International Archives of the History of Ideas 208
Archives internationales d'histoire des idées

Ofer Gal
Raz Chen-Morris *Editors*

Science in the Age of Baroque

Science in the Age of Baroque

ARCHIVES INTERNATIONALES D'HISTOIRE DES IDÉES

INTERNATIONAL ARCHIVES OF THE HISTORY OF IDEAS

208

SCIENCE IN THE AGE OF BAROQUE

Edited by

Ofer Gal • Raz Chen-Morris

For further volumes:
http://www.springer.com/series/5640

Ofer Gal • Raz Chen-Morris
Editors

Science in the Age of Baroque

Springer

Editors
Ofer Gal
Unit for History and Philosophy of Science
University of Sydney
Australia

Raz Chen-Morris
Science, Technology and Society
Graduate Program
Bar Ilan University
Israel

ISSN 0066-6610
ISBN 978-94-007-4806-4 ISBN 978-94-007-4807-1 (eBook)
DOI 10.1007/978-94-007-4807-1
Springer Dordrecht Heidelberg New York London

Library of Congress Control Number: 2012953369

© Springer Science+Business Media Dordrecht 2013
This work is subject to copyright. All rights are reserved by the Publisher, whether the whole or part of the material is concerned, specifically the rights of translation, reprinting, reuse of illustrations, recitation, broadcasting, reproduction on microfilms or in any other physical way, and transmission or information storage and retrieval, electronic adaptation, computer software, or by similar or dissimilar methodology now known or hereafter developed. Exempted from this legal reservation are brief excerpts in connection with reviews or scholarly analysis or material supplied specifically for the purpose of being entered and executed on a computer system, for exclusive use by the purchaser of the work. Duplication of this publication or parts thereof is permitted only under the provisions of the Copyright Law of the Publisher's location, in its current version, and permission for use must always be obtained from Springer. Permissions for use may be obtained through RightsLink at the Copyright Clearance Center. Violations are liable to prosecution under the respective Copyright Law.
The use of general descriptive names, registered names, trademarks, service marks, etc. in this publication does not imply, even in the absence of a specific statement, that such names are exempt from the relevant protective laws and regulations and therefore free for general use.
While the advice and information in this book are believed to be true and accurate at the date of publication, neither the authors nor the editors nor the publisher can accept any legal responsibility for any errors or omissions that may be made. The publisher makes no warranty, express or implied, with respect to the material contained herein.

Printed on acid-free paper

Springer is part of Springer Science+Business Media (www.springer.com)

Contents

Part I Order

Part II Vision

Part III Excess

Chapter 1
Baroque Modes and the Production of Knowledge

Ofer Gal and Raz Chen-Morris

Introduction: The Great Opposition

In his *Art of Painting* (Fig. 1.1) Johannes Vermeer opens a curtain and lets the observer peek at the crafting of an image. The lavishly dressed painter, his back to the observer, is busy putting to paper the figure of Clio, the muse of history, draped in blue and holding the symbols of her art: the book and the trumpet. On the wall behind her is an elaborate map of the Netherlands (the west facing up), framed by miniature depictions of Dutch towns. Vermeer "juxtapose[s] two kinds of pictorial image" wrote Svetlana Alpers of this painting in her deservedly celebrated analysis: an image fraught with "meanings (art as emblem)" on the one hand, and on the other—an image which serves as a careful "description (art as mapping)" (Alpers 1984, 166). But Vermeer is not commenting on art alone. "The aim of Dutch painters was to capture on a surface a great range of knowledge and information about the world" (Alpers 1984, 122), and Vermeer is setting a contrast between two modes of knowledge: the theatrical, poetic, historical narrative represented by Clio; and the visual exactness and immediacy of the *descriptio*—the mathematically drawn, factual map (Alpers 1984, 119–123; 166–167).

The contrast observed by Alpers is one which we have been taught to expect. It is the opposition historians draw between the two great cultural products of the seventeenth century. On one of its hands stands the style ascribed to Vermeer and his contemporary artists:

> Forceful and occasionally forced paradox; violent contrast; reliance on sensual detail, particularly color and touch, to indicate moral condition and religious theme; deliberate

O. Gal
Unit for History and Philosophy of Science, University of Sydney, Australia
e-mail: ofer.gal@sydney.edu.au

R. Chen-Morris
Science, Technology and Society Graduate Program, Bar Ilan University, Israel
e-mail: chenraz@mail.biu.ac.il

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_1,

Fig. 1.1 Vermeer's *The art of painting*

> distortion of regular structures to produce the asymmetric effect of baroque art; and unity of thought more dependent on imagery than on logic. (White et al. 1971, 391)[1]

On the other hand of the contrast stands *The Formation of the Modern Scientific Attitude* (Hall 1962), which Vermeer represents by the map, with its

> rigorous standards in observing and experimenting. By insisting that it deals only with material entities in nature, it excludes spirits and occult powers from its province. It distinguishes firmly between theories confirmed by multiple evidence, tentative hypotheses and unsupported speculations. It presents … a picture of nature … in which all available facts are given their logical, orderly places. (Hall 1962, xi)

The juxtaposition is striking. Seventeenth century art is supposedly 'sensual', 'distorted' and 'paradoxical'; seventeenth century budding science is 'rigorous,' 'orderly' and 'logical'. The two primary cultural movements of the period are neatly arranged in exactly symmetrical opposition.

But as Alpers also notices, for Vermeer there is no opposition. The route by which he leads the spectator's eye conveys, rather, an entanglement of the various modes of representation. It is a smooth progression from under the drawn stage-curtain;

by the drapes, mask, open book and sketchbook on the table; behind the painter's back; through the depiction of Clio's laurel, just appearing on the canvas; back to the person of Clio, with her withdrawn eyes; and finally to the map. The map itself is not only a precise *descriptio*, but also a highly ornamented object of beauty, fraught with historical allusions. Clio, on the other hand, does not tell a story: she is a model, a station in the winding, laborious effort by which a visual image is produced. The drawn painter is clearly a model himself, representing Vermeer in his best garb, not the working clothes he would have worn for the occasion.

It is not a progression that Vermeer depicts, but a tense coexistence. He shares the urge to shed masks and representations and reach at the simple truth beyond them, he admires the success that the map represents, and he is keenly aware of the lures and dangers of masks and fables. But he is not willing to forgo "sensual detail;" the "scientific attitude" remains an integral part of his "imagery." For Vermeer, as Alpers put it, "observation is in fact … inseparable from craft" (Alpers 1984, 167): customs, emblems and masquerade always accompany 'facts'.

Vermeer represents the smooth cultural continuum between the peculiar visual sensitivities which historians name 'Baroque' and the intellectual fashioning they recognize in the emergence of early modern science. The emblematic practices of the 'New Science'—mathematical theorization; observation employing high-power instruments; meticulous empirical inquiry—all were intrinsically embedded in the rich tapestries of Baroque "forceful … paradoxes." Operating a telescope, drawing a geometrical depiction of physical motion or carefully collecting natural specimens was no less 'Baroque' than the hyperbolic metaphors in literature or dramatic light and shadow play in the visual arts. The late Fernand Hallyn noted that the early phases of the New Science adopted late mannerist irony as the poetical mode of structuring its world view (Hallyn 1993, esp. 152–61). As the papers collected here demonstrate, his insight could be much more generally applied: much of the new ways of producing knowledge about the natural world in the seventeenth century was fundamentally 'Baroque'.

The Papers[2]: Shades of Baroque

The essays collected in this volume attempt to make sense of the 'Baroque Science' couplet; to understand the emergence of the New Science during the seventeenth century as an integral part of the high culture of the period. They explore themes common to the new practices of knowledge production and the rapidly changing culture surrounding them, as well as the obsessions, anxieties and aspirations they share, such as the foundations of order, the power and peril of mediation and the conflation of the natural and the artificial. The essays also take on the historiographical questions involved: the characterization of culture in general and culture of knowledge in particular; the use of generalizations like 'Baroque' and the status of such categories; and the role of these in untangling the historical complexities of the tumultuous seventeenth century.

The first section directly engages with the historiographical issues. John Schuster studies the place of scientific inquiry within Baroque culture not as a question of style but of structure. Following Maraval's historiography and Bourdiue's cultural analysis he redraws the scientific revolution as part of the general crisis of the seventeenth century and presents the evolvement of the new natural philosophy integrating physical causal accounts with mathematical rigor as "a crisis within a crisis." The dynamics of this intellectual process, he argues, are internal to developments within late Renaissance philosophy of nature, yet the rhythm of the crisis and the terms with which the actors analyze the nature of the crisis and its implications are taken from the general Baroque ambience.

Koen Vermeir elects to explore the notion of 'Baroque Science' through the very concept of Baroque Style that Schuster eschewed. Examining Athanasius Kircher's experimentation with a sunflower clock through this prism, he brings to life the complex of experimental attitude, magnetic natural philosophy, natural magic and spiritual speculation which Kircher embedded in this wondrous device. The Baroque perspective allows Vermeir to explore the extraordinary realm which the sunflower clock occupies, between the artificial and the natural, operating invisible spiritual forces and material mechanisms simultaneously.

Vermeir and Schuster take important strides towards understanding the New Science as rooted within the surrounding Baroque culture, and at the same time shed new light on this very culture, now understood as the breeding grounds of the New Science. Ofer Gal's paper takes an even bolder step in this direction, turning his attention from the interplay between general high culture and its sanctioned forms of knowledge to the very core of the New Science and its canonical heroes, Kepler and Newton. Analyzing the development of the inverse square law of universal gravitation, the paradigmatic 'mathematical law of nature', Gal shows how the aspirations to unearth the simple harmonies underlying and determining the operations of the world machine, which this law represented to Kepler, were gradually frustrated and abandoned. For Newton, the inverse square law became a tool of approximation, rooted in and gaining its authority from human practice: the price of the thorough mathematization of nature was relinquishing the certainty and perfection that mathematical knowledge was expected to provide. It is this complex of anxieties, tensions and compromises that Gal terms 'Baroque'.

Like Schuster's, Gal's concept of Baroque is not that of a style, at least not in the sense one may find in Wölfflin's idealistic "history of vision" (Wölfflin 1950, e.g. 12). It is not a predicate of a final product (be it a painting or a mathematical equation), but a remark on the intellectual motivations and strivings which such a product embeds and their place in the wider cultural arena. Inquiring into Baroque modes of vision, the papers in the second section similarly look at sensitivities and practices rather than completed artistic or scientific artifacts. In this, their approach is reminiscent of Ludwik Fleck's notion of "*thought* style."[3] Fleck's concept is very much a visual one; it a "readiness for directed perception" (Fleck 1979, 92) and for Raz Chen-Morris this "readiness" of the New Science is Baroque in its conscious, if often uncomfortable, conflation of the boundaries between imaginary and empirical. Chen-Morris brings this conflation to light in a study of the emerging paradoxical awareness that "nothing," i.e., intangible and invisible entities, can be manipulated and utilized as a cornerstone of a new natural philosophy. Around this disturbing awareness new practices of observation were molded, allowing shadows and artificially produced images to take precedence over direct experience of solid material bodies as the basis for empirical knowledge.

The deep indebtedness of the new observational practices to Baroque visual sensitivities is picked up by Paula Findlen, who turns from the paradox of nature's shadows to the wonder of nature's drawing. Findlen follows Agostino Scilla's attempt to make scientific sense of fossils by, as she puts it, "replicat[ing] nature with a painter's eye." Scilla expected the project of minute visual depiction to produce a causal account of fossils' formation in stone, and his easy transgression of the epistemological boundaries between science and art were founded on a Baroque metaphysical conviction: fossils were both natural physical objects and representations of divine art.

The conflation between nature and art at the heart of the Baroque empirical project receives more attention in J.B. Shank's analysis of Torricelli's celebrated barometer. To be established as a natural possibility, a vacuum had to be *seen*, but in order to be presented visually it had to be created as an artificial *fantasia*. Shank also offers another way to think about the moniker 'Baroque': as designating not the New Science itself but the historical attempt to look at it from a new perspective, liberated from the never-relinquished epic of progress. The 'Baroque' perspective, he claims, will allow one to see Torricelli's instrument not as an emblem of science to come but as an authentic representative of its own time and place.

Alan Salter moves from the Galilean instrumental empiricism to the hands-on medical tradition and examines a different conflation in the epistemology of William Harvey. Reading through the forced Aristotelian rhetoric of Harvey's published work, Salter follows Harvey's novel visual epistemology, fashioned on artisanal practices and blurring the demarcating lines between scholarly and practical medicine. In the immediate, intuitive visual acquaintance that the shepherd and the midwife develop with the object of their knowledge through experience and repetition, Harvey finds a model of reliable empirical knowledge, free from the distortions of categories and philosophical systems. Just as Shank's Torricelli answers to the challenges of his surrounding court and scientific milieu rather than to the shape of things to come, Salter's Harvey is a conscious and sophisticated user of the cultural resources of his household and his trade rather than a blind follower of an ancient tradition. Their complex, tense engagement with visual epistemology identifies Harvey, Torricelli and Galileo as sharing the cultural challenges and resources we have termed 'Baroque'.

The last section concentrates on the variety and the excess which absorbed and exasperated the practitioners of the Baroque empirical project. John Gascoigne's study of Iberian natural history of the 'new world' brings to light one major source of that "fascinating but unruly detail." The Americas and far reaches of Africa and Asia supplied Europeans with such a wealth of rare and bizarre fauna and flora that it not only challenged traditional natural history, but threatened the Baconian project of observation and induction by "sheer scale and number." The common notion of the mathematically-dominated 'scientific revolution', Gascoigne argues, fails to capture the all-important role that these riddles and their attempted solutions played in the formation of the New Science.

One attempt at controlling the disturbing human and natural variety of seventeenth century new global expanse was the search for a universal geographical measure, which Nicholas Dew narrates and examines. It is the conflation of “political, theological, and humanistic concerns” going into the scholarly conception of measurement that Dew designates ‘Baroque’. His account of the cultural and scientific negotiations that shaped the metrological project of the Académie Française reveals again the crucial Baroque conflation between nature and art at the heart of the New Science: for seventeenth century savants, manmade pendulums and bee-made honeycombs shared the same ontological space and competed for the same epistemic roles.

The Académie is also the site of Victor Boantza’s contribution. In his account, the Baroque anxiety over—and fascination with—variety and detail is set off by the “irreducible vastness of the realm of chymical phenomena.”

For the French chymist Samuel Duclos, Boantza shows, Robert Boyle’s corpuscular, physically-based chemistry was not advancement but a betrayal of “what constituted distinctly chymical knowledge and the ways of its production.” Using Bernard Fontenelle’s analysis of the debate in a surprising way, Boantza reverses the customary roles of his heroes. For Duclos, he argues, it was *Boyle’s* “physico-chymical reconciliation [which appeared] discordant and paradoxical, an incongruous Baroque middle-ground” between the precision, simplicity and openness Boyle avowed, and the speculative theorizing and secrecy he practiced.

Finally, Rivka Feldhay studies the most emblematic of the Baroque gestures of the New Science: Galileo’s interpretation of his telescopic observations. Mobilizing Louis Marin’s work on the discourse of representation around the King’s portrait and Walter Benjamin’s analysis of Baroque theatre, Feldhay shows how Galileo employed “Baroque forms of representation and allegory” to bridge “the gap between ‘seem’ and be’.” Reaching for similar allegorical means of signification in his inquisition trial, Galileo found himself immersed in the Baroque mode of “honest dissimulation.” For the seventeenth century *savant*, allegory and mediated representation were indeed an indispensable way of scholarly life.

Conclusion: Dilemmas and Anxieties

The coupling of *Baroque* and *Science* defies both the still-triumphalist historiographies of the *Scientific Revolution* and the slight embarrassment that the *Baroque* represents for most cultural-national histories of Western Europe. It signals a methodological interest in intellectual tensions, the compromises they necessitate and the anxieties they cause, rather than in self-affirming narratives of success and failure. It provides an opportunity for reflective critique of our historical categories which is valuable in its own right.

As the papers demonstrate, however, there are also very particular insights to be gained from replacing the epic oppositions—between the decadent and the progressive; the rigorous and the playful; the orderly and the contorted—with an open-ended analysis of the dilemmas that the New Science shared with, and imposed on, the high culture of its time. Relieved of the myth and considered together, as obviously-related aspects of the same era in the history of European culture, the two sides of the couplet shed revealing light on each other.

For us, the editors, the appeal of the phrase 'Baroque Science' lies especially in capturing the powerful tensions expressed by masters of the Baroque like Vermeer (as well as Rubens and Bruegel, Bettini and de Gheyn, with whom we deal in another place); tensions which are crucial, first and foremost, for understanding the evolution of early modern science. With the unavoidable risk of over-simplification, we can summarize these as three inter-related paradoxes driving, rather than hampering, "The Formation of the Modern Scientific Attitude:"

- The empiricism of the New Science originated, and is still remembered, as the demand that knowledge be based on sense experience; a corollary of Baroque fascination with the particular, the detailed and the sensual. From late sixteenth century on, however, empirical practices turned dramatically *away* from the trust in the acquisition of knowledge through the senses. In its stead, early modern empirical inquiry developed a growing reliance on the mediation of artificial instruments: lenses and screens for observation; mechanical and pneumatic devices for experimentation. Moreover, the champions of instrumental empiricism justified the mediation of instruments by rejecting the immediacy of the senses themselves. The price of the great empirical prowess of the New Science was thus the Baroque paradox that troubled Vermeer and his contemporaries: the recognition that all empirical knowledge is fundamentally mediated; that nature could only be approached by art.[4]
- This reversal of the relations between the natural and the artificial, a familiar Baroque theme, was reflected also in the other grand achievement of the New Science: its physical-mathematical ordering of heavens and earth. Mathematics—the science of harmonies and perfect structures—was employed with the hope of deciphering God's perfect design for the world. However, the mathematical techniques and procedures that allowed the success of mathematized natural philosophy turned increasingly obscure and artificial, and in place of divine harmonies they revealed an assemblage of isolated, contingent laws and constants. Vermeer's ornate map expressed an idea the new savants came to acknowledge: that mathematical order was manmade and enforced.[5]
- Essentially mediated and brazenly wrought, the knowledge provided by the New Science, with all its marvelous success, could no longer lay claim to direct acquaintance with the objects of nature. In their stead, it produced its own objects: distant stars; infinitesimal magnitudes; the spring of air and the collision of particles. This, for the Baroque savant, was perhaps the most baffling paradox of all: objective knowledge relied on the mind's creative, '*poetic*', engagement, or in other words—on the imagination. But the imagination, the faculty of images and phantasms, has always been the source of error and delusion, driving the passions to melancholy and from there—to madness. The theories of the passions sprouting from the middle of the seventeenth century express the attempt to resolve this

> dilemma with a paradoxical reversal of the order of knowledge: the assurance that *reason*, detached from material nature and dependent on the imagination, does not lead us completely astray, had to be entrusted with the orderly functioning of the passions, which direct the human *body* through the vicissitudes of nature and are sanctioned by its survival.[6]

These are, indeed, "Forceful … paradoxes" and "violent contrasts," but by no means should they be taken as decadent pastimes of a declining culture. They represent real dilemmas, and their attempted resolutions, of a culture of knowledge vigorously re-shaping itself. Nor were the major difficulties in establishing the dramatically novel modes of knowledge, or the anxieties they gave rise to, a ferment of skepticism. Rather, they were dynamic engines of change, domesticated but still present as an active challenge to the ambitious and self-confident systems of knowledge of the self-declared 'enlightenment'. Enforcing order in the face of threatening chaos, blurring the boundaries between natural and artificial and mobilizing passions in the service of objective knowledge, is our contention, the New Science is a Baroque phenomenon.[7]

Notes

1. For classical treatments of Baroque in literature and plastic arts see: Wölfflin 1950; Panofsky 1995; Wellek 1963; Maravall 1986; Croce 1990. Rare examples of applying Baroque to science can be found in: Sigerist 1929, 148 and Collingwood 1945, 4; in music history and theory the term 'Baroque' came to refer to a later period. In recent years Walter Benjamin's notion of Baroque culture as a crucial element in the formation of modernity received much theoretical attention: *cf*. Benjamin 1963; Buci-Glucksmann 1994; Bal 2001; and Grootenboer 2005.
2. The essays collected here stem from a workshop held in Sydney on February 2008 and sponsored by the Australian Research Council grant DP0664046: *The imperfection of the Universe: Music, Mathematics, Technology and the Order of Nature in Baroque Science*.
3. For more recent discussions of styles of thought see: Crombie 1994; Hacking 1982, 1992a, b; and Schweber and Wachter 2000.
4. *C.f.* Gal and Chen-Morris 2010a, 2010b, 2011.
5. *C.f.*Gal and Chen-Morris 2005, 2006 and 2012.
6. *C.f.*Gal and Chen-Morris 2012, Chapter 7.
7. *C.f.*Gal and Chen-Morris 2012.

References

Alpers, Svetlana. 1984. *The art of describing: Dutch art in the seventeenth century*. Chicago: The University of Chicago Press.

Bal, Mieke. 2001. *Quoting Caravaggio: Contemporary art, preposterous history*. Chicago: University Of Chicago Press.

Benjamin, Walter. 1963. *Ursprung des deutschen Trauerspiels*. Frankfurt: Suhrkamp Verlag.

Buci-Glucksmann, Christine. 1994. *Baroque reason: The aesthetic of modernity* (trans: Camiller, P.; intr: B.S. Turner). London: Sage Publications.

Collingwood, R.G. 1945. *The idea of nature.* Oxford: Clarendon Press.

Croce, Benedetto. 1990. *Benedetto croce: Essays on literature and literary criticism* (trans. and ann: Moss, M.E.). Albany: State University of New York Press.

Crombie, Alistair C. 1994. *Styles of scientific thinking in the European tradition*. London: Duckworth.

Fleck, Ludwik. 1979. *Genesis and development of a scientific fact* (trans: Bradley, F. and T.J. Trenn), ed. T.S. Kuhn and R.K. Merton. Chicago/London: The University of Chicago Press.

Gal, Ofer, and Raz Chen-Morris. 2005. Metaphysical images and mathematical practices: The archaeology of the inverse square law part I. *History of Science* xliii: 391–414.

Gal, Ofer, and Raz Chen-Morris. 2006. The use and non-use of mathematics: The archaeology of the inverse square law part II. *History of Science* xliv: 49–68.

Gal, Ofer, and Raz Chen-Morris. 2010a. Baroque optics and the disappearance of the observer. *Journal of History of Ideas* 71(2): 191–217.

Gal, Ofer, and Raz Chen-Morris. 2010b. Empiricism without the senses: How the instrument replaced the eye. In *The body as object and instrument of knowledge*, ed. O. Gal and C. Wolfe, 121–148. New York: Springer Verlag.

Gal, Ofer, and Raz Chen-Morris. 2011. The controversy over the comets: What was it really about? In *Controversies in the scientific revolution*, ed. Victor Boantza and Marcelo Dascal, 33–52. Amsterdam: John Benjamins.

Gal, Ofer, and Raz Chen-Morris. 2012. *Baroque science.* Chicago: Chicago University Press.

Gal, Ofer, and Raz Chen-Morris. 2012. Nature's drawing. *Synthese* 185(3): 429–466.

Grootenboer, Hanneke. 2005. *The rhetoric of perspective: Realism and illusionism in seventeenth-century Dutch still-life painting.* Chicago: The University of Chicago Press.

Hacking, Ian. 1982. Language, truth and reason. In *Rationality and relativism,* ed. M. Hollis and S. Lukes, 48–66. Oxford: Blackwell: [Enlarged in: Styles of scientific reasoning, In *Post-analytic philosophy*, ed. J. Rajchmann, and West C, 146–165. New York: Columbia University Press, 1985].

Hacking, Ian. 1992a. 'Style' for historians and philosophers. S*tudies in History and Philosophy of Science Part A* 23(1): 1–20.

Hacking, Ian. 1992b. Statistical language, statistical truth and statistical reason: The self-authentification of a style of scientific reasoning. In *The social dimensions of science*, ed.E. McMullin, 130–157. Notre Dame: University of Notre Dame Press.

Hall, A.R. 1962 (2nd ed.). *The scientific revolution 1500–1800: The formation of the modern scientific attitude*. London: Longman.

Hallyn, Fernand. 1993. *The poetic structure of the world: Copernicus and Kepler*. New York: Zone Books.

Maravall, José Antonio. 1986. *The culture of the baroque: Analysis of a historical structure* (trans: Cochran, Terry). Minneapolis: University of Minnesota Press.

Panofsky, Erwin. 1995. What is baroque? In *Three essays on criticism*, ed. Irving Lavin, 17–90. Cambridge: MIT Press.

Schweber S., and M. Wachter. 2000. Complex systems, modelling and simulation. *Studies in History and Philosophy of Modern Physics* 31(4): 583–609.

Sigerist, Henry E. 1929. William Harvey's Stellung in der Europaeischer Geistesgeschichte. *Archives der Kulturgeschichte* 19: 158–68.

Wellek, René. 1963. The concept of baroque in literary scholarship. In *Concepts of criticism*, 69–127. New Haven: Yale University Press.

White, Helen C., et al. 1971. *Seventeenth-century verse and prose*, vol. 1. New York: The Macmillan Company.

Wölfflin, Heinrich. 1950. *Principles of art history: The problem of the development of style in later art*. Mineola: Dover Books.

Part I
Order

Chapter 2
What Was the Relation of Baroque Culture to the Trajectory of Early Modern Natural Philosophy?

John A. Schuster

Abstract This paper attempts to answer the question posed in its title, by focusing attention on the institution and contested field of discourse of natural philosophy, and its processes of change in the early and mid Seventeenth century. Following the seminal work of José Antonio Maravall, Baroque culture is taken as a set of concerted responses to a wide religio-political crisis. The paper then argues that this period saw a veritable 'crisis within a crisis' occurring in natural philosophy and its cognate and subordinate disciplines, with recruitment of 'Baroque' aims, styles and rules of contestation into natural philosophy by competing players. It is also suggested that some of these Baroque 'cultural genes' survived in the subsequent history of natural philosophy, and thence, following its disintegration, into the social dynamics of the emergent modern sciences, shaping their agonistic natures.

Introduction: Thinking About "Baroque Science"

There are three broad options available for approaching the question, "What, if anything, were the relations between the Baroque and (the process of) the Scientific Revolution?" (1) With historians of fine art, music, literature and architecture, one could define Baroque style and trace its expression across creative domains including natural philosophy and the sciences. (2) With social and political historians in the manner of, say, Carl Friedrich, one could delineate a social or cultural period as Baroque, so that concrete relations might be delineated between this culture and contemporary natural philosophising.[1] The idea would be that in a culture or social formation more than artistic expression is at stake—for example, forms and norms of social interaction, resources for self-understanding and public expression by

---

J.A. Schuster (✉)
Campion College and University of Sydney, Sydney, NSW, Australia
e-mail: john.schuster@sydney.edu.au

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_2,

actors, organisational forms and styles, any of which might affect natural philosophy and its subordinate sciences. (3) One could try to show that there is something importantly Baroque about the end product[s] of the Scientific Revolution, in modern science as a whole, perhaps.

It is fair to say that the Baroque Science Project—as conceived by the Editors of this volume and exemplified in their Introduction and respective substantive contributions—offers novel findings along all three of the above options (provided that one does not see option (1) in terms of surface stylistic analogies, but rather in terms of commonalities at deeper epistemic and cultural grammar levels). This paper is more limited in scope. It explores possibilities in option (2), based on the belief that the Scientific Revolution was a complex process of change played out within the domain of natural philosophy and its subordinate sciences. Hence, whatever one might mean by Baroque 'science' or a Baroque shaping or influence on science needs to be found by exploring the structure and dynamics of natural philosophising, whilst simultaneously exploring what was Baroque culture. This also means that this paper concerns a much shorter time segment than the Editors take into view. It deals with the second and third generations of the Seventeenth century, where many scholars locate both the climax of Baroque culture and a moment of intense competition and turbulence within the realm of European natural philosophising—the problem being precisely what relations, if any, existed between the two. (Nevertheless, in the penultimate section of this paper we shall also uncover a possible larger meaning for Baroque science, along the lines of option (3), but only after the main argument has been set out.)

It should also be noted at the outset that this paper is experimental. It was conceived and written in response to the challenge and problematic of the Baroque Science Project, and, as will be seen, its structure and argument depend upon accepting and articulating further certain conceptions current in parts of the literatures on the Scientific Revolution and general Seventeenth century intellectual history. I take this paper as a beginning and a spur to myself and others, not as a definitive statement. That is the spirit in which earlier versions were presented at conferences and workshops of the Baroque Science Project variously held under the auspices of the Editors at the University of Sydney between 2006 and 2009, and that is the manner in which drafts of the paper have been read and constructively criticised by participants therein, and by other colleagues.

In keeping with its exploratory tenor, the first thing question needing to be addressed about the structure of this paper is this: How to deal with existing conceptions of Baroque culture and that old chestnut, a 'general crisis of the Seventeenth Century'? This is because attempts to speak about a Baroque cultural or social epoch are easily conflated with allusions to a 'general crisis'. Part of the problem is that social historical discussions of the Baroque, such as Carl Friedrich's, do not address the issue of crisis on a sustained historiographical level, whilst crisis theorists use evidence about Baroque art, but do not deal with the Baroque as a social or cultural formation.[2] However, there is a way of working with a conception of Baroque culture, properly related to the turbulence we denominate by the term 'general crisis of the seventeenth century', and done in such a way that both issues can be related to the structure and dynamics of natural philosophy. The result may be some progress on the question, ‘*Is there any relation between* ‘*the Baroque*’ *and (the process of) the* ‘*Scientific Revolution?*’ For the sake of the experimental historiographical argument of this paper, our starting point is the profound work of José Antonio Maravall, in his *Culture of the Baroque: Analysis of a Historical Structure*.[3]

Maravall interprets Baroque culture as a largely deliberate program of culture, put forward, in slightly differing fashions in different states, by elite blocks of monarchical and aristocratic interests who perceived threats to status, social hierarchy, social order and religious orthodoxy from mainly urban ‘middling’ classes and groups, exercising resistance to political and religious centralisation and in favour of their own interests. This occurred under conditions, emergent in the later sixteenth century and heightening in the next, of urban population expansion and more readily available new communication technology, which helped create more acute and transmissible senses of both problems and of their possible subjection to human solution.[4] These and other phenomena he nominally packaged under the shorthand label ‘general crisis’. Maravall is interested in what went into this manufactured, commodified culture product, and also in the lived experience of people born into/living in it, because, once it was widely established, of course, individuals lived and experienced their world through its forms and categories.

Thus, Maravall relates his interpretation of the Baroque to the idea of a general crisis of the seventeenth century, by making the former the overall unintended result of concatenations of particular, intentional elite responses to, or perceptions of, the latter. From the analyst’s standpoint, Baroque culture for Maravall is what he calls an “historical structure”, by which he means, a constructed interpretive framework taken to have real reference, which coordinates, explains and takes account of the relevant known phenomena and manifestations, and whose own trajectory is in turn historically explicable. At no point does Maravall claim that the general crisis of the seventeenth century is a similar sort of ‘structure’ He is saying that the best we can do is take appropriate and well documented types of turbulence and conflict in the period, the ones most likely taken notice of by Baroque actors, and *label* them the general crisis.[5]

Following Maravall’s strategy, I will relate my interpretation of natural philosophy and the process of the scientific revolution to his interpretation of Baroque culture and the general crisis. I shall deal with natural philosophy as a model or “historical structure”; and, like Maravall, I shall *denote* turbulent, worrying phenomena by the term general crisis. In the case of natural philosophy, however, the term ‘crisis’ would denote a specified and evidentially supported phase or state of play in this specific sub-culture. Exploring the structure and dynamics of early modern natural philosophising and its attendant more narrow sciences, we find that the so-called Scientific Revolution falls into several stages. One of these, in the early to mid seventeenth century, had a particularly turbulent, ‘crisis-like complexion’. Because this turbulent or critical period took place well within the space and time of Maravall’s Baroque crisis, this phase of natural philosophising constituted a ‘crisis within a crisis’.

Thus, I aim to show by articulation of Maravall's approach, that there was an early to mid seventeenth century watershed crisis of natural philosophy inside the more general crisis. Natural philosophy did not become Baroque in any simple or straightforward sense. The impacts of the Baroque on natural philosophising; or, better put, the recruitments of Baroque culture into natural philosophising, will be approached by examining how the sub-culture or field of natural philosophising worked, and how its structure and dynamics led to, and were inflected by, this crisis phase in its own history. To these ends, we shall analyse the structure and dynamics of natural philosophy in section "Constructing the Category of Natural Philosophy—Natural Philosophising as Culture and Process", followed in section "Phases and Stages in the 'Scientific Revolution' Seen as an Unfolding Process in the Field of Natural Philosophising" by a periodisation of the Scientific Revolution into stages in the evolution of natural philosophising and its attendant narrower, subordinate sciences. Then section "The Dynamics and Rules of Natural Philosophical Contestation During the 'Crisis Within a Crisis' Phase" will deal with the peculiarly turbulent and contested nature of natural philosophy in the crisis period, leading to our exploration in section "Recruitment of Baroque Behaviours, Norms and Identities?" of appropriations of Baroque culture into natural philosophising during that period. Finally, in section "An Additional, Surprising, Conjectural Finding", a further discovery will arise concerning the themes of this paper.

Constructing the Category of Natural Philosophy—Natural Philosophising as Culture and Process

To inquire about the 'Scientific Revolution' and 'the Baroque', we need both to formulate our key categories and to design a workable periodisation concerning the trajectories traced by the entities and processes those categories arguably denote.

In the early modern period the central discipline for the study of nature was natural philosophy.[6] In the first instance natural philosophy is an actor's term, but, if we metaphorically treat natural philosophy as an *iceberg*, actors' usages are merely the tip. We must also theorise the bottom of the iceberg, by modelling the structure and dynamics of the game of natural philosophising, including points that did not or could not have been known to the players. So, I model Early modern natural philosophy as a dynamic, elite sub-culture and field of contestation, theorising about its structure, dynamics and its process over time.[7]

When one 'Natural philosophised' one tried systematically to explain the nature of matter, the cosmological structuring of that matter, the principles of causation and the methodology for acquiring or justifying such natural knowledge (Fig. 2.1).[8] The dominant genus of natural philosophy was, of course, Aristotelianism in various neo-Scholastic species, but the term applied to alternatives of the various competing genera: neo-Platonic, Chemical, Magnetic, mechanistic or, later, Newtonian. Early modern natural philosophers learnt the rules for natural philosophising at university whilst studying hegemonic neo-Scholastic Aristotelianism. Even alternative systems

The field, sub-culture or tradition of Natural Philosophy

Nature is a coherent unity, to be studied systematically under the dimensions of 1.matter 2.cosmos 3. causation 4. method

- Hegemonic Scholastic Aristotelianism[s]

Challenger Genera:

- *Neo-Platonic varieties, high tide 1580-1620*
- *Mechanistic varieties, high tide 1640-80*
- *Newtonianism[s] from 1690s*

Fig. 2.1 Natural philosophy—generic structure, competing genera

followed the rules of this game. All natural philosophers and natural philosophies constituted one sub-culture in dynamic process over time.

We should not simply equate “natural philosophy” to Scholastic Aristotelianism. Nor should we accept that after about 1660 natural philosophy died and was replaced by an essentially different activity, Science.[9] At its climax in the early and mid seventeenth century—during the ‘crisis within a crisis’—the Scientific Revolution was a set of transformations, a civil war, inside the seething, contested culture of natural philosophising. That culture continued to evolve under internal contestation, and external drivers, and variously elided and fragmented into more modern looking, science-like, disciplines and domains, plural, over a period of 150 years from 1650.[10]

That there was a European culture of natural philosophising depended upon a High Medieval development of world historical import—the establishment of a European system of universities all teaching variants of a Christianised Aristotelian corpus in logic and natural philosophy.[11] This fact continued and evolved into the early and mid seventeenth century.[12] Although specific concepts constitutive of Scholastic Aristotelianism were displaced during the seventeenth century, this occurred inside the continuing, contested life of the larger field or tradition of natural philosophising.

A Scholastic Aristotelian education taught that nature has a coherent, systematic unity; that nature not only can be studied by specific means but that correspondingly systematic knowledge of it can be obtained.[13] This template for natural philosophising also applied to all jostling species of natural philosophical challengers to Aristotelianism. Additionally, Scholastic Aristotelianism framed the way in which other disciplines were conceived, and related to each other, and to natural philosophising. The positioning of natural philosophical claims in relation to other enterprises always involved two routine manoeuvres: the drawing and re-drawing of boundaries and the making of linkages.[14] This created the ‘objective field of possible moves’ in which natural philosophers carried out their own specific systematising and linking strategies—claiming new linkages or defending older ones—depending upon their respective aims and skills.[15]

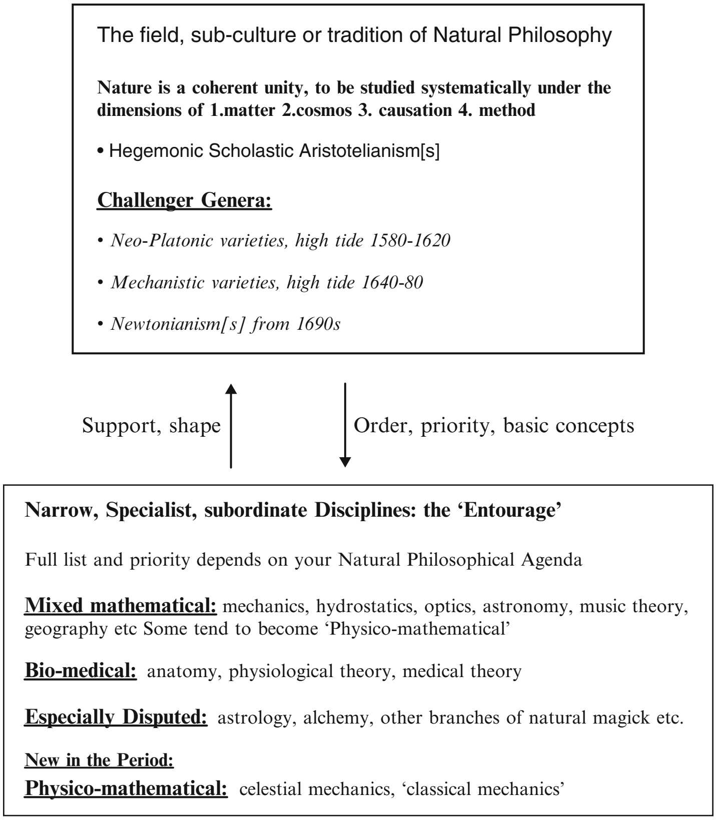


Fig. 2.2 Generic structure of natural philosophy and possible entourage of sub-ordinate fields

One may think of the subordinate disciplines as an *entourage* of more narrow traditions of science-like practice (Fig. 2.2): These included the subordinate mixed mathematical sciences, as well as the bio-medical domains, such as anatomy, medical theorising and proto-physiology in the manner of Galen. The members of this entourage changed over time. In the seventeenth century, some were disputed; some were created; all changed; new or revamped entourage members evolved.

In a given system of natural philosophy: (1) the particular entourage of subordinate disciplines lends support to and can even shape the system; while (2) the system determines the selection of and priority amongst entourage members, and imposes core concepts deployed within them.

Limitations of space prevent my detailing the five elements of theorising making up my model of the structure and dynamics of natural philosophy.[16] However, one dimension of the model, dealing with natural philosophy as a *dynamic and evolving sub-culture* needs to be mentioned. To explicate this notion, I invoke Marshall Sahlins' way of analysing cultures as dynamic historical entities in terms of their mechanisms of change and adaptation over time to exogenous and endogenous challenges. Developing an historical category of culture in anthropology, he argues that cultures display specificity of response to outside impingement; they are not simply imprinted upon or pushed around. The dynamics of response, over time, characterises the culture.[17]

Similarly, my model of natural philosophising includes conceptualising it as a sub-culture, tradition or field in dynamic process—defined over time by the resultant of its players' combats over claims, where some claims involve attempts to respond culturally to variously perceived, and represented, contextual structures and forces, threats and opportunities. These moves are not determined by a universal logic, may express considerable novelty, but remain specific to the (evolving) culture.[18]

Phases and Stages in the 'Scientific Revolution' Seen as an Unfolding Process in the Field of Natural Philosophising

Let us consider a sketch periodisation of the flow and dynamics of natural philosophising. It marks out the central plot of the Scientific Revolution and will allow us to assess its relations with the Baroque.

The periodisation categories are [19]:

1. The Scientific Renaissance, 1500–1600.
2. The Critical Period (or Natural Philosophical Crisis inside a Larger Crisis), 1590–1660.
3. The Period of Relative Consensus, Muting of Systemic Conflict, New Institutionalisation, and Incipient Fragmentation of the Field, 1660–1720. (Abbreviated as CMF Period below.)

The Scientific Renaissance displays in the subordinate sciences of the 'entourage', as well as in natural philosophy, many of the scholarly aims and practices which already characterized the treatment of classical literature, history and languages in earlier stages of the Renaissance. Established humanist practices of textual recovery, editing, translation, commentary and printing increasingly focused on the scientific, mathematical and natural philosophical heritage of classical antiquity.[20] These developments mark the first stage and essential pre-condition for the further process of the Scientific Revolution. There was a marked increase in the recovery, reconstruction and extension of the existing subordinate entourage sciences, the timing of which differed from field to field.[21] This took place amid the catalyzing influence of the pedagogical and philosophical assault on Scholastic philosophy; the reassertion of Platonising modes of thought which helped revalue mathematics as the key to knowledge; and the more general trend toward recasting the ideal of knowledge in the image of practice, use and progress, rather than contemplation, commentary and conservation.

In natural philosophy a wide and confusing array of non- or anti-Aristotelian approaches was made available through the recovery, assimilation and publication of alternatives. Outside of the universities, in princely courts, print houses and workshops of master artisans, the literate practitioner and the practical intellectual could be set at odds with School philosophy and reach for rhetorical tools against it. 'Orthodox' Scholastic Aristotelianism, however, remained central to the education of all men, and even enjoyed renewed vigor throughout the sixteenth century.

The **Critical Period (or Phase of 'Crisis within a Crisis')** of the Scientific Revolution (roughly 1590–1650) saw a conjuncture unique in the history of pursuit of natural knowledge, whether in classical antiquity, medieval Islam or Renaissance Europe: On the one hand Kepler, Galileo, and Descartes led an accelerated conceptual transformation in the subordinate entourage sciences—optics, mechanics and astronomy as well as the cognate, mathematics. On the other hand, in natural philosophy the tendencies corrosive of Aristotelianism took on greater urgency. There was heightened, often desperate competition amongst systematic natural philosophies (some tied to utopian and irenic programs of religious, social and intellectual reform), issuing in the construction and initial successful dissemination of the mechanical philosophy. The Renaissance themes of the re-evaluation of practical knowledge and the desire for domination of nature sounded now more urgently and in a new key, as figures such as Bacon and Descartes systematically assimilated them to natural philosophical programs.[22]

Out of this proliferation and climactic struggle amongst advocates of competing systems there emerged varieties of mechanical philosophy. By the mid-seventeenth century the cultural dominance of Aristotelianism collapsed (although it continued supreme in most universities for another generation). The mechanical philosophy, in several species, became the dominant genus. Hence my image of a 'civil war in natural philosophy', with multiple regime change: from Aristotelianism to mechanism, which had averted a threatened neo-Platonic take over.

This is precisely where the change in natural philosophy needs to be linked to the 'general crisis of the seventeenth century' and its heightened political, religious and intellectual turmoil. Following Maravall's strategy, we place the turbulence and contestation within the field of natural philosophising within the larger crisis. Educated men with natural philosophical interests recognised an imperative to find, and install, the 'proper' system of natural philosophy, because it was widely believed that the 'correct' program for natural knowledge would *ipso facto* provide needed support for 'correct' religion, as well as a set of directives for improvement of the moral and practical aspects of life. This powered the proliferation of alternative programs to Aristotelianism, and shaped the emergence of mechanism out of the competitive turbulence thus created. The stakes—political, moral and religious—inside the natural philosophical field were high.[23] That there was no consensus on 'correct' religion casts a poignant light on this struggle and explains its intensity, as well as, to some degree, its lack of closure.

The following **'CMF' Period** (1660–1720) is distinctive for the muting of public systematic contestation, especially in the new 'scientific' institutions; for the widespread acceptance of loosely held varieties of the mechanical philosophy; and for the endemic melding of these variants to Baconian rhetoric of method and experiment.[24] Under this new, looser umbrella of natural philosophical commitments a 'research' primacy was granted to new (rather than co-opted) experimentation. Natural philosophers found themselves doing some of their natural philosophising within the confines of new institutions in ways advantageous to them in institutional *and* natural philosophical terms. These institutions were additional nodes in the Europe wide field of natural philosophising, not the exclusive ones, and, they were not incubators of an essentially new, unified Science, replacing a natural philosophising supposedly barred from their precincts.[25]

The ironic upshot of the 'civil war in natural philosophising' was that, on the one hand, the entire field of natural philosophising became more autonomous of other cultural forms such as theology, as well as other branches of philosophy, whilst, on the other hand, it began a long process of fragmentation into a number of more modern looking, semi-autonomous, diverse and narrow special domains or disciplines of natural inquiry, which begin to look like sciences in our modern sense.

The Dynamics and Rules of Natural Philosophical Contestation During the 'Crisis Within a Crisis' Phase

Our overview of the three phases has prepared us to anatomise the types of natural philosophical contestation and competition prevalent during the critical or crisis phase of the Scientific Revolution. This will allow us to focus on how some Baroque norms, modes of behaviour and identity formations may have been recruited into the natural philosophical struggle during the 'crisis within a crisis' phase.

We begin by noting what amounts to an objective condition of the field[26]: that virtually all natural philosophical utterance, by any player, was ultimately referred back to a template initially learned through neo-Scholastic training in Aristotelian natural philosophy. Superimposed upon this in the critical period was the fact that Scholastic Aristotelianism provided the target of strategies of displacement, whilst competition amongst members of different broad genera of natural philosophising—Aristotelian, neo-Platonic, Magnetic, qualitative atomistic, and finally mechanistic—also heated up.[27] We can model the dynamics of this increased contestation, thus elucidating the rules (negotiable of course) of such engagement.

Articulation on Subordinate Disciplines: Grammar and Specific Utterance

The existence of a field of natural philosophising and its entrenched Aristotelian templates was a 'grammatical' given. However, individual natural philosophers had differing interests and skills within the *entourage* of subordinate disciplines. Each natural philosopher, even Scholastic Aristotelians, had to set priorities amongst entourage members and link them conceptually to his natural philosophy, creating a characteristic linkage pattern. The practice of a subordinate discipline under the aegis of a particular genus of natural philosophy was colored by the nature of that conceptual linkage. The upshot was, metaphorically speaking, a dialectic of grammar and particular utterance, which becomes especially interesting to examine under conditions of heightened contestation amongst the players, as in our critical period.

Consider the mixed mathematical fields, under Aristotelianism, where they were considered to be intermediate between natural philosophy and mathematics and subordinate to both.[28] For example, for Aristotelians, the investigation of the physical nature of light would fall under natural philosophising, an issue of invoking appropriate principles of matter and cause. In contrast, the mixed mathematical science of geometrical optics studied ray diagrams, in which geometrical lines represented rays of light, and phenomena such as the reflection and refraction were dealt with in a descriptive, mathematical manner, which was, according to Aristotle, incapable of providing causal explanations.[29] Thus the grammatically hegemonic Scholastic viewpoint dominated the question of the relation of mixed mathematical fields to natural philosophising. However, as the competition amongst differing approaches to natural philosophy heated up in the early seventeenth century, some natural philosophers hostile to Aristotelianism proposed a more central explanatory role for mathematics in natural philosophy, and some sophisticated Scholastic Aristotelians began to loosen the Aristotelian marginalisation of mathematics as non-explanatory. A competitive dynamics eventuated around attempts to bend the template, or ‘declaratory’, rules of subordination of the mixed sciences to Aristotelian natural philosophy.[30]

Similarly, in geometrical astronomy, the fine details and elaborate geometrical tools of Ptolemaic astronomy eluded plausible realistic interpretation, offering merely appearance-saving geometrical models, rather than natural philosophical explanations in terms of matter and cause. However, at a deeper grammatical level the fundamental concepts of Ptolemaic astronomy were shaped by Aristotelian natural philosophy: the finite earth-centered cosmos, the distinction between the celestial and the terrestrial realms, the primacy of uniform circular motion. Hence, there were some, albeit thin, linkages of a causal and matter theoretical nature that grounded Ptolemaic astronomy and linked it to its ‘parental’ Aristotelian natural philosophy. But, when Copernican astronomy became hotly debated in the later sixteenth and early seventeenth centuries, it was not as an instrumental predictive device, but as a system with realistic claims about the cosmos, implying the need for a non-Aristotelian natural philosophy, able to explain its physical workings.[31] This illustrates how the articulation of a subordinate field to one’s brand of natural philosophy involved acceptance or bending of the template Aristotelian rules, and also dictated that the discipline in question be conceptually flavored in terms of matter and cause explanations derived from that favored natural philosophy.[32]

But, there was a more radical gambit in articulating a natural philosophy to a putatively subordinate field. An entire natural philosophy could be launched, or differentiated off from a broader genus, by borrowing its core conceptual and normative resources from a now privileged more narrow discipline.[33] Articulating one’s natural philosophy to a favoured interpretation of a favoured discipline invited counter moves, because natural philosophical opponents were stimulated to co-opt and 'sanitise' (of opposing natural philosophical valencies) the domain in question. In such strategic natural philosophical battles entire subordinate disciplines and their value structures were at stake.[34]

Find or Steal Discoveries, Novelties or Facts, Including Experimental Ones

In the critical period there was competitive production of novel experiments and facts, accompanied by scrambles to co-opt and reinterpret others' claims, whether amongst nominal members of the same natural philosophical genus, or across such families. Note, first of all, that any given natural philosophy was capable of stimulating new developments—discoveries of fact, production of new instruments or experiments—conditioned and shaped by the natural philosophy in question. Aristotelians continued to contend about experimental discoveries and instruments well into the middle of the seventeenth century. The novelties in Gilbert's work heavily conditioned by, and in turn affecting the shape of, his neo-Platonic natural philosophy are well known.[35] Similarly, Kepler's, optical, astronomical and celestial mechanical discoveries were shaped by his version of a neo-Platonic philosophy of nature.

The increasing imperative to pursue novelties and embed them within one's own natural philosophical agenda did not simply involve filling cabinets of curiosities. *To be important in the history and dynamics of natural philosophising, novelties had to be pursued and coveted within and for natural philosophical purposes.* Moreover, appropriation or negation was tactical: If a discovery or claim was particularly significant in the architecture of a competing system, it had to be appropriated, down played, reinterpreted or neutralised.

For example, Harvey's ultra significant claims about the motion of the heart and blood became a target in an extended game of inter-systemic competitive football: Descartes was happy to appropriate Harvey's epochal, yet clearly Aristotelian based claims to the discovery of the circulation of the blood and motions of the heart, radically altering the latter (to the point of arguably contradicting it) to fit his mechanistic program in physiology. Within his radical Chemical natural philosophy Fludd endorsed the discovery of his friend Harvey, but invested it with mystical connotations that only aficionados of his natural philosophy could appreciate. The tactical cross fire thickened when Gassendi, a mechanist competing with Descartes, tried to refute Fludd's interpretation of the circulation, before going on to reaffirm, against Harvey (and Descartes) the Galenic pores in the septum of the heart on the basis of claimed first hand witnessing of anatomical facts! For Gassendi this *Galenic claim* vindicated the identity of venous and arterial blood, one of Harvey's central claims. Hence, with Harvey, Gassendi endorsed the 'anatomists' way' of firsthand experience, yet also preserved a key tenet of Galen, the 'physiology expert', whom both Harvey and Descartes were determined to displace.

In the critical period the players were happy to co-opt, and reinterpret, each others' claims. Symbolic capital was not assigned only to new matters of fact.[36] Borrowing and renegotiating facts and discoveries were endemic, because, and this is crucial, the contest was about systematic natural philosophical advantage, not the toting up unique, novel discoveries.[37]

Bend or Brake Aristotle's Rules About Mathematics and Natural Philosophy: The Gambit of 'Physico-mathematics'

It is often said that Kepler, Galileo, Descartes and others tried to 'mathematicise science'. These developments are better understood as products of contestation and renegotiation in one corner of the natural philosophical field, involving challenges to the dominant Aristotelian template rules about how the mixed sciences should relate to natural philosophy. This problem involves our making use of the category 'physico-mathematics', which, like natural philosophy, is both an actors' term from the time, and a category to be historiographically fleshed out. Recall the examples we have seen in section "Articulation on Subordinate Disciplines: Grammar and Specific Utterance" of attempts to articulate geometrical astronomy and optics much more closely to anti-Aristotelian natural philosophies, bringing the matter and cause dimensions of the natural philosophy into play inside the target discipline. This is what one means by players attempting to render the mixed mathematical disciplines more physico-mathematical. It is not the *mathematisation* of natural philosophy, but the *physicalisation* (tighter natural 'philosophication') of disciplines Aristotelianism held to be merely instrumental and non-explanatory.

Outcroppings of 'physico-mathematical' initiatives began to appear in the sixteenth century, for example, regarding the natural philosophical status of mechanics.[38] The heightened natural philosophical contestation of the early seventeenth century intensified the proliferation, and competition amongst, physico-mathematical gambits, a number of which can be identified.[39] Physico-mathematicians (Galileo, Kepler, Descartes, Gilbert, Mersenne and Beeckman to name the usual suspects) hostile to Aristotelianism claimed that mathematics could play an explanatory role in natural philosophy. This demanded further articulated accommodation, between their respective innovations in the mixed mathematical sciences and their respectively favoured natural philosophies.

Consider how the traditional mixed mathematical field of geometrical optics developed 'physico-mathematically' inside the natural philosophical turbulence in the early seventeenth century: In their optical work Kepler (1604) and Descartes (1637) each sought closer articulation between optical innovation on the one hand and natural philosophical explanation on the other. New natural philosophical theories of matter and cause were taken more intimately to control technical details in geometrical optics, and in turn, technical details in geometrical optics exerted pressure on the exact nature of those natural philosophical claims about matter and cause.[40] Under such pressures geometrical optics evolved into a much more 'physico-mathematical' discipline, in which innovating natural philosophers extracted natural philosophical capital out of optical work, whilst unintentionally there emerged at each turn a denser, relatively more independent domain of physico-mathematical optics—*a disciplinary area was crystallising as a function of being batted around in the natural philosophical ruck.*

Finally, it is useful to ask what it means to talk about players within the field of natural philosophy obeying, or bending 'rules'. The physico-mathematicians were rebels, but not in the sense of intending the destruction of natural philosophising, but rather attempting to renegotiate the rules of the natural philosophical game. So, by the first third of the seventeenth century, the given, template-derived rules about the status of the mixed mathematical sciences were the subject of an unprecedentedly vexed debate and a turbulent state of play.

"Hot Spots" of Articulation Contest: Additional Causes and Effects of a Field in Crisis

Just as the overall intensity and 'spatial' extent of contestation increased in the 'crisis within a crisis' phase of the Scientific Revolution, so new sites of inflammation of contestation appeared, which may be termed 'hot spots' in the field of natural philosophising. A dual process of change took place, involving, on the one hand, the target—the subordinate science, theory, instrument, novelty or discovery in question[41]—and, on the other hand, the natural philosophies contending to exploit the target. One example of a hot spot involved the claims of Harvey discussed above. Not only were they contested, and revised, by natural philosophical combatants for natural philosophical ends; but, over the next two generations, a domain of experimental physiological inquiry emerged at this site. Thus later seventeenth century English experimental natural philosophers investigated issues about 'cardiology', the functions of respiration, the blood, the lungs and the atmosphere.[42] A new, relatively autonomous domain of inquiry started to crystallise, as often happened from hot spots, although in this case it suffered a foreshortened and ultimately abortive trajectory.

The most important hot spot was located where astronomers and natural philosophers tried to articulate realist Copernicanism to natural philosophical claims. Copernicus himself, with his realist claims for his astronomical theory, had been *de facto* attempting what we can now discern as a 'physico-mathematical' move. His theory of astronomy had natural philosophical implications contradicting the prevailing Aristotelianism, and in effect demanding a systematic replacement of major components of astronomical practices and conceptual structures, although Copernicus himself offered nothing substantial along these lines. A hot spot developed in the natural philosophical field, between systematic natural philosophical theorising and the formerly relatively tame sub-ordinate mixed science of geometrical astronomy, only when some later players took Copernican realism more seriously for their own reasons and agendas.

Supporters of realist Copernicanism needed to adduce a framework of non-Aristotelian natural philosophy, a new theory of matter and cause, adequate to explaining the heliocentric cosmos. They bid to radicalise the grammar of relation between mixed mathematics and natural philosophical explanation. The entire late sixteenth and early seventeenth century debate over realist Copernicanism (culminating in Kepler's and Descartes' discourse of 'celestial physics') constituted an inflamed site within the natural philosophical field—no realist Copernicanism, no inflammation. But why be a realist Copernican unless you intend a quite radical overhaul of Aristotelian natural philosophy as such?[43] Furthermore, it was only in articulations of natural philosophy onto realist Copernicanism that the possibility of a 'physico-mathematical' astronomy arose, in the form of the emergence of a new domain, celestial mechanics, with Kepler and Descartes, [44] and the physicalisation of certain astronomical questions.[45] Not only was the old mixed mathematical science of Ptolemaic astronomy passing, but the very genus 'astronomy as mixed mathematics' was giving way to physico-mathematical problematics in astronomy and celestial mechanics.

The Mechanics of Responding to 'Outside' Challenges and Opportunities

We have focussed upon the critical or crisis phase in natural philosophising so that we can link it to the larger crisis of the age. Maravall constructed a model of Baroque culture, as a concerted elite response to the relevantly perceived aspects of the 'general crisis of the seventeenth century'. We, in turn, ask how the natural philosophical turmoil of the early and mid seventeenth century was shaped by players' responses to that same general crisis, and whether, in the process, elements of the then crystallising Baroque culture also were recruited into play in natural philosophy.

It is easy to cite testimony about the cultural, political, religious and identity desperation of the day as signs of a general crisis.[46] But, without a clear sense of how our object of study, natural philosophy, was affected by these larger circumstances, we have not illuminated the possible collateral shaping of 'natural philosophy in crisis' by the supervening Baroque culture. In modelling terms, that is, in terms of constructing historical categories and interpretative structures, this problem runs as follows: How should we think through the causal role[s] of features of a larger socio-political-religious crisis of the sort posited by Rabb and Maravall?

After two generations of development of methodological criticism from both the school of Quentin Skinner and the school of post-Kuhnian sociology of scientific knowledge, we cannot appeal to the 'influence' of ideas upon other ideas; nor can we revisit vulgar Marxism, wherein social and economic structures imprint corresponding constellations of ideas upon leading thinkers, who just happen to be cultural dopes.[47] However, our model of natural philosophising offers a solution. The way to deal with 'contextual drivers, shapers or causes of thought' is built into our model of a dynamic agonistic field or tradition, in which competing players deploy resources, and follow (or attempt to revise) rules of engagement, in order to construct claims. The modelling here follows Sahlins' conception of the historicity of cultural dynamics discussed above in section "Constructing the Category of Natural Philosophy—Natural Philosophising as Culture and Process", by extending the idea of natural philosophical players competing over articulations of their preferred natural philosophy onto subordinate fields. The players in the natural philosophical tradition responded to 'outside' or 'contextual' challenges and forces by deciding to bring them into play, inside the field, in the form of new claims, skills, material practices or values. The 'things' being brought in had to be represented by actors in appropriate form—the arguably objective existence of contextual structures and processes that we model and explain did not cause, imprint or 'influence' thoughts about natural philosophy by natural philosophers. Rather, appropriately thinkable/writable representations of things about contextual structures and features were mobilised, used, reshaped and deployed in natural philosophical claims by players of the natural philosophical game.[48] I term these actions by actors 'the articulation of natural philosophical claims upon things at the boundary of the field' and I envision the process as described by Sahlins.[49]

Hence we are now talking about the shifting ways in which players accounted, acted upon, and competed over what they took to be the boundaries of the field at any given moment.[50] There were no fixed, essential boundaries of the field of natural philosophising; no permanent, consensual actors' account of what was inside natural philosophy and what was outside: what was relevant to natural philosophising and what was not. Rather, (1) the utterances of dominant figures and groups tended to create, and recreate, a 'leading or hegemonic picture' of those boundaries and how to articulate natural philosophy onto them, whilst (2) articulation upon boundaries was an essential part of the competitive dynamics of the field. The university neo-Scholastic Aristotelians' possession of dominant institutions was crucial; but, competitors challenged the way dominant players articulated utterances to boundaries in order to [re-]define the field. The dominant utterances in the field carried a particular *selection, weighting and thematisation* of articulations on boundaries. Challengers could reorder these selections, weightings and contents, and also modify existing articulations, or bring in new ones.

For example, in the university teaching of Aristotelianism, a virtual articulation was present to whatever version of orthodox religion dominated that particular polity and university. However, the traditional exclusion of discussion of theology in the undergraduate course meant that this articulation was tacit, not thematised in the body of undergraduate natural philosophical teaching. In effect a rule existed about not explicitly articulating natural philosophy to theology from the natural philosophers' side of the fence. But, competing utterances from non- and anti-Aristotelian challengers could mobilise explicit and deeply developed articulations onto religion. To bring in religion explicitly involved devising new utterances, new articulations in depth and degree of thematisation in accord with favoured religious and theological commitments, claims and agendas.[51] This gives a more precise meaning to a Maravall-type formula that '*some natural philosophers responded to a perceived crisis with cultural moves inside natural philosophy*'.[52] Contending players, with differing agendas, were always making out the boundaries and relations of the field, by articulating utterances upon (their selection and weighting of) boundary structures and discourses.

If outside entities and forces seemed to some to be particularly threatening and challenging (if, hence, a crisis was in progress), the variety, intensity and scope of competing articulations would rise, and it did.

To recur to the stage of 'crisis within a crisis', we can say the following: A genuine sub-culture of natural philosophy existed, in which systems of nature had significant and contested articulations to religious, political and social discourses. The equally really existing contextual problems and tensions (labelled the 'general crisis') were interpreted by players through the filter of natural philosophising, thus suggesting that the problems of the age had some of their basis in natural philosophical contention and dissensus. This raised the stakes in finding and enforcing the 'true' philosophy of nature, since natural philosophy was arguably part of these problems and part of their solutions. Hence the proliferation of desperate and daring initiatives in neo-Platonic, alchemical, magical and Hermetically tinged natural philosophy, which in turn, elicited the equally sweeping, desperate as well as sudden invention of corpuscular-mechanism. Hence, also, a sense of a crisis of natural philosophising, within a larger general crisis.[53]

Recruitment of Baroque Behaviours, Norms and Identities?

Following Maravall we now ask: '*Were elements of Baroque culture, identity and behaviour templates recruited into the field of natural philosophy; were there phenomena inside natural philosophising in its critical phase that make sense as normal cultural mores of otherwise 'Baroque-ified' intellectuals*'*?* That is, we look for Baroqueness in the very weave of the processes of the game of natural philosophising.[54] The answers fall under overlapping categories of 'challenges to Scholasticism'; 'bending and breaking of rules'; 'the politics of heroic identity and honour', and 'shifting images and self-understandings of reason'.

Trying to run rings around Scholastic institutions and thinkers was a Baroque proclivity, although not a new pastime.[55] But unlike Renaissance humanism, early and mid seventeenth century natural philosophising displayed specific forms of anti-Aristotelianism focussed on *strategies of displacement of hegemonic Aristotelianism within a continuing and contested game of natural philosophy*. Many desired regime-change within the culture of natural philosophising, not the destruction of the game as such. These are the players Stephen Toulmin picked out as the anti-Renaissance, self-proclaimed heroes of intellectual and cultural salvation.[56] The culture of Renaissance humanism would have to go, as well as the institutional hegemony of neo-Scholastic Aristotelianism. But one did not have to be Descartes, Hobbes or Bacon to be involved in Baroque-looking rule breaking and bending. The vogue of seeking out novelty and discovery, not a salient feature of Scholastic commentary and disputation,[57] meant that natural philosophers were under pressure to change as the entire field became more contested and turbulent. Neo-Scholasticism taught 'don't change the mixed mathematical sciences and their relation to natural philosophy'. But some bold innovators tried to do so, fomenting and exploring the domain of physico-mathematics. Neo-Scholasticism said ‘don’t explicitly articulate natural philosophical claims on religious/political challenges, agendas and debates’. But some innovators tried to do so. Neo-Scholasticism held *de facto*, but strongly, ‘don’t bring in “inappropriate” values, aims or players, particularly anything related to practical arts, material practice, instruments, and images and rhetoric concerning the status and value of same’. But many bold and aggressive, hence Baroque-looking, innovators did.

Entrenched rules, norms and practices, reproduced from generation to generation, were under threat of reformation, deformation or rejection. The self image, self-understanding, and public posturing of the rebels and challengers was one of isolated, heroic, honour seeking, black and white decisive decision-making and action-taking. We easily label as ‘Baroque personalities’ the political and military figures of the age—Richelieu, Wallenstein, Gustavus Adophus, Maurice of Nassau, and Olivares—who, engaging their particular businesses in the general crisis, displayed these cultural identity garments whilst forging new or revised concretions of power (and its legitimations). The highest stakes natural philosophical players similarly displayed these traits.[58] To contest for systemic hegemony meant that one stood against the rest, including the massed ranks of neo-Scholasticism. Heroic effort was required, perhaps poignantly (Baroquely?) overlaid with intimations of tragic failure. We cannot know the delicate biographical cum psychological channels through which the Baroque identity and protocols came to be lived and expressed. But, for many players the situation in natural philosophising seemed to demand such self-understandings and public imaging, and the presence of such personalities further enflamed the field.[59]

In sum to comport oneself in a Baroque manner involved a striving to test and remould styles and norms, entangled with a search for identity and agenda in line with images of heroic struggle and individual honour and fame. The more radical the breaking with available styles and norms, and the more daring and honour/fame seeking the intended identity, the more Baroque the performance. If you were a natural philosopher in the ‘crisis within a crisis’ phase, numerous avenues were open to you to pursue and express such traits, whilst natural philosophising: *Is natural philosophy to become mathematical? Is good and true natural philosophy to be decided more in terms of co-opting and explaining novel discoveries? Can natural philosophy articulate to political theory, medicine, theology or not, on whose terms? Is natural philosophy meant to produce useful results?* What then in relation to these questions is the role and identity of the natural philosopher? All these channels were potentially open, and various gambits available within them. Hence the overall goal of replacing Aristotelianism by producing the really best and truest natural philosophy became supercharged, with Baroque culture elements in play. The ‘crisis within a crisis’ phase was lived through and played out, thus, by men inside natural philosophising who often acted, expressed and understood themselves in Baroque cultural terms.

Finally, it’s possible that part of the self-understanding and public imaging of competitive players in the ‘crisis within a crisis’ phase had to do with shifting notions of reason relatable to the Baroque. Consider Bernier and Boileau in 1671, cited in Hazard,[60]

> Whereas for some years past an obscure person, who goes by the name of Reason, has been attempting to make forcible entry into the schools of our University; and whereas said person, aided and abetted by certain comical quidnuncs calling themselves Gassendists, Cartesians, Malebranchists, vagabonds all of them, designs to arraign, and then expel Aristotle…

This has nothing to do with nineteenth century characterisations of rationalists (Descartes/Malebranche) and empiricists (Hobbes, Gassendi). 'Reason' here denotes an active, competitive, anti-Scholastic and (in the later seventeenth century sense) 'critical' reason—anti-authority, anti-credulity and anti-self illusion. All these terms can be imputed to the self-understandings of our earlier crisis players. This kind of 'reason', as a self-understanding and public stance, was arguably older than Hazard made out, and not totally opposed to everything Baroque. Rather, it was the obverse side of that self-aggrandising, often desperate competition in natural philosophising, which was partly Baroque in temper and cultural garb. The identity struggles and comportment of our natural philosophical players, their competitive plays and their understandings of reason are hence arguably all of a cloth which had Baroque culture threads running through it. Perhaps Baroque culture was not all about emotion and manipulation of the senses. When recruited into core of natural philosophical conflict—by specific intention, or through the already formed personalities of the players—'the Baroque' was also about 'reason' in new senses of critical, competitive, and against the existing rules and authorities. Within the context of natural philosophical contestation delineated in section "The Dynamics and Rules of Natural Philosophical Contestation During the 'Crisis within a Crisis' Phase", this type of 'reason' maps onto the image of the lonely, heroic combatant, wielding 'reason' (according to some method of his own devising) as a weapon to win the natural philosophical game, thus closing down the perceived 'crisis within a crisis'.

An Additional, Surprising, Conjectural Finding

At this point our inquiry yields a surprising speculation: What if one could link the culture and dynamics of the modern sciences to key elements of the structure and dynamics of natural philosophising, including some of Baroque provenance? Perhaps certain traits of the modern sciences express competitive, cognitive and rhetorical genes first implanted in European thought during the 'natural philosophical crisis within a general crisis', and which are partially Baroque in tenor. This corresponds to the third type of result envisioned by the Baroque Science problematic discussed in section "Introduction: Thinking About "Baroque Science"".

Consider an ideal typical model of the agonal dynamics of modern scientific disciplines, grounded in reflection on findings in contemporary history and philosophy of science, and sociology of scientific knowledge (SSK).[61] A more nuanced reading of Thomas Kuhn's model of science dynamics is a good entry point.[62] In simplistic readings of Kuhn, one has rigid—frozen—paradigms facilitating puzzle solving research, until dysfunction, crisis and revolution install a new puzzle solving paradigm, equally rigid. Against this, post-Kuhnians have explicated "normal science" dynamics using micro-sociological tools.[63] In this approach the cultural resources in play in a tradition of research, are constantly subject to re-negotiation and modification. Suppose a problem is solved by advocating a shift in some aspect of 'the paradigm', however so slight. This means the problem solution involves feedback alterations to the paradigm—conceptual, instrumental, normative. Such alterations—if negotiated into place by the expert community[64]—carry over into subsequent rounds of problem-solving, where further alterations may be negotiated. Post-Kuhnian historians and sociologists of science call such negotiated alterations of the paradigm 'discoveries', when they involve the conceptual/theoretical 'objects of inquiry' in the discipline[65], rather than, say, its instrumental techniques and standards, or norms of adequate procedure and argument.[66] Modern scientific disciplines thus display historically unique, and peculiar, tradition dynamics. They are defined by the fact that tradition modifying alterations are constantly sought, and fought over. This modelling also highlights the rhetoric that players use in self-understanding their roles and moves, and in representing them to each other, and to wider publics. Such 'method-talk' concerning isolated, non-tradition bound heroic discoveries does not accurately represent how the sciences work as agonal, novelty producing traditions. Rather, it is an accounting rhetoric used within the sciences as part of the mechanisms of contestation and accounting for change.[67]

Whilst most SSK research involves case studies, Pierre Bourdieu offered a general model of the social and organisational processes in the sciences, relating them to their knowledge-making, knowledge-breaking dialectic.[68] Bourdieu places members of a scientific tradition as players in a 'field', in a peculiar agonistic relation, involving an economy of material and symbolic resources, strategies and positions. Bourdieuian players seek a monopoly of the cognitive and social power at stake in their particular field: They have certain amounts of symbolic and material resources (or capital) which they can deploy, strategically, in attempts to secure more resources and more power over the determination of the social and cognitive stakes at risk in the field in the next rounds of play. Given their different positions, resources and hence strategies, players attempt to produce claims that are both achievable within the limits of their symbolic capital and likely to prove significant and attractive to their competitors. These peers accredit such work by taking it up and redeploying it in their own construction of bids. What Bourdieuian players play for—the production of non-trivial, new claims that might be taken up and used by peer-competitors—maps directly onto the post-Kuhnian conception of ongoing negotiation into place of 'discoveries' which shift the terms of practice in subsequent rounds of research. But it is crucial to understand that for Bourdieu a 'system of objective relations' exists at any given moment amongst the positions already won and occupied in the field, via previous rounds of struggle. Bourdieu insists that the system of relations should not be reduced to or conflated with the micro-interactions and moment to moment strategies 'which it in fact determines'.[69]

Melding the post-Kuhnian and Bourdieuian models, we can see modern natural sciences as agonistic traditions, manufacturing and negotiating novel shifts of tradition practice, and awarding credit for these shifts, using a rhetoric of individual methodologically based heroic discovery. But where did this come from? I conjecture that the crisis phase in natural philosophising (itself partaking variously and diffusely of Baroque culture) left in the dynamics and culture of the field certain competitive practices and accounting rhetorics that survived in the ultimate descendants of early modern natural philosophy, the modern sciences, partially shaping their uniqueness as traditions

In section "Phases and Stages in the 'Scientific Revolution' Seen as an Unfolding Process in the Field of Natural Philosophising", we noted that an ironic upshot of the 'civil war in natural philosophising' was that natural philosophising as a whole became more autonomous of other cultural forms, whilst it also began a process of fragmentation into a number of diverse and narrow special domains or disciplines of natural inquiry. The formerly more coherent, if internally contested, domain of natural philosophising began to fragment into, and *débouche* onto, a suite of successor, more narrow domains. Over the course of the next century natural philosophy faded and died, and these modern sciences emerged.[70]

Perhaps this slow but powerful process toward fragmentation of natural philosophy into successor disciplines, first unleashed during the crisis within a crisis, carried the élan of continuous competition and contestation from the earlier period right into the structure and dynamics of the successor fields, along with the heroic methodological accounting rhetoric.[71] Perhaps transcribed into the successor fields were the peculiar agonal dynamics according to which the a scientific tradition exists for the purpose of producing accredited novelty, a gene first expressed, in confused and desperate form, during the heated contestation of the critical phase.

Conclusion

This inquiry has taken two paths: On the one hand, viewing the Baroque as a cultural epoch in European history, we have tried to understand early modern natural philosophy, its dynamics and phases, in relation to the picture of Baroque culture and the general crisis painted by Maravall. On the other hand, we have found an intriguing hint that the modern natural sciences, as such, bear distinct cultural genes descending, ultimately, from the culture of the Baroque and the period of 'civil war in natural philosophy'. Modern sciences are by historical standards very odd beasts. They are continuously reproduced expert traditions whose very dynamics, and *raison d'être* in rhetoric and in practical activity, consists in the unremitting, competitive and concerted struggle to construct, and have re-implanted into the tradition, *significantly tradition-altering achievements*, which are proffered on a contested basis, and only have effect after being revised and negotiated into place by peer competitors of the initial proponents. In other words, both the actual, messy, competitive and political 'mangle of practice' inside scientific traditions, and the channels of crisp method rhetoric through which they are understood and accounted for, seem, on close inspection, to bear just legible hallmarks that say—"*forged by somewhat rebellious* *master practitioners in the white heat of the early to mid seventeenth century natural philosophical crisis, with some ingredients in part borrowed at that time from the supervening Baroque culture*”.

Notes

1. Friedrich (1962)
2. So, on the one hand, Friedrich mixed into his discussion of a Baroque ‘age’, culture or society, allusions to phenomena that may be taken as direct evidence of building crisis. Similarly, on the other hand, Theodore Rabb (1975) in his first of two sojourns into the territory of the general crisis, discussed Baroque art half a dozen times, but he never theorised the Baroque as an ‘age’, culture or epoch. Rabb’s recent work, *The Last Days of the Renaissance* (2006) effectively does away with the term crisis. Instead, marshalling much the same evidence, he argues for an early seventeenth century phase of turbulence within, and inflection of, various ‘Renaissance’ structures and processes, leading to different conditions in the later seventeenth century.
3. I owe this reference to Simon Schaffer, who pointed out its potential relevance to the Baroque Science Project problematic in conversation October 23 2007. Schaffer was pointing to the possible relevance to my natural philosophy ruminations of Maravall’s (1973) notion of kitsch, commodified, dramatic display, a staple of Baroque culture, as redeployed by Clark (1992) in relation to kitsch experimental displays in university teaching in late Baroque Germany.
4. Maravall, insists that there are generic properties of his interpretation applicable, with local national twists, to ‘Baroque culture’ as a pan-Western European phenomenon.
5. What Maravall takes to be included in his general crisis is very close in description, given social and state structure differences, to what Trevor-Roper long ago termed the ‘crisis of court vs. country’ in England. Similarly the trends in state structure, inter and intra state conflict, religious fission and warfare, and cultural pessimism that Rabb (1975) built into his crisis thesis, have chords and echoes in Maravall’s vision. The seminal papers by Hobsbawm (‘The Crisis of the Seventeenth Century’) and Trevor-Roper (‘The General Crisis of the Seventeenth Century’) on the crisis thesis first appeared in *Past and Present* and then were reprinted in Aston (1967) pp.5–95.
6. Peter Anstey and John Schuster, “Introduction” to Anstey and Schuster (2005) To place the evolution of natural philosophy at the centre of one’s conception of the Scientific Revolution is not novel, and more scholars are realising the value of such a perspective, but neither is it obvious or agreed upon in the scholarly community. H. Floris Cohen’s massive survey of the historiography of the Scientific Revolution (Cohen 1994) illustrates that the term ‘natural philosophy’ has been endemically present in the literature, but not systematically theorised, often serving as a synonym for ‘science’. Recent attempts to delineate the category of natural philosophy and deploy it in Scientific Revolution historiography include Schuster (1990, 1995a); Schuster and Watchirs (1990); Andrew Cunningham (1988, 1991); Cunningham and Williams (1993); Dear (1991, 2001a, b); Peter Harrison (2000, 2002, 2005); and John Henry (2002).
7. The same historiographical strategy needs to be applied to other’ terms such as, ‘physico-mathematics’, natural theology, mixed mathematics, method and natural theology (see Dr Larissa Johnson-Aldridge—*Kaleidoscopic Natural Theology: The Dynamics of Natural Theological Discourse in Seventeenth and Early Eighteenth Century England*, UNSW, unpublished dissertation, 2009). This strategy is applied throughout my forthcoming study of the young Descartes: *Descartes Agonistes, Physico-Mathematics, Method and Mechanism 1618–33* Dordrecht, Springer, 2012. Materials tending toward the findings of the latter are contained in Schuster (1995a, 2000a, b, 2005); Gaukroger and Schuster (2002).
8. Knowledge was not actually discovered and demonstrated by method—see Bachelard (1949), Kuhn (1970), Feyerabend (1975); Schuster and Yeo (1986b); Schuster (1986, 1993) and others.

Rather, method discourse provided universally understood packaging and rhetorical framing for claims of natural philosophical type, and by means of the tools of logic provided natural philosophical players, as subjective agents, the technical capability for reflexively criticising, comparing, overthrowing and radically reworking the claims of others and of themselves.

9. Schuster and Taylor (1996, 1997); Schuster (2002).
10. Other contemporary knowledge systems, such as natural history and natural theology also need to be theorised in this manner and the entire set examined for their dynamics and articulations over time. I have written several overviews of the Scientific Revolution in this style. Schuster (2002), also Schuster and Watchirs (1990); and Schuster (1990). Recently the latter work was translated into Chinese and published for the Chinese HPS market in an anthology on the Scientific Revolution edited by Liu Dun and Wang Yangzong (2002), pp.835–869.
11. David C. Lindberg, for instance, asserts that "For the first time in history, there was an educational effort of international scope, undertaken by scholars conscious of their intellectual and professional unity, offering standardized higher education to an entire generation of students." Lindberg (1992) p.212.
12. Concerning late scholastic education at the turn of the seventeenth century see Maclean (2007) and Des Chene (1996): Following their work, I hold that most of what we conceive of as the process and the products of the 'Scientific Revolution' took place within patterns of change, internal contestation and contextual shaping in this evolving field or culture of natural philosophising.
13. Schuster (1990, 1995a, 2002), Schuster and Watchirs (1990); Schuster and Taylor (1997)
14. Cf. Anstey and Schuster, 'Introduction' to Anstey and Schuster (2005). We shall refine the concept of boundary-work, including how we think about players' contestation about it, below in section "The Mechanics of Responding to 'Outside' Challenges and Opportunities".
15. This manner of conceptualising a competitive creative 'field' of course derives originally from the seminal and suggestive work of Bourdieu (1971a, b, 1975). Cf. below Notes 26 and 69.
16. The five theoretical dimensions of the model, are: (i) natural philosophy as intellectual tradition in the manner of post-Kuhnian science dynamics with a dash of Skinner; (ii) as competitive creative field (Bourdieu); (iii) as an evolving field of claims governed by rules of utterance, (Foucault, 1969); (iv) as an historically dynamic sub-culture of the larger culture (Marshall Sahlins); (v) and as a network of institutions (Mertonian sociology as refracted through my work with Alan Taylor on the 'organisation of the experimental life' at the early Royal Society: Schuster and Taylor 1996, 1997). These are developed in my current work in progress on "A Guide to Historiographical Technique and Pitfalls: The Scientific Revolution and Beyond".
17. Sahlins (1993) esp. pp. 25,15. "[Cultural orders] reveal their properties by the way they respond to diverse circumstances, organising those circumstances in specific forms and in the event changing their forms in specific ways. Here, then, in a historical ethnography—an ethnography that extends, say, over a couple of centuries—here is a method for reconciling form and function in a logic of meaning, for discovering the relatively invariant and mutable dimensions of structures….the currently fashionable idea that there is nothing usefully called 'a culture'—no such reified entity—since the limits of the supposed 'cultures' are indeterminate and permeable…paradoxically…misreads a cultural power of inclusion as the inability to maintain a boundary. It is based on an underestimate of the scope and systematicity of cultures, which are always universal in compass and thereby able to subsume alien objects and persons in logically coherent relationships." Shapin (1992) speaks of sciences as cultures in process in analogous ways.
18. On internalism/externalism, Schuster (2000b).
19. For more details, and somewhat varying emphases, see Schuster (1990, 2002) and Schuster and Watchirs (1990).
20. Schuster 1990; Dear 2001b, chap 2; Gaukroger 2006, pp.139–48; Eisenstein 1979, vol 1.
21. In mathematical astronomy the Renaissance phase is discernible from the late fifteenth century with the work of Peurbach and Regiomontanus, whilst in mathematics and geometrical optics the pace of the Renaissance phase only accelerates in the later sixteenth century In geometry this development included not only improved texts and commentaries on Euclid's *Elements*,

but the recovery, translation and edition of the texts of higher Greek mathematics, of Apollonius, Archimedes, Pappus, and most importantly for the further maturation of algebraic thinking, the work of Diophantus. Anatomy and medical theory followed more closely upon astronomy, the programme of editing and publishing the complete body of Galen's works culminating in the 1520s and 1530s. (Schuster 1990)

22. Rossi (1970); Ravetz (1974); Schuster (1990). The latter two build on the richly suggestive early thesis of Lenoble (1943), while taking their cues about the turbulence in wider intellectual circles from Popkin (1964) and Rabb (1975). More recently, the idea of a crisis period in the gestation of modern science has been articulated in great detail by H. Floris Cohen (2010), pp. 403–440. His long awaited study offers a tightly argued diachronic model of multiple overlapping and interacting 'transformations' in European nature-knowledge, flowing forward from an initial set of crucial transformations in the generation of Galileo, Kepler, Bacon and the younger Gassendi and Descartes. This initial movement broke with a 'Renaissance' phase of development, which had not yet displayed strong indications of reaching beyond what similar recoveries of classical natural philosophy and mathematics had achieved in medieval Islam or late medieval Europe. A crisis of legitimacy immediately ensued, in the middle decades of the seventeen century. Cohen traces this crisis in several dimensions within the realms of natural philosophy and the subordinate fields, rather than seeing it, in the manner of Rabb and Popkin, as a larger cultural crisis with an epiphenomenal echo in 'Science'. According to Cohen's account, this crisis momentarily threatened to abort any significant further development. However, it was contingently if sufficiently overcome to permit Cohen's subsequent waves of transformation to eventuate. Thus, Cohen's new model of the Scientific Revolution, reflects and further articulates the idea of a pivotal moment of crisis in the process (as well as the conception of a 'Renaissance' phase in the process).
23. The founders of mechanism hoped to resolve the conflict of natural philosophies in a way which was to them cognitively progressive, but religiously and politically conservative. Accordingly, mechanism was neither the finest fruit of detached, rational 'modern' thought finally asserting itself to end 'the confusion', nor was it simply or directly, the reflection of some long rising merchant, administrative or craftsman-technologist groups, who for some contingent reason invented mechanism between 1630 and 1650. (Schuster 1990)
24. As to Newton, I hold that we misunderstand the rhythm of the development of early modern science by focusing too intently upon Newtonian celestial mechanics and physics. Our periodisation, focussing on the trials of natural philosophy, should take this into account, seeing the process in terms of three phases or moments, punctuated, contingently by Newton, rather than aiming for him. See Schuster and Watchirs (1990), Schuster (1990).
25. For the claims in the second half of this paragraph and their wider historiographical implications: Schuster (2002), Boschiero (2007), Schuster and Taylor (1997); Schuster and Watchirs (1990).
26. The term 'objective' is used here in the sense of Bourdieu (see above, Note 15 and below Note 69), whereby we denote the (model-derived) organisation and dynamics of a competitive field, existing above and beyond the immediate control, or even necessarily the understanding, of actors in the field, and not capable of being instantly or unilaterally modified by the actions of such players in their respective micro contexts. These notions may be related back to the 'iceberg' metaphor offered earlier.
27. It has been obvious since Lenoble's (1943) work that families of natural philosophies competed in respect of the values, aspirations and religious resonances they endorsed and condemned; see also Rattansi (1963, 1964) and Easlea (1980). The fact that natural philosophy had that entourage of subordinate, more narrow traditions of science-like practice, however, resulted in a much fiercer competition and contestation than even the traditional literature suggests. The situation was actually more like every man for himself, as natural philosophers of similar genealogical stripe—neo-Platonic, proto or emerging mechanist, 'magnetic', or chemical—competed with each other as well: Kepler vs. Fludd; Descartes vs. Gassendi vs. Hobbes; Libavius and other latter day Paracelsians vs. the heritage of Paracelsus himself.
28. The term 'mixed mathematics' was used by Scholastic Aristotelians to refer to a group of disciplines intermediate between natural philosophy, which dealt with those things that change

and exist independently of us, and mathematics, which deals with those things that do not change but have no existence independently of us, since numbers and geometrical figures have (contra Plato) an existence only in our minds. (Aristotle, *Metaphysics* Book E.) A physical account of something — such as why celestial bodies are spherical — is an explanation that works in terms of the fundamental principles of the subject matter of physics, that is, it captures the phenomena in terms of what is changing and has an independent existence, whereas a mathematical account of something — such as the relation between the surface area and the volume of a sphere — requires a wholly different kind of explanation, one that invokes principles commensurate with the kinds of things that mathematical entities are. (Aristotle *Posterior Analytics*, 75a28-38; Cf. 76a23ff and *De caelo* 306a9-12.) In the *De caelo*, 297a9ff, for example, we are offered a physical proof of the sphericity of the earth, not a mathematical one, because we are dealing with the properties of a physical object. In short, distinct subject matters require distinct principles, and natural philosophy and mathematics are distinct subject matters. However, Aristotle also recognises subordinate or mixed sciences, telling us in the *Posterior Analytics*, 75b14-16, that 'the theorem of one science cannot be demonstrated by means of another science, except where these theorems are related as subordinate to superior: for example, as optical theorems to geometry, or harmonic theorems to arithmetic.'

29. Cf Aristotle *Physics*, 194a10: geometrical optics 'investigates mathematical lines, but qua physical, not qua mathematical.'
30. The point of introducing the unusual term 'declaratory' will be become more clear in section "Bend or Brake Aristotle's Rules about Mathematics and Natural Philosophy: The Gambit of 'Physico-mathematics'" below, where we see how certain natural philosophical rebels tried to renegotiate, rather than destroy, the formulations of these rules as they received them through their Scholastic Aristotelian educations into natural philosophy.
31. However, the articulation of a natural philosophy to a mixed science could be much looser than the Copernican example implies. As just noted, under Aristotelianism geometrical optics consisted largely in geometric ray diagrams, their rules of construction and a set of canonical puzzles, such as the behavior of mirrors, the rule governing refraction, the explanation of the rainbow and other curious optical effects. Hence, virtually any natural philosophical theory of matter could have been used to provide an explanatory 'voice over' for this science. Only during the critical phase of the Scientific Revolution, in the optical work of Johannes Kepler and René Descartes, was there sought a closer interaction between optical theorising and problem solving, on the one hand, and natural philosophical explanations, on the other.
32. This operated at an individual basis, but over time, such moves could themselves aggregate and form patterns of largely unintended change in the subordinate disciplines in question.
33. What were constructed were still natural philosophies, within the common field of natural philosophising, but the Aristotelian limitations on the rules or terms of construction were being radically challenged and shifted. Beeckman's corpuscular mechanism keyed to a reading and amplification of dynamical interpretations of mechanics, as in the pseudo Aristotelian *Mechanical Questions*. Descartes' corpuscular-mechanism, surprisingly was keyed in part to the purely static mechanics and hydrostatics of Stevin (and Archimedes) much overlayed as it developed with material from his own 'physical' optics. (Gaukroger and Schuster 2002; Schuster 2000a, 2005)
34. So, versions of the Chemical philosophy depended for both technical and value orientation on the notion of a spiritualised yet practically productive alchemy, thus powerfully expressing moral-psychological aspirations—a search for redemption through esoteric knowledge and successful practice. These powerful sentiments were partially shared, and certainly co-opted in the programs of Bacon, Descartes and their later seventeenth century followers, for whom the nature and 'control' of alchemy was therefore a particularly strategic issue: The values and aims which Paracelsianism and later the Chemical philosophy invested in alchemy were co-opted, sanitised of radical political and religious resonances and made acceptable to intellectually progressive but socially conservative elites, a ready audience for the mechanical philosophy. Chemical phenomena were de-spiritualised and reduced to applied mechanistic matter theory, whilst the search for personal justification and social benefit would now be achieved through proper method and well grounded results, rather than esoteric insight and wisdom.

35. William Gilbert (1544–1603) in his *On the Magnet* (1600) suggested a new natural philosophical agenda and content, built on exploiting and metaphorically extending important experimental work he had done on the magnet and magnetic compass. Adopting neo-Platonic notions such as the Earth's magnetic soul as an all-pervading spiritual power, he reinterpreted the craft knowledge and lore of miners and metallurgists, to argue that lodestone is the true elemental nature of the earth; that the earth is a gigantic spherical magnet; and that, since magnetic force is an immaterial force, the magnetic nature of the entire earth amounts to a cosmic soul or intelligence—capable of moving, or at least spinning the earth. Similarly, he insisted that his knowledge was productive of useful results, most notably improving the understanding and use of the magnetic compass in navigation.
36. For example, Gassendi's observational claim only confirms Galen, and is subservient to the larger natural philosophical contestation in which he is involved.
37. Descartes' extended strategic encounter with Gilbert's work on magnetism illustrates all the above points. What was novel in Gilbert's experimentation was co-opted by Descartes without the addition of a single new experiment. For Descartes the nub of the encounter lay elsewhere. Gilbert's natural philosophical exploitation of the magnet was dictated by his concern to establish a novel system of Magnetic natural philosophy of distinctly neo-Platonic flavour and embodying and supporting a modified Tychonic cosmology. This was the 'significance' of the magnet work that had to be appropriated, reframed, and tamed to the imperatives of Descartes' program. Gilbert's natural philosophising of the magnet was too important and impressive a gambit in the natural philosophical field to be ignored by his natural philosophical competitors. Descartes' efforts were directed at re-glossing Gilbert's experimental work in mechanistic terms, rather than at extending the number and type of magnetic experiments. He replaced Gilbert's story of the cosmos making and binding role of the spiritual magnetic force with a mechanist's story of an equally cosmic magnetism which was now the purely mechanical effect of a species of corpuscle of particular, and peculiar, shape and size, moving in and through suitably configured aggregations of ordinary 'third matter'.
38. Hattab (2005), following Laird (1986), Rose and Drake (1971).
39. There were competing varieties of physico-mathematics, running from the conservative version of some leading Jesuits mathematicians [Peter Dear (1995)]; the more radical reading of the classical texts in mechanics — such as the statics and hydrostatics of Archimedes, or the pseudo-Aristotelian *Mechanical Questions* as part of or relevant to natural philosophy; through to the more innovative schemes of Kepler's celestial physics, a new physico-mathematical domain; Beeckman's linking of an emergent corpuscular mechanism to dynamical interpretations of the simple machines; Descartes' very radical attempts to ground a corpuscular-mechanism and determine the principles of its doctrine of causation (laws controlling force and determination of motion) through exploitation of hydrostatical and optical inquires of a physico-mathematical character; [Gaukroger and Schuster (2002), Schuster (2000a, 2005)] and Galileo's sui generis new science of motion as well as his more piecemeal physico-mathematical excursions, identified by Dear.
40. Kepler practised geometrical optics under a neo-Platonic natural philosophy and conception of light. (Lindberg 1976) He got brilliant results in the theory of the camera obscura, theory of vision, and, to some degree, the theory of refraction and the telescope. Descartes emulated Kepler's technical optical achievements but abandoned the underlying neo-Platonic natural philosophical program. Instead he practised geometrical optics under his version of a mechanical conception of light. He achieved a simple and workable version of the law of refraction and a general theory of lenses. Conversely, as I have shown, essential details of Descartes' mechanistic system were shaped by his optical successes. Schuster (2000a, 2005). See also Gal and Chen-Morris (2010)
41. Hence the salience of significant novelties and discoveries, immediately up for first time contestation in the field. As interesting novelties emerged across increasingly dynamic and interrelating subordinate fields, the struggles over them increased. Merely gazing at, or hording or collecting curious new facts may have been a popular pastime, but it was not central to the natural philosophical agon—contention about curiosities was!
42. Frank (1980), Anstey (2000).

43. The rhythm of this process is fascinating, and important. Copernicus staked his claims upon the truth value of 'cosmic harmonies'. Copernicus himself was either too timid, or unprepared, to recast his astronomical theories in natural philosophical terms of cosmic matter and cause. Instead it was Tycho Brahe, who, toward the end of the century kicked off the eventual crisis of natural philosophy/astronomy articulation by linking his favoured version of quasi Copernican astronomy to significantly altered (Aristotelian) claims in natural philosophy. Gilbert weighed into the contest with arguably the most innovative natural philosophical vision of his generation. Then in short order Kepler subsumed his brand of Copernicanism within physico-mathematical explanations which in turn resided at the centre of his neo-Platonic natural philosophy. The situation was similar with Descartes, for in *Le Monde* he staked the truth of his natural philosophy on the truth of his version of a physically explained Copernicanism. (Schuster 2005; Gaukroger 1995)
44. As I have shown elsewhere Schuster (2005), in his *Le Monde*, Descartes had a complex articulation strategy spanning astronomy, optics and a new challenging utterance in natural philosophy.
45. Such as: What is the nature of the earth as a planet— what can be gathered about the earth, for example, about its structure, its magnetism (Gilbert), its tides (Galileo and Descartes), the nature of local fall, that might support its construal as a planet amongst planets and allow for the motions Copernicanism required of it; what causes the celestial motions; what physical role does the sun (and all stars in multiple planet system versions of Copernicanism) play in those motions; does the nature and behaviour of comets throw any light on these problems, and so on. Jacqueline Biro has recently shown how sixteenth century technical developments in mathematical geography, which potentially had implications for such questions, were only grudgingly granted by the Scholastic Aristotelians, but eagerly seized as a resource by natural philosophers advocating Copernican cosmology, such as Copernicus, Gilbert, Bruno, Galileo and Descartes. (Jacqueline Biro 2009).
46. Writing long ago, and dealing with opposite chronological ends of the period, Henri Pirenne (1936,—actually written whilst detained by the Germans 1916–17, Eng trans 1939) and Paul Hazard (1935, English trans. 1963) each captured poignantly the religiously centered critical desperation and life or death imperative to choice. Compare, for example, Pirenne p.583 on the later sixteenth century and Hazard p.221 on the later seventeenth; that is depictions respectively from early on in the presumed general crisis and near to its supposed close.
47. Nor do we want to follow intellectual history practice, for example, Popkin (1964) with his hypostatised, growing then resolved 'sceptical crisis', and simply give thick enough, untheorised descriptions so that a de facto and largely tacit explanation emerges something along the psychologistic lines of 'great thinkers somehow get it into their heads to address the great challenges hanging about in the cultural atmosphere, and hence their intellectual output somehow reflects or is shaped by them'.
48. That is, we historians can model, by evidence based conceptual construction, macro entities such as social, economic and political structures and processes. We may attribute objective existence in the past to such models if they are well formed, grounded, and consensually agreed by expert modellers. But these [models of] macro entities should not be granted direct causal efficacy over the thoughts of historical actors by influence or imprinting. And, they should be taken as having been known by actors, if at all, only in terms of their own discourses and representations, which also need to be studied, and modelled by historians. For example, there undoubtedly was a rise in the import and scope of the practical arts in the sixteenth century, which itself needs explanation through macroscopic drivers, economic, military and political, and which contemporaries partially represented in their own terms and for their own reasons. We only know about these macro forces and structures through our own evidence based modelling; actors only knew parts of them through their own representations. So, our models of the drivers will not directly explain the discourse of, say, a Bacon: its occurrence in time, content, form or motives. For that we need to start by modelling the field of contention in which the actor was moving, as in our study of natural philosophy, and the particular actor's likely trajectory through it, and finally his gambits in articulating upon boundaries, where, to

complete this circle of interpretation, we understand what underlay the boundaries in terms of our own contextual modelling.

49. The term articulation is used here to deal with 'external' forces or drivers in partial emulation of the young Foucault, as in *The Archaeology of Knowledge* (1969). All my modelling obviously grants much more reality to individual actors and their intentions than doctrinaire Foucauldianism would. That is because a wider range of resources are invoked in my model building. Cf above Note 16.
50. To this end, I have also benefited from post-Kuhnian sociology of scientific knowledge scholars' concept of 'boundary work' in disciplines or professions (Gieryn 1983), but, as some readers will sense, my conceptions of boundary maintenance and work upon field or disciplinary boundaries are wider, more historical and very much tempered by a much modified 'Foucault' passed through the filter of Bourdieuian sociology of agonistic fields.
51. This is what we mean by challenging the choice, depth and weighting of an articulation. Similar points attach to politics, or more particularly to issues about the nature and role of 'the state', and the contemporary tortured issues of sovereignty, Church governance vis à vis the state, and issues of civil order and legitimate rebellion (all of which could count as elements in a larger 'crisis' perceived and responded to by some natural philosophical players). Most Aristotelian teachers of natural philosophy in the university environment would have left largely unsaid within natural philosophy its linkages to the local political status quo, and to the institutional arrangements that supported the very existence of that particular university and its natural philosophical functions. A Bacon or Hobbes, however, articulated natural philosophical utterance in part upon such particular evaluations and agendas of these political issues. But this is not to say that politics or political doctrines or agendas 'influenced' the natural philosophical utterances of Bacon or Hobbes. Rather, it is to say in the first instance that within the field of natural philosophy they saw fit to mobilise and deploy such articulations in an effort to win the natural philosophical agon, and through it, partially to support their properly political aims, now recursively expressed, amongst other ways, through natural philosophy.
52. Similarly it can be argued that the practical arts and their practitioners did not influence natural philosophers, but rather that certain natural philosophers articulated their natural philosophical utterances in part upon resources from and about the domain of practical arts.
53. Cf. Schuster (1990) pp. 237–38, which can now be reframed using the more developed model of natural philosophical competition presented in this section of the present paper.
54. Friedrich and others who have studied the Baroque as a culture stress that the Baroque was about rule bending and rule breaking, as well as about especially self-regarding and anguished matters of identity and honour. Friedrich (1962) especially chapter 2 ; Maravall (1973); Rabb (1975) who treats these phenomena under his category of rising 'crisis'.
55. Clark (1992)
56. In his *Cosmopolis: The Hidden Agenda of Modernity*. (Toulmin 1990)
57. Which of course is not to say that none went on in Scholastic circles, teaching and textbooks, only that it was not the leading edge of these phenomena, rather the reluctant follower. (Gascoigne 1990; Reif 1969; Schmitt 1973; Brockliss 1981)
58. With the exception of the gentle, genial Gassendi, a man for that reason well recognised by historians as interestingly generationally displaced (too late for the scientific renaissance, too early for the age of consensus, muting and fragmentation). (Brundell 1987) I thank my colleague Dr Barry Brundell, MSC, for enlightening informally expressed insights about the personality and likely outlook of Gassendi.
59. Importantly, acting Baroquely inside the natural philosophical crisis, did not mean one's writings have to be notably Baroque by the standards of later literary classification. Descartes, particularly the young Descartes whom I have been studying, is a fine example of all this. From 1618 and age 22 Descartes, operating at first under his similarly inclined mentor, Beeckman, was a thrusting rebel against the official university rules about the scope and application of mathematics, without an as yet well defined, maturely expressed, cause in the game of natural philosophy. Beeckman and Descartes were thumbing their noses at scholastic natural philosophical rules about the status and role of mixed mathematics, and even the ideal of systematisation. Correlatively, they

were willing to take on board the vague, but trendy concept of physico–mathematics, and in Descartes' case, his home cooked version of the already circulating idea of a 'universal mathematics', as well, inflating them with aspiration and bravado. Descartes soon went even further, to a putatively world–beating new analytical method. At each stage Descartes was well pleased. To fancy himself a 'physico-mathematician', then a 'universal mathematician', gave him firm placement in a cultural debate, and provided a sense of who he was intellectually (and particularly as some special specimen of a mathematicising natural philosopher). After 10 years of these endeavours, and self-inflations; that is, after several notable little technical successes and a sequence of ever more grandiose fantasy agendas: 'physico-mathematics', 'universal mathematics', and, 'the method', it all blew up with the unfinished later portions of the *Regulae* in 1628–9, at which point he realised he was actually meant to be a radical version of his own Jesuit scholastic mentors in systemic natural philosophy, leading to an equally rebellious agenda along these more customary lines of systemic natural philosophising. That makes a rather Baroque looking story line through the natural philosophical currents of the day, without anything Descartes produced in those years, or in the *Discourse* which retails an ideal version of his life story to that point, being particularly Baroque in the literary sense.

60. Hazard (1935) p.119. Hazard's conception of a late seventeenth century 'crisis of the European mind' ushering in the Enlightenment may seem contrary to later crisis theories, such as those of Rabb (1975) and Popkin (1964), and similarly averse to the position put in this paper. The resolution, is this: there was an historical hysteresis (in the Sartreian sense, Sartre 1963, pp. 64, 75 and part III passim) between natural philosophical crisis (which paralleled the hot state structural and religious-political-military turbulence of the early and mid seventeenth century) and the later, wider 'Hazard cultural intellectual crisis'. The latter was marked by the prior changes in natural philosophy, and played out largely in the new pan-European literary and cultural media as a density of quarrels and controversies, not as a set of life and death social, civil and inter-state confrontations and conflicts. The Hazard crisis, marked by the rise of deism, 'criticism' and sceptical rationalism; the proto-Enlightenment 'war' on tradition, superstition and unreasonable authority; and the quarrel of ancients and moderns, would not have occurred had not, amongst other things, the trajectory of natural philosophy already unfolded as it had earlier in the century; or if state structures and the inter state-system had not crystallised into their post 'general crisis' forms. Perhaps it was not a crisis at all, just the opening phases and rapidly ramifying crystallisation of new, wider cultural forms, the emergence of the 'Republic of Letters' as it were, with a widened reading public and enlarged (open or clandestine) publishing domains. The third, or 'CMF', phase natural philosophy, attendant sciences and new organisations were part of the furniture in the salons in which Hazard's crisis was argued out, for these developments had been forged by prior 'crisis inside a crisis' if you will.
61. The model presented below is ideal typical. It is not meant to capture the precise social and cognitive dynamics of any particular modern (that is, post early nineteenth century) scientific discipline. As an ideal model, it invites complexification on a case by case basis by considering variants and emerging long term shifts affecting the sciences as a whole. One suspects that the sorts of ideal models arising from post-Kuhnian thinking in HPS and SSK are better attuned to what Ravetz (1971) called the classical academic science of the late nineteenth and early twentieth century, rather than the industrial/military science of the mid and late twentieth century or the emerging post-modern transdisciplinary sciences of today.
62. Kuhn, properly understood, was fully committed to the idea that the sciences are many, not one Science, and that his theorising was aimed at providing an ideal typical account of how any given mature science functions, the motor of tradition dynamics in any given science as it were. He also aimed to provide a broad, macroscopic mapping of the trajectories of the sciences over time. Cf. Schuster (1995c), chapters 15 and 16; Schuster(1995b), chapter 8.
63. Ravetz (1971); Schuster (1979); Barnes (1982); M. Mulkay (1979). Latour and Woolgar (1979); Knorr-Cetina (1981); Collins (1985).
64. Of course the form of the discovery claim negotiated into place, and accounted back to the presumed individual discoverer, can differ greatly from that originally published, let alone imagined, by the first inventor[s] of the claim.

65. The expression "(intellectually constructed) objects of inquiry" is Ravetz's (1971) term of art in his own early and brilliant sophistication of Kuhn's original model of 'normal science'.
66. This post-Kuhnifies the partially separate development of the so-called attributional model of scientific discovery. (Brannigan 1980, 1981; Schaffer 1986); For a textbook level exposition of a case study of these issues of post-Kuhnian notions of discovery and 'revolution' see Schuster (1995b) chap 4 and 5.
67. On the politics and rhetoric of method see: Schuster (1984, 1986, 1993); Schuster and R. Yeo (1986b), pp.ix–xxxvii.; Richards and Schuster (1989). Method-talk is flexibly used by players inside science to account for achievements, failures and allocate credit. It is part of the self-identity of many practicing scientists and an important part of the public imaging of science and its constituent disciplines.
68. Bourdieu (1971a, b, 1975). Needless to say his model is an ideal type to which empirical fields approximate.
69. By the "objective" systematic state of the field at any moment of play, we take Bourdieu to mean that the field exists as an analyst's model, a historian's model of the internal political economy of the field at a given moment in the style of historiographical category construction alluded to above. As in any model in historiography—for example my model of natural philosophy, or Maravall's model ('structure') for Baroque culture, or the post-Kuhnian model of research dynamics in a scientific tradition—it is an intellectual construct, category, constellation of concepts, constructed using social theory, bits of other historical findings, and appeals to evidence about the field or discipline in question. It then functions, as Bourdieu (and Maravall) suggest, as the ultimate object of study and as an explanatory resource for understanding particular plays and processes in the field.
70. On this Schuster and Watchirs (1990); Schuster (1990, 2002); Schuster and Taylor (1996, 1997). The domains that emerged in this process included: the complexly evolving master science, classical mechanics; new versions of the old mixed mathematical fields, now crystallised as more experimental and 'physico-mathematical'; and a host of emergent experimental fields which solidified further in the eighteenth century.
71. An important dimension of this result was the fact that as this process continued, actors' legitimatory and packaging rhetorics (typically rhetorics of method) evolved to meet the needs of players with these new sorts of aims and agendas. For example a method–discourse concerning 'speculative' vs. 'experimental' (natural) philosophy flourished in late seventeenth century England and was deployed, mainly by self-styled advocates of the latter, against real or imagined adversaries of the former stripe as a way of positioning themselves and their work in a field still inhabited not only by themselves, but by others, including a few players and texts of overtly theoretical, systematic and contentious natures (Anstey 2005).

References

Anstey, Peter. 2000. Descartes' cardiology and its reception in English physiology, In *Descartes' natural philosophy*, ed. Stephen Gaukroger, John Andrew Schuster, and John Sutton, 420–444. New York: Routledge.

Anstey, Peter. 2005. Experimental versus speculative natural philosophy. In *The science of nature in the seventeenth century*, ed. Peter R. Anstey and John A. Schuster, 215–242. Dordrecht: Springer.

Aston, Trevor. 1967. *Crisis in Europe 1560–1660*. Garden City: Doubleday.

Bachelard, Gaston. 1949. *Le rationalisme appliqué*. Paris: Presses Universitaires de France.

Barnes, Barry. 1982. *T.S. Kuhn and social science*. London: Macmillan.

Biro, Jacqueline. 2009. *On earth as in heaven: Cosmography and the shape of the earth from Copernicus to Descartes*. Saarbrucken: VDM Verlag.

Boschiero, Luciano. 2007. *Experiment and natural philosophy in seventeenth-century Tuscany*, Studies in history and philosophy of science 21. Dordrecht: Springer.

Bourdieu, Pierre. 1971a. Intellectual field and creative project. In *Knowledge and control: New directions for the sociology of education*, ed. M.F.D. Young, 161–188. London: Collier-MacMillan.

Bourdieu, Pierre. 1971b. Systems of education and systems of thought. In *Knowledge and control: New directions for the sociology of education*, ed. M.F.D. Young, 189–207. London: Collier-MacMillan.

Bourdieu, Pierre. 1975. The specificity of the scientific field and the social conditions of the progress of reason. *Social Science Information* 14: 19–47.

Brannigan, Augustine. 1980. Naturalistic and sociological models of the problem of scientific discovery. *The British Journal of Sociology* 31: 559–573.

Brannigan, Augustine. 1981. *The social basis of scientific discoveries*. Cambridge: Cambridge University Press.

Brockliss, Laurence. 1981. Aristotle, Descartes and the new science: Natural philosophy at the University of Paris, 1600–1740. *Annals of Science* 38: 33–69.

Brundell, Barry. 1987. *Pierre Gassendi*. Dordrecht: Reidel.

Clark, William. 1992. The scientific revolution in the German nations. In *The scientific revolution in national context*, ed. Roy Porter and Mikulas Teich, 90–114. Cambridge: Cambridge University Press.

Cohen, H.Floris. 1994. *The scientific revolution: A historiographical inquiry*. Chicago: University of Chicago Press.

Cohen, H.Floris. 2010. *How modern science came into the world: Four civilizations, one 17th century breakthrough*. Amsterdam: Amsterdam University Press.

Collins, Harry. 1985. *Changing order*. London: Sage.

Cunningham, Andrew. 1988. Getting the game right: Some plain words on the identity and invention of science. *Studies in History and Philosophy of Science* 19: 365–389.

Cunningham, Andrew. 1991. How the *Principia* got its name; or, taking natural philosophy seriously. *History of Science* 24: 377–392.

Cunningham, A., and P. Williams. 1993. De-centring the "big picture": The origins of modern science and the modern origins of science. *British Journal for the History of Science* 26: 407–432.

Dear, Peter. 1991. The church and the new philosophy. In *Science, culture and popular belief in Renaissance Europe*, ed. S. Pumfrey, P.L. Rossi, and M. Slawinski, 119–139. Manchester: Manchester University Press.

Dear, Peter. 1995. *Discipline and experience: The mathematical way in the scientific revolution*. Chicago: Chicago University Press.

Dear, Peter. 2001a. Religion, science and natural philosophy: Thoughts on Cunningham's thesis. *Studies in History and Philosophy of Science* 32: 377–386.

Dear, Peter. 2001b. *Revolutionizing the sciences: European knowledge and its ambitions 1500–1700*. Princeton: Princeton University Press.

Des Chene, Dennis. 1996. *Physiologia. Natural philosophy in late Aristotelian and Cartesian thought*. Ithaca: Cornell University Press.

Dun, Liu, and Wang Yangzong (eds.). 2002. *Chinese science and the scientific revolution: Selected writings on the Needham problem and related issues*. Shenyang: Liaoning Education Publishing House.

Easlea, Brain. 1980. *Witch-hunting, magic and the new philosophy: An introduction to the debates of the scientific revolution 1450–1750*. Sussex: Harvester Press.

Eisenstein, Elizabeth. 1979. *The printing press as an agent of change: Communications and cultural transformations in early-modern Europe, vol. 1*. Cambridge: Cambridge University Press.

Feyerabend, P.K. 1975. *Against method*. London: New Left Books.

Foucault, Michel. 1969 [1972]. *The archaeology of knowledge* (trans: Sheridan, Alan). London: Tavistock.

Frank, Robert G. 1980. *Harvey and the English physiologists: Scientific ideas and social interaction*. Berkeley: University of California Press.

Friedrich, Carl J. 1962. *The Age of the Baroque: 1610–1660*. New York: Harper & Row.

Gal, Ofer, and Raz Chen-Morris. 2010. Baroque optics and the disappearance of the observer: From Kepler's optics to Descartes' doubt. *Journal of the History of Ideas* 71: 191–217.

Gascoigne, John. 1990. A reappraisal of the role of the universities in the scientific revolution. In *Reappraisals of the scientific revolution*, ed. David Lindberg and Robert Westman, 207–260. Cambridge: Cambridge University Press.

Gaukroger, Stephen. 1995. *Descartes: An intellectual biography*. Oxford: Oxford University Press.

Gaukroger, Stephen. 2006. *The emergence of a scientific culture*. Oxford: Clarendon.

Gaukroger, Stephen W., and John A. Schuster. 2002. The hydrostatic paradox and the origins of Cartesian dynamics. *Studies in History and Philosophy of Science* 33: 535–572.

Gieryn, Thomas. 1983. Boundary-work and the demarcation of science from non-science: Strains and interests in professional ideologies of Scientists. *American Sociological Review* 48: 781–795.

Harrison, Peter. 2000. The influence of Cartesian cosmology in England. In *Descartes' natural philosophy*, ed. S.W. Gaukroger, J.A. Schuster, and J. Sutton, 168–192. London: Routledge.

Harrison, Peter. 2002. Voluntarism and early modern science. *History of Science* 40: 63–89.

Harrison, Peter. 2005. Physico-theology and the mixed sciences: The role of theology in early modern natural philosophy. In *The science of nature: Patterns of change in the culture of natural philosophy in the seventeenth century*, ed. P. Anstey and J. Schuster, 165–183. Dordrecht: Springer.

Hattab, Helen. 2005. From mechanics to mechanism: The *Quaestiones Mechanicae* and Descartes' physics. In *The science of nature in the seventeenth century: Changing patterns of early modern natural philosophy*, ed. Peter Anstey and John Schuster, 99–129. Dordrecht: Springer.

Hazard, Paul. 1935. (Eng. Trans 1952, 1963) *The European mind 1680–1715* (*La Crise de la conscience Européenne*). New York: World Publishing Co.

Henry, John. 2002. *The scientific revolution and the origins of modern science*, 2nd ed. London: Macmillan.

Knorr-Cetina, Karin. 1981. *The manufacture of knowledge: An essay on the constructivist and conventional character of knowledge and cognition*. Oxford: Pergamon.

Kuhn, Thomas S. 1970. *The structure of scientific revolutions*, 2nd ed. Chicago: University of Chicago Press.

Laird, W.R. 1986. The scope of Renaissance mechanics. *Osiris* 2: 43–68.

Latour, Bruno, and Steve Woolgar. 1979. *Laboratory life, The social construction of scientific facts*. London: Sage.

Lenoble, Robert. 1943. *Mersenne ou la naissance du mécanisme*. Paris: J. Vrin.

Lindberg, David. 1976. *Theories of vision: From Al-Kindi to Kepler*. Chicago: University of Chicago Press.

Lindberg, David. 1992. *The beginnings of western science*. Chicago: University of Chicago Press.

Maclean, Ian. 2007. *Logic, signs and nature in the Renaissance*. Cambridge: Cambridge University Press.

Maravall, José Antonio. 1973. *The culture of the Baroque: Analysis of a historical structure* (Eng Trans, 1986). Minneapolis: University of Minnesota Press.

Mulkay, Michael. 1979. *Science and the sociology of knowledge*. London: George Allen & Unwin.

Pirenne, Henri. 1936. (Eng trans 1939) *A history of Europe from the invasions to the XVI century*. London: George Allen & Unwin.

Popkin, Richard. 1964. *The history of scepticism from Erasmus to Descartes*. New York: Harper & Row.

Rabb, Theodore K. 1975. *The struggle for stability in early modern Europe*. New York: Oxford University Press.

Rabb, Theordore K. 2006. *The last days of the Renaissance & the march to modernity*. New York: Basic Books.

Rattansi, P.M. 1963. Paracelsus and the Puritan revolution. *Ambix* 11: 24–34.

Rattansi, P.M. 1964. The Helmontian-Galenist controversy in seventeenth century England. *Ambix* 12: 1–23.

Ravetz, J.R. 1971. *Scientific knowledge and its social problems*. Oxford: Clarendon.

Ravetz, J.R. 1974. Science, history of, In *Encyclopaedia Britannica*, 15th Edition, *Macropedia*, vol 16, pp. 366–72. Chicago: Encyclopaedia Britannica Inc.

Reif, Patricia. 1969. The textbook tradition in natural philosophy, 1600–1650. *Journal of the History of Ideas* 30: 17–32.

Richards, Evelleen, and John A. Schuster. 1989. The myth of feminine method: A challenge for gender studies and the social studies of science. *Social Studies of Science* 19: 697–720.

Rose, Paul Lawrence, and Stillman Drake. 1971. The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance culture. *Studies in the Renaissance* 18: 65–104.

Rossi, Paolo. 1970. *Philosophy, technology and the arts in the early modern era*. New York: Harper and Row.

Sahlins, Marshall. 1993. Goodbye to *Tristes Tropes*: Ethnography in the context of modern world history. *The Journal of Modern History* 65: 1–25.

Sartre, Jean-Paul. 1963. *Search for a method*. New York: Alfred Knopf, Inc.

Schaffer, Simon. 1986. Scientific discoveries and the end of natural philosophy. *Social Studies of Science* 16: 387–420.

Schmitt, Charles. 1973. Towards a reassessment of Renaissance Aristotelianism. *History of Science* 11: 159–193.

Schuster, John A. 1979. Kuhn and Lakatos revisited. *British Journal for the History of Science* 12: 301–317.

Schuster, John A. 1984. Methodologies as mythic structures: A preface to the future historiography of method. *Metascience* 1–2: 15–36.

Schuster, John A. 1986. Cartesian method as mythic speech: A diachronic and structural analysis. In *The politics and rhetoric of scientific method: Historical studies*, ed. John A. Schuster and Richard R. Yeo, 33–95. Dordrecht: Reidel.

Schuster, John A. 1990. The scientific revolution. In *The companion to the history of modern science*, ed. R. Olby et al., 217–242. London: Croom Helm.

Schuster, John A. 1993. Whatever should we do with Cartesian method: Reclaiming Descartes for the history of science. In *Essays on the philosophy and science of René Descartes*, ed. S. Voss, 195–223. Oxford: Oxford University Press.

Schuster, John A. 1995a. Descartes *Agonistes* new tales of Cartesian mechanism. *Perspectives on Science* 3: 99–145.

Schuster, John A. 1995b. An introduction to the history and social studies of science. (Open Learning Australia, 1995). On open access at http://descartes-agonistes.com.

Schuster, John A. 1995c. The scientific revolution: An introduction to the history and philosophy of science (Open Learning Australia,1995). On open access at http://descartes-agonistes.com.

Schuster, John A. 2000a. Descartes 'Opticien': The construction of the law of refraction and the manufacture of its physical rationales, 1618–29. In *Descartes' natural philosophy*, ed. Stephen Gaukroger, John Schuster, and John Sutton, 258–312. London: Routledge.

Schuster, John A. 2000b. Internalist and externalist historiographies of the scientific revolution. In *Encyclopedia of the scientific revolution*, ed. W. Applebaum. New York: Garland Publishing.

Schuster, John A. 2002. 'L'Aristotelismo e le sue Alternative.', In *La Rivoluzione Scientifica*, ed. D. Garber, 337–357. Rome: Instituto della Enciclopedia Italiana. English translation available at http://descartes-agonistes.com.

Schuster, John A. 2005. Waterworld: Descartes vortical celestial mechanics and cosmological optics—A gambit in the natural philosophical Agon of the early seventeenth century. In *The science of nature in the seventeenth century: Patterns of change in early modern natural philosophy*, ed. P. Anstey and J.A. Schuster, 35–79. Dordrecht: Springer.

Schuster, John A., and Alan B.H. Taylor. 1996. Seized by the spirit of modern science. *Metascience* 9: 9–26.

Schuster, John A., and Alan B.H. Taylor. 1997. Blind trust: The gentlemanly origins of experimental science. *Social Studies of Science* 27: 503–536.

Schuster, John A., and Graeme Watchirs. 1990. Natural philosophy, experiment and discourse in the eighteenth century: Beyond the Kuhn/Bachelard problematic. In *Experimental inquiries: Historical, philosophical and social studies of experiment*, ed. H.E. LeGrand, 1–48. Dordrecht: Reidel.

Schuster, John A., and Richard R. Yeo (eds.). 1986a. *The politics and rhetoric of scientific method: Historical studies*. Dordrecht: Reidel.

Schuster, John A., and Richard R. Yeo. 1986b. "Introduction" to Schuster and Yeo 1986, pp. ix–xxxvii.

Shapin, Steven. 1992. Discipline and bounding: The history and sociology of science as seen through the externalism-internalism debate. *History of Science* 30: 333–369.

Toulmin, Stephen. 1990. *Cosmopolis. The hidden agenda of modernity*. New York: The Free Press.

Chapter 3
"Bent and Directed Towards Him": A Stylistic Analysis of Kircher's Sunflower Clock

Koen Vermeir

Est Haec, ut Clytie, studiosa Pedissequa SOLIS*;*
Sol DEUS est, SOLIS *Lumen* AMANTIS *amat.*

Edward Benlowes, Theophila

Abstract In this article, I explore the potential of style concepts, and especially the concept of the baroque, for the history of science. I argue for a pragmatic theory of style that avoids the traditional problems of essentialist or idealist style concepts. A pragmatic style concept is very useful for describing larger cultural structures, based on resemblances between different practices, especially if evidence of concrete circulations of knowledge is lacking. Style concepts such as the 'baroque' are not only relevant for discerning large scale structures, but they can also be an indispensable tool for historians of science to make sense of particular practices or objects. I illustrate this by analysing one of the most striking marvels of the baroque: a clock made from a sunflower plant. The historiography has analysed this object as part of the controversy around copernicanism. In order to come to grips with this object, however, it is important to embed it in its baroque context. From studying the meanings of clocks, magnetism and sunflowers in different practices, certain resemblances come to the fore. These resemblances point at a broader 'baroque culture', which in its turn helps us to better contextualise and understand the sunflower clock.

The historical argument in this paper was first presented at the XXVI Symposium of the Scientific Instrument Commission, Harvard University & MIT, Cambridge, Massachusetts, 6–11 September 2007.

K. Vermeir (✉)
Laboratoire SPHERE (UMR 7219), 5 rue Thomas Mann - Case 7093,
75205 Paris Cedex 13, France
e-mail: koen.vermeir@univ-paris-diderot.fr

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_3,

Kircher's Sunflower Clock Reassessed

In 1673, Nathaniel Wanley described in his popular book *The wonders of the little world* some of the most curious objects of his age: a magnetic clock, and clocks made from a sunflower plant or a sunflower seed which, put in a basin, would follow the movements of the sun.

> There was at Leige, Ann. 1635. a Religious, and industrious man of the Society of Iesus, named Linus, by birth an English man, he had (saith Kircher) a Phial or Glass of Water, wherein a little Globe did float, with the twenty four letters of the Alphabet described upon it, on the inside of the Phial, was an Index or Stile, to which the Globe did turn and move it self, at the period of every hour, with that letter which denoted the hour of the day successively: as though this little globe kept pace and time with the heavenly motions. And Kircher himself, had a vessel of water in which (just even with the surface of the water) were the twenty four hours described. A piece of Cork was set upon the water, and there in were put some seeds of the Heliotrope, or Sun-flower, which like the flower it self did turn the Cork about, according to the course of the Sun, and with its motion, point out the hour of the day. (Wanley 1673, 226)

Indeed, the Jesuit philosopher and mathematician Athanasius Kircher (1602–1680) published a description and illustration of one of these curious instruments (Fig. 3.1) as part of a huge scholarly tome on magnetism, the *Magnes sive de arte magnetica opus tripartitum* (1641). There were descriptions of other wondrous instruments in the book, but this one stood out in its singularity and evoked a controversy back in the seventeenth century as well as in contemporary historical scholarship. The description of the construction of this 'heliotropic horoscope' can be paraphrased as follows:

The sunflower is attached to a big cork (ABC) and both are put in a large basin filled with water. They are fastened to a pivot in the middle so that the cork and the sunflower can turn around it, as if around an axis, easily and without obstructions. Make sure, however, to use a sunflower that is imbued with a high efficaciousness and power. Around the stem and root one can fasten woollen bandages, which, when let into the water, absorb it and feed the plant, so that the sun does not dry it up. By daybreak then, the author explains, one should expose the machine to the open air and direct the face of the flower to the face of the sun. Since nothing can cause resistance anymore, the sun will turn the flower with ease, according to its own movement, by attracting it. In this way, the author claims, the flower circles the hours and a pointer (F) attached to the centre of the flower (T) will indicates the hour on the clock dial (DE) which is constructed around the flower (Kircher 1641, 736–37) (Fig. 3.1).

Kircher argued that his newly invented clock indicated the time in an optimal way, because the pivot allowed the flower to turn unhampered towards the magnetic attraction of the sun. Heliotropism, Kircher thought, was a special instance of vegetal magnetism. This meant that the clock would even work at night, contrary to the common sundial, because the magnetic influence of the sun was not weakened by material barriers. On the other hand, Kircher explained, there were also some disadvantages with the clock. A slight breeze would quickly distort its movement, for instance. Furthermore, this vegetal clock would only last about a month before

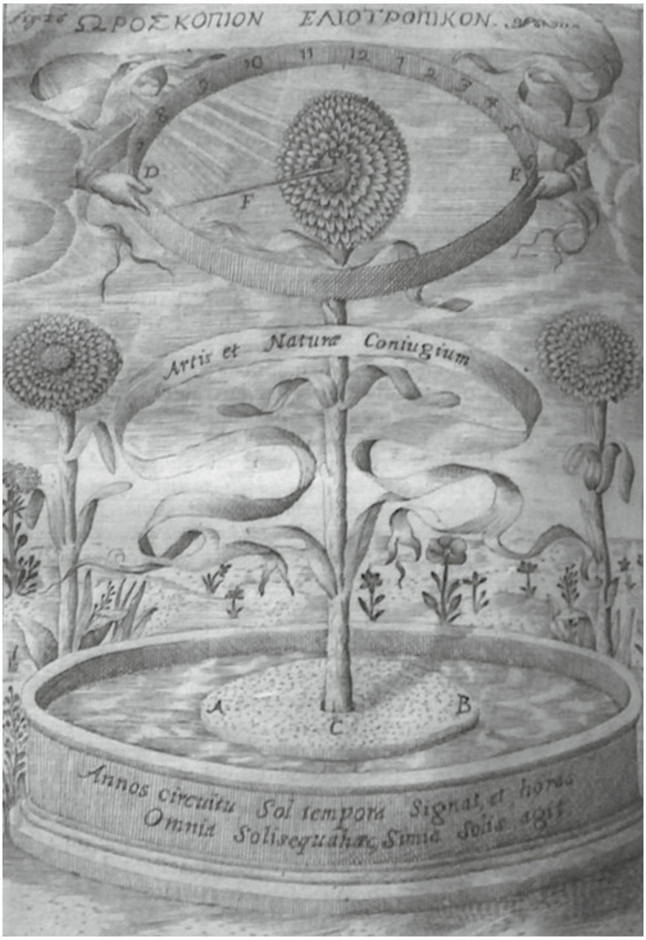


Fig. 3.1 Athanasius Kircher’s ‘botanical horoscope’ or ‘heliotropic horoscope’ (or ‘sunflower clock’) (Kircher 1641, 736) (Courtesy K.U. Leuven, Maurits Sabbebibliotheek)

withering away. Therefore, Kircher also proposed other clocks that worked by means of a similar ‘magnetic’ principle.

One clock worked by virtue of some kind of heliotropic material purchased from a mysterious Arab in 1633 that Kircher propped upon a cork, similar to his ‘botanical horoscope’ described above. Accounts of Kircher’s previous experiments and demonstrations as described in letters by contemporaries indicate that, from 1633 onwards, he had been experimenting with heliotropic roots and sunflower seeds, both of which supposedly turned towards the sun by means of a magnetic principle.[1] These clocks were better than a full size sunflower clock because they were handy and could be protected by a glass case. On the other hand, the magnetic virtue inherent in the vegetable kernel of these clocks was often affected by the water in which

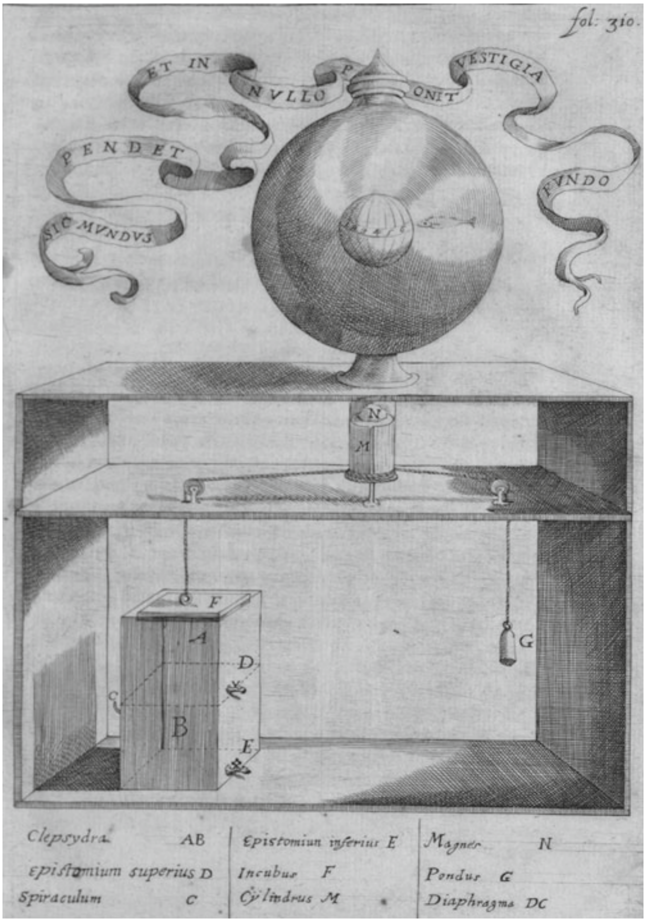


Fig. 3.2 The hidden mechanism behind Linus' and Kircher's clock. (Kircher 1641, 310) (Courtesy K.U. Leuven, Maurits Sabbebibliotheek)

it lay, and the clocks only lasted a few hours. Kircher again devised a new kind of clock, this time functioning by means of a magnet. Magnets, Kircher suggested, also turned with the sun because of an inherent 'sun-turning' (or 'heliotropic') property. These magnetic clocks were imperishable and he noted that they were described in the section on magnetic natural magic in the same book (Kircher 1641, 309–12 and 342–56).

If we look up this section, however, a hidden mechanism behind the magnetic clock is exposed (Fig. 3.2). Instead of a celestial magnetism, a water clock caused a magnet to revolve underneath the table, which made in its turn rotate the magnet in the glass globe. If his clocks made from sunflowers, sunflower seeds or other heliotropic roots were constructed by similar trickery, what are we to believe of Kircher's account of celestial and heliotropic magnetism? Should we believe Kircher's statement that he had constructed a clock out of a sunflower, much to the admiration and

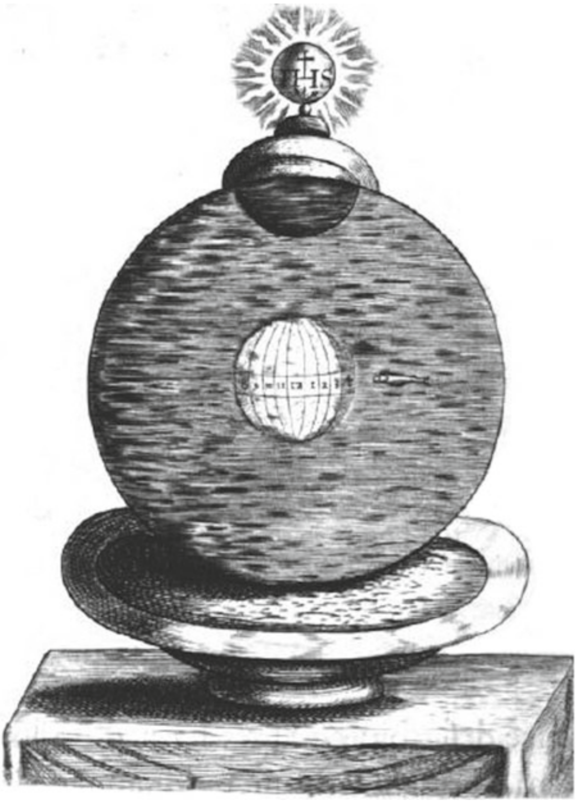


Fig. 3.3 Francis Linus’ clock as represented by the Jesuit Silvester Pietrasancta in his *De Symbolis Heroicis* (Pietrasancta 1634, 146) (Courtesy K.U. Leuven, Maurits Sabbebibliotheek)

delight of the spectators?[2] If he really demonstrated such an instrument, did he intend it to be just a playful trick, or were his demonstrations meant to be meaningful in another way?[3]

Many historians have tried to come to terms with Kircher’s instruments. One of the latest innovative assessments of Kircher’s sunflower clock was made by Hankins and Silverman, who brilliantly elaborated on the results of previous work by Monchamps, Drake, Hine, Baldwin and Bedini.[4] Like these authors, Hankins and Silverman focus on the reception of Kircher’s clock in the republic of letters, in order to come to a better understanding of its meaning. In 1633, the famous scholar Claude Fabri de Peiresc had witnessed some of Kircher’s demonstrations with vegetal and magnetic clocks in Avignon, and the mathematician Francis Linus, a fellow Jesuit, had constructed a similar magnetic clock in Liège (Fig. 3.3). At some moments in his examination of such clocks, Peiresc had suspected that there was trickery involved. One of Kircher’s clock devices turned out to be nothing more than an ordinary compass, and to make it indicate the right time, one had to adjust it manually all the time.[5]

On the other hand, Peiresc was also fascinated by the theoretical implications of such an instrument and wanted to believe in its existence, or at least in its possibility. He thought that these clocks could be used in support of the heliocentric theory and he hoped to employ them in his attempts to get clemency for Galileo. Peiresc set into motion his scholarly network and corresponded with Gassendi, Mersenne, Rubens and others on this issue. Galileo himself was less taken in by this idea than Peiresc and politely indicated to him that in all probability there was a hidden mechanism. Descartes, informed by an enthusiastic Mersenne, reacted at first rather sceptical, although he did not judge the effect impossible. Years later, he would write to Huygens that clocks driven by sunflower seeds were merely tricks, as his own attempts at constructing one had been unsuccessful (Drake 1967, 45–56; Hankins and Silverman 1995, 14–36).

Hankins and Silverman argue that the clocks were not primarily meant as 'clocks'. They explain that in building and demonstrating his sunflower seed and magnetic clocks Kircher tried to 'illustrate' (by trickery) the effects of a natural celestial magnetism. Gilbert had supported Copernican theory by arguing that the earth turned around the sun because of a magnetic interaction. According to Hankins and Silverman, Linus' and Kircher's clocks were constructed to demonstrate the earth's motion, 'for if the little sphere inside the liquid-filled glass globe rotated by a magnetic influence, then by analogy the sphere of the earth at the center of the celestial sphere would likewise rotate by its magnetism as Gilbert had claimed' (Hankins and Silverman 1995, 22). Linus and Kircher knew that it did not work in practice, but their demonstration was a demonstration 'by analogy'. It was only an illustration, not an experimental proof. Hankins and Silverman argue that Kircher was forced to reveal the trick behind the hoax magnetic clock of Linus, a fellow Jesuit, because the heliocentric doctrine had been convicted in 1633. He now had to undermine any support for the hypothesis that the sun moved the planets and the earth by magnetic forces.

Hankins and Silverman definitively establish, by means of a close study of Peiresc's manuscripts, that Peiresc viewed Kircher's clock as a possible demonstration of Copernicanism, but their interpretation runs into incongruities when they ascribe this as a purpose to Kircher.

First, if after Galileo's condemnation Kircher had to expose Linus's artifice, because it could be used in defence of copernicanism, it would be contradictory that he would construct and demonstrate his own analogous clocks in 1633 and would also still present them a decade later in print.[6] Furthermore, Kircher gave his sunflower clock, supposedly running by a celestial magnetism, a prominent place in his book even if in other passages he explicitly argued against Gilbert's magnetic defence of Copernicanism. In the *Magnes*, Kircher explicitly objected to extrapolations from small magnets to planetary bodies, and he denied that the earth was a big magnet.[7] Hankins and Silverman admit the problem: 'Kircher repudiated the moving earth and continued to demonstrate his sunflower clock […] without any apparent conflict' (Hankins and Silverman 1995, 33). As I will argue below, Kircher's sunflower clock involved a different kind of analogy.

Second, Kircher's sunflower clock or Linus' magnetic clock could not directly be used in an analogical argument for heliocentrism. Indeed, if the earth would turn around like the sunflower or like the magnet in the clock, the same spot of the earth would always be directed towards the sun (like the flower of the sunflower). In that case, the sun would stand still in the sky causing perennial day and night on the two opposite sides of the earth.[8] The analogy also did not work for another magnetic clock described above. Here, the hour hand was fixed (to the north) and the circle of hours had to be turned. If this clock would have been interpreted by contemporaries in analogy with the cosmos, the earth (magnet) would be stationary while the heavenly spheres would turn (Kircher moved that part of the device with his hand). (Hankins and Silverman 1995, 23–5).

Third, Hankins and Silverman assume that Kircher would have preferred to keep the mechanism of Linus’ clock secret and that the exposal was enforced and an unfortunate exception. They suggest that Kircher almost never exposed the real workings of his instruments. In fact Kircher explained the hidden tricks behind many of his apparatus. Kircher’s clocks belong to the tradition of natural and artificial magic, and he knew well the different ways of creating wonder.

Fourth, although I can see some plausibility in Hankins and Silverman’s idea that Kircher and Linus demonstrated a natural phenomenon by analogy, I find it less plausible that they would go so far as to defend Copernicanism – a contentious theory also in the years before Galileo’s condemnation – by means of illusion and trickery.[9] Furthermore, at various points in the *Magnes*, Kircher pointed out that one should not go too far in making analogies between magnetism and other natural phenomena, especially in cosmology.

In this paper, I will take a different approach than Hankins and Silverman. Instead of providing a micro study of the immediate reception of Kircher’s clock, I will look at broader cultural connections and interactions. I will propose a ‘stylistic analysis’ in order to uncover other meanings than those accessible to a contextual analysis in the narrow sense.[10] In contrast to the current style concepts in the history of science (e.g. Ian Hacking’s ‘styles of reasoning’; Hacking 2002), what I call a ‘stylistic analysis’ is a bottom up approach, which looks at concrete similarities between different practices.[11] A first aim of the paper is to develop a basic notion of style and to test the fruitfulness of a stylistic analysis for the history of science. A second aim is to use this method to shed new light on the interpretative problems posed by the particular example of the sunflower clock. A third aim is to contribute to explorations of the notion of the baroque in the history of science.

The Baroque Style

The Problem of Style

The concept of style has an intricate history (Ginzburg 1998). Our ideas about style are predominantly influenced by the theories of the great art historians of the nineteenth century, such as Burckhardt and Wölfflin. These were the great ‘discoverers’ of unified styles, such as the renaissance and the baroque. They treated a period in its entirety, and attempted to integrate its artistic expressions with other social and cultural forms. Heirs of an idealist philosophy, they posited something like a *Zeitgeist* that determines the essence of a period and which can only be intuited by the enlightened thinker. In 1788 already, Johann Forkel had declared that there exists a 'spirit' or 'manner of thought' that determines the characteristics of an epoch (Forkel 1788, II, 696). This idealist 'spirit of the age' has become unintelligible for us, however, and it is unclear how the contemporary historian could integrate it in his work (Mueller 1954, 431–2).

It is impossible to develop a full-blown theory of style here, but in order to make possible a fit between the art historian's notion of style and the current historiography of science, some conceptual work needs to be done (cf. Wessely 1991), and I will briefly sketch my approach in the following paragraphs. The classical idealist concept of style agrees badly with the mainstream of current historiography of science, which is based on the notion of scientific practice.[12] The most profound development of the notion of practice originates in the philosophy of Wittgenstein, which has continued to inspire historians and philosophers of science.[13] I believe that Wittgenstein's philosophy can be a new starting point for us to think about style in such a way that style concepts can become useful tools for the historiography of the sciences.[14]

On the one hand, Wittgenstein hardly writes about style, and it is only from some scattered fragments that we can deduce that he was interested in styles. Wittgenstein thought that style was important and that there was an 'aesthetic' dimension to all human practice.[15] In the following, I will follow up on this view by suggesting that all practices have a style (although not necessarily in an aesthetic sense). On the other hand, Wittgenstein's typical thought experiments and abstracted examples seem to eliminate style elements in his descriptions of practices. Therefore, I will go beyond Wittgenstein's writings in order to be able to give the onset of a style concept that can be integrated in the historiography of scientific practices.

I suggest that we understand style as a *way of doing* something. This corresponds to a common sense idea of what style is about. It is a way of performing a certain practice. When a practice consists in doing something, a style refers to the way of doing it.[16] There are different styles of painting, dancing, cooking, driving, walking, and so forth. A difference in style thus refers to a particular *variation* of a practice. First, what is important is that a change in style does not change a practice into a different practice. Different ways of doing something are still ways of doing *that* thing, ways of performing *that* practice. Such variations do not challenge the identity or identification of a practice. Second, different practices can *share* the same style. On a different level, therefore, we express similarities between ways of performing different practices also by means of style concepts. Such style concepts are about inter-praxis comparisons and connect different practices. Style, therefore, does not only refer to a particular way of doing something, on a more general level, it can also refer to a set of similarities in style elements between different practices. In order to distinguish between the similarities that constitute a practice (Wittgenstein's 'family resemblances'), I refer to the similarities that constitute a style as 'resonances'. While the first kind of similarities is about *doing similar things*, the second kind is about *similar ways of doing different things*.[17]

Finally, one can find styles on any level of analysis, from a particular way of singing to an epochal style such as the 'baroque'. What kind of style we are referring to will have to be clear from the context. While a narrower style concept refers to variations within a practice, style in a more extended sense is transversal to practices, it involves relations between practices. It is important to note, however, that what counts as a 'style' and what counts as a 'practice' often depends on the historian's perspective and on the questions the historian wants to solve. There is, therefore, a wide variety of elements that may characterise a style or a practice.[18] 'Practice' and 'style', as I use the terms, are therefore certainly not 'essentialist' categories but their application depends on the particular context.[19]

Style concepts imply a certain flexibility as well as invariance. The similarities and resonances between practices allow for variety, but in order to identify a style and associate a certain concept (such as 'baroque', 'romantic') with it, these similarities need to be relatively stable. An occasional resonance between two practices does not yet constitute a style. In order to be able to identify a style, multiple instances of such resonances have to be found, and they need to be embedded in broader networks of similarities. These similarities have often been reified by the historical actors into conventions or 'rules'. Such rules, of course, do not necessarily determine a practice, but as relatively stable style elements, they can help us in discerning intra and inter-praxis relations. In the arts, certain techniques, such as the dynamical relations between figures, the use of light, shadow and colour or symbolic references can come to constitute a set of 'rules', that are more or less stringent, that get transmitted and lend stylistic coherence to a certain body of work. Sometimes, these conventions are even explicitly formulated in a set of formal rules, which can be normative for centuries (cf. the debate between the ancients and the moderns). We should not forget, however, that actual practices are often relatively independent of such theoretisations and reifications in 'rules' and 'characteristics'. In order to understand actual styles, we should pay close attention to the ways concrete practices are performed.

The pragmatic notion of style that I am proposing here is in close agreement with the theory of style proposed long ago by James Ackerman. Styles are not discovered, according to Ackerman, but are created by the historian in a process of abstraction.[20] The pragmatic aspect of his theory becomes particularly clear when he treats the concept of style as a tool, and when he dismisses the question "what is style?" in favour of a definition of style that 'provides the most useful structure for the historian of art'. Style concepts do not refer to 'historical essences' but are a tool for historians to make sense of a lot of information and to integrate different practices in larger structures (cf. Ackerman 1962, 228). In particular, Ackerman sees the concept of style as a means of establishing relationships between different works of art. I would shift Ackerman's focus on works of art to (artistic and other) *practices*, stressing the pragmatist approach as well as broadening the concept of style and opening it up for other disciplines.

Talking about practices also implies making reference to social groups, conventions and institutions. This will enable us to avoid an idealist framework and to relate our new style concept to social structures.[21] Because of their transversal character, styles will refer to different kinds of social groups than practices.[22] One of the difficulties intrinsic to the Wittgensteinian approach is the delineation of these practices and social groups. In this case, this can be seen as a strength as well as a weakness. It gives the pragmatic notions of 'style' and of 'practice' a flexibility so that they can be applied to micro-historical levels of analysis as well as to large structures. Just like the notion of practice, which can span analytic units of different size (e.g. from the practice of 'chiselling a lens' to 'mathematics' in general), the concept of style can be used to describe, for instance, a particular style of writing as well as large stylistic concepts such as the baroque.

The Baroque Problem

The baroque is a very controversial instantiation of a style. The term is considered to be problematic not only in current day art history; scholars from the 1950s already referred to 'the baroque problem'.[23] Oppenheimer remarks: 'even the art historians admit that they made a mess of the term' (Oppenheimer 1951, 259). The main problem is that it seems to be impossible to reach agreement about what the baroque is. For every paper that proposes a new definition, there is another one that contradicts it, leading many scholars to conclude that the concept better be abandoned (Menashe 1965; Parr 2001). Menashe remarks that while one author sees the baroque as a moderate, rational and reflective style, balanced and harmonious, another proclaims that the baroque was irrational and emotional (Menashe 1965, 334). Ever new reassessments of the 'the baroque problem' have been published, and the most recent was announced when this article went into print (Hills 2011).

Bernard Heyl has a healthy view on the matter (Heyl 1961). He argues that attempts at finding a unitary and totalizing style, which can be distinguished in any expression of the period, have failed. According to Heyl, attempts at restricting the baroque to only one feature, such as a 'dynamic drive', a 'crisis of thought', a fusion of 'simplicity and excess', a 'paradoxical fusion between rationality and irrationality', or a 'vision of the infinite', are also misguided.[24] Reducing the baroque to one characteristic, or to one specific period of time, is untenable, undesirable, and in any case, scholars will not be able to reach agreement on what this one characteristic or delineated period should be. In response, some scholars have abandoned such large categories and they only deem it legitimate to refer to specific small trends. This is not a solution either. Many historians and art historians will insist that we need concepts that help us to provide a 'bigger picture' (for the history of science, see Secord 1993).

A pragmatic theory of meaning would deny the presence of a precise and simple referent of 'the baroque'. This might be annoying, but we should come to understand that it is inevitable. The meanings of a concept (such as 'baroque') are constituted by the various uses by different historians, and its imprecision and complexity are inherent to it. Even the attribution of contradictory attributions to a concept does not necessarily make it illegitimate. The legitimacy and fruitfulness of certain uses of the concept of the baroque will therefore have to be borne out in specific historical studies. Furthermore, the baroque as well as other style concepts are often implicit guiding ideas for historians and may inform their work unwittingly (cf. Findlen 2003). It is thus important to make explicit the problems and opportunities presented by such style concepts.

The usefulness of style concepts is further confirmed by the remarkable skill of connoisseurs and art historians in recognising personal styles as well as broader styles (such as 'baroque') in practice, often with an astonishing accuracy.[25] They are able to recognize such a style without necessarily being able to describe conclusively its distinguishing features. Indeed, it is a centrepiece of Wittgenstein's philosophy that we often recognize something without being able to define or even describe it. Furthermore, for Wittgenstein, aesthetic questions are very different from empirical questions, and trying to find essentialist definitions of aesthetic categories will only cloud the important issues (Hagberg, 2007).

The second major problem, specific to the notion of the baroque, is that 'baroque' is not an actor's category. Just this fact does not render the category illegitimate, however.[26] It is true that actor's categories are often a good way to comprehend past practices, because they point to the distinctions and meanings as understood by the historical actors.[27] On the other hand, the unreflective use of actor's categories, especially if these are controversial terms, might cause one to accept unthinkingly the ideological positions of the actors. One should be very careful, for instance, in using categories used in a historical controversy as historiographical categories for analysing the relevant practices.

It has often been remarked on that there is a tension between the categories and theoretical principles defended by early modern theoreticians and actual artistic practices in the seventeenth century. Classical principles were still expounded, and the authority of the ancients was still unchallenged (Sayce 1985, 251). This adherence to Classicism should be understood on a rhetorical level, however, as lip service to the authority of classical norms that were still upheld, because at the same time, these principles were frequently being violated in practice. Many debates of the period, e.g. the controversy between the ancients and the moderns, or controversies about the legitimate forms of opera and theatre, can be seen as an effect of this tension. In particular if there is a tension between theoretical formulation and contemporary practice, analyst categories – such as the baroque – will be useful for characterising it.

The question remains how the notion of the baroque can be used in a fruitful way. The baroque, understood as a selection of a stable set of resonances between style elements in different practices, has a descriptive and an explanatory function. First, the baroque can be used as shorthand for the resonances that we find important and interesting. According to a Wittgensteinian approach, we should identify paradigmatic cases of 'baroqueness', and start from there, instead of looking for definitions.

In this paper, I will start with the sunflower clock (and Kircher's work in general), as a paradigmatic case of 'baroque' knowledge practices.[28] We should keep in mind that notions such as the baroque are heuristic tools that serve to make certain points, arguments and connections more clearly. Furthermore, different notions of the baroque – on a smaller or a larger scale – might be employed, depending on what resonances the historian wants to focus on or depending on what questions he or she desires to solve. One could also make a distinction between sub-styles, such as a northern and southern baroque, for instance. These different conceptions of the baroque will have to be clarified (but not necessarily defined) in particular cases. Their fruitfulness for the history of science is still to be explored.[29] We need a close study of the similarities between style elements or of sets of conventions that are recurrent in different practices, related to specific social groups, and of the means used to establish and transmit rules, norms and authority, to gain a fuller understanding of the baroque.

Second, the baroque also has an explanatory function. The resonances that we single out by the notion of the baroque probably exist because of concrete interactions and exchanges between the practices in question. The older style concept explained these similarities by means of an unintelligible essence, *Zeitgeist* or mentality. I would see these similarities, excluding those that arose by accident or as an illusion of perspective, as the result of very concrete circulations of knowledge. These circulations are not always traceable, however. One of the purposes of style terms like 'baroque' would be to help to establish plausibilities of interaction and influence between different practices at a stage when there is not enough data available to ascertain actual knowledge circulations.[30] Indeed, a sensibility to stylistic issues will allow us to frame hypotheses of circulation between different practices. Apart from exchanges of knowledge, similarities between certain 'ways of doing' of course also depend on similarities between material constraints and socio-economic conditions between different practices, which will have to be clarified. Style concepts, then, will be fruitful heuristic tools that can point to unexpected interactions or to similarities in material and socio-economic conditions between different practices.

A Stylistic Analysis

The aim of the final part of this paper is twofold. First, I will pursue a stylistic analysis of the sunflower clock, and by extension of Kircher's practices as an early modern polymath, hoping to better elucidate their meanings. Second, I will inquire into how this bottom-up stylistic analysis relates to the 'baroque'. A stylistic account gives us insights into different ways of doing (early modern) mathematics and natural philosophy. In order to see and understand these variations better, we should look at resonances with other practices and domains. This is therefore not an analysis of the immediate context of this clock, but we will embed the sunflower clock in a much broader cultural setting, by pointing at connections with many different practices and by integrating them into a broader 'baroque' style.

It is important to recognise in this respect that the sunflower clock embodies a specific way of doing natural philosophy and mathematics. Kircher published a description and illustration of his sunflower clocks in a volume on mixed mathematics and natural philosophy. As a hypothesis, one could venture that Kircher’s work is indicative of a ‘Jesuit style’ of natural philosophy and mathematics. In order to explore this hypothesis, we should make comparisons with other Jesuit practices outside mathematics and natural philosophy, and we should contrast the ‘Jesuit style’ with other ways of doing mathematics and natural philosophy. Because a number of exceptions readily come to mind (Christopher Clavius as a Jesuit who does not work in Kircher’s style, for instance, and the work of Juan Caramuel y Lobkowitz, not a Jesuit, which resembles Kircher’s), I will not pursue this approach here.

As an alternative, I propose to look at the vicissitudes of one particular object and to study how it is used in different practices and how it acquires similar or different meanings. For a stylistic analysis, one should, firstly, pay close attention to variations in the use of this object in one practice (to find stylistic differences), and secondly, look at similarities in meaning and use with other practices (to find inter-praxis resonances). In the case of the sunflower clock, the variations of its meaning and use within the practice of mathematics and natural philosophy in the middle of the seventeenth century have to some extent already been developed by Hankins, Silverman and others. In my reading of the reception of Kircher’s clock by his fellow mathematicians, the reactions of Mersenne, Descartes and Galileo point at a basic misunderstanding, or better, at a different *way of doing* mathematics. This indicates that their style of mathematics and natural philosophy is different from Kircher’s. In the following paragraphs, I will explore the resonances between Kircher’s use of the sunflower clock in his own mathematical practice and in other contemporary practices.

How can we do this concretely? On the one hand, the sunflower clock (and the related magnetic clocks) is a rare object that cannot be found in many different practices. Representations of it are limited and there are only very few accounts of such clocks being demonstrated. I will therefore look at the uses and meanings of its three crucial elements: the sunflower, a clock and a magnetic interaction, which combined make the object into what it is. Because Kircher’s sunflower clock is such a surprising object, its meaning even in Kircher’s own practice is not very clear. It will therefore be useful to start with a hypothesis about the meaning of Kircher’s sunflower clock and see if this is borne out by a stylistic analysis. In contrast with existing interpretations, my hypothesis is that Kircher’s sunflower clock is neither an experiment in the ‘Baconian’ sense as e.g. propounded by the Royal Society, nor a demonstration of Copernicanism; it is rather a religious display. I think the instrument should be read as an emblem, and it is part of a typically baroque culture of emblematics and spectacular demonstrations in which everything is combined, including the sciences, in a *Gesamtwerk* to honour God and promote the Counter-Reformation. Kircher’s religious or emblematic style of mathematics and natural philosophy is therefore significantly different form the style in which Peiresc, Mersenne, Descartes and Galileo operate.

On the copperplate of the sunflower clock, printed in Kircher’s *Magnes* (Fig. 3.1), a divine light attracts the flower and God’s hands are holding the hour plate. Kircher’s sunflower seed clock also had a prominent place in the Kircherian museum at the Collegio Romano, and was mentioned on the first page of the catalogue, published in 1678. The clock, which they had put in the brightness next to a window, followed the 'Lord of the light', according to De Sepibus, hinting at the divine nature of the light (De Sepibus 1678, 1). When Pietrasancta described Francis Linus' original clock, the probable source of Kircher's own devices, he already referred to the 'arcane' driving force of the clock as 'a certain love'. What is more, his description of Linus' clock was a digression in a work on emblematics and about different ways of conveying meanings obliquely (see e.g. Fig. 3.5). The relevant section treats of diverse methods of secret writing, including a magnetic dial made for transmitting hidden messages. If we follow up on these indications, an emblematic interpretation of Kircher's sunflower clock becomes plausible if based on Kircher's own work, his immediate context and sources. In the next sections I will show that this approach is corroborated by the resonances we find with other practices, such as the literature of wonders, botanical texts, humanism, political literature, emblematic works, and devotional practices.

Clocks

Although we tend to think of clock metaphors in association with a mechanical universe, leading to deism and finally to a disenchantment of the world, in the early modern period, the concept of a clock had strongly religious overtones. As a Jesuit, Kircher knew this tradition very well. Clocks were very useful for religion, in regulating monastic life, determining the times for prayer and in calculating the religious calendar. But it would be a mistake to suppose that the primary purpose of water-clocks and sundials was to tell the time: they were part of an allegorical and religious tradition. Designed for aesthetic and religious reasons, they simulated the regular movements of the heavens and the gods (cf. Kircher 1652, Tome 1, 115; De Solla Price 1964, 13).[31] Otto Mayr has shown that mechanical clocks too were meant to mirror divine creation and to represent universality and the cosmos. Dante, for instance, used the image of a mechanical clock 'to invoke feelings of religious ecstasy that are almost erotic' (Mayr 1986, 30–32), and used it as an analogy for the Christian paradise.

Justus Lipsius (1547–1606), the great Flemish classicist, compared the workings of God with a mechanical clock: we can observe their outer blessings, without knowing or understanding their secret internal causes (Mayr 1986, 44). This interpretation prefigures a dominant baroque theme that brings the multiplicity and variation of the appearances to our attention. Clocks connected the immanent and the transcendent in embodying the uniformity of ideal movements. At the same time, the clock stood for regularity and ritual, for divine authority that brings order to human life. Life and the universe were seen as a clockwork or machine created by a divine clockmaker. The relation between art and nature was also being rethought in this period (Close 1969; Dear 1995; Weeks 2007). Significantly, the banner on

Kircher's representation of the sunflower clock presents it as 'a marriage of art and nature'. If the world was a clock, the cosmos and man-made machines had certain essential characteristics in common. Indeed, Kircher himself held that the artificer is nature's ape. The book of nature, in which God manifests himself, can be imitated in artifices. Similar anagogical techniques to read the book of nature can therefore be applied to man-made instruments, and Kircher hints at such meanings in various places (cf. Vermeir 2007 and the references given there).

Only in the seventeenth century, mechanical philosophers appropriated clocks to give a provocative meaning to the idea of a mechanical or clockwork universe. This created a tension between the old and the new interpretation of the clock which made it particularly charged with meanings and associations. The construction of vegetal and magnetic clocks, in contrast to the prevailing mechanical versions, was therefore especially pertinent in its contrast to the import placed on the mechanical clock by the mechanical philosophy. While clocks attained a particular significance, the baroque restored the allegorical and religious tradition of which they were part, connecting the immanent with the transcendent. It is therefore no surprise that Kircher prominently constructed vegetal, hydraulic and magnetic clocks, as well as various sun and moon dials, supporting renewed political and religious meanings (Fig. 3.4).[32]

Magnetism

Kircher's *Magnes* was an exhaustive compendium of magnetic knowledge. The book is subdivided into three parts: (1) on the nature and properties of the magnet, (2) on its applications in astronomy, natural magic, geography, navigation, etc., and (3) on its hidden workings throughout the world. The ten sections of part three deal with the magnetism of the earth, the planets, the stars, the elements, the different parts of the earth, the tides, the plants, medicine, music, love and God. For Kircher, all these kinds of magnetism, defining different disciplines, were interconnected, and he visualized them as linked in a magnetic chain (cf. Fig. 3.5). For my argument, the section on vegetable magnetism is particularly interesting, because it is here that the sunflower clock finds its place in print.

The baroque was contemporaneous with the age of magnetism. Although the phenomenon of magnetism was already known since ancient times, Gilbert's *De Magnete* (1600) had put magnetic phenomena into the spotlights. More and more phenomena that were difficult to understand became conceptualized in terms of magnetism. Magnetism was seen as an occult power because natural philosophers were only able to study its effects, without understanding the causes. In the seventeenth century, magnetism became a substitute for all kinds of other occult powers, such as sympathy, medical efficacy and gravity, and was used to explain the workings of the weapon salve, mummies, medical remedies and the divining rod (cf. Fludd 1638, 260). Long treatises were written to expose the magnetic structure of the cosmos. While Gilbert had treated the earth as a macrocosmical magnet,

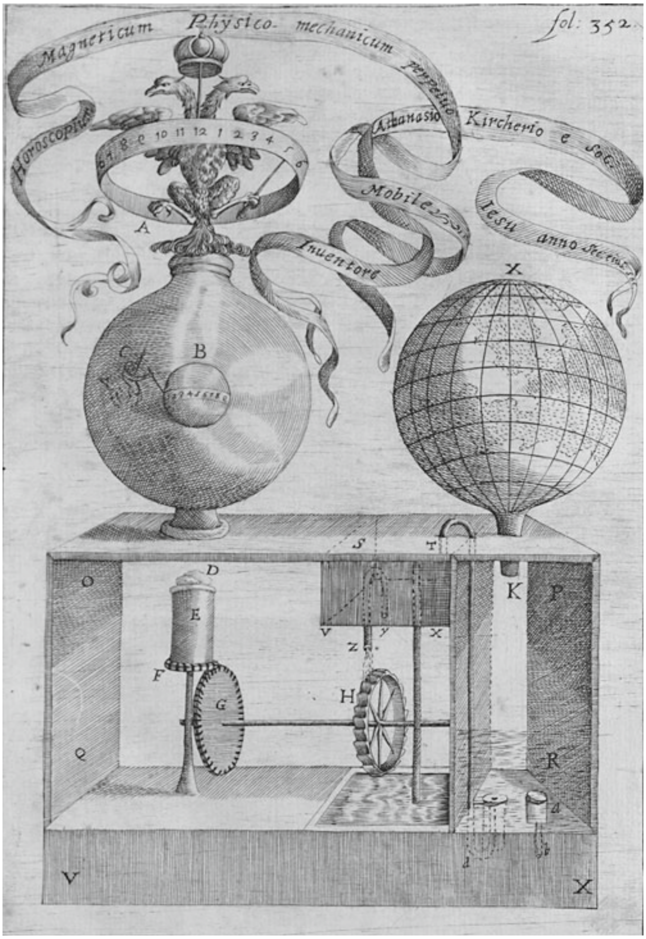


Fig. 3.4 Another of Kircher's magnetic clocks, this time driven by means of a new hydraulic mechanism. This clock figures world and cosmos, with the Habsburg eagle – symbol of the counterreformation – presiding on top (Kircher 1641, 352) (Courtesy K.U. Leuven, Maurits Sabbebibliotheek)

Fludd's *Mosaical Philosophy* (1638) argued that the magnet was the archetype of the whole universe (Kassell 2007; Debus 1964).

Natural philosophers and savants before Kircher, such as Gilbert, Crescentio and Cabeo, had already compared the magnet with plants. They used plant experiments to illustrate specific properties of the loadstone. Kircher was careful in not extending the analogies between magnetism and vegetative life too far, and accepted only a few. Upward vegetative growth and downward growth of roots was analogous to magnetic polarity; the geometrical distribution of branches and roots was similar to the way the magnet distributed its virtue; sympathies between plants were similar to

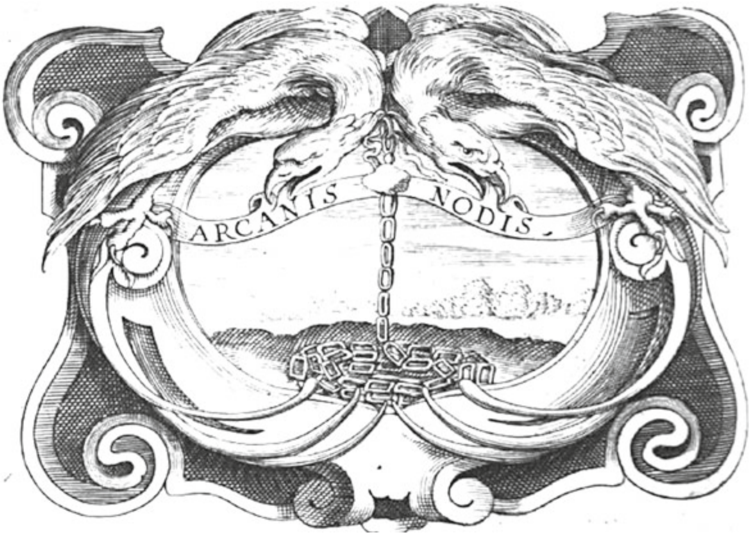


Fig. 3.5 The magnetic chain kept together by a divine magnetic power in Pietrasancta (1634, 400) (Courtesy K.U. Leuven, Maurits Sabbebibliotheek)

magnetic attraction; and the power of medical herbs to suck out poison was similar to the power of the magnet to attract iron. In particular, Kircher believed that heliotropism was analogous to the capacity of the magnet to align itself to the north. According to Kircher, the attractive force of the sun was ‘spiritual’, not material, as thick clouds did not hamper the sun’s efficacy in rotating the flowers (Kircher 1641, 698–750; Baldwin 1987, 338–58).

Kircher devotes a section of his book to love as a magnetic phenomenon, comparing the attraction between lovers with magnet and iron. But it is divine love that interests Kircher most. In the epilogue of the *Magnes*, Kircher extends the realm of magnetism into theology (Leinkauf 1993). In the seventeenth century, this theme is much elaborated by poets, philosophers, lawyers, emblem makers and divines, but few go so far in elaborating a full ‘magnetic theology’ as Kircher (Vermeir 2008). In Fig. 3.5, for instance, taken from Pietrasancta’s emblem book, we see an important trope from magnetic theology: the magnetic chain with links that do not interlock but keep together by a magnetic power, which was interpreted as the divine power connecting the world by arcane nodes. Kircher himself characterises God as ‘the central magnet of all things’ (Kircher 1654, table of contents), from which everything emanates and diffuses by means of divine magnetic rays. This ‘central Magnet’ is identified with the divine Trinity: its *attractive* power with the Father; its *ordering* capacities with the Son (the Word or Wisdom); and its *connective* power with the Holy Spirit (divine Love). It is the divine Wisdom that exerts a magnetizing attraction on man’s disquieted soul.

Sunflowers

Sunflowers were still relatively new in Europe in Kircher's time. They had come from the Americas in the sixteenth century and before the nomenclature standardised, they had many names, such as Indian Golden flour, Peruvian Chrysanthemum, Clythie or Heliotrope. The first illustrated scientific reference was probably in the 1568 edition of the *Florum* by Rembert Dodoens, a well known Flemish physician and botanist. Although the imagery of the sunflower assumed unprecedented importance in the seventeenth century – it symbolised Louis XIV, for instance, the absolutist Sun King – it did not enter an empty scene. It became part of a complex context where flowers and animals had acquired multiple meanings over centuries (Ashworth 1990). Animals and plants were known for their medicinal properties, but they also embodied moral, political and religious meanings. The sunflower was such a conspicuous plant that it soon repressed other plants in cultural representations, and it usurped the meanings associated with heliotropic plants.

Because of the virtue that attracted the sunflower to the sun, the flower came to stand for the constancy of honest love that follows its object everywhere. In Anthony van Dyck's *Self Portrait With a Sunflower,* the painter was compared to a sunflower, faithfully turning to his patron, Charles I, by love inspired by the royal 'magnetic' attraction. In Pietrasancta's *De Symbolis Heroicis* (1634, 266), the sunflower stood for the queen following the king, while it was stressed that this following did not make her inferior. The sun was the noblest object imaginable, and besides the worldly sovereign, it often represented God or Christ. It was thus natural that the sunflower came to stand for the love of the human creature to his creator, which gave it constancy and direction (see the epigraph). In one of his lectures, for instance, the Church of England clergyman Robert Bolton writes: 'By this time, being thus told and truly informed in the mystery and mercy of the Gospell, the poore wounded and weary soule begins to bee deepely and dearely enamored of Iesus Christ. […] So that now the current of his best affections, and all the powers of his humbled soule are wholly bent and directed toward him, as the Sun-flower towards the Sun; the iron to the load-stone; and the load-stone to the Pole-star' (Bolton 1631, 326–7).

In *A theatre of politicall flying-insects* (1657), a book that explains the various meanings of the bee, Samuel Purchas also mentions the sunflower and compares it to iron turning to the loadstone, to the loadstone turning to the polestar, and to how the 'domestick honey-Bees embrace and affect the Queene-Bee. […] Thus are the current of his best affections and all the powers of his humbled soule bent and directed towards Him.'(Purchas 1657, 320) The cultural prominence of sunflowers as a symbol of the love for God can also be seen in many emblems of the seventeenth century. In the *Amorum Emblemata* (1608), created by the Dutch painter and humanist Otto Van Veen, the sunflower is only the general symbol of love. But in his later *Amoris Divini Emblemata* (1615), the sunflower came to stand for divine love. In Hermann Hugo's *Pia Desideria* (1624), we can find a beautiful example of an emblem which combines the divine attraction of magnetic needle and sunflower represented together (Fig. 3.6).[33] The good soul renounces worldly love for a

Fig. 3.6 Hugo (1690), 168. The accompanying caption reads: ‘I am my Beloved’s, and his desire is towards me. Cant. 7.10.’ Here, the reference to the *Canticle of Canticles* also connects divine and worldly love (Courtesy K.U. Leuven, Maurits Sabbebibliotheek)

devotion to the divine. In this she is guided by the sympathetic power of sunflower and magnetic needle. Again, the constancy and the safe direction are stressed. Part of the accompanying text reads as follows:

> So by strange sympathy, the faithful *Steel*
> Does the lov’d *Pole’s* magnetic infl’ence feel,
> By whose kind conduct the safe *Pilot* steers
> A steddy course, till the wish’d Port appears.
> So the find *Hyacinth* pursues the *Sun*,
> *Pleas’d* at his rise, *griev’d* when the race is done (Hugo 1690, 171)

A Baroque Instrument

By means of a stylistic analysis of sunflowers, magnetism and clocks, it is possible to find intriguing resonances between early modern devotional works, botanical treatises, natural histories, books of emblems, artworks as well as natural philosophical and mathematical texts. Each of these three objects was in vogue and had a particular significance in the seventeenth century. Clocks embodied the tension between mechanical and religious worldviews, magnetism was newly theorised and became a paradigmatic power, while sunflowers, newly introduced from the Americas, conquered the gardens of the continent and symbolised the love of the subjects to their worldly or heavenly Lord. We can see a pattern here: these objects acquired similar meanings, even if they were used and represented in different practices. These meanings were not independent but they reinforced each other. Because of the constant, repetitive and reliable turning of heliotropic flowers to the sun, for instance, they were sometimes called the 'clocks' of rustics, and the attractive powers in play were considered similar to those of the magnet. These symbolic meanings, crystallizing and embodying divine power (and royal authority) were stable: these representations circulated and certain conventions developed around them. Taken from different contexts, Kicher put these objects together in his wonderful invention, the sunflower clock, and their meanings coalesced into a multifaceted but somewhat paradoxical whole.

To what extent can we call the sunflower clock 'baroque'? In our stylistic analysis, existing style concepts such as the baroque have a double role. On the one hand, the baroque is part of the general framework in which our analysis finds its place. One does not work in a void, of course, and one has to take into account existing meanings and scholarly discussions related to style concepts such as the baroque that inevitably guide our thinking. On the other hand, our approach is a bottom up analysis, looking to identify resonances between different practices. This analysis can stand on itself, detailing interesting and maybe localised style elements, but the results of this analysis can also become part of discussions about larger style concepts such as 'the baroque'. They can revisit the general framework of analysis and broaden or modify the concept of 'the baroque' itself. These two roles of our analysis clarify and inform each other, like in a hermeneutic circle

In this paper, I started from the hypothesis that Kircher's work is paradigmatic for the baroque. In particular, his way of using the sunflower clock as part of his natural philosophical and mathematical work is very peculiar, and I will explore below how it fits into 'the baroque'. Second, I also aimed at clarifying and contributing to the development of a bottom up approach to the concept of the baroque. What does Kircher's style of natural philosophy teach us about the baroque? As expected, his work fits a number of characteristics of the baroque and our analysis reinforces their importance. I have drawn special attention, however, to the role of religious spectacular demonstrations and experimentations in natural philosophy and mathematics. The meanings I explored can be seen to converge in a central religious allegory, which makes it possible to understand the sunflower clock as a rhetorical statement in a charged religious context.[34] To turn lost souls again towards god was in fact the central aim of the Jesuit mission, and their natural philosophical and mathematical pursuits were also an expression of this aim. Kircher uses an emblematic approach towards experimental natural philosophy, which for him is part of a *Gesamtwerk*, combining the arts, sciences and nature, to glorify God. Is this approach specific for Kircher? Some of Kircher's particularities are of course singular and should not be reduced to his context. Nevertheless, our stylistic analysis shows strikingly stable resonances with other practices and contexts which make it possible to see the contours of a style that goes beyond Kircher's individual traits.

The meaning of the sunflower clock and of Kircher's practices will gain more depth if connected to other relevant characteristics that are often attributed to the baroque. What is striking in the case of the sunflower clock (and especially the sunflower-seed clock) is that it could not possibly have 'worked'. This makes it harder for us to ascribe a 'common sense' meaning to it (Vermeir 2006). Hankins and Silverman show that Kircher probably used tricks to deceive his spectators. Here the illusionism of the baroque, its play with tricks and paradoxes, becomes crucial for understanding the sunflower clock. During the baroque, a chasm was constructed between appearance and reality. They did not map on each other in any straightforward sense anymore. Indeed, in the baroque, people became fascinated with deceptions and optical illusions, scepticism was in vogue, and the world itself was conceived of as a theatre. Furthermore, a new focus on the appearances, also in the experimental philosophy, allowed natural philosophers to connect various phenomena analogously without having to investigate the deeper causes. Magnetism became the exquisite reference point for this approach. At the same time, the attractive force of magnetism could be understood allegorically as a spiritual and even divine power.[35] The magnet came to stand for the divine ordering principle in the cosmos, connecting the worldly and the heavenly.

Hankins and Silverman have argued that Kircher's exposal of the mechanism of Linus' clock was an unfortunate exception. In contrast, I think that Kircher was an adept in the complicated rhetoric of veiling and unveiling that was central to the baroque (Vermeir 2012). In 1625 already, at the Jesuit College in Heiligenstadt, he had devoted a great deal of care to the mise-en-scène of a theatrical play. The spectacle was so extraordinary and the 'special effects' were so successful that it was rumoured about that black magic was involved. To a select group of visitors, however, Kircher demonstrated how his special effects worked.[36] One aspect of baroque spectacle consisted in provoking wonder by showing the hidden mechanical contrivances behind marvellous phenomena. Kircher employed these two kinds of wonder where appropriate. It is not an accident that his magnetic clocks figure in the section on magnetic natural magic.

Kircher did not explain the trick behind his sunflower clock. There was no mechanical contrivance behind it that could be shown. In this case, the rhetoric worked in a different way. The theatricality of Kircher's practices should be understood in the context of the baroque as a spectacular style, as exemplified in Counter Reformation performances and Jesuit theatre (Findlen 2003; Norman 2001; Valentin 1978; McCabe 1983).[37] These spectacles had many layers of meaning – including symbolical and allegorical. Not the deeper principles, the back stage machines or the theatrical tricks, but rather the appearances as they were represented on the stage were meant to symbolise more profound meanings. The same can be said of the sunflower clock. In this case, the breaking down of the attribution of a common sense meaning points directly at the baroque intuition that there is not just a hidden contrivance but also a deeper meaning behind the phenomena. The religious allegorical meanings of Jesuit spectacles find their counterpart in the constituent elements of the sunflower clock, and this is confirmed by our study of the different meanings attributed to them. The clock, magnet and sunflower were emblems, full of symbolism and allegorical meaning. They represented the infusion of the divine power, order and love in the visible world.

These resonances should of course still be placed in larger patterns, that the again can be captured in larger style concepts, such as the 'baroque' (and which is described in a body of historical work on the baroque), in order to understand their full meaning.[38] Ideally, to come to understand specific historical practices or objects, one would have to describe the whole culture of which these practices or objects were part (as Wittgenstein (1966, 7–8) wrote about aesthetic terms: 'to describe their use […] you have to describe a culture'). Of course, it is unfeasible to actually describe the full and infinite complexity of a culture. By necessity, I will have to restrict myself to a few aspects that I need for my argument. My short confrontation of Kircher's work with some baroque characteristics has therefore only been pragmatic and partial (in a double sense of incomplete and from a particular perspective). Furthermore, the existence of spectacular demonstrations of religious truths and political power, which were also performed in the sciences, is of course no necessary or sufficient condition for 'the baroque'. Nevertheless, it is a style element that fits with other characteristics that are generally considered baroque and a stylistic analysis can be used to explore further and enrich discussions about the baroque.

A final point to be stressed is that the baroque flourished in a time when savants still negotiated what experiments, demonstrations and instruments were supposed to do. A consensus was being built about the relevant norms and rules for such practices, and different social groups had their own ideas about this. What was a botanical clock supposed to do? Some Jesuit mathematicians clearly had a different view on instruments, experiments and demonstrations than had Descartes or Galileo. They were not necessarily anti-modern (which would be an anachronistic designation), but one might speak of conflicting styles of doing natural philosophy and mathematics. One possible research theme would be to try to relate them to different distinct currents of the baroque (cf. Alpers 1983).[39] Here, I did not want to go into this direction. My aim was rather to give a bottom up approach to styles, a method that avoids overarching generalisations, without denying the possibility to frame results in a larger set of connections (without positing a totalizing whole) in order to get a better grasp of its meaning. Despite misunderstandings about the aims and workings of instruments, the reactions of Peiresc and Mersenne illustrate that there was a multiplicity of views, besides outright support or dismissal, and a close study of the controversy shows an historical density and richness that complicates any simple opposition between worldviews.[40]

Conclusion

Hankins and Silverman have given us a brilliant close study of the reception of Kircher's sunflower clock by his contemporaries. In this paper, my approach has been different. I have argued that there are some incongruities in their interpretation and I think that a micro reception-study might sometimes miss some relevant aspects that can be gleaned from what I call a ‘stylistic analysis’. I have conceptualised a style in the narrow sense as a ‘way of doing’, and changes in style indicate variations within a practice. Style in a more extended sense is transversal to practices, it refers to similarities between ‘ways of doing’ in different practices. A stylistic analysis focuses on relevant resonances between different practices that at first sight do not seem to be connected. A stylistic analysis of the sunflower clock therefore involved an exploration of the similarities in meanings that the three constituting elements – clock, magnetism and sunflower – acquired in different practices. These meanings were conventional and the resonances were stable enough to be part of a style.

I have shown how the meanings of clocks, magnetism and sunflowers converged. Indeed, the idea of a sunflower clock was maybe not so far-fetched after all. Taken together, they enhance the plausibility of a symbolic reading of Kircher’s sunflower clock. Artefacts can have many meanings, and the multiple religious meanings ascribed to clocks, magnetism and sunflowers constitute in part the meaning of Kircher’s instrument. This instrument, I would argue, can therefore be seen as a hieroglyph or illustration of metaphysical and theological truths. They were *metaphorical instruments*, or maybe better ‘anagogical’ or ‘tropological’ instruments, that had to be interpreted according to the right exegetical techniques.

For Kircher, I would suggest, the core function of the sunflower clock was not to tell the time. I think that Hankins and Silverman were right when they claimed that Kircher was using the sunflower clock to make a rhetorical point. I only disagree about the point Kircher tried to make. I believe the Kircher and other members of the Jesuit order to which he belonged used analogical demonstrations to illustrate and visualize the central aspect of their worldview: Catholic religion. This is all the more plausible since the Jesuits possessed well established hermeneutical tools to deal with the use of metaphors and analogies. We should interpret the meaning of Kircher’s artefacts also in the context of Jesuit apologetics and it is clear that they used scientific instruments to represent catholic meanings and values in their battle for lost souls (Vermeir 2007). Accepting the ‘baroqueness’ of Kircher’s instruments helps us to avoid positivist or functionalist readings of past technologies.

The reception of Kircher’s sunflower clock was complex, and people differed in ascribing meanings to it. The ‘new philosophers’ mistook the Jesuit’s instruments as demonstrations of a natural magnetism, while many of them might have been meant as demonstrations of a ‘supernatural’ magnetism. This does not mean that these philosophers would agree with such a religious use of natural objects or scientific instruments, or that everyone attributed the same meanings to them. Seventeenth-century culture was complex and heterogeneous, and this should not be concealed by general notions such as ‘the baroque’. Henry More, for instance, shows in *The Grand Mystery of Godliness* that he is not favourably disposed towards the traditions that connect religion to sunflowers. He writes: ‘That the two great Luminaries of the world have a very strong influence upon all sublunary bodies is very plain, and upon some more peculiarly then others; and yet without any suspicion of Religion in them. For what *Religion* can there be in the *Heliotropium* that winds about so with the Sun?’ (More 1660, 49–50) We are left to imagine his reaction to Kircher’s philosophical and theological exploits.

Notes

1. For more on Kircher's clock and the controversy it generated, see Hankins and Silverman (1995), Drake (1967), Monchamps (1892), Bedini (1969), Baldwin (1985), Hine (1982) and Hine (1988).
2. At the time, a sunflower clock was a plausible instrument, since the heliotropic movement of the sunflower has been noted by many, but from a twentieth century perspective, a clock like that seems implausible. First, botanists and biologists today have pointed out that the sunflower only shows this motion as long as its flower does not bloom. There are indications, however, that this happens only because a part of the sunflower's stem stiffens and cannot turn anymore. So Kircher's setup might actually allow the sunflower to turn again. Second, we now believe that heliotropism is not the effect of magnetism but of blue light. This means that the clock cannot work inside a house, contrary to what Kircher claims. Stanton and Galen (1993).
3. Note, however, that our stylistic approach will not reduce the question of meaningfulness of an action or text to the intention of the author.
4. See note 1 for references. The latest account is by Angela Mayer-Deutsch (2010), a paper I received when the current article went into print.
5. My description of Kircher's clocks in the previous paragraphs is a reworking of some paragraphs of Vermeir (2006).
6. Galileo was sentenced on 22 June 1633 and Kircher demonstrated his sunflower clock on 3 September 1633.
7. The Jesuit Jacques Grandami (1645) similarly argued that magnetism could not support Copernicanism by experimentally demonstrating that magnets did not rotate around their axis.
8. Pietrasancta (1634, 147) does compare the different spheres in Linus' clock with different heavenly bodies (earth, sun, spheres), but he does not pursue a consistent analogy and the passage cannot be read as a defence for Copernicanism.
9. For Kircher's changing attitudes to Copernicanism, see Siebert (2006).
10. With a 'contextual analysis in the narrow sense', I mean an analysis that only takes into account the local context and related circulations of knowledge of an episode. Of course, I am not arguing against such a contextual analysis, but I only want to point out that a stylistic analysis will uncover different material. Furthermore, my stylistic analysis might very well be called a contextual analysis in the broader sense.
11. Hacking's approach is top down, and although his styles are historical, they also have an *a priori* character. Crombie's (1994) original approach was more bottom up, but he too was guided by a priori conceptions necessary to identify these large scale inflexible styles.
12. For a description of the importance of the practical turn in the humanities, see Schatzki et.al. (2001), Stern (2003); and in the historiography of science, see Pickering (1992), Rheinberger (2007).
13. See esp Bloor (1983), and the application of this interpretation of Wittgenstein in historical study in e.g. Shapin and Schaffer (1985), p. 14–15. For a different interpretation of Wittgenstein and its application in history and sociology of science, see Lynch (1993), esp. Ch. 5. Note, however, that there is no consensus on a 'theory of practice' in history and philosophy of science. For an exploration of a 'Wittgensteinian' theory of practice, see Schatzki (1996), in which especially Chapter 4 is relevant.
14. For other notions of style relevant for the history of science, see Wessely (1991), Crombie (1994), Hacking (2002). For a critical analysis, see Kusch (2010). Although it is possible to fit these approaches into my concept of style as a way of doing or as variation of a practice, these style concepts are not so relevant for my purposes. Older theories of style (Fleck 1935; Chevalley 1935; Granger 1968) are more relevant to my approach. For an overview, see Gayon (1996).
15. For an analysis of Wittgenstein's aesthetics, see Hagberg's 'Wittgenstein's Aesthetics' and Hagberg 1985. See also Wittgenstein (1966).
16. How practices are identified depends on the use of concepts and of language in general and vice versa (cf. Wittgenstein's 'family resemblances'). If a practice is characterised as doing 'something' (e.g. the practice of 'cooking'), it corresponds to a concept of that 'something'

(the concept of 'cooking'). On this level, practices are not just brute activities, but are already related to conceptual frameworks. (It follows that in order to avoid anachronism, historians should describe past practices with reference to past conceptual frameworks and actor's categories).

17. The notion of 'resonance' aptly describes the similarity between different practices (the similar ways of doing different things), because of the analogy with two different strings that resonate when there is a similarity (ratio) between them. For an alternative use of the concept of resonance, see e.g. Greenblatt (1990).
18. On the one hand, if one is interested in analogical reasoning, e.g. one can call this a practice: a practice that involves searching good analogies. The kinds of analogies one finds might distinguish different styles: one style of analogical reasoning would find analogies with inanimate natural objects, for instance, while another style would consistently seek to make analogies with human behaviour. On the other hand, if one is interested in early modern natural philosophy, one can distinguish analogical reasoning, syllogistic reasoning or experimental trials as different styles of practicing natural philosophy.
19. Cultural conventions, however, usually determine what counts as practice and what counts as style. Historians are often interested in disciplines or institutions that have a certain continuity over time and are studied diachronically (the history of mathematics, natural philosophy, painting or architecture) and styles – or the similarities between ways of doing these things – are therefore often synchronic. Style concepts such as renaissance, baroque or romanticism are therefore habitually used to indicate periods (and are often confused with periodizations).
20. The idea that styles are abstractions, together with a bottom-up approach to styles, should preclude the temptation to *reduce* events, authors or milieus to stylistic categories such as the baroque.
21. There is some controversy about the extent in which Wittgenstein represents a 'social turn'. The sociology of scientific knowledge interprets practices as grounded in social interactions while others see the notion of practice as more fundamental. See also Maravall (1986), for an analysis of the baroque grounded in social structures.
22. Suppose that one is interested in the history of early modern mathematics, and imagine that 'Jesuit mathematics' or 'Royal Society mathematics' constituted specific ways of doing mathematics. In that case, the Jesuits or the Royal Society would be social groups singled out by a stylistic criterion, while early modern mathematicians are the social group associated to the practice of mathematics. Of course, in many cases, stylistic criteria would not neatly single out a social group that we can easily label (such as 'Jesuit' or 'Royal Society', for instance).
23. See e.g. Wellek (1946), Hatzfeld (1949), Geers (1960), Menashe (1965), Hatzfeld (1975). The literature here is of course enormous.
24. I took these examples of characteristics from the abundant literature on the baroque.
25. This does not exclude the possibility of mistakes and forgeries, of course.
26. For more reflections on anachronism and actors' categories, see Jardine (2000).
27. See also note 16.
28. The question then remains whether one should include less evident cases (such as Descartes, Boyle, Huygens and other seventeenth century luminaries) as part of a baroque style. That there is some disagreement on this point does not strike me as problematic.
29. For some notable attempts, see Eriksson (1994), Høyrup (1997a); and especially Høyrup (1997b) on Kircher and Caramuel.
30. A possible constraint on the selection of resonances that may constitute a style could be, for instance, the plausibility of interaction and circulation of knowledge between the domains in question. In that case, only those resonances that plausibly result from actual interaction and circulation (excluding chance resonances) can be used in order to develop our style concepts.
31. De Solla Price also argues that sundials seldom had their hour-lines numbered but the equator and tropical lines were modelled as if the sundial was the representation of the earth.
32. The religious and political meaning that the Jesuits embodied in their clocks is nicely illustrated by a story of intercultural exchange: When the Jesuits brought mechanical clocks to China in the

seventeenth century, Murata argues, the Chinese did not use them as instruments for time measurement and time-keeping because they rejected the religious and political worldview embodied in the clocks (Murata 2003, 242–45; Landes 1983, 44–7). Clocks had political significance symbolizing the absolutism that came into prominence in the seventeenth century.

33. Similar sunflower emblems can be found in e.g. de la Feuille (1691); de Harduwijn (1629); anonymous (1690); Suderman (1724); and on compasses pointing to God, see e.g. Cats (1618); Hooft (1611); van Leuven (1629).
34. A more refined analysis is necessary in order to distinguish between protestant and catholic styles in this respect. In a first analysis, and somewhat generalizing, it seems that the catholic version starts with religious dogma and metaphysical theory, which it expresses in mathematical objects or natural philosophical performances, while protestant styles are more based in experimental practice (cf. Vermeir 2008).
35. The study of magnetism also played a complex role in the increasing importance of the dichotomy between the material and the spiritual, sometimes blurring and at other times reifying this distinction.
36. See Kircher's autobiography in the appendix of Langenmantel (1684), p. 32–3.
37. Also particular practices of secrecy, dissimulation, illusion, fraud and trickery can be seen as part of the baroque. See e.g. Vermeir (2012), Snyder (2009), Nummedal (2007).
38. This also implies that the fashionable practice of using baroque style elements today is not really part of a baroque style. One or two style elements do not make a large style such as baroque. Their embedding in other practices, as well the tissue of similarities, resonances and connections, will be very different from the seventeenth century baroque. The reuse of style elements should therefore be called 'neo-baroque'.
39. If one would like to follow this path, it should be noted that these were not necessarily incompatible, let alone incommensurable, and that participants cannot be neatly divided according to a north-south division. (This is against Paul Feyerabend (1984), for instance, who argues that there is incommensurability between different styles in art and science.)
40. This indicates that despite the presence of major currents in a baroque style, their permeability and interactions make strict distinctions in particular cases impossible and make it important to frame these currents in a still broader stylistic framework.

References

Ackerman, James. 1962. A theory of style. *The Journal of Aesthetics and Art Criticism* 20: 227–237.

Alpers, Svetlana. 1983. *The art of describing: Dutch art in the seventeenth century*. Chicago: University of Chicago Press.

Anonymous. c1690. *Emblemata amatoria*.

Ashworth, William. 1990. Natural history and the emblematic world view. In *Reappraisals of the scientific revolution*, ed. D.C. Lindberg and R. Westman, 303–332. Cambridge: Cambridge University Press.

Baldwin, Martha. 1985. Magnetism and the anti-copernican polemic. *Journal for the History of Astronomy* 16: 155–174.

Baldwin, Martha. 1987. *Athanasius Kircher and the magnetic philosophy*. PhD diss., University of Chicago, Chicago.

Bedini, Silvio A. 1969. Seventeenth century magnetic timepieces. *Physis* 11: 37–68.

Bloor, David. 1983. *Wittgenstein: A social theory of knowledge*. New York: Columbia University Press.

Bolton, Robert. 1631. *Instructions for a right comforting afflicted consciences*. London: Thomas Weaver.

Cats, Jacob. c1618. *Proteus.*

Chevalley, Claude. 1935. Variations du style mathématique. *Revue de Métaphysique et de Morale* 3: 375–384.

Close, A.J. 1969. Commonplace theories of art and nature in classical antiquity and in the Renaissance. *Journal of the History of Ideas* 30: 467–486.

Crombie, Alistair Cameron. 1994. *Styles of scientific thinking in the European tradition: The history of argument and explanation especially in the mathematical and biomedical sciences and arts*. London: Gerald Duckworth.

de Harduwijn, Justus. 1629. *Goddelycke wenschen.*

de la Feuille, Daniel. c1691. *Devises et emblemes*.

De Sepibus, Georgius. 1678. *Romani Collegii Societatis Jesu Musæum Celeberrimum*. Amsterdam: Janssonius-Waesbergen.

De Solla Price, Derek J. 1964. Automata and the origins of mechanism and mechanistic philosophy. *Technology and Culture* 5(1): 9–23.

Dear, Peter. 1995. *Discipline and experience: The mathematical way in the scientific revolution*. Chicago: The University of Chicago Press.

Debus, Allen G. 1964. Robert Fludd and the use of William Gilbert's *De Magnete* in the weapon salve controversy. *Journal of the History of Medicine* 19: 389–417.

Drake, Stillman. 1967. A long-lost letter of Galileo to Peiresc on a magnetic clock. In *A letter from Galileo*, ed. B. Dibner and S. Drake, 45–56. Norwalk: Cn. Burndy Library.

Eriksson, Gunnar. 1994. *The Atlantic vision. Olaus Rudbeck and Baroque science*. Canton: Science History Publications.

Feyerabend, Paul. 1984. *Wissenschaft als Kunst*. Frankfurt: Suhrkamp.

Findlen, Paula. 2003. Scientific spectacle in Baroque Rome: Athanasius Kircher and the Roman College Museum. In *Jesuit science and the republic of letters*, ed. M. Feingold, 225–284. Cambridge: MIT Press.

Fleck, Ludwik. 1935. *Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einführung in die Lehre vom Denkstil und Denkkollektiv*. Basel: Schwabe.

Fludd, Robert. 1638. *Philosophia Mosaica*. Rammazenius: Gauda.

Forkel, J.H. 1788. *Allgemeine Geschichte der Musik*. Leipzig: Schwickert.

Gayon, Jean. 1996. De la catégorie de style en histoire des sciences. *Alliage* 26: 13–25.

Geers, Gerardus Johannes. 1960. Towards the solution of the baroque problem. *Neophilologus* 44: 299–307.

Ginzburg, Carlo. 1998. Style as inclusion, style as exclusion. In *Picturing science, producing art*, ed. C.A. Jones and P. Galison, 27–53. London: Routledge.

Grandami, Jacques. 1645. *Nova Demonstratio Immobilitatis Terrae Petita Ex Virtute Magnetica*. La Fleche: G. Griveau.

Granger, Gilles-Gaston. 1968. *Essai d'une philosophie du style*. Paris: Armand Colin.

Greenblatt, Stephen Jay. 1990. *"Resonance and wonder" in idem, learning to curse: Essays in early modern culture*, 161–183. Routledge: London.

Hacking, Ian. 2002. *Historical ontology*. Cambridge: Harvard University Press.

Hagberg, Garry. Wittgenstein's Aesthetics. 2007. In *Stanford Encyclopaedia of Philosophy* (http://plato.stanford.edu/entries/wittgenstein-aesthetics/).

Hankins, Thomas L., and Robert J. Silverman. 1995. *Instruments and the imagination*. Princeton: Princeton University Press.

Hatzfeld, Helmut. 1949. A clarification of the baroque problem in the Romance literatures. *Comparative Literature* 1: 113–139.

Hatzfeld, Helmut. 1975. Problems of the Baroque in 1975. *Thesaurus* 30: 209–224.

Heyl, Bernard C. 1961. Meanings of Baroque. *The Journal of Aesthetics and Art Criticism* 19: 275–287.

Hills, Helen (ed.). 2011. *Rethinking the Baroque*. London: Ashgate.

Hine, William. 1982. The Mersenne-Kircher correspondence on magnetism. *Proceedings of the Annual Meeting of the Western Society for French History* 10: 106–117.

Hine, William. 1988. Athanasius Kircher and magnetism. In *Athanasius Kircher und seine Beziehungen zum gelehrten Europa seiner Zeit*, ed. J. Fletcher, 79–97. Wiesbaden: Harrassowitz.

Hooft, Pieter Corneliszoon. 1611. *Emblemata amatoria*. Amsterdam: Ianszoon.

Høyrup, Jens. 1997a. Reflections on the baroque in the history of science. *Physis* 24: 675–694.

Høyrup, Jens. 1997b. Barocco e scienza secentesca: un legame inesistente? *Analecta Romana Instituti Danici* 25: 141–172.

Hugo, Hermann. 1690. *Pia Desideria*. Bonwicke: London.

Jardine, Nicholas. 2000. Uses and abuses of anachronism in the history of the sciences. *History of Science* 38: 251–270.

Kassell, Lauren. 2007. Magic, alchemy and the medical economy in early modern England: The case of Robert Fludd's magnetical medicine. In *Medicine and the market in early modern England*, ed. M. Jenner and P. Wallis. Basingstoke: Palgrave Macmillan.

Kircher, Athanasius. 1641. *Magnes sive de arte magnetica.* Grignani: Rome.

Kircher, Athanasius. 1652–1655. *Oedipus Aegyptiacus*, 3 vols. Mascardi: Rome.

Kusch, Martin. 2010. Hacking's historical epistemology: A critique of styles of reasoning. *Studies in History and Philosophy of Science* 41: 158–173.

Landes, David. 1983. *Revolution in time: Clocks and the making of the modern world*. Cambridge: Belknap.

Langenmantel, Hieronymus Ambrosius. 1684. *Fasciculus epistolarum*. Augsburg: Utzschneider.

Leinkauf, Thomas. 1993. *Mundus combinatus. Studien zur Struktur der barocken Universalwissenschaft am Beispiel Athanasius Kirchers SJ (1602–1680).* Berlin: Akademie Verlag.

Lynch, Michael. 1993. *Scientific practice and ordinary action*. Cambridge: Cambridge University Press.

Maravall, José Antonio. 1986. *Culture of the Baroque*. Minneapolis: University of Minnesota Press.

Mayer-Deutsch, Angela. 2010. Magnetische Uhren bei Athanasius Kircher, Francis Line und Galileo Galilei. Stationen einer Debatte um Weltbilder. In *Imagination und Repräsentation: zwei Bildsphären der Frühen Neuzeit*, ed. H. Bredekamp, C. Kruse, and P. Schneider. Paderborn: Wilhelm Fink Verlag.

Mayr, Otto. 1986. *Authority, liberty & automatic machinery in early modern Europe*. Baltimore: Johns Hopkins University Press.

McCabe, William. 1983. *An introduction to the Jesuit theatre*. St. Louis: Institute of Jesuit Sources.

Menashe, Louis. 1965. Historians define the Baroque: Notes on a problem of art and social history. *Comparative Studies in Society and History* 7: 333–342.

Monchamps, Georges. 1892. *Galilée et la Belgique: essai historique sur les vicissitudes du système de Copernic en Belgique*. Sint-Truiden: G. Moreau-Schouberechts.

More, Henry. 1660. *An explanation of the grand mystery of godliness.* Flesher: London.

Mueller, John H. 1954. Baroque: Is it datum, hypothesis, or tautology? *The Journal of Aesthetics and Art Criticism* 12: 421–437.

Murata, Junichi. 2003. Creativity of technology: An origin of modernity? In *Modernity and technology*, ed. T. Misa, P. Brey, and A. Feenberg, 227–253. Cambridge: MIT Press.

Norman, Larry. 2001. *The theatrical Baroque*. Chicago: University of Chicago.

Nummedal, Tara. 2007. *Alchemy and authority in the Holy Roman Empire*. Chicago: Chicago University Press.

Oppenheimer, Max. 1951. Tendencies and bias in Baroque literary studies. *The Modern Language Journal* 35: 258–262.

Parr, James A. 2001. A modest proposal: That we use alternatives to borrowing (renaissance, baroque, golden Age) and leveling (early modern) in periodization. *Hispania* 84: 406–416.

Pickering, Andrew. 1992. *Science as practice and culture*. Chicago: University of Chicago Press.

Pietrasancta, Silvester. 1634. *De Symbolis Heroicis Libri IX*. Antwerpen: Plantijn-Moretus.

Purchas, Samuel. 1657. *A theatre of politicall flying-insects wherein especially the nature, the vvorth, the vvork, the wonder, and the manner of right-ordering of the bee, is discovered and described*. London: Thomas Parkhurst.

Rheinberger, Hans-Jörg. 2007. *Historische Epistemologie zur Einführung*. Hamburg: Junius Verlag.

Sayce, R.A. 1985. The use of the term baroque in French literary history. *Comparative Literature* 10: 246–253.

Schatzki, Theodore. 1996. *Social practices. A Wittgensteinian approach to human activity and the social*. Cambridge: Cambridge University Press.

Schatzki, Theodore, Karin Knorr Cetina, and Eike Von Savigny (eds.). 2001. *The practice turn in contemporary theory*. London: Routledge.

Secord, James (ed.). 1993. The big picture. special issue. *British Journal for the History of Science* 26: 387–483.

Shapin, Steven, and Simon Schaffer. 1985. *Leviathan and the air-pump*. Princeton: Princeton University Press.

Siebert, Harald. 2006. *Die große kosmologische Kontroverse. Rekonstuktionsversuche anhand des Itinerarium extaticum von Athanasius Kircher SJ (1602–1680)*. Stuttgart: Franz Steiner Verlag.

Snyder, Jon. 2009. *Dissimulation and the culture of secrecy in early modern Europe*. Berkeley: University of California Press.

Stanton, Maureen L., and Candace Galen. 1993. Blue light controls solar tracking by flowers of an alpine plant. *Plant, Cell & Environment* 16: 983–989.

Stern, D.G. 2003. The practical turn. In *The Blackwell guide to the philosophy of the social sciences*, ed. S. Turner and P. Roth, 185–206. Oxford: Blackwell Publishing.

Suderman, Jan. c1724. *De godlievende ziel.*

Valentin, Jean-Marie. 1978. *Le Théâtre des Jésuites dans les pays de langue Allemande (1554–1680): Salut des âmes et ordre des cités*, 3vols. Bern: Peter Lang.

van Leuven, Ludovicus. c1629. *Amoris divini et humani antipathia*.

Vermeir, Koen. 2006. The reality of failure. On the interpretation of success and failure in (the history and philosophy of) science and technology. In *Variantology II. On deep time relations of arts, sciences and technologies*, ed. S. Zielinski and D. Link, 335–360. Cologne: Walter König.

Vermeir, Koen. 2007. Athanasius Kircher's magical instruments: An essay on science, religion and applied metaphysics. *Studies in History and Philosophy of Science* 38(2): 363–400.

Vermeir, Koen. 2008. Magnetic theology as a baroque phenomenon. Presentation for the baroque science workshop, University of Sydney, 16 Feb 2008, and made available online.

Vermeir, Koen. 2012. Openness versus secrecy? Historical and historiographical remarks. *The British Journal for the History of Science* 45(2): 165–188.

Wanley, Nathaniel. 1673. *The wonders of the little world, or, A general history of man in six books.* Basset: London.

Weeks, Sophie. 2007. Francis Bacon and the art–nature distinction. *Ambix* 54: 101–129.

Wellek, Rene. 1946. The concept of Baroque in literary scholarship. *The Journal of Aesthetics and Art Criticism* 5: 77–109.

Wessely, Anna. 1991. Transposing 'style' from the history of art to the history of science. *Science in Context* 4: 265–278.

Wittgenstein, Ludwig. 1966. *Lectures and conversations on aesthetics, psychology, and religious belief.* Oxford: Basil Blackwell.

Chapter 4
From Divine Order to Human Approximation: Mathematics in Baroque Science

Ofer Gal

Abstract The Inverse Square Law (ISL) of Universal Gravitation is the epitome of the great achievement of mathematical natural philosophy. But what exactly was this achievement? Newton and his followers presented it as the discovery of the simple, perfect laws underlying all seemingly-unruly phenomena. For Kepler, the first to introduce the ISL into natural philosophy (as the law of the decline of light with distance), mathematics was indeed the human means to decipher God's perfect harmonies, but through the seventeenth century this belief gradually eroded. For Newton, the ISL became a tool of approximation, rooted in, and gaining its authority from, human practice: the mathematization of nature required relinquishing the certainty and perfection that mathematical knowledge was expected to provide.

Kepler and Newton

> "… Here ponder too the Laws which God/Framing the universe, set not aside/But made the fixed foundations of His work" (Halley's *Ode to Newton*, Newton 1687, p. xiii)
>
> "A catechist announces God to children, and a Newton demonstrates Him to the learned." (Voltaire 1901, 7:80)

This is the way in which Newton and his disciples wanted his great achievement to be remembered: as the submission of all phenomena to a small set of exact mathematical laws. These laws, Halley and Voltaire avowed, constituted a simple, perfect and harmonious structure underlying all seemingly unruly phenomena, a structure which was the divine blue print for the universe. They had to be mathematical because mathematics was the way the catechism was revealed to the sages;

O. Gal (✉)
Unit for History and Philosophy of Science, University of Sydney, Australia
e-mail: ofer.gal@sydney.edu.au

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_4,

the science of simple, perfect structures, human reason in its closest approximation of the divine.[1]

The two diagrams below suggest that this self presentation was not completely unfounded. They suggest that the assumption that simplicity of causes must underlie the complexity of phenomena, and that deciphering this simplicity is the role of mathematics, were not only philosophical afterthoughts for Newton and the Newtonians. Rather, they were working principles that he picked up from the tradition of mathematized natural philosophy developed through the seventeenth century and which Kepler was instrumental in shaping.

Seventy year separate these diagrams and they are different in audience and goal. Kepler's (Fig. 4.1) is public and in print—it opens the 1609 *Astronomia Nova.* Newton's (Fig. 4.2) is private and hand-drawn—part of a 1679 letter to Robert Hooke (Newton 1960, 2:308). Kepler is aiming to convince the general astronomical public that the geostatic system, whether in its Ptolemaic or Tychonic version, is untenable. Newton is suggesting to his correspondent Hooke that his—Hooke's—idea that planetary motions are a "compounding" of inertial motion and solar attraction (Hooke1960, 2:297) is fundamentally flawed. Kepler's diagram is based on a careful calculation from the geostatic theory he thinks obsolete; Newton, on the other hand, feigns a quantitative theory he does not really have and fabricates a construction (*Cf.* Gal and Chen-Morris 2006, 33–48).

Yet the structure of the argument these diagrams embody is essentially the same and very much in line with Halley and Voltaire's pronouncements. Both depict a hypothetical planetary orbit, suggested by the theory under consideration, and both expect their audience to immediately perceive the orbit as obviously absurd, and eschew the theory that produced it. And why is the orbit obviously absurd? Because it is *chaotic*. Because, to quote Kepler, "these motions, continued farther, would become unintelligibly intricate, for the continuation is boundless, never returning to its previous path" (Kepler 1609, 119). As Newton will put it later, Hooke's idea means that "there are as many orbits to a planet as it has revolutions." And for both writers the argument ends here—an "unintelligibly intricate" orbit is *prima facie* unacceptable.

An absolute trust in simplicity and orderliness is entailed in these diagrams. It is such a strong assumption, that it needs no explication: the reader—Hooke or the general readership—is expected to accept the impossibility of the theory by merely looking at the complexity of the orbit. And it is so strong, that it seems to spill over from the level of causes to that of phenomena: it is the orbit that is complex, complain Kepler and Newton, but it is complex enough not to allow a simple cause.

However, I intend to demonstrate, the idea of Newton's approach to mathematics and natural order that we inherited from the likes of Halley and Voltaire is incorrect. These twin beliefs; in simple harmonies and in the power of mathematics to discover them, were Kepler's, but no longer Newton's. For Kepler, the diagram and the argument it supports represented a genuine commitment: these beliefs guided him and were embedded in his work throughout his career. For Newton, on the other hand, they were already, or soon to become, a commonplace. He could use this type of 'argument from order' effectively, but by the time he sends his sketch to Hooke

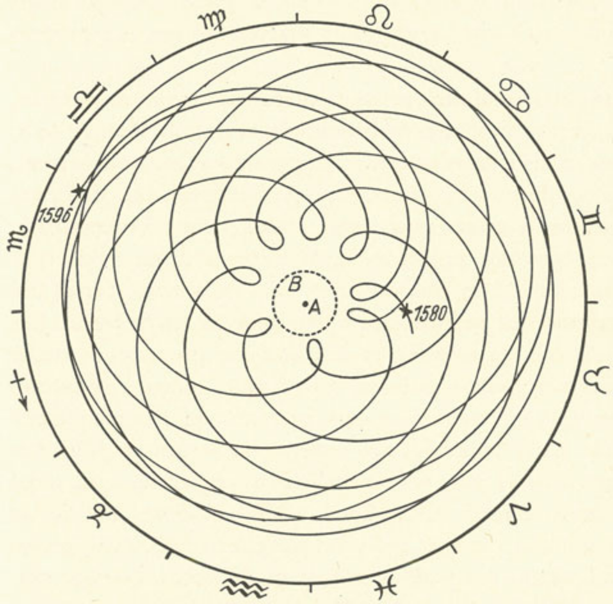
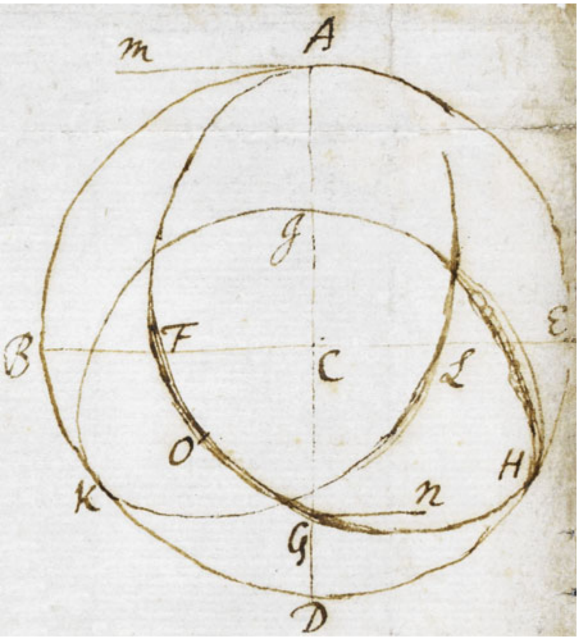


Fig. 4.1 Kepler's *Astronomia Nova*

Fig. 4.2 Newton's Letter to Hooke



(13 December 1679), it represents no more than a rhetorical *topos*, which he easily forgoes once it conflicts with his problem-solving strategies. For all practical intents and purposes, Newton's universe is imperfect and far from simple. For Kepler it is the perfection of mathematics which makes it the proper medium through which to express the beauty, majesty and absolute perfection of the Creator and His creation—the magnificent *Harmonices Mundi*. In diametric opposition, Newton's work takes the turn that would lead him to the *Principia* once, following the correspondence with Hooke, he adopts the chaotic view expressed in the following, so called "Copernican Scholium:"

> The whole space of the planetary heavens either rests … or moves uniformly in a straight line, and hence the communal centre of gravity of the planets … either rests or moves along with it. In both cases … the relative motions of the planets are the same, and their common centre of gravity rests in relation to the whole of space, and so can certainly be taken for the still centre of the whole planetary system. Hence truly the Copernican system is proved a priori. For if the common centre of gravity is calculated for any position of the planets it either falls in the body of the Sun or will always be very close to it. By reason of this deviation of the Sun from the centre of gravity the centripetal force does not always tend to that immobile centre, and hence *the planets neither move exactly in ellipse nor revolve twice in the same orbit. So that there are as many orbits to a planet as it has revolutions* … and the orbit of any one planet depends on the combined motion of all the planets, not to mention the action of all these on each other. But to consider simultaneously all these causes of motion and to define these motions by exact laws allowing of convenient calculation exceeds … the force of the entire human intellect. Ignoring those minutiae, the simple orbit and the mean among all errors will be the ellipse. (Herivel 1965, 297.)

Many interesting observations can be offered about this scholium. Not the least of them is that its main point is squarely denied in the *Principia*: "the aphelia of the planets are at rest," (Newton 1687, 943) Newton brazenly claims in the General Scholium, even though he had dedicated Section 9 of Book I to deal with "the motion of bodies in mobile orbits, and the motion of the apsides" (Newton 1687, 534. See below). Indeed, the Copernican Scholium has been omitted from the *opus magnum* altogether, in which the rhetoric of perfection and accuracy is carefully maintained: "nature is simple" Newton reiterates at the beginning of Book III, it "does not indulge in the luxury of superfluous causes" and is "ever consonant with itself" (Newton 1687, 794; 795); "gravity towards the sun … decreases *exactly* as the squares of the distances" he concludes on the last page (Newton 1687, 943. Italics added). This is particularly telling because the scholium is central to Newton's *De Motu Sphæricorum Corporum in Fluidis*—one of the final drafts of the *Principia*. It also has clear merit as a summary of Newton's mechanical cosmology, and Newton was keen to provide such a summary to himself and his audience: he has written the "System of the World" twice, in two different modes and with different persuasive strategies in mind (Newton 1687, 793).

The discrepancy between practical acknowledgement of irreducible complexity and the insistent public avowal of discoverable, fundamental simplicity is a cultural phenomenon of utmost importance; part of the legacy of early modern science that its modern successor has never reflected on. But my interest in the following is not the discrepancy but the mathematical practices. I will try to demonstrate that the sentiment expressed by Newton in the scholium is a genuine expression of the metaphysical commitment embedded in his work, to which he is fully aware. In other words, I will argue that the accolades Newton coaxed from his followers capture what Kepler was aiming at, but not what Newton finally achieved.

Two additional points have to be noted concerning this scholium. The first is that it is not skepticism that Newton suggests. He does concede that "to consider simultaneously all these causes of motion and to define these motions by exact laws allowing of convenient calculation exceeds … the force of the entire human intellect." The import of this declaration, however, is not a lamentation of the limits of "the force of the entire human intellect." It is, rather, an introduction of what this intellect *is* capable of and should be expect to provide: "Ignoring those minutiae, the simple orbit and the mean among all errors." It is within our powers to decide what to "ignore." Newton can hardly be suspected of skepticism; my point is not that he is short of epistemological confidence but that he bases this confidence, in the Copernican Scholium and his subsequent work, on our active capacity to mathematically approximate a "mean among all errors."

The other point is that, like the two diagrams above, Newton's discussion seems to be on the level of phenomena—the motions of the planets—and one may read him as implying that there is, behind these phenomena, a level where "exact laws" rule. That the scholium narrates the effects of "centripetal forces" around a "center of gravity" may suggest that by "exact laws" Newton refers to the inverse square law (henceforth: ISL) as "the fixed foundations of His work." In this scholium, however (and I will argue later: in his use of ISL in general), this is not what Newton is advocating. There are "causes," in the plural, and the laws are to "*define* these motions" affected by them. These are human made laws that Newton refers to here, designed for "allowing of convenient calculation." The world painted by Newton in this purposefully-suppressed scholium is dominated by the specter of "as many orbits to a planet as it has revolutions," and his promise is not to find their "foundations" but only "the mean among all errors." The ISL, I will try to demonstrate, is such a "mean."

Kepler and Perfection

The dream of "foundations" could of course be very real. For Kepler, the aspiration to arrive, through mathematics, at the simple, divine infrastructure of our world was a genuine commitment (Cf. Field 1988; Stephenson 1994a, b). The trust in universal harmony and an effective belief in the power of mathematics to discover it is not merely a metaphysical, epistemological or religious position for him, but a working strategy. It is a 'working metaphysics', assumed in the argument based on the *Astronomia Nova* diagram and embedded in this work throughout. In his *Mysterium Cosmographicum,* published 13 years earlier, Kepler provides his most explicit expression of both: the universe, he tells his readers, is "complete, thoroughly ordered and most splendid" (Kepler 1621, 96–97). It is simple, and its structure necessary.

Kepler's mathematical inquiry is strictly structured by these assumptions. His question in the *Mysterium* is why there are exactly six planets, and his answer is that there are exactly five perfect solids. Thus, since the distances between the planets correspond to these solids, namely—since the proportions between the planets' distances can be shown to correspond to the proportions between the solids (for Tycho has demonstrated that there cannot be any material solids in the heavens)—then the number of planets has been explained—the mathematical directly accounts for the physical.

But what kind of san explanation is this? Why should abstract mathematical proportions account for a material fact? Why should the aesthetic value of these proportions be evidence for their truth? Guided by the metaphysics of order, Kepler suggests two complementary answers to this question. Either:

> God, like one of our own architects, approached the task on constructing the universe with order and pattern, as if it were not art which imitated Nature, but God himself had looked to the mode of building of Man who was to be (Kepler 1981, 53–55)
> Or:
>
> it is by some divine power, the understanding of the geometrical proportions governing their courses, that the stars are transported through the ethereal fields and air free of the restraints of the spheres. (Kepler 1981, 167)

Either mathematics is God's own blueprint for the universe, or the planets themselves are using the "geometrical proportions governing their courses" to navigate the empty vastness of the heavens. Kepler never gives up on the former possibility (in the 2nd edition of the *Mysterium* from 1621 he would use this idea as a proof for creation and the existence of God—see below). The latter is an awkward assumption; that the rationality of the structure can only affect the material realm if that realm (or elements of it, like the celestial bodies) is also endowed with rationality, and Kepler largely retreats from it in the *Astronomia Nova*. But, against common wisdom, Kepler never eschews the mathematical enthusiasm of the *Mysterium* when he adopts the physicalism of the *Astronomia Nova*.[2] This is how he himself analyses his intellectual development in a note he adds to this paragraph in the second 2nd edition of the *Mysterium*, published unchanged (apart from added annotation): "So indeed I supposed," he says, concerning the rationality of the planets,

> but later in my *Commentaries on Mars* [the *Astronomia Nova*] I showed that not even this understanding is needed in the mover. For although definite proportions have been prescribed for all the motions … by God the Creator, yet those proportions between the motions have been preserved … not by some understanding created jointly with the Mover, but by … the completely uniform perennial rotation of the sun [and] the weights and magnetic directing of the forces of the moving bodies themselves, which are immutable and perennial properties. (Kepler 1621, 168–169)

Note what it is that Kepler thinks he defended in the *Astronomia Nova*. The notion of perfect proportions remains untouched; changed is only the mechanism by which these proportions are "preserved." The mechanism preserving them is simple in itself: the rotation of the sun and the magnetism of the planets are "completely uniform [and] perennial." The mathematics—the analysis of the proportions between the solids and the consequent distances and periods—is left to safeguard the "complete, thoroughly ordered and most splendid universe."

Newton and the Moving Aphelia

It does not mean, of course, that Kepler was unaware of the difficulties in applying his grand mathematical scheme to the minute details of observation. Quite the contrary. The major part of the *Mysterium* is dedicated to this very task; in particular, to finding a place for the eccentricities of the planetary orbits between the nesting polyhedra. The request for the latest values of the eccentricities was the pretext for his correspondence with Brahe, leading to their illustrious collaboration. The point, however, is exactly this: Kepler excuses the eccentricities by fitting them into the mathematical model constructed according to independent principles. For Kepler, the world has a universal, harmonic, perfect structure, which can be discovered by a-priori, mathematical considerations, and into which one then has to fit the embarrassing particularities of the empirical.

Compare now Kepler's genuine pursuit of "Laws which God/Framing the universe, set not aside" to Newton's transformation of the argument embedded in the drawing sent to Hooke, which in its original form was so similar in structure to the anti-geostatic argument of Kepler's diagram. The final version of this is to be found in Propositions 43–45 of the first book of the *Principia* (Newton 1687, 534–545):

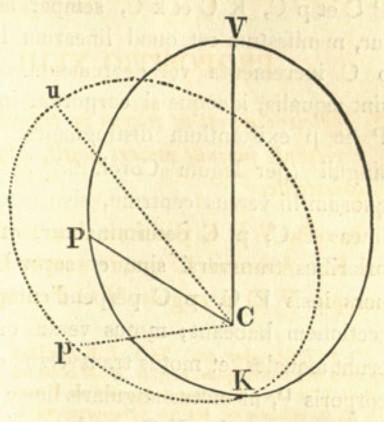
> If a body, under the action of a centripetal force that is inversely as the square of the height, revolves in an ellipse having a focus in the center of forces, and any other extraneous force is added to or taken away from this centripetal force, the motion of the apsides that will arise from that extraneous force can be found out … and conversely. (Ibid, 544)

Newton turns on its head his previous suggestion that the consequent motion of the apsides invalidates Hooke's proposal "of compounding the celestiall motions of the planetts of a direct motion by the tangent & an attractive motion towards a central body" (Hooke 1960, 2:297). Now, it is the ability to calculate this motion that assures him of the power of his mathematics and the validity of those very physical assumptions he adopted from Hooke.

But this new ability hinges on changing the very notion of an orbit and the way mathematics is used to construct it, and the change implies that the assumptions of perfection and simplicity underlying the original argument were abandoned. This is borne in the particulars of Newton's mathematics.

The crucial change transpires when Newton stops treating moving apsides as a sign that a trajectory is *not* an orbit. Instead, they become a property of *a particular kind* of orbit; a revolving orbit. Newton affects this in Proposition 43 by teaching his readers "to find the force that makes a body capable of moving in any trajectory that is revolving about the center of forces in the same way as another body in the same trajectory at rest" (Newton 1687, 534). Given orbit VPK (Fig. 4.3) with center of force at C, Newton shows how to construct for each point P on this closed curve (the diagram suggests it is a Keplerian ellipse, but nothing in the proof refers to or depends on this) an identical curve through *up* at a constant angle PC*p*, equal to VC*u*. Now, since VPK is an orbit around C, namely P is moving about C towards which it is drawn by centripetal force, it follows that area VCP is proportional to time. This is the very first theorem Newton proved in the *Principia* (Ibid, Prop. 1

Fig. 4.3 Newton's *Principia*, Proposition 43



Th. 1, 444–446). It is Kepler's law of areas, his so-called 2nd law, generalized from an empirical approximation into a mathematical truth about all bodies revolving around a center of force.[3] Additionally, by the conditions of the propositions, angle VC*p* is proportional to angle VCP. Hence, the area described by C*p* will be proportional to area VCP, and therefore proportional to time.

The proof that C*p* is sweeping areas proportional to time is what turns "the figure *u*C*p*" into a sector of an orbit around C. This follows from Newton's second theorem, according to which "every body that … by a radius drawn to a point … describes areas around that point proportional to time, is urged by a centripetal force tending toward that same point" (Ibid, Prop. 2 Th. 2, 446–448.). In other words, not only does every body orbiting around a center of force abides by the area law, the complementary is also true: every body abiding by the area law around a given point is describing an orbit around that point as a center of force, so *up*K is an orbit:

> the body, being always at *p*, will move at a perimeter of the revolving figure *u*C*p*, and will describe its arc *up* in the same time in which another body P can describe the arc VP, similar and equal to *up*, in the figure VPK. (Ibid, Prop. 43, 534)

Proposition 43 is pivotal not only because it allows Newton to claim control over the concept of a revolving orbit, but also because it formulates the difference between the stable and the revolving in terms of the difference of the laws of centripetal force creating them. In the next proposition he calculates exactly what this difference is:

> The difference between the forces under the action of which two bodies are able to move equally—one in an orbit that is at rest and the other in an identical orbit that is revolving—is inversely as the cube of their common height. (Ibid, Prop. 44, 535)

The transformation of the unruly, hence impossible, trajectory of the letter to Hooke into an orbit is thus an impressive show of skill and an important achievement; it is the culmination of Newton's treatment of "motion of bodies in orbits whose planes pass through center of forces" (Ibid, 545), after which he moves on to deal with oscillation. So when, in the General Scholium, he reverts to the claim that "the aphelia of the planets are at rest," it is not because he shies away from complex trajectories.

But what exactly has Newton achieved? C*p* obeys the areas law simply by virtue of C being a center of force, and "figure *u*C*p*" is sector of an orbit by virtue of obeying the areas law. But the body at *p* does not travel along the dotted line. Newton makes it very clear in his illustration to Proposition 44 (Ibid, 537): the body at *p* does not continue to *k* but to another point *m*. *up* is not a real trajectory—it is only a copy of VPK around one known point *p* of the real trajectory. The real trajectory, like that in the drawing sent to Hooke, has no regular line of apsides. Moreover—VPK itself is not a real trajectory; there is no body that moves on that curve. Both trajectories—at rest and revolving—are mathematical fictions. The orbit itself, the real trajectory in which the body is travelling, does not possess any mathematical status; it is not a recognizable curve and Newton does not presume to draw it. One may say that Newton returns the real trajectory to the status of mathematical and theoretical irrelevance it had in traditional astronomy, before Kepler has turned astronomy into a study of real motions and their causes. "There are as many orbits to a planet as it has revolutions," says Newton, so instead of real trajectories he is studying points like V and *p*—presumably locations to be determined from observations—and fictive orbits constructed for calculation purposes. Of course, the end of the calculation is a brilliant realization of Kepler's hope for *physica coelestis*: the mathematics provides causal account, relating real bodies by forces that impact their motions. But the original justification for this hope, the idea of simple mathematical infrastructure, has disappeared. Mathematics is not embedded in the behavior of the revolving body; it is only a sophisticated means to decipher it.

In propositions 43–45 of the *Principia* Newton put regularity into trajectories which follow no regular curve and have no stable line of apsides—they turn them into orbits. But it is *artificial* regularity; it is the work of art, the construction of fictive orbits. This regularity is the assurance that some features of these orbits can be determined by reason, but the determination comes by the application of art rather than by the discovery of rational or simple foundations.

Kepler's ISL

Kepler's planets required a rational orbit they could follow. Even when they followed those orbits mechanically, rather than by their own navigation, the orderly curve they were to draw in the heavens served as the equivalent of a final cause. What keeps Newton's body in its trajectory is not the draw of mathematical property but a universal physical property: mutual attraction between all parts and particles of matter. True, the most fundamental law governing the relations between this attraction and cosmology—the area law—is a geometrical law. But this law promises neither underlying simplicity nor resultant order. The law is so general that it is independent of both the causing force and the caused trajectory, so it tells nothing of either. This trajectory does not have to be any recognizable curve. It does not even need to be a closed curve—it holds, for example, for bodies on hyperbolic trajectories, arriving and then retreating to infinity.

There is, of course, a mathematical side to that universal property of matter: the mutual attraction declines by the square of distance. It is tempting to take the ISL as the mathematical foundations Halley and Voltaire were hailing Newton for discovering. In the following section I will demonstrate that this is not the case. The ISL never fulfills for Newton the foundational role that mathematical structures fulfilled for Kepler in either metaphysics or physical–mathematical practices, nor does it carry for Newton any of the mathematical certainty that Kepler required of his foundations.

The ISL is a particular case in point because it allows an unmediated comparison of the use of a mathematical law of nature in Kepler and Newton and highlights the change in the import of mathematics in the order—or ordering—of the universe. This is because it was Kepler who first introduced the ISL into mathematized natural philosophy, as the ratio between light and distance in his *Optical Part of Astronomy* of 1604 (Henceforth: *Optics*)[4]:

> just as [the ratio of] spherical surfaces, for which the source of light is the center, [is] from the wider to the narrower, so is the density or fortitude of the rays of light in the narrower [space], towards the more spacious spherical surfaces, that is, inversely. For … there is as much light in the narrower spherical surface, as in the wider, thus it is as much more compressed and dense here than there. (Kepler 1604, 10)

Though embedded in traditional mathematical optics, Kepler's ISL is a mathematical–physical law of a new kind: a law which captures causal properties in strictly mathematical terms. Five years after completing the *Optics*, Kepler attempted to demonstrate the prowess of such laws in the full-blown *physica cœlestis* of the *Astronomia Nova*, and the physical considerations, we shall see, made the ISL a difficult case in point. The import of light, the courier of the sun's powers to the planets, suggested it to Kepler as a perfect analogy by which to conceptualize his *virtus motrix*—the solar force stirring the inertial planets—but the same physical considerations forced him to abandon the analogy. In the *Optics*, however, Kepler derives the ISL directly from the spherical expansion of light: "there is as much light in the narrower spherical surface, as in the wider, thus it is as much more compressed and dense here than there." Kepler's light is a quasi-physical entity which has "density," but its physical properties can be inferred directly from its mathematical ones because it is also a uniquely mathematical entity:

> the spherical is the archetype of light (and likewise of the world); the point of the center is in a way the origin of the spherical solid, the surface the image of the inmost point, and the road to discovering. The surface is understood as coming to be through an infinite outward movement of the point out of its own self, until it arrives at a certain equality of all outward movements. The point communicates itself into this extension, in such a way that the point and the surface, in a commuted proportion of density and extension, are equal. (Ibid, 19)

The physical properties of light follow directly from its mathematical attributes because light is a substantiation of geometry. It is not simply that light happens to expand spherically—it is the embodiment of sphericity; "the spherical is the archetype of light." The mathematical structure of nature is the materialization of divine mathematical archetypes: "God the Creator had the Mathematicals with him as archetypes from eternity in their simplest divine state of abstraction" (Kepler 1621, 127, fn. 2), and through its mathematical essence, light serves as conduit of this divine mathematical structure into the world of matter.

For Kepler this is a solution to a problem that troubled him already in the *Mysterium* of 1596: "I shall have the physicists against me" he worried there, "because I have deduced the natural properties of the planets from immaterial things and mathematical figures" (Ibid, 122). The planets, we saw, required a mathematical path to follow, whether rationally or mechanically, and this mathematical structure had to be embedded in nature in a way that would make scientific and metaphysical sense. In 1621 Kepler was so proud of his solution to the problem of the physicalization of mathematics that in a note he added to the new edition of the *Mysterium* he celebrated it as an empirical argument for the existence of God, strong enough to defeat the greatest challenger of all:

> in the end Aristotle [would have to be] persuaded that splendid and plainly necessary causes for this matter could be derived from the harmonies as if from an archetype, [and] would accept with the fullest agreement the archetypes and, since they are ineffectual in themselves, God as the architect of the universe. (Ibid, 127, fn. 2)

Kepler's solution itself, however, is not my main point, nor his success in finding one. Most important is that he recognizes a problem and seeks a solution; that he looks for a justification for the efficacy of his mathematics and finds it in the metaphysics of divine simplicity and harmony—the metaphysics we learned to attribute to Newton. As argued, this metaphysics is not limited to reflection but determines Kepler's actual use of mathematical laws. In the case of the ISL, it provided the motivation and justification for the application of the law from optics to celestial dynamics, and it also set the limits of this application. The spherical dissemination of agency from the sun to the planets and its decline with distance suggest that light may be the *virtus* by which the sun makes the planets, made of inert matter, move about it. But these very geometrical considerations finally convince Kepler that light can, at best, serve as analogy—it cannot be one and the same with the solar motive force. The planets' velocity is inversely proportional to their distance from the sun, not to the square thereof, he reasons. Additionally, light is dispensed spherically, and the motive force, apparently—only in the plane of the ecliptic. The geometrical make-up of the two types of solar emanation—the mathematics embedded in their nature—is different, so the motive force cannot follow the ISL, hence cannot be light (Kepler [1609] 1992, 372–399).

Kepler does not attempt to apply the ISL to the calculation of the motion of the planets. When it comes to the actual plotting of orbits, his mathematical tools and techniques are those of traditional astronomy, and offer no place for a dynamic law. The ISL is not another mathematical tool for Kepler; it is an expression of the mathematical structure embedded in nature; Kepler is attempting to identify and characterize the force that moves the planets, and the mathematical properties of this force are where he expects the necessity and harmony of the divine archetypes to assert themselves. The commitment to God's geometrical "fixed foundations" dictated a fit between law and phenomena that, in astronomy, the ISL could not provide.

The ISL After Kepler

Kepler's torturous way of legitimizing his mathematical 'physics of the heavens' failed to impress even his popularizer and most ardent admirer, Ismaël Boulliau. Perhaps his Catholicism relieved Boulliau from Kepler's protestant worries about God and mathematical perfections, or perhaps it was the benefit of another generation of mathematized dynamical thought, but Boulliau simply could not see the point in Kepler's vacillations. "On the rocks of these hesitations," he exclaims in the *Astronomia Philolaica* of 1645, Kepler "crashes his very astronomy into shipwreck" (Bouliau 1645, 24), and suggests both geometrical and physical arguments to save the forsaken analogy between light and *virtus motrix*. Boulliau finds it almost hard to distinguish between them, using 'species'—that in the *Astronomia Nova* Kepler carefully reserved only to the solar force[5]—to denote light, and assaults every one of Kepler's diffident distinctions between the two.

Boulliau's patronizing tone suggests that he did not realize how daunting the task of making mathematics explanatory was for Kepler. He (Boulliau) never took up a similar challenge, and his geometrical and physical speculations remained completely distinct from each other, even if adjacent. Another 20 years later, when Robert Hooke was attempting to follow Kepler's footsteps, he found himself facing very similar difficulties and, like Kepler, was restrained from making full physical use of the ISL by the physical-geometrical considerations from which this ratio was born. Not that Hooke had much patience for neo-Platonic worries. In his 1665 *Micrographia* he seamlessly imports the ISL from light to gravity in the following parenthesized remark:

> [I say Cylinder, not a piece of a cone, because … that triplicate proportion of the shels of a Sphere, to their respective diameters, I suppose to be removed by the decrease of the power of Gravity] (Hooke 1665, 227. Cf. Gal and Chen-Morris 2005)

Hooke is concerned here with the Tychonic problem of the implications of atmospheric refraction on astronomical observations, and he conducts Torricelli-style experiments in order to calculate the size and density of the atmosphere. This off-hand argument allows him to approximate the height of the column of air above his mercury tubes: the decline of "the power of gravity" by the square of the distance means that instead of a truncated cone (in which the volume is proportional to the cube of height), he can calculate the column as a cylinder (namely—as if its volume is proportional to the height of the atmosphere).[6]

This almost frivolous use of mathematical approximation is already quite removed from Kepler's grave hesitations about the way the perfection of his geometry reflects the perfection of creation, the way perfect geometry is distributed into the imperfect physical realm (through light), and what all this allows by way of physical–mathematical hypotheses. But Hooke's application of the ISL actually has more in common with Kepler's attitude than might be assumed. As I have shown in a different place (Gal 2005), the only justification he has for the move is exactly the geometrical analogy: like light, gravity, and with it the atmosphere, expands spherically. The image of spherical 'explosion' of agency or active principle from center towards periphery, which produced the ISL for Kepler, is exactly what is on Hooke's mind when he inquires about the behavior of light in the atmosphere and how the atmosphere itself is constituted by gravity. Like Kepler, he treats the agency as operating on the enveloping "shells" and consequently feels entitled to apply the law for the decline of light to the decline of gravity. But, again like Kepler, these very considerations prevent him from making real *physica cœlestis* use of the ISL. In 1673 he promises a

> System of the World … answering in all things to the common Rules of Mechanics [which] depends on three Suppositions. First, That all Cœlestial Bodies Whatsoever, have an attraction or gravitating power towards their own Centers, whereby they attract not only their own parts … but … also … all the other Cœlestial Bodies that are within the sphere of their activity; and consequently that not only the Sun and the Moon have an influence upon the body and motion of the Earth, and the Earth upon them, but that [all the planets], by their attractive powers, have a considerable influence upon its motion as in the same manner the corresponding attractive power of the Earth hath a considerable influence upon every one of their motions also. The Second Supposition is this, That all bodies whatsoever that are put into a direct and simple motion, will so continue to move forward in a streight line, till they are by some other effectual powers deflected and bent into a Motion, describing a Circle, Ellipsis, or some other more compound Curve Line. The third supposition is, That these attractive powers are so much the more powerful in operating, by how much the nearer the body wrought upon is to their own Centers (Hooke 1930, 57–60)

Hooke's mechanical "System of the World" was to be based on the suppositions of universal attraction, Cartesian inertia and a mathematical force law: "these attractive powers are so much the more powerful in operating, by how much the nearer the body wrought upon is to their own Centers." All is ready to apply the ISL for the decline of "attractive powers" with distance, the law he so easily imported from light to gravitation eight years earlier in the *Micrographia*. But Hooke declines the opportunity: "what these several degrees [of decline]" he adds, "I have not yet experimentally verified." The image of spherical 'explosion' that related gravity to light seems to him inapplicable to the notion of "attractive power," as it seemed to Kepler concerning his motive force. So, like Kepler, he refrains from applying the law for decline from solar illumination to solar attraction. Hooke is not committed to Kepler's notions of mathematical order, but the similar geometrical reasoning which leads him to adopt the ISL prevents him from turning it into a flexible algebraic operator in the calculation of orbits.

Newton's ISL

This is exactly what Newton does. Establishing the ISL in two different ways (as should now be obvious, he did not need to 'discover' it), neither aspiring nor presuming certainty or divine harmonies, he can, "ignoring minutiae," find "the simple orbit and the mean among all errors." In the Third Book of the *Principia* and in various *scholia* and prefaces, Newton often presents the ISL as a paradigm of mathematical certainty injected into empirical investigation, and his disciples adopted this rhetorical stance. But in less public reflections like the Copernican Scholium, and more importantly in Newton's actual practice, the ISL is taken as a contingent empirical fact, which mathematics allows to approximate and then to flexibly employ.[7]

This is far from claiming that Newton takes these procedures lightly or skeptically. Quite the opposite: they take arduous effort and reflect a firm conviction in the power of mathematics to produce reliable knowledge. The legitimacy of this knowledge, however, is founded and attained very differently than Kepler or Halley presumed it was. Newton infers the ISL, first, by plugging Kepler's 'harmonic law' into a geometrical proportion relating orbits and periods to centripetal forces. This equation, however, he only proves for circular, uniform motion. He then infers the ISL, independently, from Kepler's 'first law'; for elliptical orbits whose center of force is at one of the foci (Cf. Gal 2002, 194–213). This demonstration, however, comprises also the difficult stipulation that a minor deviation of the sun from the focus – well below the empirical resolution – will make it completely wrong.

Newton derived an ISL from Kepler's harmonic law in the 1660s, and perfected the procedure in the same version of *De Motu* from which the Copernican Scholium is taken (Cf. Gal 2002, 197–206). Using a few fast-and-loose moves which Kepler would have hardly recognized as the "Mathematicals," which "God the Creator had with him as archetypes from eternity," Newton establishes a geometrical expression for the centripetal force which would make a body revolve uniformly in a circular orbit: $f \propto AD^2/R,$ where *AD* is an infinitesimal arc (Cf. Gal 2002, 174). He then adds five corollaries, all simple derivations from this expression. He assumes uniform motion, so *AD* is proportional to the body's velocity. Thus, combining $AD \propto V$ with $f \propto AD^2/R,$ it follows that:

Corr. 1. $f \propto V^2/R$.
Since the velocity of rotation is inversely proportional to the period of revolution, *i.e.*, $V \propto 1/T$, this is equivalent to:

Corr. 2. $f \propto R/T^2$.

Combining these two proportions, Newton can construct a force law—a ratio between force and distance—for *any* given ratio between the radius of the orbit and the period of revolution, and he demonstrates this capacity by providing three different ones:

Corr. 3. if $T^2 \propto R$, then f is distance-independent,
Corr. 4. if $T^2 \propto R^2$, then $f \propto 1/R,$ and
Corr. 5. if $T^2 \propto R^3$, then $f \propto 1/R^2$.

"The case of the fifth corollary holds for the celestial bodies … astronomers are now agreed"[8] he adds, almost as an afterthought (Herivel 1965, 260, translation on 279).

Newton has no use for the geometrical imagery that provided Hooke and Kepler with justification for their mathematical-causal claims. He has no difficulty accepting that the physical follows the mathematical; that the actual law governing the force is simply what one finds by installing the empirical data into an abstract mathematical formula. This outcome is contingent, as Newton stresses by running through possible force laws following imaginary data. The stress on contingency is important to him: the five corollaries of the *De Motu* are expanded to nine in Proposition 4 of the *Principia*. The case in which "the periodic times are as the $^3/_2$ powers of the radii" is just the sixth of them, and the language distinguishing this particular option in the consequent scholium is hardly more excited than in *De Motu*:

> The case of corol. 6 holds for the heavenly bodies (as our compatriots Wren, Hooke, and Halley have also found independently). Accordingly, I have decided that in what follows I shall deal more fully with questions relating to the centripetal forces that decrease as the squares of the distances from centers. (Newton 1687, 452)

Which force law to "deal more carefully with," Newton declares, is a matter of choice; he had "decided" on the ISL. This is not a mere turn of phrase: given that the proof is limited to "the centripetal forces of bodies that describe … circles with uniform motion" (Ibid, Prop. 4, 450), its application to the elliptical orbits and changing velocities of the primary planets *is* a decision, and not a trivial one.

The point is *not* that Newton allows himself a convenient tolerance in 'massaging' the empirical data into whatever mathematic apparatus is at his disposal. Quite the contrary. As he states in the Copernican Scholium, achieving "the mean among all errors" is the very task he undertakes in the *Principia*, and one which he carefully defines in the previous proposition and its corollaries:

> Proposition 3, Corollary 2: And if the areas are *very nearly* (*quam proxime*) proportional to the times, the remaining force will tend towards body T *very nearly*.
>
> Proposition 3, Corollary 3: And conversely, if the remaining force tends *very nearly* toward body T, the areas will be *very nearly* proportional to the times. (Ibid, 448–9)

Creating mathematical order in complex orbits by way of approximation is not a manner of tolerating inaccuracies or an assertion of epistemological pessimism. It is, rather, a demand: the mathematics and the observations should fit *quam proxime*. Namely: if an exact force law gives an ideal orbit, an approximate one should give an orbit within the resolution of the empirical data. Newton does not feel obliged to legitimize his physical use of mathematics the way Kepler does, but the commitments he accepts as part of this epistemology of controlled complexity also exact an unavoidable price. The *quam proxime* requirement all but prohibits demonstrating the ISL from the empirical data and Kepler's 'laws' directly, because very different laws can produce heavenly motions which are "very nearly" identical.

The problem presents itself most acutely in Propositions 10 and 11, as Newton completes his instruction of how "to find centripetal forces" (Ibid, sec. 2, 444) and moves to "the motion of bodies in eccentric conic sections" (Ibid, sec. 3, 462).[9] Using the same proto-infinitesimal techniques of the *De Motu* Newton proves in Proposition 10 that for a body traveling in an elliptical orbit, "the law of the centripetal force tending towards the center of the ellipse" is as the (changing) distance of the body from the center of force (Ibid, 459). In the next proposition, no. 11, he proves that if "the centripetal force [is] tending towards a *focus* of the ellipse," it will be inversely as the *square* of the distance (Ibid, 462–3). In other words, if the sun is in the center of the planets' elliptical orbits, gravity *increases* with distance; if the sun is at the focus of these orbits, gravity *declines as the square of this distance*. Mars' is the most eccentric of planetary orbits, and still, as calculated by Kepler, it deviates so little from the circular, that the sun is *both* "very nearly" at the center *and* very nearly at the focus. Obviously, gravity cannot both increase with distance and decrease with its square.

Newton's demand that his approximation would fit '*quam proxime*' does not express a failure to apply simple mathematics to complex nature. Rather, it is a particular constraint that Newton puts on both sides—mathematics and empirical data: the mathematical law should not provide an idealization of the natural motion, but a trajectory that approximates a particular curve. For the ISL to be a demonstrated law of nature, it is not enough to deduce it from Kepler's (idealized) first or third laws—the force law needs to converge towards ISL as the orbits converge to Kepler's first law. The relation between the ISL and the ellipse fails this criterion. But there is no fact of the matter as to whether the orbit is an eccentric circle or ellipse. After all, "the planets neither move exactly in ellipse nor revolve twice in the same orbit," so Newton is free to prove the ISL from the circle, as he does in corr. 6 (corr. 5 of *De Motu*). "The simple orbit and the mean among all errors [is] the ellipse," but simplicity is only one of the considerations which Newton applies in choosing the mathematical order to apply to nature. Given the empirical data and the *quam proxime* requirement, the ellipse does not allow one to distinguish between the various force laws, all possible, all contingent, that could create these rather than any other orbits. There is no over-arching concept of underlying simplicity to compel Newton to accept one approximation over another.

So when writing "I have decided [to] deal more fully with … centripetal forces that decrease as the squares of the distances from centers" Newton refers to a very serious decision. It is a similar decision whether to trust Kepler's first law, lean on the proof of the ISL from the ellipse, and breach the *quam proxime* requirement, or lean on the proof of the ISL from Kepler's third law, but assume the planetary motions are in circular orbits and uniform velocities—a patently false assumption.

Newton chooses the latter. Although he is very expansive in demonstrating the capacity of his mathematics to handle orbits along various conic sections and complex curves, his work with the real planetary orbits always assumes motion "in the circumference of a circle." To do so within the *quam proxime* requirement, Newton develops a very complex theorem (proposition 7) which allows him "to find the law of centripetal force tending toward any given point" inside this circular orbit. Expanding it on the basis of the preceding propositions, George Smith transformed Newton's geometrical proportion into modern algebraic notation in which force is inversely as:

$$\left(\frac{SP}{a}\right)^5+3\left(1-\varepsilon^2\right)\left(\frac{SP}{a}\right)^3+3\left(1-\varepsilon^2\right)^2\left(\frac{SP}{a}\right)+\left(1-\varepsilon^2\right)^3\left(\frac{SP}{a}\right)^{-1}$$

Where S (Fig. 4.4) is the hypothetical position of the center of force (the sun in the solar system), P—the position of the moving body (the planet), a—the diameter of the orbit (AV in the figure) and ε—its eccentricity (the distance of center of force—the sun—from the geometrical center to the obit). As Smith acutely points out, "SP

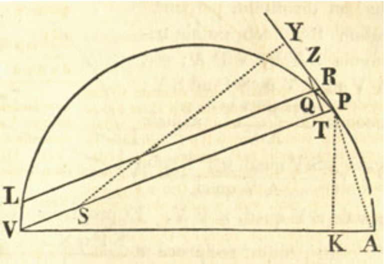


Fig. 4.4 Newton's *Principia*, Proposition 7

to the power of 2 is nowhere to be found in this expression" (Smith 2002, 40). Newton has no commitment to the ISL as representing anything beyond a convenient approximation. The expression as a whole, however, converges towards SP^2 the closer the eccentric circle can be seen as an approximation of an ellipse with the center of force at a focus—it provides that gravitation will be 'very nearly' proportional to $1/r^2$ if the planetary orbits are very nearly ellipses and the sun very nearly at their focus. The ISL was a feature of divine infrastructure for young Kepler—a reified 'mathematical'; it became a partially-flexible geometrical structure for Hooke; it has become sophisticated means of approximation is Newton's *Principia.*

Conclusion

In his seminal paper "Newton and the Fudge Factor" Sam Westfall argued that "not the least part of the *Principia*'s persuasiveness was its deliberated pretense to a degree of precision quite beyond its legitimate claim" (Westfall 1973, 751–2). To create this lure of precision, Westfall showed in great detail, "Newton brazenly manipulated the figures" (Ibid, 755) in determining the velocity of sound and the precession of the equinoxes, and in the all important demonstration that "the attraction holding the moon in its orbit is quantitatively identical to the cause of heaviness at the surface of the earth" (Ibid, 752).

This latter issue is of particular interest to us. "The law of universal gravitation," Westfall claims, "rested squarely on the correlation of the measured acceleration of gravity at the surface of the earth with the centripetal acceleration of the moon" (Ibid, 752). The former was accomplished by Huygens, following Mersene, by finding the length of the pendulum beating seconds. The latter Newton calculated by estimating the distance the moon would fall towards the earth in 1 min. To produce the level of precision he required, Westfall demonstrates, in this and the other examples, Newton was "doctoring the correlation" (Ibid, 754). As shown above, Newton allowed himself even more. "Gravity towards the sun … decreases exactly as the squares of the distances" he declares in the concluding lines of the General Scholium. This "is manifest from the fact that the aphelia of the planets are at rest", he continues, even though his is fully aware that the aphelia are anything but at rest, that indeed "there are as many orbits to a planet as it has revolutions." (Newton 1687, 943.)

Westfall is half bemused, half awed by Newton's audacity in "mending the numbers" (Westfall 1973, 757). The discussion above reveals, however, that the moves Westfall calls "more public relations than science" (Ibid, 755) are fundamental to the way Newton perceives the role of mathematics in the application of order to nature. Westfall accepts the textbook view that "Newton had shown that a system of planets orbiting the sun in accordance with Kepler's three laws entails a centripetal attraction towards the sun that varies inversely with the square of the distance from the sun" (Ibid, 752). But Newton's System of the World is based on a 'doctored' proof. This is so in its popular form, designed as the second book of the *Principia* but discarded by Newton for being too accommodating to the unschooled, and published only posthumously (in English translation) as *A Treatise of the System of the World* (1728). It is as just as true in its final, formal version as Book Three of the *Principia*. The claim that the force attracting the planets to the sun follows the ISL is proved by applying to Kepler's 3rd law a proportion that was only proved for uniform, circular motion. Not surprisingly, Newton's primary example for his System is provided by the moons of Jupiter, which are the most orderly of the solar system.

When Newton writes that "gravity towards the sun … decreases *exactly* as the squares of the distance as far out as the orbit of Saturn" he is addressing the public (Newton 1999, Bk. 3 General Scholium, 943. Italics added). Nowhere in the *Principia* is such a claim supported or applied. And when he writes that this "is manifest from the fact the aphelia of the planets are at rest" he also knows this is overstated at best (Ibid). In the original *System of the World* he referred, instead, to "the very slow motion of [the planets'] apses", and the Copernican Scholium argues that such motion is, in principle, necessary (Newton 1728, 24). But this flexible and approximate use of mathematics is neither reckless nor a show of epistemological despair. It reflects exactly the way Newton perceived his science: the human enforcement of mathematical order on messy nature.

"Nature and Nature's laws lay hid in night," wrote Pope, "God said, "Let Newton be!" and all was light" (Pope 1797, 2:403). What Pope had in mind was Kepler's Renaissance dream of divine order. Newton's achievement was largely indebted to relinquishing this dream in the name of the enforced order of the Baroque.

Notes

1. For the emergence of the concept of natural law in the seventeenth century see Steinle (1995). The literature on the religious aspects of the New Science and its laws of nature is too extensive to be accounted here; for a recent analysis and bibliography see Gaukroger, *The Emergence of a Scientific Culture*, 2006 esp. Chs. 2 and 4. An interesting aspect of this religiosity is brought to light by Brockey's account of the Jesuits' use of these ideas in their attempts to convert the high cultures of East Asia. See his *Journey to the East* (2007).
2. For the persistence of the metaphysics of the *Mysterium* in Kepler's later work see Voelkel (2001).
3. On the import of the proof of Kepler's area law in the *Principia* and its drafts see De Gandt 1995.

4. For Kepler's originality in introducing the Inverse Square Law, how it related to traditional optics and how it was converted to mechanics by Robert Hooke, see Gal and Chen-Morris (2005, 2006)
5. In Kepler's *Optics* "species of things" simply means light. *C f.* Gal and Chen-Morris (2010).
6. Hooke leaves it to the reader to do the calculations, but he clearly means that the decline is by the square of the distance. Otherwise the paragraph makes no sense.
7. The question of the difference between practice of approximation and rhetoric of perfection here is fundamental to the understanding of late seventeenth century science as part of its cultural context. It deserves a full treatment in a different place.
8. "Casus corolarij quinti obtinet in corporibus cœlestibus … jam statuunt Astronomi."
9. In the following I am much indebted to George Smith's excellent analysis of these theorems in "From the phenomenon of the Ellipse to an Inverse-Square Force: Why Not?" (2002).

References

Boulliau, Ismaël. 1645. *Astronomia Philolaica*. Paris: Simeon Piget.

Brockey, Liam Matthew. 2007. *Journey to the east: The Jesuit mission to China 1579–1724*. Cambridge: Harvard University Press.

De Gandt, François. 1995. *Force and geometry in Newton's Principia*. Princeton: Princeton University Press.

Field, J.V. 1988. *Kepler's geometrical cosmology*. London: The Athlone Press.

Gal, Ofer. 2002. *Meanest foundations and nobler superstructure: Hooke, Newton and the compounding of the celestiall motions of the planetts*. Dordrecht: Kluwer.

Gal, Ofer. 2004. Hooke, Newton, and the trials of historical examination. *Physics Today* 57(Aug.): 19–20.

Gal, Ofer. 2005. *Hooke's Programme*: Final thoughts. In *Robert Hooke: Tercentennial studies*, ed. Michael Hunter and Michael Cooper. London: Ashgate.

Gal, Ofer, and Raz Chen-Morris. 2005. Metaphysical images and mathematical practices: The archaeology of the inverse square law part I. *History of Science* xliii: 391–414.

Gal, Ofer, and Raz Chen-Morris. 2006. The use and non-use of mathematics: The archaeology of the inverse square law part II. *History of Science* xliv: 49–68.

Gal, Ofer, and Raz Chen-Morris. 2010. Baroque optics and the disappearance of the observer. *Journal of the History of Ideas* 71(2): 191–217.

Gal, Ofer, and Raz Chen-Morris. 2012. Nature's drawing. *Synthese* 185(3): 429–466.

Gaukroger, Stephen. 2006. *The emergence of a scientific culture: Science and the shaping of modernity, 1210–1685*. Oxford: Oxford University Press.

Halley, Edmund. [1687] 1962. Ode to Newton. In *The Principia*, revised by Florian Cajori (trans: Motte, Andrew). Berkeley: University of California Press.

Herivel, J.W. 1965. *The background to Newton's Principia*. Oxford: Clarendon.

Hooke, Robert. 1665. *Micrographia.* London: *Jo*. Martyn and Ja. Allestry, printers to the Royal Society; digital facsimile, The Warnock Library, Octavo Digital Rare Books.

Hooke, Robert. [1673] 1930. Manuscript in Hooke's collection at the Trinity Library. Reproduced in R.T. Gunther. *Early Science in Oxford*, vol. 10, pp. 57–60. London: Dawson of Pall Mall.

Hooke, Robert. 1960. Robert Hooke to Isaac Newton, November 24, 1679. In *The correspondence of Isaac Newton*, ed. H.W. Turnbull, 7 vols. Cambridge: Cambridge University Press.

Kepler, Johannes. 1937. In *Gesammelte werke 1571–1630*, ed. Walther von Dyck and Max Caspar. München: C. H. Beck.

Kepler, Johannes. [1604] 1968. *Ad Vitellionem paralipomena, quibus astronomiae pars optica traditur*. Frankfurt: C. Marnius, and Heirs of J. Aubrius; facsimile reprint, Brussels.

Kepler, Johannes. [1596 1st ed., 1621 2nd ed.] 1981. *The secret of the universe* (*Mysterium cosmographicum*) (trans: Duncan, A.M.; commentary by E. J. Aiton). New York: Abaris Books.

Kepler, Johannes. [1609] 1992. *New astronomy* (*Astronomia Nova*) (trans: Donahue, William H.). Cambridge: Cambridge University Press.

Newton, Isaac. 1728. *A treatise of the system of the world*. London: F. Faryam.

Newton, Isaac. 1960. Isaac Newton to Robert Hooke, December 13, 1679. In *The correspondence of Isaac Newton*, ed. H.W. Turnbull, 7 vols. Cambridge: Cambridge University Press.

Newton, Isaac. [1687] 1999. *The Principia* (trans: Cohen, I.B. and Whitman, A.). Berkeley: University of California Press.

Pope, Alexander. 1797. *The Works of Alexander Pope, Esq*, 9 Vols.

Smith, George E. 2002. From the phenomenon of the ellipse to an inverse-square force: Why not? In *Reading natural philosophy: Essays in history and philosophy of science and mathematics*, ed. David Malament, 31–70. Chicago: Open Court.

Steinle, Friedrich. 1995. The amalgamation of a concept – Laws of nature in the new sciences. In *Laws of nature: Essays on the philosophical, scientific and historical dimensions*, ed. Friedel Weinert, 316–368. Berlin: de Gruyter.

Stephenson, Bruce. 1994a. *Kepler's physical astronomy*. Princeton: Princeton University Press.

Stephenson, Bruce. 1994b. *The music of the heavens: Kepler's harmonic astronomy*. Princeton: Princeton University Press.

Voelkel, James R. 2001. *The composition of Kepler's Astronomia nova*. Princeton: Princeton University Press.

Voltaire. 1901. *The works of Voltaire, A contemporary version* (Critique and Biography by John Morley, notes by Tobias Smollett, trans. by William F. Fleming), 21 Vols. New York: E.R. DuMont.

Weinert, Friedel (ed.). 1995. *Laws of nature*. Berlin: Walter de Gruyter.

Westfall, Richard S. 1973. Newton and the fudge factor. *Science*, New Series, 179 (4075): 751–758.

Part II
Vision

Chapter 5
"The Quality of Nothing:" Shakespearean Mirrors and Kepler's Visual Economy of Science

Raz Chen-Morris

Abstract The gradual dissolution of early modern trust in vision as a source of knowledge of the natural world reached its climax towards the end of the sixteenth century. The outlines of this distrust are poignantly expressed in some of Shakespeare's major plays. Shakespeare's protagonists initiate a thorough investigation into what kind of knowledge is possible in a world of apparitions and visual deceptions. In his 1604 treatise on optics Kepler confronts similar doubts, suggesting a new visual economy based on "unsubstantial" shadows "similar to nothing", artificially produced within a camera obscura. In his short 1611 musings with the six-cornered snowflake, Kepler suggests mathematical ways for the observation, measurement and manipulation of *nihil* (i.e., Nothing). Confronting this new visual economy and the ensuing epistemological difficulties early modern natural philosophers suggested along with their scientific methods, a new poetics that allows the mind's eye to intuit a new sort of knowledge founded on a new super-sensory sight.

Introduction

The turn of the seventeenth century witnessed a growing fascination with modes of depiction of entities beyond the grasp of human perception such as infinity and nothingness.[1] This fascination, due perhaps to the revival of Lucretian poetics,[2] was shared by painters, mathematicians, natural philosophers and poets alike. While it is

---

Shorter versions of this paper were presented on different occasions: *Embodying Shakespeare*, Univerrsity of Utago, 7–9 February, 2008; *Baroque Science Conference*, University of Sydney, February 15–17, 2008; *Cohn Institute at 25 -Celebration Colloquium*, Tel Aviv University, May 11–13, 2008. The final version was written during my stay as a Rosenblum short term fellow at the Folger Shakespeare Library during the summer of 2009.

R. Chen-Morris (✉)
Science, Technology and Society Graduate Program, Bar Ilan University, Israel
e-mail: chenraz@mail.biu.ac.il

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_5,

difficult to trace any direct contacts between these disparate discussions, this paper aims to outline a framework to analyze the manner in which "nothing" resonates through late Renaissance and early Baroque high culture. I use the term "resonance" following Dror Wahrman's suggestion to understand "resonance as a gauge of conditions of communication and transmission, one that distinguishes … enunciations or practices that have little echo…from those that are picked up to be reproduced or mirrored or objected to or bounced around again and again and that thus continue to reverberate against a background buzz of similar enunciations or practices." Wahrman defines the task of a cultural historian as reconstructing the peculiar sound box that allows certain resonances to reverberate using and weaving freely "fragments from many texts".[3] This is a risky historiographical maneuver, facilitating a dialogue on a hermeneutic level between texts that usually treat different subjects in different contexts. In order for such an enterprise to succeed the historian has to take into account the particular intent and contexts, yet letting the shared issues and anxieties that haunt these cultural products to surface. In our case following the reverberations of nothing through various cultural products of the first decade of the seventeenth century provides a rich background tapestry for the emergence of new practices of observation that include both a new ontology of what is observed, and a new ethics of how to perform an observation.

Shakespearean Mirrors and the End of Renaissance Science

In Shakespeare's *King Richard the Second*, at the moment of climatic confrontation between Richard and the usurper Bolingbroke, the deposed king asks for a mirror:

> Give me the glass, and therein will I read,
> No deeper wrinkles yet? Hath sorrow struck
> So many blows upon this face of mine,
> And made no deeper wounds?

(Shakespeare 1597, 533, IV.i.275–278)

Richard, frustrated by the seeming inadequacy of his well-formed face and his own internal feeling of disintegration, throws the mirror to the ground. Together with the shattered mirror, the whole governing metaphor of mirroring splinters. Mirrors, in scholastic discourse, were significantly used as an analogy of the eye. This analogy braced the veridicality of sight as the epistemological foundation for human cognitive ability: the forms of the visible objects are stamped on the eye complete and undistorted just as they appear on the mirror's plane surface. Avicenna, the great Muslim philosopher wrote that the eye is like a mirror and the "visible object is like the thing reflected in the mirror by the mediation of air or another transparent body…if a mirror should posses a soul, it would see the image that is formed on it" (Avicenne 1955–58, 2:60, in Lindberg 1976, 49). The eye as a mirror guarantees the wholeness of perception and the correspondence between the internal imaging and external reality. Albertus Magnus embraced Avicenna's description and designated the eye a *speculum animatum*, asserting that "You know, however, that species is visible in the eye, the same as in a mirror, that is a clean and polished [surface] repeatedly receiving and representing the form, just as the thing"[4] (Magnus 1968, 7:122. See also Anzulewicz 1999). The eye as a mirror is no mere metaphor for scholastic philosophers but a physical analogy for the passage of images and their reception in the organ of sight. Dante summarizes this analogy in his *Convivio* (*The Banquet*, a philosophical essay written 1304–07): "The movement of the visible form along the medium is completed in the water found in the pupil of the eye, because the water is backed by a surface, rather like a mirror, which is made of glass backed with lead"(Akbari 2004, 130–131).

Medieval scholastics expanded this identification and applied the act of mirroring not just to sight but to linguistic signification and its role in religious knowledge as well. Understanding the Pauline motto *per speculum in aenigmate* as referring to the way knowledge is mediated through Christ the Word in this life, they combined this belief with a verbal species of sign theory derived from classical antiquity by way of the *trivium*. "This classical sign theory provides them with their mental tools, with the Stoic conviction that words are accurate signs of the things they signify, and with the Aristotelian certainty that sense data conduce authentically to knowledge of prior and nonsensible realities" (Colish 1983, 221 ff). The analogy of mirroring to sight remained central also in Renaissance culture, and Alberti mentions the notion of the eye as an animated mirror in his *De pictura* (Alberti 1991, 41). This equation is most notable in Brunelleschi's demonstration of his method of artificial perspective, in which he drew a panel of the Baptistery in Florence. One could see the painted building in perspective only by looking through a small peep-hole in the back of the panel, seeing the painting reflected in a mirror held in the viewer's outstretched hand. The mirror in this demonstration was the exact equivalent of the viewer's eye, guaranteeing its visual judgment[5] (C.f. discussions in Kemp 1978; Damisch 1994, esp. 88–164; Summers 2007, 61–67).

The shattered mirror in *Richard the Second* turns this dream that sight and language reflect and emulate reality into a nightmare. The notion of correspondence, whereby the internal human world mirrors and is reflected in the external macrocosm, comes to a dead end. In Richard's own words:

> For there it is, cracked in a hundred shivers.
> Mark, silent king, the moral of this sport:
> How soon my sorrow hath destroy'd my face
>
> (Shakespeare 1597, 533, IV.i. 288–290).

The silence of the king's inner self can be approached only through an act of violence and playful seriousness (the sport of shattering glass). To this performance Bolingbroke responds, that if external reality does not reflect one's inner states then the fallacy is in one's pretensions, and one's affections are mere shadows:

> The shadow of your sorrow hath destroy'd
> The shadow of your face. (Ibid, 291–292)

Bolingbroke alludes to the words of Sir John Bushy in his attempt to console the queen in the preceding Act, promising her that her grief and fear at her husband's departure are mere anxiety and do not signify any real approaching troubles.

Renaissance perspective with its promise of correct depiction of visual reality is turned into an awkward and anamorphous illusion:

> Each substance of a grief hath twenty shadows,
> Which shows like grief itself, but is not so;
> For sorrow's eye, glazed with blinding tears,
> Divides one thing entire to many objects,
> Like perspectives which rightly gaz'd upon,
> Show nothing but confusion; ey'd awry,
> Distinguish form. [6] (Ibid, 518–519, II.ii.14–20)

Just as the awkward perspective, the shattered mirror in the hands of Richard shows nothing but shadowy illusions that have no truth value whatsoever. Bolingbroke's words attempt to wave off the fragmented reflections and to cling to a matter of fact account of the political event in which an incompetent king is deposed by his better rival. Richard's response turns Bolingbroke's phrase on its head: the only truth lies invisible and silent within; and the external world is a world of shadows:

> Say that again.
> The shadow of my sorrow? Ha! let's see!
> 'Tis very true: my grief lies all within;
> And this external manners of laments
> Are merely shadows to the unseen grief
> That swells with silence in the tortured soul. (Ibid, 533, IV.i.293–298)

Yet Richard is no medieval Neo-Platonist. His world is not the world of monastic cloisters aiming at a complete divorce from material reality in favor of a transcendental divine illumination. Later in the play, confined in his cell at Pomfret Castle, Richard exclaims:

> I have been studying how I may compare
> This prison where I live unto the world;
> And, for because the world is populous,
> And here is not a creature but myself,
> I cannot do it. Yet I'll hammer it out. (Ibid, 539, V.v.1–5)

With these words Shakespeare captures the main epistemological discontent of late sixteenth century thought: One can know only oneself, yet that same self is silent and invisible, beyond language and sensory experience.[7] Human ambition to know external reality is vain and will lead to disaster, in the words of Montaigne:

> Oh man … There's not one so shallow, so empty, and so needy as thou art who embracest the whole world. Thou art the Scrutator without knowledge the magistrate without jurisdiction, and when all is done, the vice of the play (Montaigne 1603, 6:252–53).

No matter for how long one is to observe the world and speculate over it, the true causes that govern it and its motions will remain beyond human reach.

> Man can not declare and express them in wordes. Some man doth neyther geve rest unto his eyes by daye nor by night, and yet can he neither fynde the cause nor the reason of Gods workes, yea the more he laboreth to seeke it, so much the lesse shall he fynde it?[8] (Pope Innocent III 1576, Ch. 10)

Is it possible to break out of this cell of the inner self? Is there a way to transform this dark silence into the foundation of a new sort of knowledge? If man is not the measure of the world and human language cannot describe it how can a commensurate scale be found to relate to the alien world? These are the questions confronted by Hamlet in his search for a new view point from which inner imaging will be identical to the hidden truth of the world. When meeting his former companions, Rosencrantz and Guildenstern, Hamlet cries out a puzzling lamentation: "I could be bounded in a nutshell and count myself a king of infinite space – were it not that I have bad dreams" (Shakespeare 1604, 1162, II.i.260–62). Towards the end of this enigmatic conversation he reiterates his lamentation over the physical universe and its majestic appearances, depicting it as nothing but "a foul and pestilent congregation of vapors" (Ibid, 1163). Not only is the material world degraded into fleeting vapors but human crown of reason made in the image of God, turns into "quintessence of dust." The ephemeral world cannot supply the human investigator with any solid vantage point from which to contemplate its secrets, and the human intellectual tools are inadequate for this investigation and search.

Hamlet's bad dreams that the contemplation of infinity has no epistemological foundation haunted late sixteenth century natural philosophy and astronomy. Copernicus' claim that the invisible, yet calculable, motion of the earth governs the heaven was followed by Tycho Brahe's observations of the comet of 1577. Tycho pointed that there are no solid orbs in the heavens, and there is nothing corporeal that the astronomer measures other than shining dots moving through a vast space.

> The celestial machine is not a hard and impenetrable body, crammed full of various real orbs, as was heretofore believed by most people. On the contrary, very fluid and quite simple, it lies open everywhere without exertion or transportation by any real spheres, to the unimpeded revolutions of the planets… while heaven offers absolutely no obstacle. … Nor does this view admit any real and inappropriate penetration of the orbs (since they really are not present in heaven, but are propounded only for the sake of teaching and understanding the subject). (Brahe 1913–29, 4:159, in Rosen 1985, 22. See also Grant 1994; Randles 1999)

Giordano Bruno, in the last two decades of the sixteenth century, followed this situation to its radical conclusion: The universe is infinite, with no centre of signification; only the true inspired philosopher can offer valid interpretation based on direct perception of reality. Bruno, accordingly, rejects optics and geometry as legitimate venues for capturing heavenly phenomena.

> I would like to know by what principle of perspective or of optics we can definitely establish the correct distance, or the greatest and smallest difference, from any variation of the diameter. … We cannot establish the true size or distance of a luminous body from its apparent size. (Bruno 1995, 139–140)

Instead, Bruno suggests that only an extra-ordinary direct experience of the heavens can provide a new foundation of astronomical knowledge. The Nolan (the persona of the true philosopher in his dialogues) has no recourse to either authority or any mediated experience:

> But in truth it signifies nothing for the Nolan that the aforesaid [motion] had been stated, taught, and confirmed before him… For he [the Nolan] holds [the mobility of the earth] on other, more solid ground of his own. On this basis, not by authority but through keen perception and reason, he holds it just as certain as anything else of which he can have certainty. (Ibid)

Traditional astronomical knowledge accepts the limits of human sensory apparatus and must satisfy itself with circuitous and flawed perception of celestial phenomena. This state of affairs is the cause for the folly of astronomers and their monstrous inventions and theories:

> The Nolan … has freed the human mind and the knowledge which were shut up in strait prison of the turbulent air. Hardly could the mind gaze at the most distant stars as if through some few peepholes, and its wings were clipped so that it could not soar and pierce the veil of the clouds to see what was actually there.

What can the Nolan suggest instead of optics, geometry and peep-hole cameras? Bruno's radical suggestion is to accept the testimony of the one who has been to the sky and had direct experience of the true nature of the universe:

> How shall we honor this man [the Nolan] who has found the way to ascend to the sky, compass the circumference of the stars, and leave at his back the convex surface of the firmament?… Now behold, the man [the Nolan] who has surmounted the air, penetrated the sky, wandered among the stars, passed beyond the borders of the world, [who has] effaced the imaginary walls of the first, eighth, ninth, tenth spheres, and the many more you could add according to the tattlings of empty mathematicians and the blind vision of vulgar philosophers. Thus, by the light of his senses and reason, he opened those cloisters of truth which it is possible to us to open with the key of diligent inquiry; he laid bare covered and veiled nature, gave eyes to the moles and light to the blind, who could not fix their gaze and see their image reflected in the many mirrors which surround them on every side. (Ibid, 88–90)

Hamlet despairs of establishing a coherent relationship between cause and event, based on the untrustworthy data he had received from his father's ghost, and embraces Bruno's solution of a wondrous flight of philosophical imagination. Like Bruno, who rejects the ghost of traditional authorities, Hamlet faces the infinite and autonomous power of human imagination. Instead of its Aristotelian dependence on external objects and sensual perception, Hamlet's (and the Nolan's) imagination can convert the dust and the loam with which one stops "a beer-barrel" into the grandeur of an Alexander[9] (Shakespeare 1604, 5.1.228ff). It can transform daily occurrences and simple objects into containers and signifiers of meaningful events. The imagination as an independent agent eradicates the possibility of a simple move from reality to meaning, supplying the human contemplator with multiple levels of possible interpretative mediators between words and things.

The waning of King Lear sets the scene for a new point of view from which Lear can measure such invisible nonentities as "Love" and "Nothing" (See also Rotman 1993, esp. 78–86; and Ostashevsky 2004). Lear begins his inquiry with the assumption that man is the measure of all things, and thus human linguistic means are adequate in capturing the unknown and imperceptible realms. When Cordelia admits that all she can say in answering her father's demand to measure her love is nothing, Lear reprimands her with "How, how, Cordelia? mend your speech a little."

In one of the subsidiary scenes, the Earl of Gloucester gives a clue as to how one can observe Nothing. His bastard son Edmond pretends to hide a letter incriminating his brother the legitimate heir. Gloucester inquires "what paper were you reading?" and Edmund replays: "Nothing, my lord." On this answer the Earl marvels:

> No? What needed then that terrible dispatch of it into your pocket? The quality of nothing hath not such need to hide itself. Let's see. Come, if it be nothing, I shall not need spectacles. (Shakespeare 1608, 1201, I.ii.32–36)

This is an obvious pun on such late sixteenth century proverbs as "A man needth not spectacles to see the Sunne shine" (Browne 1582 E3v).[10] Gloucester, with this double negation, discloses the truth. In order to observe what is unobservable, that is nothing, one actually needs lenses, as artificial means that go beyond the capability of human senses.[11] Towards the end of the play, as Lear carries in his arms the body of Cordelia, the power of artificial lenses and mirrors is fully revealed. Lear realizes that speech cannot be mended and he cries out "Howl, howl, howl!" Lear protests against the inadequacy of human language in reflecting his feelings and of the human eye in seeing reality, stressing the first vowel in the shape of a zero:

> O, you are men of stone: Had I your tongues and eyes, I'd use them so that heaven's vault should crack. (Shakespeare 1608, 1238, V.iii.257–59)

However, neither the human eye nor human language can penetrate the heavens to come face to face with the mystical Nothing (the Kabalistic *Ayin*) that governs life and death. In order to penetrate into these depths of nothingness Lear needs artificial means of observation:

> I know when one is dead and when one is alive; she's dead as earth. Lend me a looking-glass; if that her breath will mist or stain the stones, why, then she lives. (Ibid, V.iii.260–264)

The only way to observe breath, that is, an almost invisible nothingness and simultaneously the essential sign of life, is to manipulate it through the application of mirrors, the device that produces false images and distorted reflections.

Kepler's Astronomical Speculations, Aristotelian Metabasis and Renaissance Imagination

Kepler picks this line of investigation and turns it into a new science. By means of this new science, Kepler aspires to capture and measure the "moving soul and infinite motion" that emanates from the sun moving the planets.[12] This moving soul is what Dante asserted to be"that lofty fantasy…the Love which moves the sun and the other stars."[13] In a sense Kepler seeks to quantify "love" in the same way as King Lear, only that he attempts to embed this "love" in the more visible "light". Kepler situates in the sun, the center of the universe and the source of its light, an *anima movens* that govern the motions of the heavenly bodies. In order to compute the operation of this localized moving soul Kepler turns to light, yet the role of light in

Kepler's celestial physics is ambivalent. In some places it serves as an analogy to the moving soul:

> [There is] a moving soul in the center of all the spheres, that is, in the Sun, and it impels each body more strongly in proportion to how near it is… Thus, just as the source of light is in the Sun, and the origin of a circle is at the position of the Sun, which is at the center, so in this case the life, the motion and the soul of the universe are assigned to that same Sun. (Kepler 1596, 199)

In other places Kepler almost identifies the *anima movens* with light: "Light and motion are connected by origin [i.e., the sun:] as well as by action and probably light itself is the vehicle of motion."[14] In any case, the association of the moving soul with light allows Kepler to regard the resulting motions of the planets not as mere mathematical hypotheses but as real physical occurrences.[15] The orbits of the planets are determined by their distance from the sun and describe necessary mathematical proportions in the heavens in the forms of the platonic solids. Kepler is well aware that this is a preposterous move and predicts the reaction of his critics against his speculations on how platonic solids are associated with physically determined planetary orbits

> I shall have the physicists against me because I have deduced the natural properties of the planets from immaterial things and mathematical figures, and furthermore because I dare to investigate the origin of the orbits out of bare (nuda) imaginary cross sections. (Ibid, Ch. 11, 122 [translation amended])

Kepler describes his critics sitting pensively, like Dürer's melancholic angel, pondering at a row of platonic solids lying in front of them, wondering how such imaginary constructs can supply scientific explanation to real physical bodies. At first glance this assumed criticism is an extrapolation of the traditional Aristotelian prohibition of *metabasis*. According to this principle one cannot "prove anything by crossing from another genus (*metabasis eis allo genos*) – e.g. something geometrical by arithmetic"[16] (Aristotle 1984, *Posterior Analytics*, Bk I, 7: 75a38, 1:122). The word *imaginatio*, however, signifies that a shift occurs in the meaning of the admonition against *metabasis*. Aristotle's criticism of the application of mathematics to physical phenomena was founded on his claim that the mind separates mathematical entities from material objects it senses. Although this is a mental operation, the existence and truth of such mathematical entities is dependant on external, physical and corporeal bodies. The observer of a physical, three-dimensional object creates an image of such an object in his imagination and this image is the substratum and the ontological anchor of any further intellectualization.

Mathematical sciences such as astronomy and optics challenged this neat Aristotelian scheme from its inception. Aristotle himself in his *Physics* asserts that: "While geometry investigates natural lines but not *qua* natural, optics investigates mathematical lines, but *qua* natural, not *qua* mathematical"[17] (Ibid, *Physics*, Bk. II, 194a 10–11, 1:331). The science of Optics is truly secondary and dependent on entities whose origin lies in another discipline. The observer of nature separates geometrical figures from the perceptual images of natural bodies; these figures are applied artificially when the need arises according to accepted rules of the intermediate disciplines such as astronomy, optics and music. Thus, in optics, the scientist constructs artificial lines and angles, although according to Aristotle’s analysis in *De anima*, in reality there are no rays, nor reflected rays or angles of incidence. The scientist, by assuming these imaginary constructs to be real, can explain away optical puzzles in order to save optical phenomena.

A further constriction on the application of imaginary mathematical constructions to natural philosophy is that in explaining natural phenomena one is condemned to limit the inquiry to visible features. Maestlin, Kepler’s teacher and mentor, attempted to legitimize the application of mathematics to a study of the heavens. His move consisted in qualifying astronomical research, contending that this concerns only apparent motions and not the real and invisible motions of the heavenly bodies (See for instance Methuen 1998, 192; Westman 2001; and Kepler 1984, 232n). According to the basic percepts of Euclidean optics the distance at which an object is seen is inversely proportionate to the size of the angle of vision and beyond a given distance it is no longer visible on account of the decrease in the size of the angle (Euclide 1959, props 3 and 5, 3–4). Sense experience yields only apparent motions of the heavenly bodies, and therefore the mathematical formal cause is applied not to a genuine and direct sense experience of the natural phenomena, but as an imaginary construct to indirect and dubious apparent motions. A few years later, in 1599, another Italian commentator – Lodovico Carbone, will bring these arguments to their extreme form, arguing that in astronomy one demonstrates either through appearances without determining whether they are causes or not, or through false premises (such as eccentrics and epicycles) assumed merely to save appearances.[18]

Kepler’s imagining stresses the autonomous existence of geometrical entities especially with regard to their application to natural phenomena. Yet, this only accentuates further the question of their epistemological reliability. In the Aristotelian context of *De anima*, the imagination is liable to errors and to the production of false images. This quality of the imagination requires the control of the soul’s faculty of rational judgment. The flaw of the imagination meant that those mental concepts, such as mathematical entities, are far removed from the reality of the physical world. Thus, for instance, Nicole Oresme, in his commentary on Aristotle’s *Politics*, remarked concerning the possibility of a universal empire:

> If everyone wished to avoid war and to obey one sovereign who could always understand every matter and judge it well and properly order everything … it would be a splendid thing, as it seems. But in fact this is like the fiction of the poet, or the speculation of the mathematician (ymaginacion mathematique). For as I have said earlier the world is not run by hypothesis. It must be taken as it is … and taking it as it is by nature, it scarcely seems possible that anyone could be a sovereign (universal) monarch and last for very long. (Babbit 1984, 63)

Mathematical hypotheses are ideal cases, but their application towards an understanding of the world (be it the natural or the political world) is dubious. Mathematical sciences can discover universal principles of beauty and order, but their relevance for the knowledge of concrete objects and events is controversial. Mathematics can supply ideal cases but when compared to “nature as it is,” it is like poetic fiction or political ideals never truly fulfilled or implemented in reality.

This critique was only one side in late medieval reevaluation of the role of the imagination in general and of mathematics in particular, in acquiring knowledge. In as much as the imagination can produce images of things never perceived by the senses, it can furnish the mind with foundations for the investigation of invisible and super-sensory entities.

In the Renaissance this quality of the imagination began to occupy a much more prominent place among the faculties of the soul. Gianfrancesco Pico della Mirandola in his *De imaginatione* (1501) argued that fantasy as a derivative of *phaos* (light) is the bridge between matter and mind. Fantasy (or imagination) is like sense in that it perceives the particular, corporeal and present; but "it is superior to sense in that, with no external stimulus, it yet produces images, not only present, but also past and future, and even such as cannot be brought to light by nature" (Pico della Mirandola 1930, 30–31).

The power of the imagination to view and observe what is not present to normal human sight was compared to artificial instruments of observation, and especially to lenses. Savanarola, in his sermon on the art of dying of 1496, argues that in order to comprehend something one must form a phantasm in the imagination. These phantasms are "the eyeglasses of the intellect." Just as eyeglasses mediate visible objects, so the imagination mediates true knowledge; and just as one needs clear lenses for observation so one needs a well ordered imagination. As opaque lenses distort visual data so the imagination governed by human passions can distort the truth. In order to control the imagination one has to form strong images that attract the imagination and move the human soul towards the Godhead.

> The strength of the fantasy moves man even against reason… If lustful things come into your fantasy, you will immediately be moved to evil. If you wish to do good and shun evil, make a strong *fantasia* of death. These are the eyeglasses I am telling you about. (Savanarola in Summers 1981, 114–15)

The notion that the imagination operates like a lens was greatly circulated in the sixteenth century. At the center of Naldini's allegory of dreams in the studiolo of Francesco de' Medici appears the feminine figure of Aurora holding a large spherical object. A careful examination of this object reveals it is a huge lens (Hamburgh 1996). The figure of Aurora embodies those moments at dawn, when according to classical tradition one can envision dreams of truth. These dreams, which allow a glimpse of the mysteries of the world, however, are not clear. The fogs that surround the dreams obstruct the slumbering mind from deciphering the riddles and grasping the message concealed in the vision. The danger is that the dreamer would glide over the visionary wings into the abyss of madness. The dreamer is prone to losing the ability to differentiate between imaginary apparitions springing from his desires and passions, and the divine message concealed in the dream's symbolic language. The lens as an artificial means, a human-made object, replaces the incompetent natural eye, as the tool to achieve this super-sensory perception of truth clouded in fantasy. In order to comprehend the inner content formed in the imagination, one has to filter them through artificial instruments shaped by the imagination. This paradoxical method seems only to distance one's inner fantasies away from external, physical reality. The lens, instead of reducing the fantastical mental image to a concrete sensible appearance, only accentuates the non-existent nature of these apparitions and specters.

Keplerian Shadows on a Wall

At the core of Kepler’s research program are imaginary mathematical entities and artificial instruments of observation. Yet in order for these to produce scientific knowledge, Kepler has to reduce the visible corporeal world into a theatre of nothing. In the preface to his magisterial work on optics Kepler declares that in observing the celestial bodies “we consider nothing but their image,” and this image is reduced to “light and shadow” on the one hand, and to “shape and quantity” on the other hand. These quantifiable illuminations are *mise en scène* of “this theatre of the world” and are signs suitable to the “human minds, likenesses of God”, assisting them in their investigations after deeper meaning. The way, however, in which humans can perceive these signs, is principally as shadows and deficiencies, that is, as no-thing that captures true knowledge of the world.

> Now, one may consider, that all the rest of Astronomy is closely associated with the motion of the Sun and the important assistance given us by the Moon, participating in the days just as in the nights, when all other means failed us: it is believed rightly that universal astronomy is born from this obscurity of the luminaries. Just as these darknesses may be the eyes of the astronomers, these defects may be a rich source for doctrines, and these “stains” may illustrate the most precise pictures on the mortal mind. O most excelled and commendable argument for all the nations about the glory of the shadow. (Kepler 1604, 16)

The only way to capture these images and shadows is by applying *camera obscura* as a micro-theatre of light and shadow. The *camera* assists the astronomer in avoiding “the inadequacy of the eyes” and is the only “sure procedure… for measuring something that happens in the sky” (Ibid, 39). Within the darkness of the *camera* the astronomer can “accomplish… what is completely impossible in clear light.” The eye cannot measure shadows as it is attracted to the strong light of the luminaries and to their visible effects, whereas within the *camera* one observe and measure the shapes and figures created by a ray of light coming through a window onto a wall. The stains of light on the wall were considered in late Renaissance culture as figments of the imagination. Leonardo da Vinci prescribed them as stimulation for the artistic invention.

> I remind you that it is worth your while to stop sometimes in order to look at the stains on walls, or… clouds… or similar things, in which, if you consider them well, you will find marvelous inventions… because in confused things the *ingegno* is stimulated to new things. (da Vinci 1956, 1:76)

Yet for Erasmus these stains on the wall are no mere playful invention of the human imagination but are representations of nothing as he explains in his *Adagia*:

> Clouds upon a Wall. In a letter to his son, Gregorius, Ausonius used the phrase ‘clouds upon a wall’ for something most similar to nothing or a dream; ‘have you ever seen a cloud painted upon a wall?’ he says. [By this] he indicates that the subject *(lemma)* of the poem subjoined to this letter is trifling and empty; for, a cloud is too unsubstantial to be expressed by colors. (Erasmus 1520, 405, 2nd Chilias, 4th Centuria, no. XXXVIII, in Panofsky 1951)

In producing the visible and corporeal world as an insubstantial shape on a wall Kepler can apply to it mathematical measurements circumventing the Aristotelian admonition against *metabasis*. Furthermore, Kepler not only reduces corporeal objects to shadows and images on the wall but defines the light as the agent that produces these stains on the wall as a two-dimensional, non-corporeal entity, for "light has no matter, weight, or resistance" (Kepler 1937, 2:8). Further more the ray of light is but a representation of its motion and thus has no material reality: "the ray is not in the transparent… but there was [a ray], or almost was" (Ibid, 40). Kepler asserts that the non-corporeal geometrical line can express and represent motion, "since motion cannot keep the one thing that is complete about it, its past" (Kepler 1611, 5). Light is the purest embodiment of motion as it penetrates the transparent medium and collides with dense surfaces "without matter or the dimension of solidity"[19] (Kepler 1937, 2:27–28).

One can sketch Kepler's main principles of his new visual economy:

A. In order for truly mathematical account of physical phenomena, one is to apply instruments of observation to reduce these phenomena to insubstantial, yet perfectly measurable, shadows and stains of light.
B. Motion is not a qualitative process of change defined by its beginning and end points, but as the vanishing continuum produced by the mobile.
C. Geometrical lines and points must be considered not as hypothetical devices or as aesthetic factors. They are embedded in the physical material realm determining its possible motions. A point at infinity is not merely a geometrical playful speculation but can be considered as a real cause in determining the path taken by light in a parabolic mirror.

Yet, Kepler has to face one further question: how can one guarantee that Nature is geometrical through and through; what vouchsafes that these procedures are not mere artificial hallucinations within the human mind. In order to confront these queries, Kepler has to assume that Nature herself is governed by a formative faculty corresponding to the human mind. In 1608 Kepler writes to David Fabricius and presents the whole natural world as suffused with this geometrically formative power:

> God has ordained certain animal faculties in this Earth, which are to be perceived as active in themselves in exuding vapours, assisting in a certain way the mind in perceiving geometrical beauty, or even discrete quantities. This is certainly that peculiar ordination of God: these faculties are Divine images, sensing the geometrical beauty, as God.[20] (Kepler 1858–71, 2:357)

Understanding these animal faculties that reside within the earthly matter supplies a powerful key to the mysteries of the universe. In fact, it provides Kepler with the true rules of the divine game:

God Himself, since because of His supreme goodness He cannot remain without occupation, has therefore played with the signatures of things, and has represented Himself in the world; and so I sometimes wonder whether the whole of Nature and all the beauty of the Heavens is not symbolized in Geometry …Just as God the Creator has played, so he has taught Nature, His image, to play, and indeed to play the same game that He has played before her[21] (Kepler 1937, 4:245–46).

As the divine game is incorporeal, Kepler's pawns have to evaporate into nothing while in play and reveal the bare grid of rules to his investigative mind. In 1611, just a few years after King Lear was first performed in London, Kepler attempted his own play with nothing.[22] In a small treatise entitled *A New Year's Gift or On the Six-Cornered Snowflake* Kepler seeks an object that will be small enough to be considered nothing, yet would "give promise of a geometrical speculation," and could "excite the desire for invisible things." After considering and rejecting different minute things and animals, while crossing over Karlsbrüke in Prague Kepler found that a snowflake is "something smaller than any drop, yet with a pattern" (Kepler 1611, 7). Snowflakes are even better than clouds as representations of nothing[23] (ibid): they both evaporate almost immediately and 'melt into nothing', or "they are entangled in larger plumes." Better still, while snow is an earthly meteorological phenomenon it is associated with astronomical research as "it comes down from heaven and looks like a star." Contemplating the flakes with their six corners and feathered radii triggers Kepler's inquiry into Nature's formative faculty:

> There must be some definite cause, why, whenever snow begins to fall, its initial formations invariably display the shape of a six-cornered starlet? (Ibid)

In order to answer this question Kepler has first to isolate the snowflake as a unique case. Other six-corner shapes in nature are formed with view to their utility. An external factor, such as cold cannot be the cause of the vapor's particles assuming the six-cornered shape of snow. Neither can material necessity resulting from the clash of their inner heat with external cold be the cause. Kepler concludes that a solution will be formulated only if he manages to "bring to light a way for the internal heat to fix the drop of vapor on three diameters, in the shape of an octahedron, or at any rate in a six-sided shape, on which matter may accumulate by condensation" (Ibid, 31) Kepler infers that the snowflake is formed over a "skeleton (so to say) of the octahedron with its three feathered diameters that intersect at right angles" (Ibid, 27). This skeleton is the bare mathematical form that operates from within the plumed particles as "the formative power" that resides in the center "disseminates itself equally in all dimensions." This formation is not random but is part of the "creator's design… preserved in the wonderful nature of animal faculties." In the case of shaping snowflakes this formative faculty operates with no obvious purpose and thus reveals itself in its pure form:

> Formative reason does not act only for a purpose, but also to adorn. It does not strive to fashion only natural bodies, but is in the habit also of playing with the passing moment. (Ibid, 33)

In detecting the way nature follows the mathematical rules of play one can gather the causes of physical processes. The formative principle in nature, or its soul (*anima*), is geometrical and seeks to fulfil itself in the orderly shape of a regular body, imitating the Creator, playing with geometrical forms. These are not merely imaginary constructs but incorporeal causes that operate from within the material realm as active agents.[24]

Underneath the multicoloured physical reality are hidden skeletal mathematical figures and shapes. These are not part of the realm of platonic Ideas but are the constitutive elements of material shapes and their motion. The natural philosopher should not be deluded by external sensible qualities but strive to explore and reveal the "nothingness", those bare mathematical figures and proportions, that govern and form material reality. The human mind can know the world not as it appears to the senses but as it can be reduced to invisible geometrical components.

The Renaissance sense of melancholic contemplation of the ever widening gap between human inner abstract intellection and external corporeal phenomena is turned on its head. Instead of Dürer's melancholic angel contemplating artificial regular solids pondering over their physical reality, Kepler reduces the physical world to bare geometrical forms and their motions.[25] Cold and heat in Kepler's analysis are not sensations but expansion and contraction, that is, motions and their direction. To know a snowflake is not to marvel like the Psalmist at its sensual similarity to wool, but to expose its skeleton of abstract geometrical structure. In concluding his "New Year's Gift" with the ironic "Nothing to follow", Kepler sets the outline of the Baroque answer to late Renaissance melancholic distress: scientific endeavor has to discard sensorial qualities and instead set its sight on "nothing". Descartes' mad melancholic persons "whose brains are so disordered and clouded by dark bilious vapors" are correct in distrusting their senses. No sensory criteria can establish a clear differentiation between wakeful states of mind and dreamy states and delusions. However, this calls not for a skeptical retreat but for a more nuanced control over one's imagination. When Kepler directed his telescope at the moon he reported to Matthias Bernegger:

> An experiment with the telescope that I carried out recently, produced a marvelous sight, altogether remarkable: cities and walls, which were circular because of the shape of the *umbra*. What more should I say? Campanella wrote his *City of the Sun*. And if we were to write a *City of the Moon*? Wouldn't it be excellent to paint the cyclopean mores of our times in lively colors, but leave the earth behind and go to the moon, for the sake of prudence? But what is the good of such evasive action, since neither More in his *Utopia* nor Erasmus in his *Praise of Folly* were so well protected that they didn't have to defend themselves? We must forsake the political tar pit and stay within the green and pleasant plains of philosophy. (Kepler 1937, 18:143)

The images refracted by the telescopic lenses are like confused insubstantial stains or clouds upon a wall. These apparitions can lead the mind towards poetic ingenious inventions that can entertain, but cannot lead to true philosophy. In order to scientifically investigate celestial phenomena the astronomer has to discipline his imagination, expurgate its playful disposition to form novelties and direct it, through careful analysis and comparison at the true structure of physical reality. One has to mobilize the imagination, without which no scientific play can ensue, yet one should be careful not to be carried away, and search beyond the imaginary symbols and differentiate between false constructions and true causes.

> I play in such a way that I do not forget that I am playing. For nothing is proved by symbols; no hidden thing is brought to light in natural philosophy, through geometrical symbols… unless by sure reasons it can be demonstrated that they are not merely symbolic, but are descriptions of the ways in which the two things are connected and of the causes of these connections. As in meteors, it is some active cause, capable of reason and Geometry that according to the rising aspect [a geometrical matter] in the heaven accommodates the modes and paroxysms of its operations, which is to excite the vapors of the subterranean humors,

> as they evaporate. The erroneous others support this by symbolization, expecting out of Saturn snow, out of Mars thunder, from Jupiter rain, and from Venus dew, out of Mercury wind, etc. But the Geometry of an aspect is an objective cause, moving the subterranean Arche to some impulse on account of which all the things mentioned, with no distinction, result – now this now that, according to the circumstances. (Ibid, 14:158)

In analysing celestial phenomena one should ignore the metaphorical and mythological significance of the heavenly bodies and their sensual qualities. All there really is in the sky are geometrical proportions and relations. In order to discover these geometrical aspects, one has to play, but this play has to be censured and meticulously adjusted to decipher the bare truth hidden underneath what appears to the senses in general and on the bodily eye in particular. Such disciplining of the imagination is exhibited in Kepler’s posthumous treatise *Somnium*. The treatise narrates a fantastic dream of witchcraft and daemons, of ludicrous travel to the moon and back during eclipse on the shadows that fall on the moon and on the Earth respectively. Yet the aim of the story is to give “an argument in favor of the motion of the earth or rather a ***refutation of the argument, based on sense perception, against the motion of the earth***” (Kepler 2003, 82. See also Chen-Morris 2005 [my emphasis]).[26] In order to overcome one’s immediate sense perception the astronomer has to muster the power of the imagination, and so to prove Copernicanism. Kepler suggests an imaginary point of view on the moon that would upset our normal perception and allow the invisible motion of the earth to be observed.

However, one cannot allow the imagination free reign, as chimeras and fantastic specters lure and attract the human mind to false games and the formation of distorted pictures of the world. In order to avoid these dangers Kepler adjuncts to his mythical narrative an extensive body of notes that direct the reader’s mind beyond the immediate fantasy to comprehend the astronomical truth disclosed within it.

Towards Baroque Modes of Observation

The natural philosophers of the first decades of the seventeenth century consigned the imagination to the acquisition of knowledge. They no longer sought to suppress it but attempted to harness it in order to observe what the eye cannot see. If as Francis Bacon contends, “it is a false assertion that the sense of man is the measure of things”[27] (Bacon 1620, Aphorism XLI, 57) then the imagination is needed to fathom a new measuring rod. The faculty of imagination, however, is traditionally associated with human passions that every so often lead it astray, producing sensual delusions. In order for the imagination to fulfill its role in the new scientific endeavor the passions must be regulated and supervised. Galileo blames his Jesuits adversaries for following “the strength of their passions”, and for failing “to notice that the contradiction of geometry is the bald denial of truth” (Galileo 1960, 164). One has to apply the faculty of imagination in order to perceive the world as a book in which “philosophy [is] written in the language of mathematics.” (Ibid, 183.) Yet the natural philosopher has to be vigilant and avoid being trapped in “the bounty of nature in producing her effects” (Ibid, 252). Early modern natural philosophers found in the *camera obscura* and the telescope the artificial scenes for disciplining human fantasy. Instead of "sensations, which… have no real existence" (Ibid, 312. See Osler 1973; Daston 1984), one can observe through these "spectacles" the true, yet unadorned, skeletal and bare geometrical discs and crescents of the planets. By concentrating the viewer's attention on shadows, light stains and ephemeral reflections and by adjusting the eye to a subsidiary role, the dangers and chimeras the passionate imagination is disposed to form are avoided. Reining the passions and attenuating the imagination reveal that the hidden and true shape of Nature is exactly the same as the geometrical figures the human mind contemplates. Scientific investigation does not require more powerful eyesight, but shunning of sensory experience altogether. Through artificial means the imagination is tamed and can be used to direct the mind's eye to know those bare and imaginary mathematical characters, and how they form the natural properties and paths of the corporeal world.

Notes

1. For the treatment of the void in Dutch still-life and its relation to Pascal's notions of emptiness and infinity see Grootenboer (2005), pp. 61–96; for the fascination of early seventeenth century Spanish literature with nothingness see Castillo (2010), pp. 37–75; for the mathematical treatment of infinity in theories of artificial perspective see Field (1997), pp. 178–234; for the treatments of infinitesimals in early modern mathematics see Baron (1969) and Blay (1998).
2. See Passannante (2011).
3. See Wahrman (2006). pp. xv–xvi.
4. "Sciam autem, quod sicut fit species visibilis in oculo, ita fit in speculo, quod est tersum politum recipiens formam et repraesentas iterate, sicut est res."
5. For a definitive account of the use of mirrors and lenses in the Renaissance see Ilardi (2007).
6. These words were extensively discussed in current literature *cf.* Gilman (1978), 88–128; Žižek (1991), 9–12.
7. For further and extensive analyses of the epistemological crisis of the late sixteenth century see Popkin (1979), Reiss (1997), esp. pp. 45–69; Clark (2007).
8. The translation of this medieval moral treatise won great popularity in late sixteenth century England.
9. "Why may not imagination trace the noble dust of Alexander till 'a find it stopping a bunghole? […] Alexander died, Alexander was buried, Alexander returneth to dust; the dust is earth; of earth we make loam; and why of that loam whereto he was converted might they not stop a beer barrel?
10. Also Misodiaboles (1596), A2f.: "You… are seene for a spectacle of follie, to those that cannot see without their spectacles".
11. As Jay L. Halio, the editor of the *New Cambridge Shakespeare's King Lear* comments: "Spectacles are a symbol of what Gloucester does need. He does not see through Edmond's plots". Gloucester does not see Edmond's invisible intentions but only the external, superficial actions that any eye can see with no aid. In order to see beyond this one needs an artificial aid. Gloucester shows himself entirely credulous. (Shakespeare 1992, 114: note to line 35); see also Heilman (1948), pp. 45, 154.
12. "Deinde, in Sole est anima movens et motus infinitus, in mobilibus decrementum motus duplex primo inaequalitas reditus, quam causatur amplitudo orbium inaequalis, etsi vigor motus esset idem in omnibus orbibus, 2. Sed jam ille vigor motus, (ut in opticis lux) quo longius a fonte est, hoc debilior est." Kepler (1937)-, vol. 13, Nr. 22, p. 32.
13. Dante's words are an allusion to Aristotle's assertion that the first principle, which is the immaterial unmoved mover, produces motion in the movable heavens through being loved: "… The one

is unmovable and the other is not. Thus it produces motion by being loved, and it moves the other things.” Aristotle, “Metaphysics”, XII, 7:1072^{b}3-4, in Aristotle (1984), 1694.
14. “Nam lux et motus utique ut origine sic etiam actibus conjunctj, et forsan ipsa lux vehiculum motus est.” Kepler (1937), Nr. 23, 13: 38.
15. In his *Astronomia nova* Kepler abandons this analogy of light and the moving soul in favor of a more abstract magnetic force. See Stephenson (1994), 68–75; Gal and Chen-Morris (2005), and 2006.
16. For extensive discussions of this theme see W. Roy Laird (1987) and (1997).
17. For a discussion of the status of geometrical lines in optics see Chen-Morris (2001).
18. See further discussions on the classification of the sciences in late sixteenth century Italian context in Mikkeli (1992) and 1997.
19. “…sine tamen materia aut soliditatis dimensione”
20. “Deum ordinasse facultates quasdam animales in his Terris, mentis quodammodo participes ad percipiendas geometricas pulchritudines, seu etiam quantitates discretas, quibus perceptis ipsae essent operosae in exsudandis vaporibus. Haec est igitur illa peculiaris Dei ordinatio, facultates illae sunt imagines Dei, percipientes geometricam pulchritudinem, ut Deum.”
21. “Dasz Gott selber/da er wegen seiner allerhochsten gute nicht feyren konnen/mit den *signaturis rerum* also gespielt/und sich selbst in der Welt abgebildet habe: Also dasz es einer ausz meinem Gedancken ist/Ob nicht die gantze Natur und alle himmlische zierligkeit/in der *Geometria symbolisiert* sey … Wie nun Gott der Schopffer gespielt/also hat er auch die Natur/als sein Ebenbild lehren spielen/und zwar eben das Spiel/das er jhr vorgespielet.” Quoted and translated in Walker (1978), 55–6. For Kepler’s sense of play and mode of inquiry see also Hallyn (1993), esp.163–202; Jaeger (1996); and Findlen (1998).
22. This short and playful treatise should be read on the backdrop of Kepler’s ongoing contention with epicurean atomism. See Boner (2007).
23. Kepler quite explicitly rejects clouds as the embodiment of nothing. He initially approaches the addressee of his treatise and comments that “accept with unclouded brow this enrichment by nothing” and then plays down Aristophanes’ “Clouds”: “Away with that panderer to vulgar scorn and ignorance, Aristophanes; what need have I of Socrates, the theme of his play?”
24. Kepler discussed the “earthly soul”, or “animated faculty” in the context of his astrological theories, especially in his *De fundamentis astrologiae certioribus*, 1602. See Field (1997). For his discussion of this animated faculty in *Harmonices mundi* see Schwaetzer (1997); and in the context of Kepler’s polemics with Robert Fludd see Boner (2006).
25. For Renaissance notion of melancholy and its relationship with mathematics see: Klibansky et al. (1964), 317–38; Elkins (1994), 166–76; and Mazzio (2004).
26. For a somewhat different interpretation of Kepler’s *Somnium* see Paxson (1999), Spiller (1999); and Swinford (2006).
27. “Falso enim asseritur, Sensum humanum esse Mensuram rerum.”

References

Akbari, Suzzane Conklin. 2004. *Seeing through the veil: Optical theory and medieval allegory*. Toronto: University of Toronto Press.

Alberti, Leon Battista. 1991. *On painting* (trans by Cecil Grayson, with an introd. and notes by Martin Kemp). London: Penguin Books.

Albertus, Magnus. 1968. De anima. In *Alberti Magni Opera Omnia*, ed. Clemens Stroick. Köln: Aschendorff.

Anzulewicz, Henryk. 1999. *De forma resultante in speculo: Die theologische relevanz des Bildbegriffs und des Spiegelbildmodells in den frühwerken des Albertus Magnus*. Münster: Aschendorff.

Aristotle. 1984. *The complete works of Aristotle*, ed. J. Barnes, 2 vols. Princeton: Princeton University Press.

Avicenne. 1955–58. *Le livre de science* (trans: Achena, Mohammad and Henri Massé), 2 vols. Paris: Les belles lettres.

Babbit, S.M. 1984. Nicole oresme: The limits of imagination. *Mediaevalia* 10: 63–80.

Bacon, Francis.1620. *Novum organum*. In *Franciscy de Verulamio, summi Angliae cancellarij instauratio magna*. Londini: Apud [Bonham Norton and] Ioannem Billium typographum regium.

Baron, Margaret E. 1969. *The origins of the infinitesimal calculus*. Oxford: Pergamon Press.

Blay, Michel. 1998. *Reasoning with the infinite: From the closed world to the mathematical universe* (trans: DeBevoise, M.B.). Chicago: University of Chicago Press.

Boner, Patrick J. 2006. Kepler's living cosmology: Bridging the celestial and terrestrial. *Centaurus* 48: 32–39.

Boner, Patrick J. 2007. Kepler v. the Epicureans: Causality, coincidence and the origin of the new star of 1604. *Journal for the History of Astronomy* 38: 207–221.

Brahe, Tycho. 1913–29. *Tychonis Brahe dani opera omnia*, ed. J.L.E. Dreyer, 15 vols. Copenhagen: Gyldendal.

Browne, Robert. 1582. A booke which showeth the life and manners of all true christians, and howe vnlike they are vnto Turkes and Papistes and heathen folke. Middelburgh: imprinted by Richarde Painter.

Bruno, Giordano. 1995. *The Ash Wednesday Supper (La cena de le ceneri),* ed. and trans. E.A. Gosselin and L.S. Lerner [Renaissance Society of America Reprint Texts 4]. Toronto: University of Toronto Press.

Castillo, David R. 2010. *Baroque horrors: Roots of the fantastic in the age of curiosities*. Ann Arbor: The University of Michigan Press.

Chen-Morris, Raz D. 2001. Optics, imagination, and the construction of scientific observation in Kepler's new science. *The Monist* 84: 453–486.

Chen-Morris, Raz 2005. Shadows of instruction: Optics and classical authorities in Kepler's Somnium. *Journal of the History of Ideas* 66: 223–243.

Clark, Stuart. 2007. *Vanities of the eye: Vision in early modern European culture*. Oxford: Oxford University Press.

Colish, Marcia L. 1983. *The mirror of language: A study in the medieval theory of knowledge*. Lincoln: The University of Nebraska Press.

Damisch, Hubert. 1994. *The origin of perspective* (trans: Goodman, John). Cambridge: MIT Press.

Daston, Lorraine J. 1984. Galilean analogies: Imagination at the bounds of sense. *Isis* 75: 302–310.

Elkins, James. 1994. *The poetics of perspective*. Ithaca: Cornel University Press.

Erasmus, Desiderius. 1520. *Adagia*. Basel: Froben.

Euclide. 1959. *L'Optique et la catoptrique* (trans: Ver Eecke, Paul). Paris: Blanchard.

Field, J.V. 1997. *The invention of infinity: Mathematics and art in the Renaissance*, Oxford: Oxford University Press.

Findlen, Paula. 1998. Between carnival and lent: The scientific revolution at the margins of culture. *Configurations* 6: 243–267.

Gal, Ofer, and Raz Chen-Morris. 2005. The archaeology of the inverse square law part I: Metaphysical images and mathematical practices. *History of Science* 43: 391–414.

Gal, Ofer, and Raz D. Chen-Morris. 2006. The archaeology of the inverse square law part II: The use and non-use of mathematics. *History of Science* 44: 49–68.

Galilei, Galileo. 1960. The Assayer. In *The controversy on the comets of 1618*, ed. and trans. Stilman Drake and C.D. O'Malley. Philadelphia: University of Pennsylvania Press.

Gilman, Ernest B. 1978. *The curious perspective: Literary and pictorial wit in the 17th century*. New Haven/London: Yale University Press.

Grant, Edward. 1994. *Planets, stars, and orbs: The medieval cosmos, 1200-1687*. Cambridge: Cambridge University Press.

Grootenboer, Hanneke. 2005. *The rhetoric of perspective: Realism and illusionism in seventeenth-century Dutch still-life painting*. Chicago: The University of Chicago Press.

Hallyn, Fernand. 1993. *The poetic structure of the world: Copernicus and Kepler* (trans: Leslie, D.M.). New York: Zone Books.

Hamburgh, Harvey. 1996. Naldini's *Allegory of dreams* in the studiolo of Francesco de' Medici. *The Sixteenth Century Journal* 27: 679–704.

Heilman, Robert. 1948. *The great stage: Image and structure in "King Lear"*. Baton Rouge: Louisiana State University.

Illardi, Vincent. 2007. *Renaissance vision from spectacles to telescopes* [Memoirs of the American Philosophical Society, vol. 259]. Philadelphia: American Philosophical Society.

Jaeger, C.Stephen. 1996. Johannes Kepler: Poetic inspiration and scientific discovery. In *Knowledge, science, and literature in early modern Germany*, ed. Gerhild Scholz Williams and Stephan Schindler, 117–130. Chapel Hill/London: University of North Carolina Press.

Kemp, Martin. 1978. Science, non-science and nonsense: The interpretation of Brunelleschi's perspective. *Art History* 1: 134–161.

Kepler, Johannes. 1604. *Ad Vitellionem paralipomena, quibus astronomiae pars optica traditor*. Francofurti: Claudius Marnius and Haeredes Ioannis Aubrii.

Kepler, Johannes. 1611. *Strena, seu De nive sexangula*. Frankfurt. English translation: Kepler, Johaness. 1966. *A new year's gift, or on the six-cornered snowflake*, ed. and trans. Colin Hardie. Oxford: Clarendon Press.

Kepler, Johannes. 1858–71. *Ioannis Kepleri astronomi opera omnia*, 8 vols., ed. Ch. Frisch. Frankforti a. M. et Erlangae: Heyder et Zimmer.

Kepler, Johannes. 1937. *Gesammelte Werke*, ed. Walter von Dyck and Max Caspar, 24 vols. Munich.

Kepler, Johannes. (1596 1st ed., 1621 2nd ed.) 1981. *Mysterium cosmographicum: The secret of the universe* (trans. A.M. Duncan, introd. and commentary by E.J. Aiton). New York: Abaris Books.

Kepler, Johannes. 1984. *Le secret du monde. Introduction, traduction et notes d'Alain Segonds*. Paris: Les Belles Lettres.

Kepler, Johannes. 2003. *Kepler's somnium: The dream or posthumous work on lunar astronomy* (Translated with a commentary by Edward Rosen Mineolta). New York: Dover Publication.

Klibansky, Raymon, Erwin Panofsky, and Fritz Saxl. 1964. *Saturn and melancholy: Studies in the history of natural philosophy, religion and art*. London: Thomas Nelson and Sons.

Laird, Roy W. 1987. Robert Grosseteste on the intermediate sciences. *In Traditio* 43: 147–169.

Laird, Roy W. 1997. Galileo and the mixed sciences. In *Method and order in Renaissance philosophy of nature: The Aristotle commentary tradition*, ed. D.A. Di Liscia, E. Kessler, and Ch Metheun, 253–270. Aldershot: Ashgate.

Leonardo, da Vinci. 1956. *Treatise on painting*, ed. A.P. McMahon, 2 vols. Princeton: Princeton University Press.

Lindberg, David C. 1976. *Theories of vision from Al-Kindi to Kepler*. Chicago: The University of Chicago Press.

Mazzio, Clara. 2004. The three dimensional self: Geometry, melancholy, drama. In *Arts of calculation: Quantifying thought in early modern Europe*, ed. D. Glimp and M.R. Warren, 39–65. New York: Palgrave Macmillan.

Methuen, Charlotte. 1998. *Kepler's Tübingen: Stimulus to a theological mathematics* [St. Andrews Studies in Reformation History]. Aldershot: Ashgate.

Mikkeli, Heikki. 1992. *An Aristotelian response to Renaissance humanism: Jacopo Zabarella on the nature of arts and sciences*. Helsinki: The Finnish Historical Society.

Mikkeli, Heikki. 1997. The foundation of an autonomous natural philosophy: Zabarrella on the classification of arts and sciences. In *Method and order in Renaissance philosophy of nature: The Aristotle commentary tradition*, ed. D.A. Di Liscia, E. Kessler, and Ch Metheun, 211–228. Aldershot: Ashgate.

Misodiaboles. 1596. *Vlysses vpon Aiax. Written by Misodiaboles to his friend Philaretes*. London: [by R. Robinson?] for Thomas Gubbins.

Montaigne, Michel de. [1603] 1965. *Montaigne's essays* (trans: Florio, John), 3 vols. London: J.M. Dent & Sons.

Osler, Margaret J. 1973. Galileo, motion, and essences. *Isis* 64: 504–509.

Ostashevsky, Eugene. 2004. Crooked figures: Zero and Hindu-Arabic notation in Shakespeare's *Henry V*. In *Arts of calculation: Quantifying thought in early modern Europe*, ed. D. Glimp and M.R. Warren, 205–228. New York: Palgrave Macmillan.

Panofsky, Erwin. 1951. "Nebulae in Pariete": Notes on Erasmus' Eulogy on Dürer. *Journal of the Warburg and Courtauld Institutes* 14: 34–41.

Passannante, Gerard. 2011. *The Lucretian Renaissance philology and the afterlife of tradition*. Chicago: The University of Chicago Press.

Paxson, James J. 1999. Kepler's allegory of containment, the making of modern astronomy, and the semiotics of mathematical thought. *Intertexts* 3: 105–123.

Pico della Mirandolla, Gianfrancesco. 1930. *Gianfrancesco Pico della Mirandola on the Imagination.* The Latin text with an Introduction, an English translation, and notes by Harry Caplan. [Cornell Studies in English, Vol. XVI]. New Haven: Yale University Press.

Pope Innocent III. 1576. The mirror of mans lyfe Plainely describing, what weake moulde we are made of: what miseries we are subiect vnto: howe vncertaine this life is: and what shal be our ende. English: H. Kirton. Imprinted at London : By Henry Bynneman.

Popkin, Richard H. 1979. The history of scepticism from Erasmus to Spinoza. Revised and expanded edition. Berkeley: University of California Press.

Randles, W.G.L. 1999. *The unmaking of the medieval Christian cosmos, 1500–1760: From solid heavens to boundless aether*. Aldershot: Ashgate.

Reiss, Timothy J. 1997. *Knowledge, discovery and imagination in early modern Europe: The rise of aesthetic rationalism*. Cambridge: Cambridge University Press.

Rosen, Edward. 1985. The dissolution of the solid celestial spheres. *Journal of the History of Ideas* 46: 13–31.

Rotman, Brian. 1993. *Signifying nothing: The semiotics of zero*. Stanford: Stanford University Press.

Savanarola, G. 1496. *Predica dell'arte del ben morire*. Florence [facs. Berlin, 1926].

Schwaetzer, H. 1997. *Si nulla esset in terra anima: Johannes Keplers Seelenlehre als Grundlage seines Wissenschaftsverständnisses*. Zurich: Georg Olms.

Shakespeare, William. 1936. (1597 first quarto and 1615 first folio) "Richard the second"; (1604 second quarto) The tragedy of Hamlet, Prince of Denmark; (1608 first quarto and 1623 first folio) "The tragedy of king Lear." All in *The complete works of Shakespeare*, ed. George Lyman Kittredge. New York: Ginn and Company.

Shakespeare, William. 1992. In *The tragedy of king Lear*, The New Cambridge Shakespeare, ed. Jay L. Halio and Philip Brockbank. Cambridge: Cambridge University Press.

Spiller, Elizabeth A. 1999. "To depart from the earth with such writing": Johannes Kepler's dream of reading knowledge. *Renaissance and Reformation* 23: 5–28.

Stephenson, Bruce. 1994. *Kepler's physical astronomy*. Princeton: Princeton University Press.

Summers, David. 1981. *Michelangelo and the language of art*. Princeton: Princeton University Press.

Summers, David. 2007. *Vision, reflection, and desire in western painting*. Chapel Hill: The University of North Carolina Press.

Swinford, Dean. 2006. *Through the Daemon's gate: Kepler's Somnium, medieval dream narratives, and the polysemy of allegorical motives*. New York/London: Routledge.

Wahrman, Dror. 2006. *The making of the modern self: Identity and culture in eighteenth century England*. New Haven/London: Yale University Press.

Walker, D.P. 1978. Kepler's celestial music. In *Studies in musical science in the late Renaissance*, ed. J.B. Tripp, 34–62. London: The Warburg Institute.

Westman, Robert S. 2001. Kepler's early physical-astrological problematic. *Journal for the History of Astronomy* 32: 227–236.

Žižek, Slavoj. 1991. *Looking awry: An introduction to Jacques Lacan through popular culture*. Cambridge: MIT Press.

Chapter 6
Agostino Scilla: A Baroque Painter in Pursuit of Science

Paula Findlen

The judicious Scilla who surpassed the condition of the painter by philosophizing … (Vallisneri 1721, 58)

Abstract Agostino Scilla (1629–1700), a disciple of Antonio Barbalunga in Messina and Andrea Sacchi in Rome, was a prolific painter, well-known numismatist, and the author of one of the most important publications on fossils in the seventeenth century. Scilla's *Vain Speculation Undeceived by Sense* (1670) helped to usher in a new understanding of fossils in the seventeenth century. Cited approvingly by Leibniz and to some degree plagiarized by John Woodward (who eventually acquired Scilla's fossils and drawings), *Vain Speculation* added further weight to the argument that fossils were remnants or imprints of living beings. This article examines Scilla's work as an important example of a painter's understanding of nature in Baroque Italy. It explores Scilla's claim to be a better interpreter of nature than any scholar in light of his relationship to the scientific communities in Messina and Rome, and his understanding of art and science in the works of Leonardo, Galileo, the *Accademia dei Lincei* and *Accademia del Cimento*.

Introduction

In 1670 a curious treatise appeared in the city of Naples entitled *Vain Speculation Undeceived by Sense*. Its author Agostino Scilla (1629–1700) was a well-known and respected painter from Messina who had personally drawn all the illustrations for his book on fossils (Scilla 1670; Accordi 1977; Morello 1979; Di Bella 2001; and

P. Findlen (✉)
Department of History, Stanford University, Stanford, CA 94305-2024, USA
e-mail: pfindlen@stanford.edu

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_6,

Fig. 6.1 “Vain Speculation Undeceived by Sense” (*Source*: Agostino Scilla, *La vana speculazione disingannata dal senso* (Naples 1670). Courtesy of Special Collections, Stanford University Libraries)

Carpita 2006). *Vain Speculation* opened with an elegant frontispiece depicting the allegorical struggle between truth and error (Fig. 6.1). Sense, represented by the god of painting Mercury bearing the eye of reason in his chest and certainly an allegorical portrait of a young artist, holds up a fossilized echinoid for inspection. He gestures expansively to the ground below to demonstrate the ubiquity of such specimens and implicitly the transparency of his understanding of their nature. By contrast, Vain Speculation is an unpredictable shape-shifter. Her hair, curling repeatedly backward into the shape of an elongated cumulus, brings to mind the remarks of Emanuele Tesauro who, in his *Aristotelian Telescope* (1654), described clouds as the ultimate play of nature as they formed and reformed themselves (Tesauro 1654). Vain Speculation is an ethereal and unfinished figure not grounded in nature's reality, let alone experience. Lost in her Mannerist delusions, she cannot understand fossils.

Dedicated to the prominent Sicilian nobleman Don Carlo di Gregorio, founder of Messina's leading literary and scientific academy, the Accademia della Fucina (1639–1678), *Vain Speculation* refuted the theories of an unnamed naturalist, in reality, the Maltese physician Giovanni Francesco Buonamico (1639–1680) (Buonamico 1668; Mangion 1971; Dollo 1979; Morello 1989).[1] It argued a fairly novel thesis: that fossils were not creations made *of* stone but creations made *in* stone. Using fossil specimens from Malta, Sicily, and Calabria, Scilla provided copious evidence that the visual similarities between fossils and the living creatures they resembled were not superficial or accidental but revealed traces of the natural processes which transformed animals and plants into petrifactions. Scilla's treatise, along with the earlier work of the Neapolitan lawyer-naturalist Fabio Colonna (1567–1640), the contemporaneous investigations of the Danish anatomist Nicolaus Steno (1638–1686) and the Royal Society's curator Robert Hooke, and subsequent research by Martin Lister, Edward Lhywd, John Ray, and John Woodward, became the cornerstone of a new understanding of fossils as an important record of the earth's history (Rudwick 1976a; Rossi 1984; and Rappaport 1997).

There are many noteworthy aspects of Scilla's interesting account of the fossil record of southern Italy and Malta. *Vain Speculation* is a marvelous example of the growing desire to combine naked-eye observation of things with an anatomical, instrumental, and philosophical understanding of the natural world in post-Galilean Italy. Its attentiveness to the relationship between human history and natural history also testifies to the impact of antiquarian studies upon science, further reminding us how two centuries of excavating, collecting, and debating the origins and meaning of ancient artifacts provided scholars with the skills to approach nature as a similarly historical record (Rossi 1984, 19–24, 35–36; and Lombardo Unpublished paper). *Vain Speculation* is also a fine instance of the development of vernacular prose as a legitimate medium in which to write science. Scilla indicated his consciousness of the Tuscan style of writing science, first developed by Galileo and perfected in the mid-1660s by the members of the Accademia del Cimento (1657–1667) in Florence and the Medici court physician Francesco Redi, when he indicated his admiration of works "written in a very Florentine manner" (Scilla 1996, 27; Nigido-Dionisi 1903, 160; Findlen 1993; Tribby 1991; Boschiero 2007). Finally *Vain Speculation* must be compared to works such as Galileo Galilei's *Sidereal Messenger* (1610) and Robert Hooke's *Micrographia* (1665), as an outstanding and early example of a text that made scientific illustrations an active part of the argument for interpreting nature.

Scilla's importance lies not only in how he interpreted fossils but also in how he depicted them. From the earliest pages of his book, he drew his reader's attention to the "many tables that I was obliged to draw for the purpose of explaining my idea with clarity" (Scilla 1996, 33; Rosand 2002, esp. 98). To paraphrase Martin Rudwick, Scilla played a crucial role in creating a "visual language" for natural history (Rudwick 1976b). By illustrating his text with 29 gorgeous copperplate engravings created from Scilla's own drawings by the Perugian artist Pietro Santi Bartoli

(1635–1700) – based in Rome and closely associated with the learned Roman antiquarian and historian of art Giovan Pietro Bellori (1613–1696) (Bell and Willette 2002, esp. 12–13, 127, 133, 143, 176–180; Carpita 2006)[2] – Scilla sought to persuade readers to *see* the fossil record differently. Richly detailed, carefully labeled, and at times offering multiple viewpoints of a single specimen, his images of fossils are among the most striking visual artifacts of seventeenth-century science. They are a fundamental window into the historical entanglements of art and science.

In order to understand the degree of Scilla's innovation, we need to consider his images in relationship to earlier traditions of depicting nature in print. While naturalists exhibited a self-consciousness about their use of images since the emergence of the illustrated natural history in print in the 1530s, arguing for the importance of depicting nature "from life" (*ad vivum*) to achieve the kind of verisimilitude that made an image a truly meaningful record of their observations, the woodcuts adorning most Renaissance natural histories did not achieve this effect without being hand colored. Even Colonna's experiments with copperplate etchings to illustrate his descriptions of plants and fossils between the 1590s and 1610s still failed to produce images that fully translated drawings into prints that would be more powerful than words (Swan 1995; Kusukawa 1997; Tognoni 2005; and Ogilvie 2006).

The failure of images, as David Freedberg has so eloquently written in his recent account of the natural historical projects of the Accademia dei Lincei (1603–1930), was one of the great paradoxes of natural history at the height of the Scientific Revolution (Freedberg 2002; Findlen 2004). Scilla's *Vain Speculation* responded to this problem by demonstrating how an illustrated natural history could indeed be persuasive at an entirely new level. Building on the work of the Lincean Colonna, who argued strongly for the cognitive function of images but had primarily created his own visual archive from the dried plants and fossil specimens in the apothecary Ferrante Imperato's famous museum in Naples rather than from living nature, Scilla experimented with new and more dynamic ways to unify words and images of fossils (Palmer 2008, esp. 246, 249). Emphasizing his professional identity as a painter, he argued that the painter's eye gave him the kind of probing insight into nature – an ability to see and interpret things better – and the essential skills to transform experience into science through his ability to describe and depict nature without mediation.

Scilla's approach to the earth's history and its visualization could not have been more different than the most important geological treatise of his generation: Steno's vastly ambitious anatomy of the earth entitled *On Solids within Solids* (1669). While the puzzling origins of Maltese tongue-stones (*glossopetrae*) – the fossilized shark's teeth also examined by Colonna in 1616 who declared them organic in origin (Colonna 1616; Morello 1979) – inspired both naturalists to reconsider the earth's history, Scilla kept his focus on the details of fossils themselves. Seeing specific manifestations of nature's operations was his goal. By contrast, Steno sought to explain the entire system of nature, *stratum super stratum*. Based on data he collected in Tuscany, he conjured up the earth's mechanical and physical transformations. Scilla evinced some interest in the layering of the earth but his primary concern was the appearance and placement of fossils; he did not attempt to explain the process by which each stratum formed. They produced complementary projects but they did not see nature in the same way.

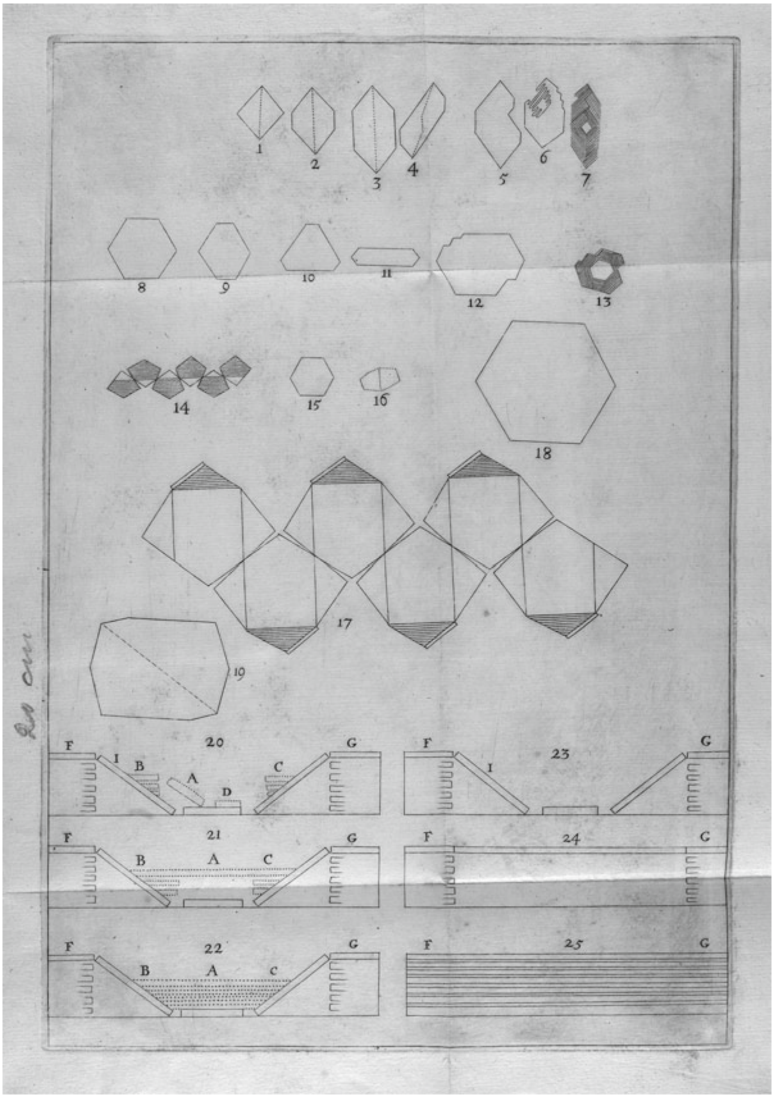


Fig. 6.2 Steno's diagram of geological stratification (*Source*: Nicolaus Steno (Niels Stensen), *De solido intro solidum naturaliter contento dissertationis prodromus* (Florence 1669). Courtesy of Special Collections, Stanford University Libraries)

Steno's geometric diagram, surely inspired by his reading of Descartes's *Principles of Philosophy* (1644), presented his argument about the organization and movement of the earth's strata in the simplest and most abstract terms (Snorrason 1986; Cutler 2003; Yamada 2006). (Fig. 6.2). His image of the earth made no attempt to depict an actual instance of the earth's stratification or anything trapped within these layers as the illustrators of Francesco Stelluti's 1637 account of the fossil woods of Umbria did by transforming Lincean field drawings into engravings and creating a map of the locations near Aquasparta where these curious specimens were found (Stelluti 1637; Scott 2001). To the extent that Steno addressed the question of fossils, he did so in his 1667 treatise on the dissection of a shark's head where he compared its teeth with the so-called tongue-stones. But this singular specimen was never really the focal point of Steno's investigation. His goal was to illustrate general principles of stratification and sedimentation in order to explain the dynamic environment in which fossils formed.

Instead, Scilla invited readers of his well-illustrated treatise to look closely at the effect of the earth's transformations on the remnants of once living things. He sought agreement with his conclusion that "the objects of our disquisition were true animals and not jokes of nature generated simply by stony substance" (Scilla 1996, 48). He asked readers to see nature through his eyes, denying the alternative theory of fossils as *lusus naturae*, whimsical simulacra of animals and plants formed spontaneously in rock either by God or that ancient divinity Nature (Findlen 1990; Bredekamp 1995, esp. 63–80; Daston and Park 1998). Scilla insisted that fossils were neither miraculous nor paradoxes but the natural outcome of how nature changed things over time. Through careful examination of the condition and location of each ruined specimen, Scilla came to a different understanding of how and why it had been formed. His beautifully rendered illustrations captured the specificity of some of the most interesting fossils in his possession which, as he elegantly put it, were "jokes of time not of nature" (Scilla 1996, 71; Ashworth 1990). In 1616 Colonna provided one image of tongue-stones in contrast to his numerous etchings of plants (Colonna 1616, 33). Scilla instead provided multiple viewpoints of this particular fossil and embedded his observations and drawings of tongue-stones in a careful study of other fossils commonly found in the Mediterranean.

Guiding his readers through the examination of nature's antiquities, Scilla repeatedly underscored the uniqueness of his vantage point. As he reminded his patron Don Carlo di Gregorio in the preface to *Vain Speculation*: "remember that this is a composition not by a scholar but by a painter who nonetheless pretends to have an eye for judging things that we can manage with greater truth that those who are mere professors of blind speculations" (Scilla 1996, 34). Years later, Leibniz would fondly recall his encounter with Scilla in Rome in 1689–1690 as an important touchstone for his own work on fossils: his *Protogaea*, composed in 1691–1693 but unpublished until 1749. Discussing similarities between Maltese and Lüneburg tongue-stones, Leibniz invoked the authority of the naturalist who in his mind had established definitively that tongue-stones were fossilized shark's teeth: *Scylla pictor* (Leibniz 1749, 48). "Scilla the painter" had given him the insight he needed and this episode offers us a richly detailed example of the role of artists in making knowledge.

The Making of a Learned Painter

How did a Sicilian painter become one of the greatest authorities on fossils during the century in which the idea of the earth's history became a subject of growing interest? In order to address this question, we need to consider how Scilla understood and sought to rethink the relationship between art and science in the age of the baroque. While not a theorist of art like his contemporary Bellori – who left us many important writings on the status of the arts in baroque Rome and had a copy of Scilla's *Vain Speculation* in his library (Perini 2000, 679) – Scilla's own understanding of painting as a kind of ocular judgment reveals the core ingredients of his argument about the role of artistic skill, especially the practice of drawing and the handling of color, in the creation of scientific knowledge. Both agreed that art was no passive imitation of nature but a creative and disciplined act of interpretation.

This understanding of the artist's role gave Scilla the authority to declare the impossibility of many things other naturalists claimed to observe. Commenting on the Paracelsian doctrine of signatures invoked by Buonamico, which explained the resemblances between things as a deliberate product of nature's artistry and design, Scilla remarked on the improbability of seeing human features or the shape of entire animals in a plant. He argued that the mandrake, the Scythian lamb, and a variety of other marvelous beings did not exist in actuality but were only marginally visible to the human eye as an act of excessive imagination. Scilla invited readers of *Vain Speculation* to envision the process in reverse through his eyes. What kind of man could one make from an anthropomorphic plant? "I am a painter," he wrote, "and I swear as a poor man that a most horrendous figure would be composed if its members were formed corresponding to this or that plant." He strongly criticized the sixteenth-century physician and philosopher Girolamo Cardano for claiming in *On the Subtlety of Things* (1550) to possess an agate with the face of the Roman emperor Galba sculpted by nature. However vaguely it resembled a human face, Scilla felt that there was no basis for concluding that it belonged to Galba. He had spent many hours examining the faces of emperors on ancient coins and spoke authoritatively on this issue. Cardano's anthropomorphic rock most certainly was not "made by Nature's brushstrokes" (Scilla 1996, 50, 56; Bianchi 1987).

Such comments presented the interpretation of fossils as a matter of aesthetic discrimination as well as disciplined observation. In contrast to the Renaissance naturalists who casually enumerated numerous instances of nature making anything and everything in stone, constantly producing *lusus naturae* and other instances of nature's mimesis to delight and confound the human intellect, Scilla saw no reason to create fantastic petrified figures. He divided fossils into two basic categories: objects which accidentally resembled other things in the most approximate sense and were therefore not "perfect drawings of the things they represent," and objects whose level of anatomical detail and specificity indicated the presence of once living beings (Scilla 1996, 55) (Fig. 6.3). He considered the alternative theory a misunderstanding of the evidence of the senses and a misuse of the metaphor of painting.

Shortly before Scilla completed his study of fossils, Bellori proclaimed the superiority of the artist to nature in *The Idea of the Painter, the Sculptor, and the Architect*, a lecture given before the Accademia di San Luca in Rome in May 1664, writing that the painter's imagination was the source of his wisdom. He would later praise Carlo Maratta, who studied painting alongside Scilla under the tutelage of Andrea Sacchi (1599–1661), for understanding that the painter needs to know enough but not too much science. Bellori argued that an aspiring artist should not "go so deeply into these aspects that he neglects others, which are very difficult and most

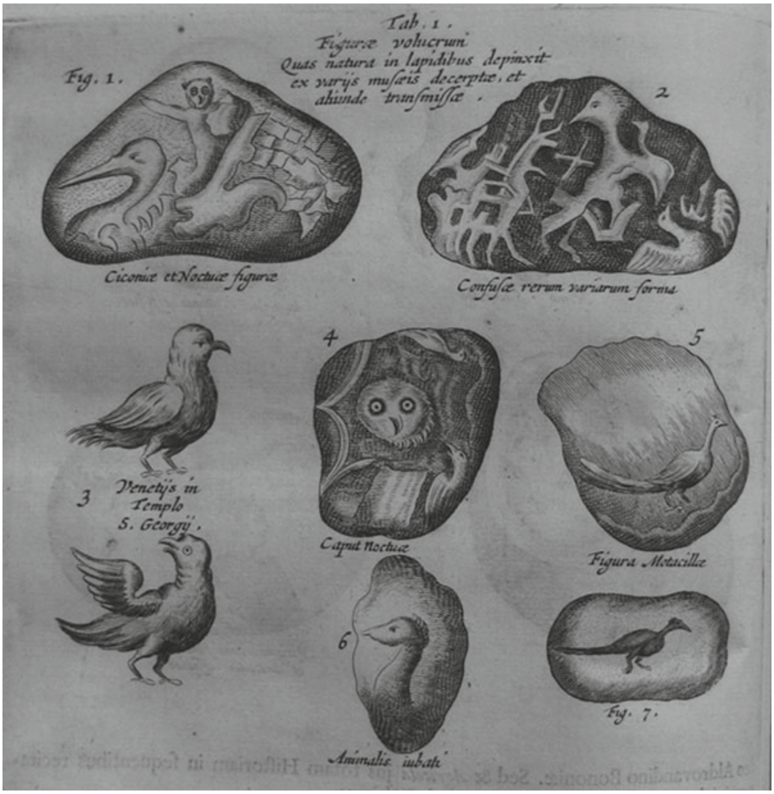


Fig. 6.3 Nature's sculptures according to Athanasius Kircher (*Source*: Athanasius Kircher, *Mundus subterraneus*, 2nd ed. (Amsterdam 1671). Courtesy of Special Collections, Stanford University Libraries)

important, in such a way that he pursues the squaring of the circle, or the nude too far below the skin" (Bellori 2005, 422). Such comments stood in direct contradiction to Leonardo da Vinci's earlier formulation of painting as a "true science" capable not simply of capturing surface perceptions but of probing the very nature of reality itself (Farago 1992, 179). Fundamentally, Bellori invited his contemporaries to consider the predicament of the learned painter. Painters benefited from their knowledge of other disciplines but he argued that they should not run the risk of immersing themselves to the point where they lost sight of their primary goal of producing art.

Understanding Scilla's project in light of the ongoing debate about the relationship between art and science helps us to see how *Vain Speculation* demonstrates Scilla's participation in the revival of Leonardo's vision of painting as a cognitive science. More immediately, it reflected the role of *disegno* in the development of the Galilean traditions of science then flourishing in the Italian peninsula, not only in Galileo's chosen areas of special interest such as astronomy and mechanics but also in the realm of medicine and natural history. Galileo had been a member of Florence's

Accademia del Disegno and was an accomplished enough draftsman to execute his own lunar sketches (Winkler and Van Helden 1992; Reeves 1997, esp. 6–11; Bredekamp 2007). He frequently analogized the relationship between the good painter and the good scientist, both of whom drew from nature. He had access to early manuscript copies of Leonardo's *Treatise on Painting*, unpublished until 1651 but in circulation the Italian cities where Galileo spent the majority of his career (Reeves 1997, 29–31, 114–116; Steinitz 1958; Pedretti 1964). Directly and indirectly, discussions of Leonardo's ideas – and more generally the science of art – became a topic of conversation in the seventeenth century, and not only for artists but for some of the most influential scientific figures of this era. The Roman project to publish Leonardo's *Treatise* was partly indebted to Galileo's interest in what his friend Federico Cesi (1585–1630), founder of the Accademia de' Lincei (1603–1630) which counted Galileo as its most famous member, called "philosophical painting" (*pittura filosofica*) (Solinas 2009).

The image of Galileo as a painter *manqué* emerged most forcefully in Vincenzo Viviani's biography of his mentor, first composed in 1654 though unpublished until 1717. Viviani wrote that Galileo "often said to friends that if at that age it had been in his power to choose his profession, he would surely have chosen painting." Viviani further emphasized Galileo's understanding of *disegno* by describing him as man whose "judgment of painting and drawing" earned the praise of leading artists such as Ludovico Cigoli and Bronzino (Viviani 1992, 82; Reeves 1997, 117). In seventeenth-century Italy the model scientific inquirer incorporated the perspective of the artist into the work of science. Scilla reflects a genealogy of the artist inaugurated by Leonardo which became incorporated into the image of the scientific inquirer embodied by Galileo. The generation of artists who sought to continue the Lincean project of *pittura filosofica* drew inspiration from his example as well as the published and unpublished projects of natural history that represented the other legacy of this Roman scientific academy, including its extensive investigation of fossils. Maurizio Marini has interpreted Scilla's painting of a hunting scene of hares and avocets observed by a watchful owl (in a private collection in Pavia) as an example of the Lincean influences on his work. Arguing that the owl's nocturnal vision – like the eye of lynx used by the Linceans used as their emblem – reflected Scilla's understanding of himself as an insightful observer of nature, Marini presents this painting as evidence that Scilla explicitly cultivated a reputation as a philosophical painter (Marini 1990, 49).

Not every seventeenth-century artist shared Scilla's passion for the scientific study of nature. In 1682 Maratta expressed the alternative view which Bellori had praised in an allegorical drawing of the phrase *tanto che basti* (just enough) (Fig. 6.4). Scilla instead presented himself as a painter with knowledge capable of generating new ideas about nature. He used his artistic training to produce a considerable corpus of paintings which, while not the subject of this essay, are certainly deserving of more detailed study than they have so far received. Yet the circumstances of his apprenticeship and artistic practice between Messina and Rome in the middle decades of the seventeenth century encouraged Scilla to believe that he had something original and important to contribute to natural history that went beyond the

Fig. 6.4 Nicolas Dorigny's engraving from Carlo Maratta's allegory of the painter's knowledge, *Tanto che basti* (Just enough) of 1682 (*Source*: Nicolas Dorigny, *L'Accademia di Pittura* (Rome 1704-1710). Department of Prints and Drawings, British Museum AN798108001 © Trustees of the British Museum)

question of creating a good illustration and delved more fundamentally into the process by which drawing itself was a form of knowledge.

Maratta's allegory of the artistic academy also included a companion sketch of a different phrase, *mai a bastanza* (never enough), to underscore the importance of antiquity in the training of an artist (Bellori 2005, 423). Scilla wholeheartedly agreed with this assessment of the significance of antiquarian studies; he was an accomplished numismatist who knew his ancient sources well. The physician Buonamico, whose August 1668 letter to Scilla on Maltese tongue-stones inspired the writing of *Vain Speculation*, considered Scilla to be one of the best antiquarians of their generation, and he was not alone in this assessment. In a self-reflective moment Scilla wrote that while painting was his profession, "my private intellect is devoted entirely to ancient medals" (Buonamico 1668, 108; Scilla 1996, 40). He pursued these interests before and after the publication of *Vain Speculation*. Scilla's unpublished *One Hundred Sicilian Cities Described with Medals*, recently rediscovered by Luigi Hyerace who is preparing a critical edition, offers important evidence of his commitment to writing the history of Sicily from the evidence of ancient Roman coins. He may well have been in the middle of this project when he completed *Vain Speculation*, since he presented his study of fossils as "an interlude in the pleasing exertion of medals" (Scilla 1996, 106; Hyerace 2001).[3] Scilla reconsidered natural history from the evidence offered by Sicilian nature just as he rewrote Sicilian history from its antiquities.

The question of physical evidence was of paramount importance in both domains of knowledge. Antiquarians honed their skills in order to discern authentic from forged antiquities, and to place objects in a precise moment in time as a prelude to writing a better version of ancient history. Scilla drew upon his numismatic skills to explain the variations he observed in fossils. "What's more, anyone who has experience of ancient medals also knows how difficult it is to find even two medals, of the same emperor, on the same side and from the same time, that are cast from the same mint" (Scilla 1996, 59; Momigliano 1950; Schnapp 1997). Human history confirmed the rule of natural history: nature did not make exact copies, making the differences between living animals and fossil remains far less puzzling than they might otherwise seem. Scilla proudly compared his inspection of fossils in the mountainous regions of southern Italy and Malta to his knowledge of the most famous artificial mound of human and natural remains: Monte Testaccio in Rome (Scilla 1996, 51, 58). His observations of this ancient Roman dump site stimulated his understanding of fossils as nature's amphorae, piling up over time and mixing with earth to create a seemingly infinite mountain of ruined things.

While advertising his credentials as an accomplished antiquarian, Scilla strongly articulated the more fundamental significance of his professional identity to the development of his work as a naturalist.[4] Science and antiquarianism were complementary expressions of learning whose simultaneous pursuit allowed him to transcend the limits of his assigned role. In this respect, Scilla sought to expand the meaning of what admirers of the greatest theorist and practitioner of baroque art, Gianlorenzo Bernini (1598–1680), called *un bel composto*, a beautiful whole (Lavin 1980, vol. 1, 6). Bernini's sculptures may not have inspired directly any of Scilla's paintings, but his goal of unifying the arts potentially stimulated Scilla's ambitions in other ways. As William Ashworth has suggested, Scilla's allegory of the revelation of knowledge from the history of the earth in the frontispiece of *Vain Speculation* may have alluded to Bernini's most reflective and personally significant sculpture, *Truth Unveiled by Time* (1645–1652), though there is also a tradition of painting this theme by Roman artists such as Pietro da Cortona which needs to be considered as well as the conventions of frontispieces in baroque Rome, including those of Athanasius Kircher.[5]

Scilla's treatise on fossils was his only scientific publication in the midst of a long and highly productive artistic career. It secured his reputation as a learned painter and complemented his status as one of Sicily's most knowledgeable antiquarians. Rather than seeing it as a diversion from these other activities, we should consider it a sublime demonstration of what it meant to move even further beyond the *paragone* of the arts which so preoccupied painters, sculptors, and architects since the Renaissance (Azzolini 2005). As natural history, antiquarianism, and experimental philosophy became more important and self-consciously methodological endeavors in the seventeenth century, the question of visual representation became a crucial subject of discussion. What could art contribute to science? To what extent were artists passive instruments of scientific vision or active participants in the making of knowledge? In many respects, there could be no better example of baroque science than this curious treatise by Agostino Scilla.

From Messina to Rome

Scilla's career indicates the many ways in which art and science intermingled in the seventeenth century to foster new approaches to knowledge. The founder of a school of painting in mid-seventeenth century Messina which included his brother Giacinto and his son Saverio, he was a prolific painter whose work can still be found today throughout Sicily and beyond. Paintings such as his *Saint Benedict Ordering the Destruction of the Idols*, which dates from the 1660s, suggest his fascination with the confrontation between truth and error which would reappear as the central theme of *Vain Speculation*.[6] The loving attention which he gave to fragments of broken antiquity in the foreground of this painting echoes the placement of fossils in the frontispiece of his book as well as alluding to his passion for antiquities. Celebrated in his own lifetime and prominently featured in histories of Sicilian painters in the eighteenth and early nineteenth centuries, Scilla has only recently begun to receive the attention he deserves from art historians as a painter who combined the classicizing impulses of the mid-seventeenth century Roman artistic community with the strong emphasis on naturalism apparent in Sicilian painting during this period (e.g. Martinelli 1978; Marini 1990; Hyerace 1999). The diverse artistic traditions he encountered between Messina and Rome, and the opportunities he found in both cities to observe and ultimately participate in some of the most exciting scientific projects of seventeenth-century natural history shaped his aesthetic sensibilities and sharpened his understanding of the role of scientific illustration. Scilla's project to interpret fossils emerged at the intersection of these experiences.

The son of a notary, Scilla began his artistic training under the tutelage of the painter Antonino Barbalunga in Messina during the 1640s. Barbalunga, himself a student of Domenichino, was so impressed with Scilla's work that he petitioned the Senate of Messina to provide his young pupil with a stipend to pursue his studies. In 1647 the teenage Scilla made the pilgrimage to Rome. He apprenticed for 5 years in the workshop of Sacchi, one of the most prominent classical painters in baroque Rome – best known for his famous fresco of *Divine Wisdom* (1629–1633) in Palazzo Barberini which has often been interpreted as a meditation on the relationship between the Barberini family and Galileo during the trial as well as an obvious homage to Raphael's vision of *Parnassus* (Memorie 1821, 139–147; Lechner 1976; Sutherland Harris 1977). The impact of this experience on Scilla was profound since it exposed him to some of the most important projects of art and science then underway.

Who besides Barbalunga determined Scilla's path to Rome? One likely answer is the Roman physician and naturalist Pietro Castelli (1574–1662), himself a product of Rome's international artistic community. Castelli first came to Messina to teach at the university in 1634 where he would found its botanical garden 4 years later. Son of the Flemish painter and miniaturist Frans van de Casteele (1541–1621), who renamed himself Francesco da Castello after migrating from Brussels to Rome where he enjoyed a successful artistic career culminating in his election as head of the Accademia di San Luca, Castelli applied the lessons he learned in the family *bottega* to the art of scientific illustration, making his own drawings of plants in the Farnese garden and of curious animals and insects wherever he found them (Carpita 2006, 349–350; Giuliani 2009). Castelli proudly advertised this fact – "I usually depict plants and animals with my own hand" – and pointedly critiqued the deficiencies of the "badly depicted image" (*icon male picta*) which contributed to much confusion and misidentification in natural history (Castelli 1652, 19, 16).[7] There is no question that he cared a great deal about the quality of images.

Disillusioned with Barberini patronage which never quite got him the kind of position he wanted in Rome, Castelli was nonetheless well connected to the circles in which Scilla traveled. He brought the practices of Roman science with him to Messina, making Sicily a destination for a number of travelers who were not only curious to see the island's volcanoes, fossils, fish, and plants but also interested in finding out what Castelli was doing in Messina.[8] At times Castelli asked himself the same question, wondering whether the prospect of a better salary and position was really worth self-imposed exile from Rome. As an artistically inclined naturalist, he may have indeed understood the teenage Scilla's potential and encouraged him to cultivate his talent in the Eternal City.

With the flight of the Barberini after the death of Urban VIII in 1644, Rome was only just in the process of reconstructing its reputation as a center of artistic patronage but there was much to be learned from its artistic legacy since the Renaissance (Haskell 1980). The presence of Nicolas Poussin (1594–1665), the greatest French painter then resident in Rome, made it an exciting location in which to theorize and practice the arts. Bellori's famous anecdote about Poussin inviting a foreigner passionate to bring home Rome's antiquities to take with him "a bit of earth and some pebbles amid the grass, with specks of porphyry and marble reduced almost to dust," which Poussin personally offered by the fistful, suggests an interesting theoretical foundation for Scilla's development as a painter with antiquarian interests who subsequently became a naturalist. Declaring Roman earth to be "the most beautiful antiquity that you could desire," Poussin not only poked fun at the collector's passion for the beautiful object but also drew attention to the earth as a repository of all things over time (Bellori 2005, 325).[9] In *Vain Speculation* Scilla described a mill on the western edge of the port of Messina whose churning of the earth disgorged colored pebbles and shells, allowing him to observe how things at the bottom of the sea might eventually rise to the surface (Scilla 1996, 41).

Scilla did what all young artists who came to Rome were supposed to do. He copied antiquities and the works of Renaissance artists such as Raphael, and gradually began to develop his own contacts with leading Roman virtuosi, artists, and antiquarians. He witnessed the return to Rome in September 1648 of the most important remaining member of the Barberini family, Cardinal Francesco Barberini (1597–1679), and his subsequent efforts to re-establish himself as one of the city's leading patrons. By then if not before, Scilla was aware of the role of Barberini's secretary Cassiano dal Pozzo (1588–1657) in developing important collaborations between artists, naturalists, and patrons in the service of the Lincean project of depicting nature. An intimate of Poussin and a correspondent and sometime patron of Castelli, Cassiano was a passionate collector and connoisseur. Cassiano and his Barberini patron had been members of the Accademia de' Lincei in its final years. Through his acquisition of the Lincean academy's books, papers, and drawings in 1633, Cassiano proclaimed himself the intellectual heir of the academy's founder Cesi. During Scilla's apprenticeship in Rome Cassiano was in the final stages of creating his *Museo cartaceo*, a vast paper museum of nature and antiquities that today stands as one of the greatest visual archives of the seventeenth century (Solinas 1989; Haskell et al. 1989; Jenkins 1992; Beneš et al. 1993; Freedberg 2002). The home he shared on Via dei Chiavari with his brother Carl'Antonio (1606–1689) was filled with books, paintings, and curiosities, and became a well-known meeting ground for artists and scientists in mid-seventeenth century Rome. It was an environment which stimulated many artists to study nature, and many naturalists to consider the role of art in science.

Was Scilla able to see these materials as a result of his association with Castelli and Sacchi? Cassiano's paper museum contained an enormous quantity of material documenting the fossil woods around Acquasparta, a small portion of which found its way into Stelluti's *Treatise on the Fossilized Mineral Wood* (1637). The majority existed in the form of unpublished notes and drawings by Cesi, Stelluti, and other members of the defunct Lincean academy in Cassiano's possession (Scott and Freedberg 2000). There was much to see and discuss in Rome which might have inspired a young painter to consider the nature of fossils though Scilla later insisted that he only read the *Dissertation on Tongue-stones* (1616) by the "most accurate Colonna" after forming his own opinion of fossils (Scilla 1996, 70). His noteworthy silence on Stelluti's *Treatise* raises the possibility that he had seen it, perhaps found it inspiring in the initial stages of his research, but ultimately disagreed with Stelluti's conclusions which fairly closely resembled those of Buonamico in arguing for nature's spontaneous creation of fossil woods. Scilla would not have wished to criticize a Lincean by name. In a poem which prefaced *Vain Speculation* the physician Giovanni di Natale praised Scilla for having a "Lincean eye" (*lincea pupilla*) (Scilla 1996, 31). There can be little doubt that Scilla's contemporaries envisioned him as an heir to this Roman tradition of art and science.

Scilla did not remain in Rome, at least not on the occasion of his first voyage to the Eternal City. He returned to Messina in 1651 – right around the time that Castelli was reminding other naturalists of the importance of drawing nature for one's self in his latest publication – and used the knowledge and skills he had acquired during his apprenticeship to develop a successful artistic career. Scilla's close association with the wealthy and learned Don Antonio Ruffo (1610–1678), arguably the greatest patron of the arts in this city, opened many doors for him. Upon taking possession of his mother's palace near the harbor in 1646, Senator Ruffo inherited an art gallery which he augmented with strategic acquisitions all over Europe. His collection of paintings, sculptures, drawings, coins, and other fine objects rivaled the princely and noble galleries that made such cities as Rome, Naples, Florence, and Venice destinations for art lovers and antiquarians on the Grand Tour. At the time of his death he possessed 364 paintings including Rembrandt's *Aristotle Contemplating the Bust of Homer* (1653), which Ruffo commissioned for the princely sum of 500 florins shortly after Scilla returned to Messina, and many other noteworthy paintings by Flemish, Spanish, and southern as well as northern Italian painters (Ruffo 1916; Rousseau 1962, 152; Haskell 1980). Ruffo's desire to create a gallery of modern masters – personally negotiating the details of many commissions with the assistance of agents and encouraging artists to create paintings in dialogue with each other through their representation of the same subject – transformed his palace into an academy of the arts whose contents were discussed well beyond Messina.

Until his flight from Messina in March 1678 after the failure of a French-assisted revolt against the Spanish by the proudly independent scions of the Messinese nobility (including Ruffo who died in France later that year), Scilla thrived on his close association with Don Antonio. He was greatly in demand for his religious paintings, portraits, "landscapes, animals, flowers, fruits, and similar things in which he was considered singular" (Memorie 1821, 142) His commissions not only took him inside Messina's churches and palaces but led him to travel throughout Sicily – including Palermo, Syracuse, and Caltanisetta – and Calabria where he not only painted but collected antiquities and fossils. Scilla also supervised aspects of the iconographic program of Palazzo Ruffo and tended to more mundane issues of the growing gallery, restoring paintings by foreign artists that were damaged in transit. His school of painting seems to have flourished inside the palace. While further research needed to be done to establish more precisely the relationship between Ruffo's patronage and Scilla's artistic career, during the period in which Scilla wrote and illustrated his treatise on fossils he was a full-fledged member of this household. The environment of Palazzo Ruffo allowed him to paint and to train other painters in one of the most stimulating and cosmopolitan visual laboratories of the seventeenth century.

Possibly modeling himself on Cassiano dal Pozzo, Ruffo cultivated a reputation as a munificent patron of the arts and sciences. His palace became the meeting place for some of the most innovative minds in mid-seventeenth century Messina. By 1662 Ruffo's palace academy counted the Bolognese physician, anatomist, and inveterate experimenter Marcello Malpighi among its members. Malpighi arrived in Messina as a highly recruited medical professor from Pisa; in 1667, the year after he left Messina to return to his native Bologna, he became the first Italian to be admitted to the Royal Society. Throughout the 1660s Malpighi's skillful dissections and microscopic investigations of humans, animals, and plants yielded numerous discoveries (Bertoloni Meli 1997). His science was all about the *details*. His exposure to Malpighi's intensive and extensive program of anatomical research gave him an acute awareness of the limits and difficulties of anatomical knowledge. Scilla observed in *Vain Speculation* that anyone examining the relationship between fossils and living organisms should bear in mind that “it is absolutely certain that we have not seen all the parts of all the animals” (Scilla 1996, 62). Inspired by Malpighi and other seventeenth-century anatomists, Scilla saw his project as a simultaneous anatomy of the living and archeology of the dead.

From 1662 until 1666 Malpighi frequented Palazzo Ruffo and lived with his student Don Iacopo Ruffo, nephew of Don Antonio. After returning to Bologna, he maintained ties with the Ruffo family. Writing to Don Antonio, Malpighi fondly recalled “the consolation that I had in your Academy and in the summer on the balconies by the sea.” He subsequently acted as Don Antonio's agent in the acquisition of paintings by the Bolognese artist Guido Reni (Adelmann 1975, vol. 1, p. 389). Malpighi also maintained his friendship with Scilla who responded appreciatively by sending “many curiosities” to Bologna in 1670. He also followed the progress of *Vain Speculation*. “I am anxiously awaiting the things that he is publishing in order to enjoy his most honored efforts,” Malpighi wrote Don Antonio (Porzio 1989, vol. 2, 1002). His admiration for Scilla's work eventually led him to promote the project with English members of the Royal Society also investigating fossils.

As the case of Malpighi demonstrates, Scilla's position in Palazzo Ruffo gave him unique access to Messina's leading scientific practitioners. His association with Castelli, who preceded Malpighi as professor of medicine at the University of Messina, paved the way for Scilla's growing reputation as an artist appreciated by naturalists. For Castelli, Scilla was part of a select group of young men whose talent he cultivated since he otherwise found Messina an unpromising and intractable environment for a foreigner. In 1652, the year in which Scilla completed his apprenticeship with Sacchi, Castelli lamented the impoverishment of intellectual life in the city:

> What really bothers me is that I am deprived of the conversation of learned men as was my usual experience in Rome where every day I learned something. Here I do not have even anyone to teach; the students are satisfied with learning little and I cannot explain my more fantastic ideas to them because they neither understand me nor do they cultivate what little philosophy they have. In Rome at least I would hear learned people debate, I would converse with curious foreigners, and I would be understood even by those who hated me (Trabucco 1996, 128).

The return of a young, Roman-trained artist, fresh from his experience of the Eternal City which included contact with the very community which Castelli claimed as his own, must have breathed new life into the aging physician's experience of Messina. He saw the virtue of employing Scilla in an ambitious project which demanded a painter's knowledge of nature.

At some point between Scilla's return to Messina in 1652 and Malpighi's arrival in 1662, Castelli commissioned the painter to assist him in illustrating a two-volume study of insects. When John Ray and Philip Skippon visited Messina in June 1664, it was in the possession of Castelli's nephew Giovanni Pietro Corvino. The early Royal Society members were sufficiently impressed by its beautiful color illustrations and content to consider the possibility of enlisting one of the English merchants in Messina to ship the manuscript for London for publication. They thought that the insects were "painted by *Castellus*'s own Hand" – a comment Ray would later repeat to Martin Lister when invoking Castelli's study as the most interesting recent contribution to entomology (Ray 1718, 361; Skippon 1732, 613–614).[10] Sicilian scholars who studied this now vanished work knew of Scilla's involvement in the project (Dollo 1984, 149). Thus when Scilla first met Malpighi he had just completed his work on Castelli's manuscript. As a result, he is an important example of an illustrator who became an author.

Scilla's growing reputation as an artist whose skills naturalists coveted is additionally supported by Malpighi's correspondence with Giovanni Alfonso Borelli (1608–1679). Borelli first came to Messina around 1635 and became a disciple of Castelli. From 1639 until 1656 when he left for Tuscany, he taught mathematics at the University of Messina; during his trips to the Italian mainland, he recruited professors of international stature such as Malpighi. After teaching at the University of Pisa and participating in Leopoldo de' Medici's Accademia del Cimento (1657–1667), Borelli returned to Messina in 1667 to the home of his patron Don Iacopo Ruffo. Until his abrupt departure in April 1672, when he found his possessions confiscated and a price on his head because of his overtly anti-Spanish politics, he played a visible if controversial role in Messina's scientific community (Bertoloni Meli 1996; Boschiero 2007, 59–91). Scilla's book was written during this final period of Borelli's residence in Messina.

Borelli already knew Scilla from his early years in Messina, probably because of Castelli. He warmly advertised Scilla's talent to colleagues in search of a good scientific illustrator. In the summer of 1664, for example, Malpighi and Borelli were in the midst of a lengthy discussion of comparative anatomy and physiology. Taking advantage of nature's bounty brought daily by fishermen to the harbor of Messina, Malpighi had been dissecting swordfish and reporting their anatomical peculiarities to Borelli who was then at the Medici court. Patiently he was developing a better understanding of their physiology from their anatomy. There were many questions to resolve. How exactly did swordfish breathe? Did its heart have one ventricle like that of sea tortoise? How did its blood circulate? He communicated his thoughts in a letter to Borelli.

Borelli found Malpighi's observations sufficiently intriguing to request a full report of the physiology of "large fish." He underscored the necessity of including drawings (*disegni*) to accompany the description, and discussed how best to acquire them. “Although you will not always have Signor Scilla at hand, at worst Don Iacopo will be able to provide some youngster who draws. Indeed you could even do it since you know that a careful version isn’t needed, just as you and I together drew the muscles of a swan in Pisa” (Adelmann 1975, vol. 1,230). Borelli’s evaluation of Scilla as the best scientific illustrator in Messina gives further weight to the posthumous appreciation of Castelli’s unpublished entomology by the English virtuosi. It also offers a compelling portrait of Scilla dissecting and drawing the bounty of the sea with the great Malpighi, his preferred assistant in the endeavor of recording nature well rather than producing a hasty and imperfect copy. We now understand better why Scilla confidently proclaimed that there were many things about animals as yet unknown. He had been observing nature at Malpighi’s side, indeed was admired by some of the best naturalists of his generation for his knowledge of fish anatomy (Boccone 1670).

By the 1660s Scilla developed a reputation as one of the most talented and knowledgeable scientific illustrators in Italy. His reputation began to travel beyond Sicily, and in the city of Messina he was now an active participant in the renewal of science then underway. When not fulfilling his artistic commissions or improving the work of other artists whose paintings hung in Palazzo Ruffo, Scilla pursued questions of natural history with his colleagues, and read deeply and widely in the natural philosophical, experimental, and natural historical literature that had so recently begun to overturn many ancient theories of nature. He was sufficiently well informed to understand the importance of referring to Galileo without actually citing him, and to thinking with Descartes without following him down any particular path other than to profess admiration for the importance of doubt in the advancement of knowledge. Scilla also expressed open admiration for modern philosophers such as Pierre Gassendi and presumably Galileo and Borelli (both unnamed) who drew inspiration from the writings of the ancient atomists Democritus, Lucretius and Epicurus. Among all the philosophers, ancient and modern, only they were considered capable of discerning the absolute truth (Scilla 1996, 81).

Don Antonio’s palace by the sea was a veritable laboratory, a beehive of activity which would have made Francis Bacon believe that Salomon’s House had been transported to a Mediterranean island rather than the Britain of the Royal Society. It was a world filled, at least temporarily, with exiles from such cities as Rome, Bologna, and Pisa who found themselves in this entrepôt of the Mediterranean, their salaries handsomely paid by municipal taxes on Messina’s silk industry, their ideas discussed by the local academy, and their prospects burnished by their association with the Ruffo family. Their conversations about the future shape of knowledge and their understanding of role of new techniques of observation and analysis offered an intensive education in virtually every major development in science during the previous century. To some degree, the political openness of Don Antonio’s academy and its distance from Rome also seems to have permitted a number of participants to put aside some of their concerns about the consequences of speaking freely. It was unusual and compelling moment, and Scilla took full advantage of everything it offered in fulfillment of his ambitions.

What role did the Accademia della Fucina play in these activities? Don Carlo di Gregorio's academy enjoyed sponsorship from the Senate of Messina as one of the city's leading cultural institutions. Founded in the same year in which the university also came under the direct control of the Senate of Messina, the Fucina fostered a literary and scientific community that was politically aligned with Messina's long-standing claims to be Spanish by choice rather than by conquest. The academy thrived as an institution recognized and supported by the Senate. Like Borelli's earlier publications financed by the academy and the Senate, Scilla's *Vain Speculation* enjoyed their sponsorship. His book was the academy's penultimate publication (Nigido-Dionisi 1903). It may have been the culmination of a scientific program that was increasingly supplanted by political concerns.

It is little wonder that Malpighi recalled his four years in Sicily with some nostalgia, having had a far briefer and more positive experience of what it offered than did Castelli who was truly embittered by his lengthy exile from Rome. He did not forget his friend the painter. Malpighi closely followed the completion of Scilla's work, expressing his eagerness to see it in print. He was well informed about the content of this book in advance of publication, and lamented the delays in binding and distribution while Scilla finished a painting. As *Vain Speculation* neared completion, Malpighi gratefully accepted the gift of some fossils from another member of the Ruffo circle – his fellow Bolognese Carlo Fracassati then teaching in Messina – in order to compare actual specimens which Scilla's drawings and analysis (Adelmann 1975, vol. 2, 443, 470–471, 501. Years later Malpighi and Scilla would reconnect in Rome when Malpighi accepted the position of papal physician in 1691. Yet even prior to this appointment Malpighi encouraged Robert Boyle and Martin Lister to read the published letter by "my friend Signor Agostino Scilla, Messinese Painter" (Adelmann 1975), vol. 3, 1136–37, 1157). Theirs was no casual relationship but a lasting friendship based on mutual respect and appreciation.

Malpighi also admired Scilla's skill as an antiquarian, including his ability to execute new and risky techniques of copying antiquities. In February 1671 Malpighi was in the midst of a discussion with Silvestro Bonfigliuoli about copying medals by making "sulfur impressions." Highlighting the risks inherent in the process of burning copies from originals, which even an accomplished Bolognese silversmith refused to do because of the potential damage it might cause, Malpighi recalled Scilla's success with this difficult technique. "I know Signor Scilla did it in Sicily to send off a copy of some of his medals" (Adelmann 1975, vol. 2, 524) Malpighi's confidence in Scilla's ability to do difficult and innovative things which confounded other artisans acknowledged Scilla's skill as a metallurgist and potentially raises the possibility that part of Scilla's confidence in his technique came from his exposure to iatrochemistry, a subject of special interest to Castelli who introduced Borelli to this science. Scilla's ability to understand the composition of things lay at the heart of his approach to natural history (Smith 2004). Malpighi appreciated Scilla's work as an experimenter because he had witnessed one of Scilla's principal experiments with ancient coins. He was an artist from whom naturalists could learn.

Scilla's *Vain Speculation* was the product of his keen observation of the remarkable physical environment of southern Italy in which the earth's history lay literally upon the surface. It was equally the result of unusual intellectual and professional opportunities. In the 1660s Messina was a thriving city for the arts and sciences with strong connections to multiple communities beyond Sicily: Rome, Florence, Pisa, Bologna, Spain, and eventually France during the brief and unsuccessful efforts of Louis XIV to support the uprising against Spain. Rather than seeing it as a world apart, we might consider its centrality to this particular moment. Scilla was in exactly the right location to become the leading expert on certain kinds of fossils, and to bring fossils into a broader conversation about the changing face of knowledge in post-Galilean Italy. In order to dig further into this story, however, we need to consider the genesis of Scilla's curiosity about the fossils of southern Italy. How did the emerging debate about the interpretation of fossils inspire Scilla to publish *Vain Speculation*? What did he hope to accomplish with his book and its images?

The Genesis of a Scientific Conversation

In the summer of 1668 understanding the nature of fossils preoccupied many naturalists and their patrons. It engendered a lively conversation that developed through the exchange of letters and specimens, especially between Sicily, Malta, and Tuscany, and was facilitated by the travels of naturalists who moved among these three worlds. By this point in time if not before, Scilla had begun to collect fossils, including the much prized Maltese tongue-stones. He had a box of them in his possession even before his exchange with Buonamico (Scilla 1996, 52–53). Fossil-hunting was hardly a surprising activity for a learned painter who already enjoyed a reputation as a collector of ancient coins, conversed regularly with physicians and naturalists, and immersed himself in the paintings, books, and curiosities of Palazzo Ruffo. It may also have been a logical outgrowth of his interest in still life painting in which shells were often depicted for their beauty, intricacy, and variety (Pomian 1990, 121–138). The decision to read and write about fossils, as well as collect and draw them, made Scilla a full-fledged participant in this important intellectual debate. Whatever the exact nature of his contributions to Castelli's lost entomology or Malpighi's comparative anatomy of fish, Scilla was in command of all the resources he needed to make himself an expert on fossils.

Scilla's friendship with one of the leading Sicilian naturalists of the time, Paolo Boccone (1633–1704) played an important role in the emergence of his voice in the fossil debate. Botanist to the Grand Duke of Tuscany, Boccone came from Palermo and had studied with Castelli. He regularly traveled between Tuscany and Sicily, becoming an important conduit of information between the two communities. In all likelihood, he inspired Scilla's admiration of Redi's experimental natural history by bringing copies of the Tuscan naturalist's recent books to Messina. Boccone presented his research on Sicilian flora and fauna, and more generally the natural history of southern Italy and the Mediterranean, as a valuable corrective to the often uninformed comments of northern European naturalists on Sicilian flora and fauna (Accordi 1975 ; Dollo 1979, 140; Dollo 1984, 205; Trinci 1988, 130). By the late 1660s he expanded the scope of his investigations to include the island of Malta.

In his publications Boccone openly professed his admiration for Scilla, whom he respectfully called the "painter and famous antiquarian of Messina." He advertised Scilla's ability as a keen observer of nature and recalled at the distance of several decades his initial pleasure in seeing "all these petrifactions" of Maltese tongue-stones in Scilla's collection (Boccone 1670, 79, 285; Boccone 1697, 180).[11] Boccone's comments reveal Scilla's interest in a wide range of natural phenomena. Scilla's patient observations and carefully reasoned conclusions about the probable function of different parts of plants led Boccone to praise him for exemplifying the "century of experiences" in which they lived. During one of his periodic trips to Messina, Boccone observed the "extraordinary leeches" which parasitically burrowed into the fleshy exterior of swordfish, thanking Scilla for bringing them to his attention. At Boccone's request, Borelli enclosed a specimen in a vial of aqua vitae to send to Florence so that "a good anatomist with a good microscope" – presumably Lorenzo Bellini to whom this letter was addressed – would observe it more closely. All of them acknowledged that Scilla deserved the credit for being the first to identify this little animal (Boccone 1670, 79–94, esp. 80, 83; 284–295, esp. 285, 290, 292).

For these reasons, Boccone was quite willing to do a favor for his friend the painter. In the summer of 1668 Boccone traveled to Malta to collect plants for a new botany of this region, many of which would later appear in his *Images and Descriptions of the Rare Plants of Sicily, Malta, France, and Italy* (1674) (Boccone 1674).[12] Both Boccone and Scilla were interested in Maltese fossils. On at least one earlier occasion, Boccone returned from Malta bearing specimens to compare with Sicilian examples which he and Scilla collected along the coast near Messina (Boccone 1670, 297–298). Boccone's return to Malta inspired Scilla to inquire about the possibility of adding more tongue-stones to his collection. Boccone understood the importance of this question because of his knowledge of Steno's research in Tuscany; he invited Buonamico to assemble "a collection of glossopetrae or tongues, as we call them here, of St. Paul, so-called serpents' eyes, shells, turbines, echnoids, bones, and other such petrified things that one finds between these rocks." He let his Maltese friend know that Scilla was especially interested in "entire masses of rocks" which contained different specimens mixed together (Buonamici 1668, 109) (Fig. 6.5). Buonamico was delighted to assist because he had been hoping to solicit Scilla's opinion of some Greek medals. Later publications reveal Boccone's evident sympathy for the position that Scilla took in the ensuing debate with Buonamico about the genesis of tongue-stones. Like Malpighi, Boccone would publicize the results of Scilla's research in the decades after the appearance of *Vain Speculation* (Boccone 1697, 181).[13]

Encouraged by Boccone, Buonamico decided to respond to Scilla's request with a letter offering his own interpretation of Maltese tongue-stones. Recently returned to Malta and fresh from his studies in the great universities of France and the Netherlands, Buonamico considered this invitation a golden opportunity to demonstrate his own expertise by participating in the debate. He enthusiastically endorsed the Jesuit polymath Athanasius Kircher's account of the genesis of fossils and the geological formation of the island of Malta in Kircher's influential, well-read, and much criticized encyclopedia on the *Subterranean World* (1665). While acknowledging the organic origins of some fossils, Kircher argued that the majority of objects

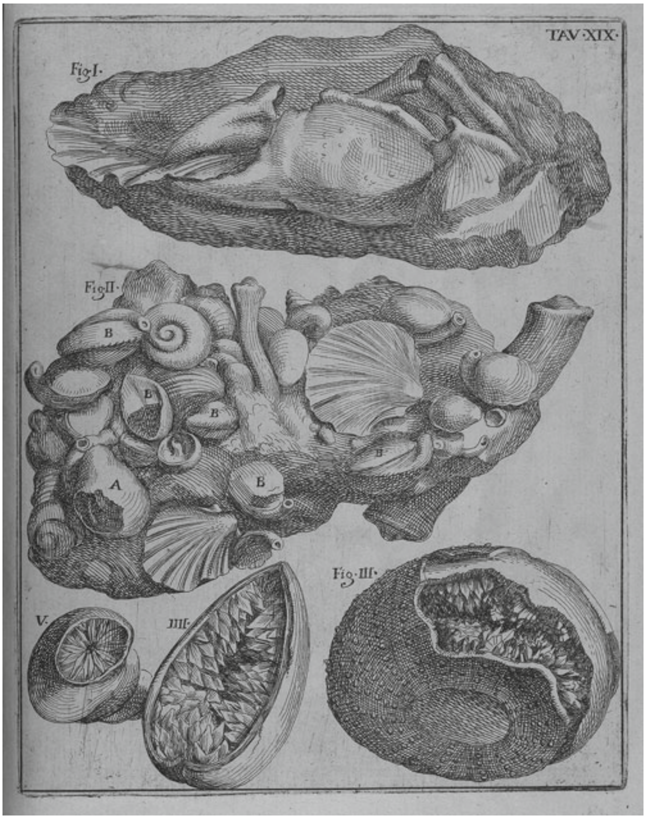


Fig. 6.5 Fossils embedded in a rocky mass (*Source*: Agostino Scilla, *La vana speculazione disingannata dal senso* (Naples 1670), Table 19. Courtesy of Special Collections, Stanford University Libraries)

found in stone were made of stone. He did not clearly distinguish between fossils that were remnants or imprints of once living creatures, and a much larger, more creatively defined category of "fossils" that were a product of nature's *vis plastica*, its ability to sculpt and mold things either by design or by chance (Kircher 1665; Gould 2004). Kircher based his knowledge of tongue-stones on his brief journey to Malta and Sicily in 1637. Buonamico proudly recalled his conversations "with the most learned Kircher" during this trip, including a detailed discussion of the island's formation (Buonamico 1668, 117). They debated whether Malta was a product of the Creation, the Flood, or the more natural effect of upwardly thrusting land emerging over time from the sea. Scilla would argue decisively for the third option, *contra* Kircher's more literally biblical account of the island's origins, incorporating it into his explanation of the quantity of fossils on the rocky slopes of the island.

Following Kircher, Buonamico also considered the nature of figured stones. While agreeing that some fossils closely approximated animals and were probably of organic origin – for instance, fossil echnoids – he observed that others took on "such extravagant and indeterminate shapes that it was impossible to make them resemble anything" (Buonamico 1668, 125) Many fossilized shells looked *like* shells but did not resemble any shells that could then be found on Malta's beaches. The absence of a directly analogous living being raised reasonable doubt about emerging theories of organic formation. Buonamico questioned how there could be so many petrified echnoids and shark's teeth when there were so few living examples. Sharks were rarely seen near the Maltese coast and sea urchins and sand dollars only occasionally washed ashore. An examination of shark's teeth raised further doubts. While Buonamico agreed that many tongue-stones closely resembled them, their irregularities in size and shape, the smoothness of many specimens, and numerous other inconsistencies rendered certain identification doubtful. Most importantly, he argued that the legendary therapeutic properties and curious appearance of Maltese tongue-stones made them a sublime expression of Divine Omnipotence *ludens in orbe terrarum*. God understood that "curious men" needed powerful healing miracles as well as natural paradoxes to investigate (Buonamico 1668, 125). The Maltese tongue-stones existed not only as a special sign of divinity but as a reminder of the limits of human knowledge confronting divine wisdom. In his explanation Buonamico carefully avoided denying the possibility that some fossils were animal petrifactions. He simply did not believe that all of them were organic in origin.

By August 28 Buonamico had completed his lengthy letter to accompany the box of specimens, and presented both items to Boccone for delivery to Scilla in Messina (Morello 1989 and 2003). During the fall of 1668 Scilla composed his response, writing an open letter to contest Buonamico's explanation of fossils. This document would become the first draft of his book. Neither Buonamico nor Scilla considered their disagreement to be personal, professing great admiration for each other throughout the exchange in a manner which seems to have gone beyond mere politeness. When Scilla published *Vain Speculation*, he deliberately chose not to attack Buonamico by name. Instead he critiqued many of the scientific authorities that his colleague cited in defense of the idea that tongue-stones were proof that Malta enjoyed God's special favor through the copious production of wonder-working petrifactions. Scilla strongly doubted that God miraculously transformed the eyes and tongues of Maltese snakes into stone not because he doubted miracles in general or Malta's historical importance as a site of early Christianity – he was careful to establish his credentials as a good Catholic regarding these issue as well as the belief in the Mosaic account of Creation as well as the biblical Flood – but because he did not think that Buonamico's proof rose to the level of a miracle (Scilla 1996, 27, 54, 71–72, 78).[14] While respectfully acknowledging Buonamico's greater empirical knowledge of Maltese nature, Scilla disagreed forcefully with his conclusions.

Despite their differing opinions on the formation of Maltese tongue-stones Scilla saw Buonamico as a potential ally more than adversary in the project of untangling their meaning. Certainly Scilla felt that Buonamico's reliance on Kircher's theory of fossil formation had led him astray yet he also recognized that his Maltese colleague did not entirely discount the eventual possibility of his own theory of organic origins nor did he always agree with Kircher on other matters. The question was how to persuade Buonamico to see fossils better. Scilla counted on Boccone to act as his trusted intermediary. Together they articulated the desirability of widening the conversation about the earth's history to include learned opinions which demonstrated the importance of local knowledge (Cooper 2007). Scilla, Buonamico, and Boccone lived amidst mountains of interesting fossils prized by other naturalists and in the midst of the rich and unstable volcanic terrain of the Mediterranean islands. They did not draw conclusions from a handful of specimens but proudly advertised the privileged perspective of the "native inhabitant" who was an eyewitness to the spectacular geology and paleontology of Sicilian, Malta, and Calabria (Buonamico 1668, 111). The Lincean project of studying fossils was born between the undulating hills of southern Umbria and the stinking sulfur pits of Pozzuoli a half-century earlier, but never traveled south of Naples. Now the field of knowledge had expanded to include all of southern Italy and the principal islands of the western Mediterranean islands. It was time for someone to write a new history of the earth and its productions from this location.

Scilla frequently reminded his readers that he lived in the midst of an extraordinary natural environment and traveled extensively within it. He knew that tongue-stones were not unique to Malta, citing many examples found throughout Sicily, especially in the vicinity of Corleone, which undermined prior arguments about their special nature. He claimed to draw his wealth of experience from conversations with Sicilian fisherman and Calabrian peasants, from beachcombing in Catania and walks through the port of Messina, and from frequent conversations with the most learned scholars throughout Sicily. He reminded readers that his artistic training made him especially capable of transmitting accurately what he observed (Scilla 1996, 39, 58, 63, 69, 89).[15] In every respect, Scilla presented himself as the ideal observer of southern Italian nature.

In 1668 Scilla was in the midst of an important conversation with Borelli which also inspired his response to Buonamico. In his original letter to Boccone, Scilla indicated that his desire for additional samples of Maltese tongue-stones was not solely for his own edification but also "to satisfy … other friends about the origin of similar figured stones" (Buonamico 1668, 110). Malpighi was no longer present, having returned to Bologna two years earlier, but Borelli was back in town and there were also other members of the Accademia della Fucina interested in fossils. Borelli's correspondence with Cardinal Leopoldo de' Medici reveals his own efforts to collect fossils during this period, undoubtedly inspired by his observations of Steno's research. The dissection of the great white shark caught in Livorno occurred in October 1666 when Borelli was still in Pisa. He knew the published account, *A Shark's Head Dissected* (1667) (Fig. 6.6). As Steno embarked on a much more comprehensive investigation of the earth's history, drawing especially upon Tuscany's geological record, Borelli forwarded southern Italian fossils to the Medici court as a point of comparison. In December 1667 he informed Cardinal Leopoldo de' Medici that he was sending "a large quantity" of petrified shells to Livorno. Borelli also wrote of his desire to see Steno in person (Dollo 1979, 327). He was curious about the varying interpretations of the Maltese tongue-stones and was

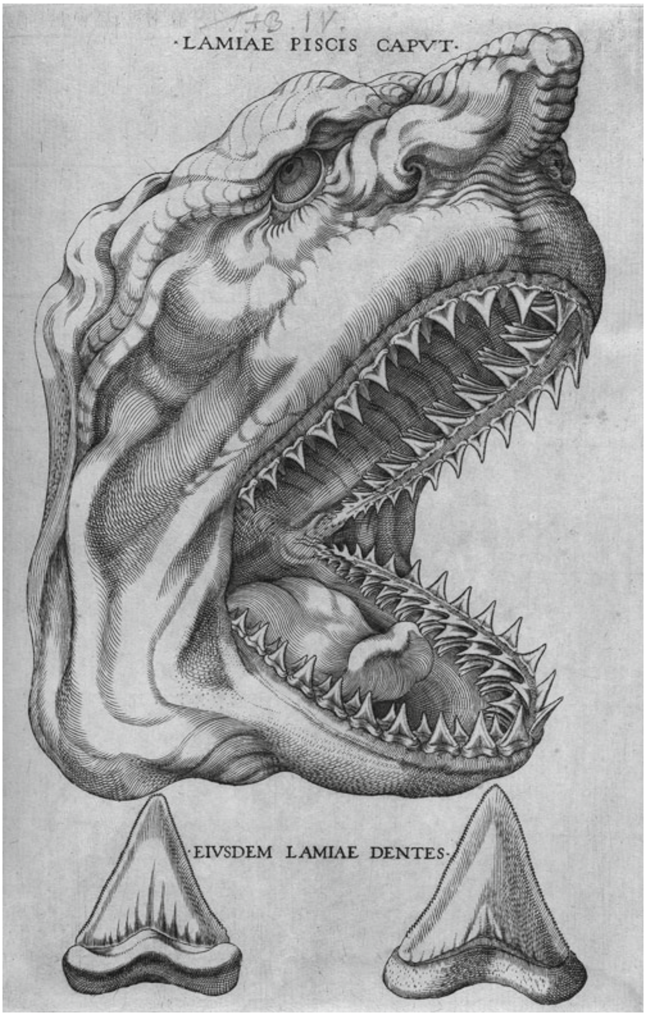


Fig. 6.6 Steno's engraving of a shark's head in comparison with Maltese tonguestones (*Source:* Nicolaus Steno, *Elementorum myologiæ specimen, seu musculi descriptio geometria. Cui accedunt Canis carchariae dissectum caput, et Dissectus piscis ex canum genere* (Florence 1667). Courtesy of Special Collections, Stanford University Libraries)

surely one of the friends whom Scilla had in mind when he wrote to Boccone requesting more specimens in the summer of 1668.

What, then, was the effect of Borelli's preoccupations on the painter from Messina? Scilla's interest in fossils seems to have emerged as part of a conversation, or rather a competition between Tuscany and Sicily engendered by Boccone and Borelli. While historians have often commented that Scilla wrote his book without having read Steno's *On Solids within Solids*, it was clearly written with an awareness

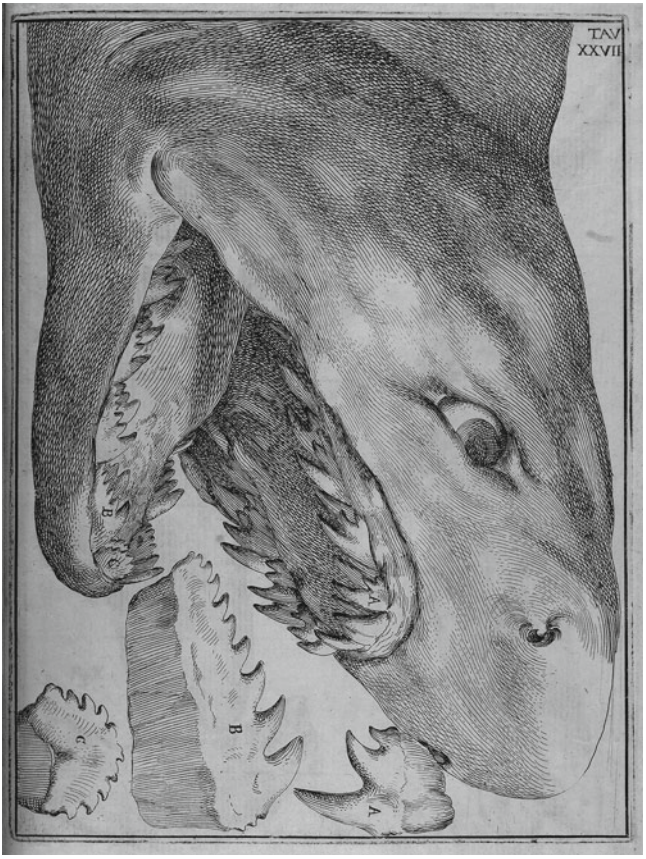


Fig. 6.7 Scilla's response to Steno's depiction of a shark (*Source*: Agostino Scilla, *La vana speculazione disingannata dal senso* (Naples 1670), Table 28. Courtesy of Special Collections, Stanford University Libraries)

of the imminent appearance of this book, not to mention concrete knowledge of the earlier publication of 1667. Buonamico, who had known Steno during their student days in Leiden, even mentioned his desire to acquire a copy of Steno's recent publication in his letter to Scilla (Buonamico 1668, 112). Curiously, Scilla did not refer explicitly to *A Shark's Head Dissected* anywhere in his text. Yet there is every reasons to belief that he read it carefully. The last two illustrations of Scilla's *Vain Speculation* were a form of visual citation that no one could have missed (Fig. 6.7).

In the captions for these illustrations, Scilla stated that he had drawn his shark's head "from life" (*al vivo)* and created a "drawing of the entire shark, not taken from another writer" (*Disegno dell'intero Pesca Vacca, non portato da altro Scrittore*). At an earlier point in his book, Scilla promised readers that he would provide a "drawing not only of the entire head … but the entire fish" because it had not been adequately described by earlier naturalists (Scilla 1996, 111, 76). In fact, he provided two illustrations of two different kinds of sharks, including a hammerhead. Everyone knew that Steno had borrowed his own image comparing a shark's head with tonguestones from the Tuscan courtier Carlo Dati's copy of the sixteenth-century papal physician Michele Mercati's beautifully illustrated and unpublished *Metallotheca*. Dati proudly advertised this fact and also encouraged Boccone to make use of Mercati's best images (Mercati 1717, xxiv-xxv, 333, 334n2; Boccone 1697, 295). We can now see how Scilla inserted a discussion of Steno in *Vain Speculation*. Critiquing Steno for using a dated illustration that was not of his own making, Scilla proudly advertised the originality of his own *disegno* as a genuine contribution to knowledge. He was improving upon *A Shark's Head Dissected*.

In the summer of 1668 Steno was completing his groundbreaking study, *On Solids within Solids*. At that very moment, Borelli was apologizing profusely to Cardinal Leopoldo for the loss of an unusually rich shipment of Sicilian, Maltese, and Calabrian fossils on a vessel which sank while navigating the waters between Messina and Livorno. In July 1668 Borelli informed Leopoldo that a smaller collection was in transit to replace the disappearance of the first shipment. He ecumenically observed that the possession of even "a few [fossils] can in every respect provide ground for and foment both one and the other opinion now in contention about the origin of the said stones" (Dollo 1979, 329). Was Scilla the supplier of the fossils which eventually found their way to Florence? He was then in the process of adding similar specimens to his own collection, and sending additional samples to Malpighi in Bologna.

The growing interest in fossils in the mid-seventeenth century made their possession critical to participation in the debate about their origins. Naturalists coveted these unusual artifacts but they almost never traveled far enough south, let alone to the islands of the Mediterranean, to acquire them personally. As a result, they relied on intermediaries for the occasional specimen. Scilla's visualization of this fossil record in his *Vain Speculation* created a compelling portrait of a virtual collection. Each specimen was carefully selected by the owner who meticulously rendered them as drawings which Bartoli transformed into subtle and richly detailed engravings, presenting them with the same vivacity and dimensionality which he normally reserved for antiquities (Carpita 2006). Keeping in mind the very real possibility that Scilla, his patron Ruffo, and even Borelli, who had been educated in Rome, knew something of Cassiano dal Pozzo's archive of natural history, perhaps the starting point for Scilla's project was the idea of a paper museum that traveled through the medium of print. They were well aware that Castelli's study of insects languished in obscurity because it could be seen only in Messina.

Scilla was equally inspired by the idea of writing a scientific letter as a mode of communication. For him as much as for Buonamico, the letter was a means of participating in more than just a local conversation. Virtually every major publication on natural history by Redi, for example, was written in the form of a letter, which became the preferred genre of mid-seventeenth scholarship (Findlen 1993; Dooley 2001) . Buonamico saw Scilla's request for specimens as an opportunity to make a uniquely Maltese contribution to the fossil debate in the form of an unpublished letter. Encouraged by Borelli, Boccone, and possibly other members of the Fucina,

Fig. 6.8 The eruption of Mount Etna, 1669 (*Source*: Paolo Boccone, *Recherches et observations naturelles* (Amsterdam 1674). Courtesy of Special Collections, Stanford University Libraries)

Scilla responded with an equally distinctive Sicilian letter which highlighted his command of empirical evidence and his more sophisticated understanding of the fossils themselves. Unlike Buonamico, he published his letter to seek a wider audience leading the eighteenth-century editor of the published version of Buonamico's letter to express outrage that Scilla would dare to publish a letter without crediting his adversary by name. Yet the real issue in 1668–1670 was not how to acknowledge his Maltese correspondent but how to ensure that Steno did not receive all the credit for writing a new history of the earth when others were arriving at similar conclusions, based in part on their appreciation of the work of Colonna and Imperato, the southern Italian naturalists of the preceding century who established the tradition which Scilla and Boccone revived. They would not let a Danish anatomist have the final word on the Maltese tongue-stones.

Shortly after Scilla completed the initial draft of his response to Buonamico, new events made the question of the earth's history even more pressing for the scholarly community in Messina. The violent eruption of Mount Etna in March 1669 catalyzed Borelli into making his own contribution to writing the earth's history from a southern Italian perspective (Fig. 6.8). From Rome, Cardinal Leopoldo asked him for an account of the volcano's activity. The result was Borelli's *History and Meteorology of the Eruption of Etna in 1669* (1670), published in the same year in which *Vain Speculation* appeared. Benefiting from the appearance of Steno's masterful account of the earth's stratification and sedimentation, Borelli presented his own highly sophisticated understanding of a volcanic eruption as a geophysical phenomenon which could be studied physically, chemically, and mathematically. His account of Etna's most recent eruption explicitly critiqued a central argument put forth by the Jesuit Kircher in his *Subterranean World*. Using evidence from Etna's lava flow and changing morphology, Borelli negated the idea of eternal mountains and perpetual subterranean fires poetically evoked by Kircher in his lushly illustrated volume (Borelli 2001; Nigido-Dionisi 1903, 200–201). Even more strongly than Scilla, he hoped to demolish the underlying premise of Kircher's natural philosophy.

Borelli's account of Etna offered the radical specter of a dynamic nature in which the extinction of volcanoes might be but a prelude to the end of the earth. Scilla's *Vain Speculation* needs to be read in relation to this foundational treatise of vulcanology because his work was fundamentally a rethinking of the earth's beginning. By 1697 Boccone was willing to state that "the earth is far more ancient than us and we do not know when mountains may have been formed" (Boccone 1697, 285) There is no doubt that Borelli and Scilla developed their publications fully cognizant of the significance of Steno's research and ready to engage with its important findings. They also wished to muster evidence to demonstrate the fallacies of Kircher's *Subterranean World*, as a premature effort to write a synthetic history of the earth when so many doubts remained about its specifics. Arguing for the importance of greater experience and methodological acumen, both Borelli and Scilla demonstrated how a different way of seeing and interpreting nature could produce a more persuasive account of the earth's history. In the process, they found themselves confronting a new set of questions which were not easily answered but since their goal was probable rather than certain knowledge, they did not feel obliged to resolve every single doubt. Instead, it belonged to the next generation of naturalists to consider more seriously the antiquity of the earth itself.

In the middle of the seventeenth century Scilla and Borelli's simultaneous publications put Sicily at the center of a new history of the earth while Boccone gathered materials for a new botany showcasing specimens from Sicily, Malta, and Calabria. The Fucina's decision to sponsor Scilla's *Vain Speculation* after their support of a number of Borelli's earlier publications, adds further credence to the idea that there was a common project underway in the late 1660s to write a Sicilian history of nature (Nigido-Dionisi 1903). Scilla's book was not simply an appendage of Borelli's project; he spoke distinctively in his own voice and drew his authority not only from his collaborations with other naturalists but from the status he enjoyed as a learned painter. At the same time, his attentiveness to the largely horizontal layering of the soil surrounding Messina and its movements after a heavy rain was not only a matter of an artist's perspective on the changing natural landscape before him but an acknowledgment of the importance of the geophysical arguments made by Borelli and Steno (Scilla 1996, 89–90). Let us return in the final section of this paper to his arguments for the painter's insight into nature.

Seeing Fossils Like a Painter

How did Scilla see fossils like a painter? In order to understand Scilla's approach to writing about fossils, we need to understand painting much as his contemporary Bellori described it: a deeply theorized kind of knowledge in seventeenth-century Italy. The question of *disegno* was never far from Scilla's mind as he looked at the specimens before him. "I will draw," Scilla declared. He encouraged friends to send him as many different kinds of fossils as possible to improve the quality of his drawings and increase the probability of his conclusions. He carefully selected and arranged the fossils in his collection to achieve the maximum impact on the viewer, and sought to faithfully transmit his impressions of what he saw. If David Rosand is correct in arguing that "in drawings we most directly encounter the artist," then Scilla's surviving drawings of his fossils offer us the best evidence of how he thought about nature. His goal was to create the best possible representation of each specimen – "a most accurate drawing" (*il disegno puntualissimo*) capable of making the argument better than words alone (Scilla 1996, 63, 88; Rosand 2002, 19; Bermingham 2000) (Fig. 6.9). Through drawing, he saw nature better and he sought to sharpen the perception of others by inviting them to look with him.

At every stage, Scilla asked his readers to consider not only the nature in front of them but also the evolution of the artist's sketchbook, filled with drawings of everything he observed (Purcell and Gould 1992, 84–94). He wished to impart one of the most fundamental lessons he had learned as an artist, namely, that nature is not uniform but can only be known through the uniqueness of its features. Scilla offered the example of the human face, recalling the many pages of drawings of different faces by artists who found the unique physiognomy of each person, indeed the changes to each person's appearance in the course of their life, to be a source of inspiration:

> Even though we are all of one species, we, in fact, vary in respect to our faces and limbs. Indeed, with age, we differ from our [earlier] selves. The same can be said of animals and fruits that are cultivated on the same tree. I pretend to be assured in one grasp of my experience of grapes, but if at times I need to paint them, I am forced to make a specific portrait down to each seed (Scilla 1996, 59).[16]

In this passage, Scilla adroitly demonstrated how the principles of good artistic practice were an essential feature of scientific observation.

The painter's habit of looking closely at every detail to create a faithful representation gave him privileged insight into nature's variety and complexity. At one point in *Vain Speculation* Scilla described breaking open pieces of fossil coral found in the hills to prove that the pale, stony exterior hid "a certain embodied tincture that assures us that it was colored red, like all of its species" (Scilla 1996, 219). Color, texture, and form were all the domain of the painter, as was the evolution from nature's rough sketch to its final product. Through brilliant use of the ordinary example, Scilla helped his readers to see that nature itself was not the alchemy of the canvas, as Kircher and others argued, but a painting with a history (Elkins 1998; Smith 2004).[17] Restoring that history would capture the stages of a fossil's development. His technique invited readers to identify the clues which allowed one to see not only what a fossil

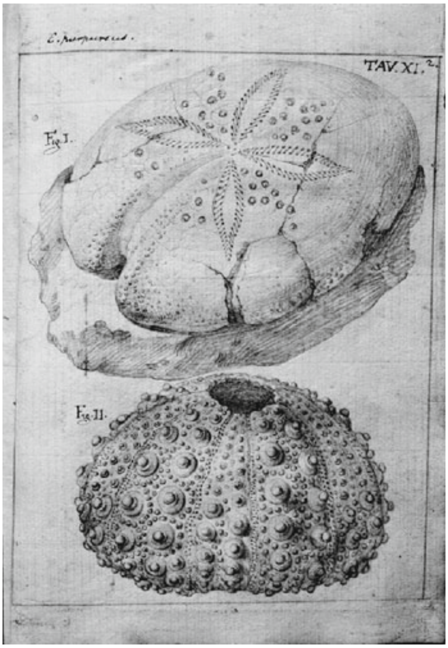


Fig. 6.9 Scilla's pen & ink drawing of fossilized echnoids in preparation for the publication of *Vain Speculation Undeceived by Sense*. Photograph © Rosamond Purcell. Courtesy of Rosamond Purcell and the Sedgwick Museum, University of Cambridge

became, but what it had been. Much like his predecessor and fellow fossil-hunter Leonardo, Scilla believed that the painter possessed the secrets of nature through the act of reproduction. Nature and a painting were but two different expressions of the creation of things in the universe. Therein lay the artist's advantage.

Scilla invited readers to contemplate the power of *disegno* in demonstrating a number of crucial features he observed through a close examination of the fossils in his possession. He repeatedly drew tongue-stones – as singular specimens to illustrate the varied appearance of fossil teeth, as a precisely positioned series of specimens to indicate their location in a jawbone, and as objects embedded in stone which itself contained the original outline of the tooth and, in some instances, even the root (Fig. 6.10). Each of these ways of viewing fossils drew upon different artistic skills, not only painting and drawing but also sculpture and casting. Scilla also used drawing to invite comparisons between specimens. Placing Maltese tongue-stones next to Sicilian examples, he underscored the "sameness of the parts" which confuted any theory about their uniqueness (Scilla 1996, 64). Scilla's virtual portrait of the tongue-stone was a sophisticated exercise in constantly adjusting the angle of vision. He also used this technique with great effect on the fossil sea urchins, sand dollars, coral, fish vertebrae, and other objects in his collection. His goal was to bring his specimens to life on paper.

In the tradition of every anatomical illustrator since Vesalius, Scilla labeled crucial details in each image to draw the reader's attention to those ingredients which he believed to be most decisive in demolishing ancient theories of fossils.

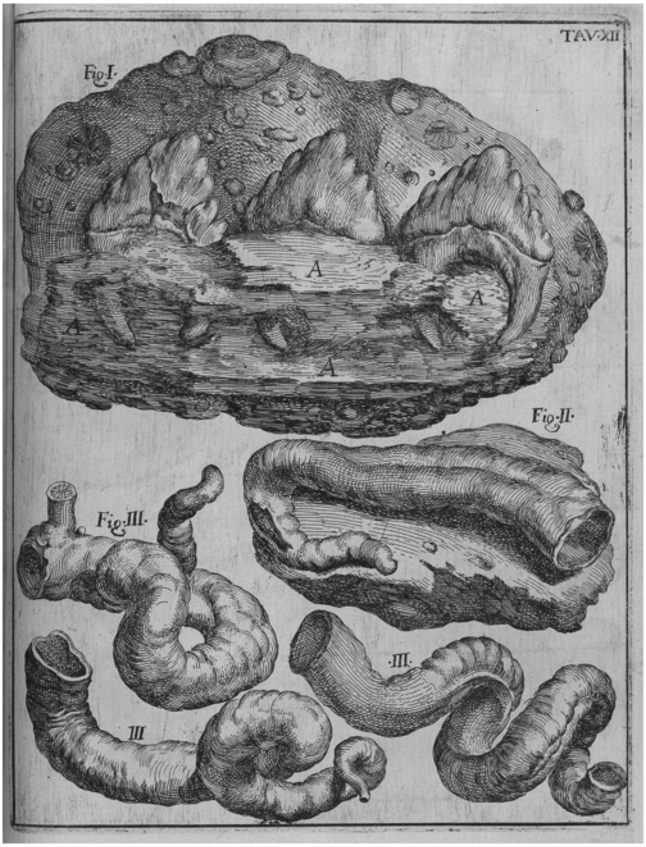


Fig. 6.10 Bartoli's engraving from Scilla's drawing of fossil teeth with the root visible (*Source*: Agostino Scilla, *La vana speculazione disingannata dal senso* (Naples, 1670), Table 12. Courtesy of Special Collections, Stanford University Libraries)

In the most sophisticated examples, he showed readers how to look at his images, drawing their attention first to one detail, and then another, until he felt that his evidence was indisputable. In his depiction of fossil vertebrae, for example, Scilla displayed his anatomical abilities by creating a "drawing of the entire spine of some fish" labeled Fig. 5 to compare a living and complete example with fossil fragments. Scilla then asked them to compare this image with Figs. 2, 3 and 4 to see "vertebrae of once living animals located below where the breast had been" and Fig. 1 to see "those near the tail" (Scilla 1996, 94) (Fig. 6.11). Yet the capstone of his natural history lesson can be observed in a detail in Fig. 6.4 which he did not mention: the projection through use of a broken line restoring the missing parts. Scilla's drawing technique also benefited from his knowledge, as an antiquarian, of how to reconstruct an object. He considered this disciplined use of the imagination critical to seeing the animal emerge from the fossil.

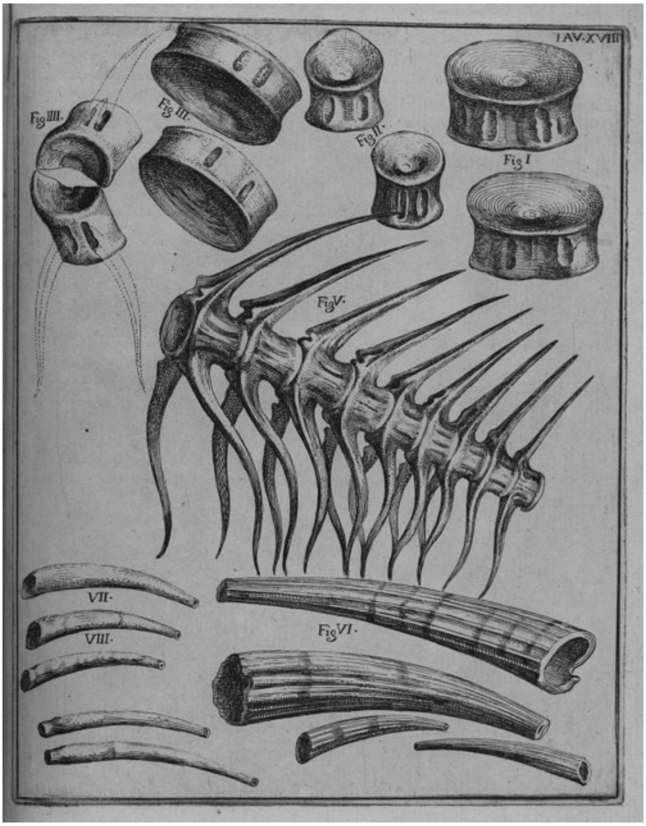


Fig. 6.11 Bartoli's engraving from Scilla's drawing of fossil vertebrae (*Source*: Agostino Scilla, *La vana speculazione disingannata dal senso* (Naples, 1670), Table 18. Courtesy of Special Collections, Stanford University Libraries)

Scilla also advocated the importance of seeing "with the help of a microscope" (Scilla 1996, 107; Trinci 1988). Many of his drawings were naked-eye observations but he also demonstrated the importance of this Galilean instrument in capturing minute details. Examining fossil sea urchins, Scilla had a critical problem to solve: what had happened to their spiny exterior? In order to find evidence that these fossils were once covered with the "most subtle spines," he had to demonstrate the existence of the spine among the fossils he could find and find evidence for how the spines attached to the sea urchin. Scilla use multiple images to resolve this problem. First, he presented a small fragment of a fossil sea urchin whose surface had been enlarged under a microscope to make an "observation of the tiny little nipples" on the entire body (Fig. 6.12). Next, he provided more complete images of sea urchins, juxtaposing these examples to drawings of somewhat larger echnoids known as "marine porcupines" (*istrici marini*) whose spines, though also broken, were far more visible. Finally, he completed the task of putting all the parts together by

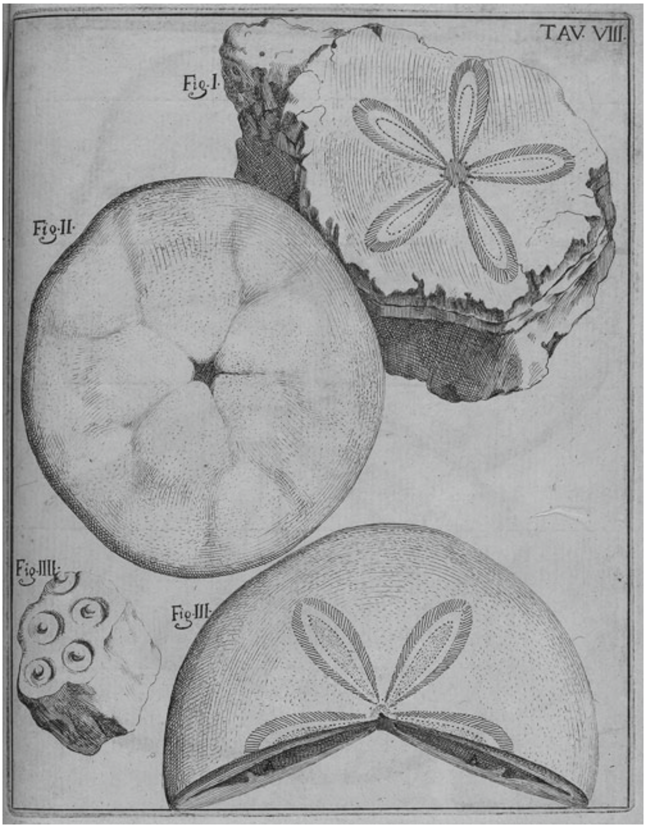


Fig. 6.12 Bartoli's engraving of Scilla's observations of fossilized sea urchins using a microscope (*Source*: Agostino Scilla, *La vana speculazione disingannata dal senso* (Naples, 1670), Table 8, Fig. 4. Courtesy of Special Collections, Stanford University Libraries)

demonstrating that the fossils "commonly and variously called St. Paul's batons" (*bastoncini … detti volgarmente e variamente di S. Paolo*) were "manifestly spines of a sea porcupine" (Scilla 1996, 88, 100–101) (Fig. 6.13). Forestalling any potential criticisms, Scilla removed the spines from a live sea urchin to establish the degree of correspondence.

With each drawing, Scilla experimented with different ways to make his argument through images. He created drawings which put the pieces back together and used drawing to reveals the pattern of the ruined object. While proudly advertising his success in depicting specimens on paper, Scilla was also willing to confess the limits of his skill in capturing the most complex examples. In one instance, he apologized for his inability to "bring forth a graceful view in drawing" of a rocky mass of miscellaneous fossils, including shells containing petrified bodies. About this fossil within a fossil, Scilla lamented, "I am not finding the way to express on paper, to satisfy the eyes of all." He contented himself with drawing the viewer's

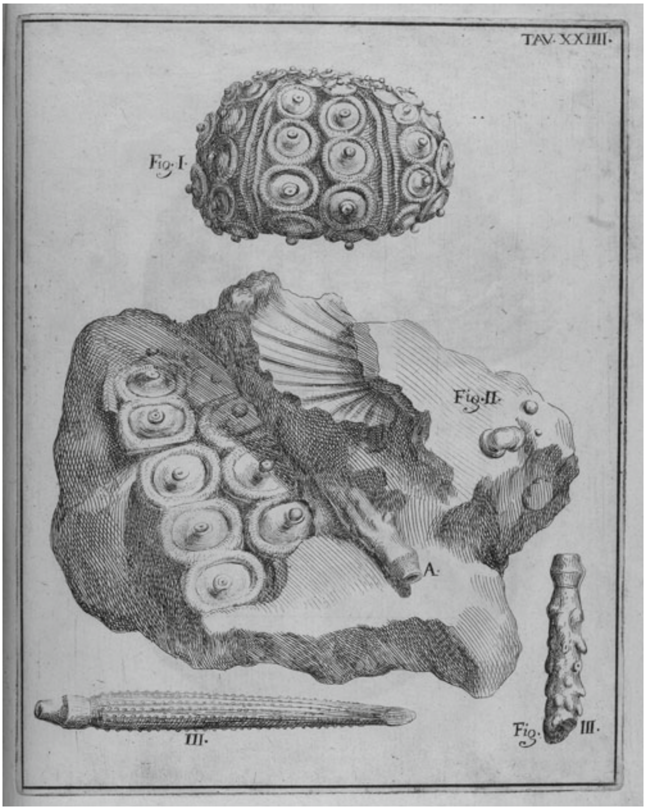


Fig. 6.13 Bartoli's engraving of Scilla's observations of "St. Paul's batons" (*Source*: Agostino Scilla, *La vana speculazione disingannata dal senso* (Naples, 1670), Table 24, Fig. 3. Courtesy of Special Collections, Stanford University Libraries)

attention to "a small crack" labeled A – the mysterious site of this elusive instance of micro-petrifaction (Scilla 1996, 96) (See Fig. 6.5). In a number of instances, he addressed the limits of drawing by describing a more tactile approach to specimens: the anatomizing of a specimen to see what lay beneath the surface, the crushing of objects to learn more about their composition. Scilla's *Vain Speculation* was both a scientific treatise on fossils and an instructional manual in the art of scientific illustration. He had a very clear vision of what artists could contribute to science.

In his own fashion, Scilla saw nature as a form of divine art and Malta became his own demonstration of God's Creation. Contradicting Kircher, Scilla argued that Malta emerged in a series of natural transformations over time. As the sea receded, layers of fossils appeared. To make his argument persuasive, he offered the analogy of artistic creation:

> And if we observe well the progress of a painter and a sculptor, we realize that first they sketch and hew everything, and that the last strokes will be the most beautiful and most

> assured. And if this is true, consider the operations of the Great Artificial Creator who colored this world perfectly with a beautiful light, and who sculpted it marvelously with his omnipotent right hand. We must admire this island as one of the strokes reserved to the power of God, who intended to beautify the most noble part of the great body of the earth with a lively and clarifying light (Scilla 1996, 54).

Rather than being the God's first creation, Malta was the last and most wondrous part of nature. The layers of fossils which encrusted its surface offered special proof of how the earth was formed.

Like the anamorphic paintings which became popular in the mid-seventeenth century, Malta was a painting of nature which required only the proper perspective to emerge into view. It was an object lesson in seeing as well as knowing. "All the observations combine with that [evidence] like an infinite number of lines which stop when directed to a point," declared Scilla (1996, 101). Living and working in Messina in the mid-seventeenth century, Scilla indeed is the vanishing point of an intellectual conversation. It is only now that we can retrace the invisible lines which place him in the midst of the early modern debate about fossils. He sought to fully realize the visual program of post-Galilean science – responding to and critiquing ways of seeing and knowing that we might indeed characterize as "baroque" – and never doubted that he could replicate nature with a painter's eye. Scilla's baroque was a product of many different conversations in play in the mid-seventeenth century. A close examination of his passion for fossils allows us to understand natural history as a discipline emerging at the crossroads between art and science, and past and present – not absolutely "baroque" in any strict sense but fully partaking in a moment that has alternately been defined as the age of the baroque, or the scientific revolution when it was of course both of these things.

Notes

1. Scilla was responding to Buonamico's letter of 28 August 1668, stating his views on fossils to accompany a box of Maltese tongue-stones. It remained unpublished for a century.
2. While there are twenty-eight numbered illustrations, two images are identified as number eleven, making a total of thirty images including the frontispiece. I owe a great debt to Veronica Carpita for recently identifying Bartoli as the engraver of these images.
3. Scilla possessed an important bronze tablet of a *Senatus Consultus* of Titus Livius which he donated to the Roman antiquarian Raffaele Fabretti in 1699. The collection of papal coins developed and published by his son Saverio (1673–1748), which surely had its origins in his father's collection, became part of the *Musaeum Christianum* founded by Benedict XIV and eventually found its way into the Vatican Museum. See Di Bella (1998, esp. 37, 39).
4. As the exact title of his recently rediscovered treatise on medals reveals, even as antiquarian Scilla advertised his professional identity: *De' Discorsi sopra alcune medaglie delle siciliane Città di Agostino Scilla Pittore* (Hyerace 2001).
5. Thanks to Bill Ashworth for suggesting this idea to me many years ago, when I first began to work on Scilla. I look forward to seeing his discussion of this subject in his forthcoming study of the visual culture of early modern science.
6. Four of the most accessible of Scilla's paintings are in the Museo Regionale in Messina. In addition to the image mentioned above, they are: *Saint Hilarion in the Arms of Death*, *Saint Gaetano Before the Virgin*, and *Saint John the Baptist*. The Galleria Regionale della Sicilia in

Palermo owns Scilla's *Epicarmus crowned by Thalia*. Scilla's *Self-Portrait* and *St Jerome* can be found in the Accademia di San Luca, Rome. The Museum of Fine Arts, Boston possesses a painting by Scilla that they have called a *Portrait of an Artist* and identified as a probable portrait of Sacchi, but is almost certainly a portrait of Scilla.

7. For an example of Castelli's standard of illustration, see his *Exactissima descriptio rariorum quarandam plantarum qu[a]e continentur Rom[a]e in Horto Farnesiano* (Rome, 1625) which Castelli published under Tobias Aldini's name.
8. The Danish anatomist Thomas Bartholin praised Castelli as the best physician by far in the city of Messina during his visit in 1644.
9. Thanks to Michael Gaudio for drawing my attention to this passage.
10. See also Ray 1848, 24 (Ray to Lister, London, 13 May 1668): "Praeter hos qui de insectis scripserunt, alicujus nominis aut pretii nullos novi; vidi aliquando in Siciliâ opus manuscriptum D. Castelli Romani in duos tomos divisum. Volumen satis spissum et grande cum figures propriâ ipsius manu delineates, quod nescio an unquam lucem publicam visurum sit."
11. We know that Boccone kept track of Scilla since he described him in 1697 as "Signor Agostino Scilla Letterato, e Pittore di molta lode, al presente in Roma."
12. There is no mention of Scilla in this book but warm references to Giovanni Francesco Buonamico as his source for Maltese plants (e.g. p. 82). See also Boccone (1697).
13. Here Boccone discusses the reasons why he is persuaded by the views of Colonna, Steno, and Scilla. By contrast, the chapter on tonguestones in his *Recherches* (Boccone 1670, 314–328) cites Colonna and extensively discusses Steno's two publications but curiously makes no mention of Scilla.
14. The legend of the Maltese tongue-stones refers to an episode in St. Paul's conversion of the Maltese to Christianity in 60 CE. Shortly after landing upon the island, Paul survived the bite of a poisonous snake which he threw into the fire. The fact that he was unharmed impressed the Maltese greatly with the power of his faith. By the Middle Ages there was a flourishing trade in Maltese tongue-stones. Widely used to counteract all manner of poison and more generally to effect supernatural cures, they bore the memory of St. Paul. See *Acts of the Apostles* (28: 2–7).
15. Boccone agreed that Corleone was also a source of tongue-stones; see Boccone (1697, 180).
16. This wonderful passage reinvents a famous episode in the history of ancient painting, recounted by Pliny in his *Natural History*. In a public contest to determine the best painter, Zeuxis, whose painting of grapes appeared so life-like that birds tried to eat them, was outdone by Parrhasius, whose painting of the curtain was so realistic that it deceived even Zeuxis.
17. On the alchemy of painting, see Smith (2007) and Elkins (1998).

References

Accordi, Bruno. 1975. Paolo Boccone (1633–1704) – A practically unknown excellent geo-paleontologist of the 17th century. *Geologica Romana* 14: 333–359.

Accordi, Bruno. 1977. Agostino Scilla, painter from Messina (1629–1700), and his experimental studies on true nature of fossils. *Geologica Romana* 17: 129–144.

Adelmann, Howard (ed.). 1975. *The correspondence of Marcello Malpighi*. Ithaca: Cornell University Press. 5 vols.

Ashworth Jr., William J. 1990. Natural history and the emblematic world view. In *Reappraisals of the scientific revolution*, ed. David Lindberg and Robert Westman, 303–332. Cambridge: Cambridge University Press.

Azzolini, Monica. 2005. In praise of art: Text and context of Leonardo's Paragone and its critique of the arts and sciences. *Renaissance Studies* 19: 487–510.

Bell, Janis, and Thomas Willette (eds.). 2002. *Art history in the age of Bellori*. Cambridge: Cambridge University Press.

Bellori, Giovan Pietro. 2005. *The lives of the modern painters, sculptors, and architects*, ed. Helmut Wohl (trans: Sedgwick, Alice). Wohl. Cambridge: Cambridge University Press.

Beneš, Mirka, et al. 1993. *The paper museum of Cassiano dal Pozzo*. Ivrea: Olivetti.

Bermingham, Ann. 2000. *Learning to draw: Studies in the cultural history of a polite and useful art*. New Haven: Yale University Press.

Bertoloni Meli, Domenico. 1996. The neoterics and political power in Spanish Italy: Giovanni Alfonso Borelli and his circle. *History of Science* 34: 57–89.

Bertoloni Meli, Domenico (ed.). 1997. *Marcello Malpighi: Anatomist and physician*. Florence: Olschki.

Bianchi, Massimo Luigi. 1987. *Signatura Rerum. Segni, magia e conoscenza da Paracelsus a Leibniz*. Rome: Edizioni dell'Ateneo.

Boccone, Paolo. 1670. *Recherches et observations naturelles*. Amsterdam: Chez Jean Jansson à Waesberge.

Boccone, Paolo. 1674. *Icones & descriptiones rariorum plantarum Siciliae, Melitae, Galliae & Italiae.* London.

Boccone, Paolo. 1697. Museo di fisica e di esperienze. Venice

Borelli, Giovanni Alfonso. 2001. *Storia e meteorologia dell'eruzione dell'Etna del 1669*, ed. Nicoletta Morello. Florence: Giunti.

Boschiero, Luciano. 2007. *Experiment and natural philosophy in seventeenth-century Tuscany: The history of the Accademia del Cimento*. Dordrecht: Kluwer.

Bredekamp, Horst. 1995. *The lure of antiquity and the cult of the machine* (trans: Brown, Allison). Princeton: Markus Weiner.

Bredekamp, Horst. 2007. *Galileo der Kunstler: Der Mond, Die Sonne, Die Hande*. Berlin: Akademie Verlag.

Buonamico, Giovanni Francesco. 1668. Lettera missiva del Signor Gio. Francesco Buonamico Maltese Dottore di Medicina, Filosofo, e Poeta diretta ad Agostino Scilla messinese Pittore, ed Academico della Fucina detto lo Scolorito. In *Opuscoli di autori siciliani.* Palermo. 1758–1771, vol. 11, 105–200.

Carpita, Veronica. 2006. Agostino Scilla (1629–1700) e Pietro Santi Bartoli (1635–1700): il metodo scientifico applicato allo studio dei fossili e la sua trasmissione ai siti e monumenti antichi. *Rendiconti dell'Accademia Nazionale dei Lincei. Classe di scienze morali, storiche e filologiche* ser. 9, vol. 17, fasc. 3: 307–384.

Castelli, Pietro. 1652. *An Smilax Aspera Europea sit eadem ac Salsa Parilla Americana*. Messina: Typis Haeredum Petri Breae.

Colonna, Fabio. 1616. De glossopetris dissertatio. In his *Purpura. Hoc est de Purpura ab animali testaceo fusa, de hoc ipso animali aliisq[ue] rarioribus testacies quibusdam.* Rome.

Cooper, Alix. 2007. *Inventing the indigenous: Local knowledge and natural history in early modern Europe*. Cambridge: Cambridge University Press.

Cutler, Alan. 2003. *The seashell on the mountaintop: A story of science, sainthood, and the humble genius who discovered a new history of the earth*. New York: Dutton.

Daston, Lorraine, and Katharine Park. 1998. *Wonder and the order of nature 1150–1750*. New York: Zone Books.

Di Bella, Sebastiano. 1998. Le collezioni romane di Saverio Scilla. *Archivio storico messinese* 76: 21–57.

Di Bella, Sebastiano. 2001. Agostino Scilla collezionista: la raccolta dei fossili. In *Wunderkammer siciliana: alle origini del museo perduto*, ed. Vincenzo Abbate, 60–66. Naples: Electa.

Dollo, Corrado. 1979. *Filosofia e scienze in Sicilia*. Padua: CEDAM.

Dollo, Corrado. 1984. *Modelli scientific e filosofici nella Sicilia spagnola*. Naples: Guida Editore.

Dooley, Brendan. 2001. *Science and the marketplace in early modern Italy*. Lanham: Lexington Books.

Elkins, James. 1998. *What painting is: How to think about oil painting using the language of alchemy*. New York: Routledge.

Farago, Claire. 1992. *Leonardo da Vinci's "Paragone": A critical interpretation with a new edition of the text in the Codex Urbinas*. Leiden: Brill.

Findlen, Paula. 1990. Jokes of nature and jokes of knowledge: The playfulness of scientific discourse in early modern Europe. *Renaissance Quarterly* 43: 292–331.

Findlen, Paula. 1993. Controlling the experiment: Rhetoric, court patronage and the experimental method of Francesco Redi. *History of Science* 31: 35–64.

Findlen, Paula. 2004. Science, art, and knowledge in seventeenth-century Rome: New work on the Accademia dei Lincei (1603–30). *Metascience* 13: 275–302.

Freedberg, David. 2002. *The eye of the Lynx: Galileo, his friends, and the beginnings of modern natural history*. Chicago: University of Chicago Press.

Giuliani, Erika. 2009. Pietro Castelli, an Italian case for European botany. In *Confronting mortality with art and science*, ed. Pascale Pollier-Green et al., 179–186. Brussels: VUB Press.

Gould, Stephen Jay. 2004. Father Athanasius on the isthmus of a middle state: Understanding Kircher's paleontology. In *Athanasius Kircher: The last man who knew everything*, ed. Paula Findlen, 198–228. New York: Routledge.

Haskell, Francis. 1980. *Patrons and painters: Art and society in Baroque Italy*, revth ed. New Haven: Yale University Press.

Haskell, Francis, et al. 1989. *Il Museo cartaceo di Cassiano Dal Pozzo: Cassiano naturalista*. Milan: Olivetti and Mondadori.

Hyerace, Luigi. 1999. Aggiunte ad Agostino Scilla. *Prospettiva* 93–94: 200–207.

Hyerace, Luigi. 2001. Agostino Scilla collezionista: le raccolte di monete, medaglie, disegni e anticaglie. In *Wunderkammer siciliana: alle origini del museo perduto*, ed. Vincenzo Abbate, 55–60. Naples: Electa.

Jenkins, Ian. 1992. *Cassiano dal Pozzo's Paper Museum*. Ivrea: Olivetti.

Kircher, Athanasius. 1665. *Mundus subterraneus*, 2 vols. Amsterdam.

Kusukawa, Sachiko. 1997. Leonhart Fuchs on the importance of pictures. *Journal of the History of Ideas* 58: 403–427.

Lavin, Irving. 1980. *Bernini and the unity of the visual arts*, 2 vols. New York and London: Oxford University Press and the Pierpont Morgan Library,

Lechner, George. 1976. Tommaso Campanella and Andrea Sacchi's Fresco of Divina Sapienza in the Palazzo Barberini. *Art Bulletin* 58: 97–1108.

Leibniz, Gottfried Wilhelm. 1749. *Protogaea sive de prima facie telluris et antiquissimae historiae vestigiis in ipsis naturae monumentis dissertation ex schedis manuscriptis viri illustris in lucem edita a Christiano Ludovico Scheidio*. Göttingen.

Lombardo, Richard. 'To satisfy the eyes of all': Agostino Scilla, 'Virtuosi,' the epistemology of sense experience, and 'la Veridica Storia.' (Unpublished paper).

Mangion, Giovanni. 1971. Giovan Francesco Buonamico scienziato e letterato maltese del Seicento. *Studi secenteschi* 12: 285–321.

Marini, Maurizio. 1990. Due nature morte di Agostino Scilla. *Quaderni dell'Istituto e Storia dell'Arte Medievale e Moderna, Facoltà di Lettere e Filosofia Università di Messina* 14: 49–52.

Martinelli, Valentino. 1978. Agostino Scilla pittore e scrittore messinese, esule a Roma. In *Scritti in onore di Salvatore Pugliatti*, vol. 5, 595–605. Milan: Giuffrè.

Memoria. 1821. *Memoria de' pittori messinesi*. Messina: Giuseppe Pappalardo.

Mercati, Michele. 1717. In *Metallotheca*, ed. Giovanni Maria Lancisi. Rome.

Momigliano, Arnaldo. 1950. Ancient history and the antiquarian. *Journal of the Warburg and Courtauld Institutes* 13: 285–315.

Morello, Nicoletta. 1989. Giovanni Francesco Buonamico and the fossils: A flood of problems. In *Italian scientists in the low countries in the XVIIth and XVIIIth centuries*, ed. C.S. Maffioli and L.C. Palm, 131–145. Amsterdam: Rodopi.

Morello, Nicoletta. 2003. The question on the nature of fossils in the 16th and 17th centuries. In *Four centuries of the word geology: Ulisse Aldrovandi 1603 in Bologna*, ed. Gian Battista Vai and William Cavazza, 127–151. Bologna: Minerva.

Morello, Nicoletta. 1979. *La nascita della paleontologia*. Colonna, Stenone e Scilla. Milan: Franco Angeli.

Nigido-Dionisi, Giacomo. 1903. *L'Accademia della Fucina di Messina (16399-1678) ne' suoi rapporti con la storia della cultura in Sicilia*. Catania: Niccolò Giannotta.

Ogilvie, Brian. 2006. *The science of describing: Natural history in Renaissance Europe*. Chicago: University of Chicago Press.

Palmer, Rodney. 2008. Making sense: Neapolitan illustrated books in the European republic of letters. In *"Napoli è tutto il mondo": Neapolitan art and culture from humanism to the enlightenment*, 243–269. Pisa: Fabrizio Serra Editore.

Pedretti, Carlo. 1964. *Leonardo da Vinci on painting: A lost book (Libro A)*. Berkeley: University of California Press.

Perini, Giovanna. 2000. La biblioteca di Bellori: saggio sulla struttura intellettuale e culturale di un erudito del Seicento. In *L'idea del bello. Viaggio per Roma nel Seicento con Giovan Pietro Bellori*, vol. 2, 673–685. Rome.

Pomian, Krzysztof. 1990. *Collectors and curiosities: Paris and Venice. 1500–1800* (trans: Wiles-Portier, Elizabeth). London: Polity.

Porzio, Francesco (ed.). 1989. *La natura morta in Italia*. Milan: Electa. 2 vols.

Purcell, Rosamond Wolff, and Stephen Jay Gould. 1992. *Finders, keepers: Eight collectors*. New York: W. W. Norton.

Rappaport, Rhoda. 1997. *When geologists were historians*. Ithaca: Cornell University Press.

Ray, John. 1718. *Philosophical letters between the late learned Mr. Ray and several of his ingenious correspondents, natives and foreigners*. London: William and John Innys.

Ray, John. 1848. In *The correspondence of John Ray*, ed. Edwin Lankester. London: The Ray Society.

Reeves, Eileen. 1997. *Painting the heavens: Art and science in the age of Galileo*. Princeton: Princeton University Press.

Rosand, David. 2002. *Drawing acts: Studies in graphic expression and representation*. Cambridge: Cambridge University Press.

Rossi, Paolo. 1984. *The dark abyss of time: The history of the earth and the history of the nations from Hooke to Vico* (trans: Cochrane, Lydia G.). Chicago: University of Chicago Press.

Rousseau, Theodore. 1962. Aristotle contemplating the bust of homer. *The Metropolitan Museum of Art Bulletin* 20: 149–156.

Rudwick, Martin. 1976a. *The meaning of fossils: Episodes in the history of paleontology*, 2nd ed. Chicago: University of Chicago Press.

Rudwick, Martin. 1976b. The emergence of a visual language for geological science 1760–1840. *History of Science* 14: 149–195.

Ruffo, Vincenzo. 1916. La galleria Ruffo nel secolo XVII in Messina. *Bollettino d'Arte* 10: 21–388.

Schnapp, Alain. 1997. *The discovery of the past* (trans: Kinnes, Ian and Varndell, Gillian). New York: Abrams.

Scilla, Agostino. 1670. *La vana speculazione disingannata dal senso*. Naples.

Scilla, Agostino. 1996. In *La vana speculazione disingannata dal senso*, ed. Marco Segala. Florence: Giunti.

Scott, Andrew C. 2001. Federico Cesi and his field studies on the origins of fossils between 1610 and 1630. *Endeavour* 25: 93–103.

Scott, Andrew C. and David Freedberg. 2000. *Fossil woods and other geological specimens*. With Documentation Provided by Francesco Solinas and Contributions by Jo Taylor. (The Paper Museum of Cassiano dal Pozzo. Series B, Natural History; part 3). Turnhout: Harvey Miller.

Skippon, Philip. 1732. An account of a journey made thro' part of the low-countries, Germany, Italy and France. In *A collection of voyages and travels*, vol. 6, 361–736. London.

Smith, Pamela H. 2004. *The body of the artisan: Art and experience in the scientific revolution*. Chicago: University of Chicago Press.

Snorrason, Egil. 1986. Nicolaus Steno, the illustrative expositor. In *Nicolaus Steno 1638-1686: A re-consideration by Danish scientists*, 191–206. Gentofte: Nordisk Insulinlaboratorium.

Solinas, Francesco (ed.). 1989. *Cassiano dal Pozzo: atti del seminario internazionale di studi*. Rome: De Luca.

Solinas, Francesco. 2009. Osservazione della natura e 'pittura filosofica' nella Roma dei primi Lincei. In *Il cannocchiale e il pennello. Nuova scienza e nuova arte nell'età di Galileo*, 225–239. Florence: Giunti.

Stelluti, Francesco. 1637. *Trattato del legno fossile minerale*. Rome: Vitale Mascardi.

Sutherland Harris, Ann. 1977. *Andrea Sacchi*. Princeton: Princeton University Press.

Swan, Claudia. 1995. *Ad vivum, near het leven*, From the life: Considerations on a mode of representation. *Word and Image* 11: 353–372.

Tesauro, Emanuele. 1654. *Il canocchiale aristotelico*. Turin.

Tognoni, Federico. 2005. Nature described: Fabio Colonna and natural history illustration. *Nuncius* 20: 347–370.

Trabucco, Oreste. 1996. "La corrispondenza tra Pietro Castelli e Marco Aurelio Severino." In *Filosofia e scienze nella Sicilia dei secoli XVI e XVII*, ed. Corrado Dollo, 109–131. Catania: Centro di Studi per la Storia della Filosofia in Sicilia, Università di Catania.

Trauman Steinitz, Kate. 1958. *Leonardo da Vinci's Trattato della Pittura: A bibliography of the printed editions 1651–1956.* Copenhagen: Munksgaard.

Tribby, Jay. 1991. Cooking (with) Clio and Cleo: Eloquence and experiment in seventeenth-century Florence. *Journal of the History of Ideas* 52: 417–439.

Trinci, Manuela. 1988. L'occhio, l'occhialino e la vista di Agostino Scilla. *Il piccolo Hans* 57: 123–146.

Vallisneri, Antonio. 1721. *De' corpi marini che su' monti si trovano*. Venice: Per D. Lovisa.

Viviani, Viviani. 1992. *Vita di Galileo*, ed. Luciana Borsetto. Bergamo: Moretti & Vitali.

Winkler, Mary, and Albert Van Helden. 1992. Representing the heavens: Galileo and visual astronomy. *Isis* 83: 195–217.

Yamada, Toshihiro. 2006. Kircher and Steno on the "geocosm", with a reassessment of the role of Gassendi's works. *Geological Society of America Special Papers* 411: 65–80.

Chapter 7
What Exactly Was Torricelli's "Barometer?"

J.B. Shank

Abstract What exactly was Evangelista Torricelli up to when, in 1644, he took mercury filled tubes and turned them upside down in other mercury filled tubs? A canonical story anchored by the larger historiographical edifice of the "Scientific Revolution" holds that Torricelli was using a new instrumental experimental approach to natural philosophy to demonstrate and measure the weight of atmospheric air. Within this frame, Torricelli's mercury *esperienza* as he called it heralds the birth of modern experimental science through his invention of the first modern barometer. This paper questions this traditional narrative by re-interpreting the archival foundations upon which this account is based. In particular, it stresses the gaps between the rather slim documentary record of Torricelli's work and the canonical interpretation of it built by the "Scientific Revolution" literature. It also uses other documentary contexts available in the same archive to give a different, though insistently historical, account of Torricelli's motivations for and conceptualization of his labors. The paper does not conclude by claiming a new and definitive revisionist interpretation of Torricelli's mercury *esperienza*, but by suggesting instead that the emphatically historical but also interpretively flexible approach to Torricelli's work pursued in this paper exemplifies a "Baroque" approach to the history of science, one that can be useful in generating fresh interpretive insights about the early history of modern science.

J.B. Shank (✉)
Department of History, University of Minnesota, Minneapolis, MN, USA
e-mail: jbshank@umn.edu

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_7,

Introduction

On 11 June 1644, Evangelista Torricelli, the Mathematician and (perhaps) Philosopher of Grand Duke Ferdinando II de Medici, wrote a letter to his good friend Michelangelo Ricci in Rome describing an *esperienza* he performed at some earlier point (Torricelli 1919–1944, 3:186–188). The Museo Galileo in Florence has an exhibit devoted to this event, and it describes the *esperienza* this way: "Torricelli filled a glass tube, open at one end, with mercury. Then, closing off the open end with a finger, he tipped the tube upside down and lowered it into a basin containing more mercury. He observed that the column of mercury descended only partially, stopping at a height of around 76 cm. Torricelli was convinced that the space created by the descent of the mercury in the tube was empty, and that the force holding up the column consisted of the pressure exerted by the air on the mercury in the basin. … The results of the mercury experiment opened a period of revolutionary change, forcing a re-examination of doctrines accepted for centuries." (Torricelli's Barometric Experiment)

This narration, especially the last sentence, echoes the understanding of these events that has become canonical in the larger history of the "Scientific Revolution." In fact, in the collection of statements that constitute the creed of this historical paradigm, few are more integral than those that declare Torricelli to have been "the inventor of the barometer," and those that make the invention of this device one of the singular (perhaps even *the* singular) moments in the revolutionary seventeenth-century birth of modern experimental science. Torricelli's name was tied to the heroic understanding of his *esperienza* of 1644 fairly quickly. Less than 20 years after the letter announcing the result to Ricci was written, Carlo Dati was already able to articulate what would become a widely used celebration in describing it as Torricelli's "*famosissima esperienza dell'argento vivo*" (Dati 1663). Modern scholarship has essentially ratified this three centuries old tradition of canonization, and anchoring this understanding is one the masterpieces of what H. Floris Cohen calls the "Great Tradition" of Scientific Revolution historiography: W. E. Knowles Middleton's magisterial 1964 *The History of the Barometer* (Floris Cohen 1994; Knowles Middleton 1964). In a set of carefully researched and argued opening chapters that draw upon the equally meticulous documentary research of Cornelis De Waard (1936), Knowles Middleton concludes that Torricelli's mercury *esperienza* deserves to be considered the invention of the modern barometer. Defining the word barometer "rather strictly as meaning any instrument, no matter how it works, used for measuring the pressure of the atmosphere," Knowles Middleton argues that in June 1644 Torricelli "decided, on what evidence we do not know, that the weight of the column of air [pressing on the mercury] varies." He then declared this understanding to Ricci, saying that, "I had wanted to make an instrument (*fare un strumento*) to show this variation." For Knowles Middleton, these statements by Torricelli to Ricci about the mercury *esperienza* constitute "real and sufficient reason for ascribing the invention of the barometer to Torricelli" (Knowles Middleton 1964, x, 29).

My project in this paper is to scrutinize this assessment and to ask anew the question of what exactly Torricelli's letter to Ricci teaches us about his work and its place in seventeenth-century thought. My analysis will part company with Knowles Middleton and the "Great Tradition" of Scientific Revolution scholarship (De Waard 1936; Knowles Middleton 1964; Rougier 2010) through its refusal to permit the imagined idea of a new experimental science coming to life *sui generis* in seventeenth-century Europe, and especially in post-Galileian Tuscany, to serve as the master hermeneutic driving my interpretation. In this respect, my argument dovetails with the recent work of Luciano Boschiero, whose research in the very same archive – seventeenth-century Tuscany – compels a reconsideration of the alleged birth of modern, instrumentalist, experimental science in the activities of the Galileo- and Torricelli-inspired *Accademia del Cimento* after 1657 (Boschiero 2007). My argument also joins with the recent scholarship on seventeenth-century experimental science, particularly the work of Peter Dear and Steven Shapin, that complicates the older narrative of instrumental experimentalism's triumphant arrival in Europe in the decades after 1600 (Dear 1995; Shapin 1994).

Sharing the revisionist impulses of this recent scholarship, I begin by recognizing that Torricelli's letter to Ricci, and the *esperienza* that it narrates, is not a self-declaring beacon of a new and emergent experimental approach to science coming to life in seventeenth-century Europe. The letter is rather a single document, one capable of many readings, but one that has remained trapped for over three centuries in one particular interpretive tradition. In this tradition, Torricelli's letter is made into the herald of a monumental arrival: the birth of modern instrument-based experimental physics. Yet nothing in the letter or its surrounding context forces us to interpret the document in these terms. Likewise, no error occurs if we instead place this letter within other interpretive frames, and view it through the lenses of other contemporary documents. Such a re-contextualization is what I propose to offer here. By giving Torricelli's letter to Ricci, and the *strumento*-produced *esperienza* that it relates, a different, but equally meticulous historical interpretation, I hope to challenge the interpretation that makes this letter revelatory of a new and modern instrumental approach to experimental science. This new interpretation of Torricelli's work is also "Baroque," I contend, not because it reveals a latent Baroque spirit manifest in Torricelli's science (although the suggestion is made that it might), but because it shows the value of Baroque interpretive maneuvers for freeing the history of science from its classical interpretive frameworks.

"Torricelli's Barometer:" The Extant Sources

To begin, let us be very precise about the documents we have, and careful in reporting the material that they contain regarding the events of June 1644. At the center of things, of course, is Torricelli's June 11 letter to Ricci announcing the *esperienza*, a letter that constitutes one of the few documentary traces that Torricelli left behind at the end of his brief – he died just after his 39th birthday – but intellectually prolific life. Like almost every other document that Torricelli bequeathed to posterity, this letter is found in the five small volumes of his collected *Opere,* virtually the only written documents available to historians seeking to understand his scientific work (Torricelli 1919–1944). With respect to his work on the so-called barometer, this small archive proves to be even smaller since the documents found here tell us very little about the nature of Torricelli's work or its genesis. The 1644 letters with Ricci in fact constitute the only direct reference to this work, and when viewed in light of these documentary realities what is striking is the very small archival foundation anchoring the larger history of Torricelli's monumental invention. Indeed, in many respects it is built solely on the interpretation of the 11 June 1644 letter alone.

Regarding the *esperienza* itself*,* we have, besides the famous letter, a letter from Ricci to Torricelli, dated the very same day in Rome, claiming great impatience "to hear about the success of those *esperimenti* that your Excellency hinted to me about (*di sentire il successo di cotesti esperimenti accennatomi da V.S.*)" (Torricelli 1919–1944, 3:189). Most have read Ricci's longing as a desire to learn about the mercury experiment, yet none of Torricelli's previous letters to Ricci (he was one his most frequent correspondents) mention that he is engaged in such a project. No other record suggesting how Ricci might have learned of Torricelli's intentions has been discovered either. Whatever he knew about the *esperienza* before receiving Torricelli's June 11 letter, Ricci responded quickly to the news of it. On June 18, he replied to Torricelli raising three objections against what he called "the *esperienza* made in proof of the vacuum (*l'esperienza fatte in riprova del vacuo*)" (Torricelli 1919–1944, 3:193). Torricelli replied in turn on June 28, and that was the end of the exchange as far as Torricelli and Ricci were concerned (Torricelli 1919–1944, 3:198–201). The correspondence of these two friends and scientific colleagues had included discussions of many matters before June 1644, and geometry occupied their attention in these early letters far more than empirical or instrumental *esperimenti*. After June 1644 this familiar conversation resumed, and no more mention of the mercury experiment was made even though the two men exchanged 53 more letters before Torricelli died in October 1647.

Knowles Middleton spends much time puzzling over this documentary silence regarding the Torricellian *esperienza*, an apparent non-reception that is further confirmed by De Waard's research regarding the immediate impact of Torricelli's letter in Italy (Knowles Middleton 1964; De Waard 1936). As Knowles Middleton quips, "in the twentieth century, medals are awarded for work of less relative importance," yet in Italy in 1644 little interest appears to have been sparked by Torricelli's demonstration (Knowles Middleton 1964, 19). How should we understand this silence, one that still leaves the 1644 *esperienza* engulfed in a textual darkness despite vigorous research efforts to remove it? Knowles Middleton suggests that the silence was intentional, rooted in a conspiracy led by savants and their political patrons to keep news of the experiment a secret. The reason for the secrecy, he claims, was fear that the demonstration might raise the hackles of the clerical authorities and trigger a repression akin to the one that had been leveled upon Galileo 10 years earlier. As he sums up his judgment:

> Now while it is clear that he was busy with his mathematical researches, it is surely impossible to believe that Torricelli … immediately lost all interest in an experiment which he must have felt to be important, and which must have suggested many others, as it did to many people all over Europe in the next two decades. Nor is it likely that Ricci would have refrained from discussing it further with his many friends in Rome, except for one consideration which outweighed all others. This consideration was prudence. We have to remember that all of those involved were in a sense disciples of the great Galileo; they would be expected to have clearly in mind what had happened to him at the hands of the Holy Office in 1633; more, their lively imaginations could foresee that what happened to their famous master would be mild (he was in fact treated with surprising consideration) compared to what might happen to them. The idea of a vacuum was anathema to the Church, and in Italy the Church was, at the moment, almost omnipotent. Better to let the whole thing drop (Knowles Middleton 1964, 31).

I cite this statement at length because one still reads assessments like this of the private motivations driving the work of Italian savants after 1633. Especially powerful is the idea that the condemnation of Galileo dropped a sopping wet blanket over the sparkling fire that was the Galileian scientific movement. Certainly the Church was a powerful force in seventeenth-century Italy, and no one should doubt that its agendas influenced the science that was pursued there. Clerics were also important players in the scientific culture of the period, and it is appropriate when examining the role of clerics in the scientific debates of the time to consider their religious calling and their ecclesiastico-political concerns when assessing their influence. Yet all that being said, there is not one shred of direct documentary evidence supporting the view that fear of religious sanctions or concerns about doctrinal orthodoxy played any role whatsoever in the history of the Torricellian *esperienza* or its reception. Recent scholarship also suggests other interpretations of these events, and without clear documentary evidence to support the claim we should be suspicious of the idea that the actors did what they did because of a fear that the Catholic Church was poised to suppress advocates of provocative natural philosophy.

In the next section of this paper, I will present arguments based on the extant documentary evidence suggesting a different understanding of Torricelli's cognitive agenda with respect to his *strumento* and *esperienza.* I will also offer a different interpretation of why it might have been received the way it was in Italy in 1644. But before turning to that discussion, let me complete this inventory of the documentary record so as to fully illuminate the documentary foundations of this history.

Moving beyond Torricelli's letters with Ricci, the next step in the story of the *esperienza*'s reception occurs in Rome, where the original letters began to circulate more broadly as soon as they were received. Among those interested in what the letters claimed to show were several clerics, including the Jesuits Athanasius Kircher and Nicolas Zucchi. Kircher and Zucchi each reported, after the fact, and at a time when claiming to be such a witness would have had more significance, that after learning of the *esperienza* from Ricci, they were given the honor of seeing a performance of it in Rome at the invitation of Cardinal Giovanni Carlo de Medici, the Grand Duke's brother. This event was said to have occured in February 1645. Who attended this demonstration, or what the circumstances of its staging were, are unknown save for three retrospective reports, two by Kircher and Zucchi, and a third by a French cleric, Emmanuel Maignan, who was also in Rome at the time (Kircher 1650, 11; Zucchi 1648, 4; Maignan 1653, 1897). Efforts to turn up any further contemporary documentation about this event have so far resulted in nothing (De Waard 1936, 170–178; Knowles Middleton 1964, 33–34).

Another of Ricci's contacts in Rome was François Du Verdus, a French envoy in the city who enjoyed the company of learned men. Du Verdus was already in correspondence with Torricelli from as early as April 1644 (Torricelli 1919–1944, 3:172), and when Ricci showed him Torricelli's letters in Rome Du Verdus wrote to Torricelli in Florence, likely in July 1644 (the precise month is not noted on the letter), asking him for more information about the *esperienza* (Torricelli 1919–1944, 3: 210–212). No response from Torricelli survives, yet based on the documents he already possessed Du Verdus produced an abridgment of the entire exchange between Torricelli and Ricci and sent it to Marin Mersenne in Paris (Mersenne 1977, 13:177–183). Knowles Middleton rightly devotes considerable attention to this missive from De Verdus since it was this abridged synopsis of the Torricelli-Ricci exchange, and not the original letters themselves, that circulated in France and transformed the 1644 Florentine *esperienza* into an international phenomenon. As Knowles Middleton sums up: "Mersenne got enough [from Du Verdus] to know how to perform the experiment and also to realize that Torricelli explained [the variation of the mercury in the tube] by means of the pressure of the air, but there were two things that he did not get: Torricelli's thoughts about the facility with which a vacuum can be reproduced, and the statement of his 'chief intention' to make an instrument to show the changes in the pressure of the air." Mersenne also received only one of the three challenges to the experiment that Ricci offered. In short, he got neither Torricelli's experiment, nor even Torricelli's own narration of it, but an after the fact synopsis written by someone who had only seen Torricelli's letters. Du Verdus also clipped from his account two aspects of the experimental narration that might be among its most important features (Knowles Middleton 1964, 35–36).

It is not clear when Mersenne received Du Verdus' letter, and while he was already in correspondence with Torricelli before this date (their first letters passed in August 1643), their epistolary conversations did not include talk of the mercury *esperienza*. Mersenne began a series of travels in Italy in October 1644, and in December he visited Torricelli in Florence. No documentary evidence of their encounter survives, but the Minim friar later – again the retrospective character of the report is significant – reported seeing Torricelli perform the *esperienza* with the glass tube during his visit. The context for this retrospective narration was a priority dispute about the rightful innovator of the experiment that erupted in France in early 1647. Mersenne's comments, as their precise language makes clear, were a contribution to that priority controversy. As he wrote, "it is certain that the vacuum was observed with a glass tube in Italy before it was in France, by, I believe, the illustrious geometer Evangelista Torricelli, who showed me the tube in the year 1644 in the admirable schools of the Grand Duchy of Tuscany. Moreover we were first informed about this observation at Rome by his particular friend Michelangelo Ricci, the distinguished ornament of the whole Academy of Geometry" (Mersenne 1647, III: 216; Knowles Middleton 1964, 37).

The reference to Ricci is a reference to the letter from Du Verdus, and it is worth noting that while Mersenne continued his travels in 1644 by going from Florence to Rome, and while he was in the city in February 1645, and was in contact with several of the people who claimed to have witnessed the *esperienza* staged by Cardinal Giovanni de Medici during that month, he never mentioned seeing or hearing about it there. Mersenne did, however, attempt, unsuccessfully it turned out, to perform the experiment with the Swedish Ambassador after his return to Paris in July 1645 (Thirion 1907–1908). He also served as an important conduit for news about the experiment throughout France. His circulation of the Du Verdus synopsis in fact led to the first successful reproduction of the experiment in France by a military engineer at Rouen named Pierre Petit. This occurred in November 1646. Petit knew Blaise Pascal, and consulted with him in accomplishing his work. When Petit's letter to Mersenne announcing the result was published as a pamphlet in late 1647, together with a rival account of the *esperienza* produced at King Wencelaus's court in Poland the same year, Pascal became a figure in this history as well (Petit 1647; Knowles Middleton 1964, 37). He was at the center of the priority controversy alluded to above that erupted in the wake of Petit's pamphlet. All the well-known controversies in France about the mercury instrument and the nature of the vacuum it was alleged to show, including Pascal's famous test of the mercury *esperienza* at the top of Puy de Dome, also occurred after November 1646 (Mazauric 1998). Meanwhile, Torricelli remained pre-occupied with other matters, and when he fell ill and died on 27 October 1647 he left the earth without having played any role whatsoever (or at least no textually documented role) in the subsequent history of his mercury *esperienza*.

One final wrinkle in this documentary story involves a letter to Mersenne written in March 1648 from Rafaello Magiotti, another of Torricelli's regular correspondents. Magiotti was a member of the Roman intellectual circles that included Ricci, Du Verdus, Kircher, and Zucchi, and while the conversation, epistolary or otherwise, that prompted his 1648 letter does not survive, the relevant passages in it make clear that Magiotti was responding to Mersenne's inquiries, perhaps triggered by the priority controversies erupting in France, "regarding the history of the quicksilver (*quanto all'istoria dell'argento vivo*)" (Mersenne 1977, 16:168–171). What the letter contained was not an account of the mercury *esperienza,* but a report of a different though related experiment performed sometime between 1640 and 1643 (all the surviving reports are retrospective and imprecise about dates) by a well-respected and well-connected Roman savant named Gaspar Berti. Berti's *esperimento* used water, rather than mercury, to produce an empty space in the top of a glass tube immersed in a vessel, and while Torricelli's quicksilver version of this *esperienza* had received a quiet reception at first, Berti's was relegated to utter oblivion until it was resurrected after 1648 (De Waard 1936, 101–110). Those who witnessed this experiment began to report what they saw after this date, and the reports included one from Maignan, who published his account in 1653, and one each from the two Jesuits noted earlier, Kircher and Zuchi, published in 1650 and 1649 respectively (Maignan 1653; Kircher 1650; Zucchi 1649). Magiotti's report to Mersenne, written in 1648, further linked Torricelli and Galileo with Berti's work, suggesting that perhaps this early water experiment was a precursor to, and even an inspiration for, Torricelli's later work with mercury (Knowles Middleton 1964, 9–18). I will have more to say about Berti's water experiment in the next part of this paper, and I only introduce it here so as to show its connection to the documentary reception of Torricelli's *esperienza* of 1644.

What themes should we draw from this complicated, if exceedingly finite, textual history? One point to emphasize is the mediated distance that separates Torricelli's actual *esperienza* from its reception as a Torricellian experimental event. Also worth stressing is the impossibility, given this mediated distance, of obtaining anything like eyewitness testimony in this case. If one remains strictly attached to the archival record, one must concede that we have no direct account of Torricelli's work at all, only a narrative report, offered after the fact as one episode in an exchange of three letters circulated as part of a broader epistolary commerce that was pursuing many different discursive and intellectual agendas simultaneously. We also have a document that narrates an event which did not seem to generate much response or interest at first, including, and especially, between the two central protagonists, Torricelli and Ricci. We also have an event that did not immediately trigger the excitement and controversies that it would later provoke, and one that only became broadly interesting to the learned public years later after the mediated reports contained in Torricelli's and Ricci's letters had been further translated into abridged summations (the Du Verdus letter to Mersenne) that were shaped by different rhetorical imperatives and epistolary agendas. We also have a case of geographical circulation, where an *esperienza* performed and textually inscribed in Florence receives its first real reception in a set of textual encounters and experiential re-creations in Rome, and then only later becomes a fully established and named "Torricellian" event in France after the textual document that takes Torricelli's *esperienza* north of the Alps (Du Verdus' letter to Mersenne) is joined there with further experimental recreations and experimental reports from Rouen and Warsaw, and a dispute about its authorship. We finally have the later solidification of the 1644 events into the authentic "*istoria della famosissima esperienza dell'argento di Torricelli"* by the members of the Cimento starting in the 1650s, and then by Carlo Dati in the next decade in his eponymous text articulating the Cimento's position (Dati 1663). The process is then completed by the twentieth-century re-canonization of the Cimento's understanding by historians such as De Waard, Knowles Middleton, Rougier, and others, and the incorporation of this interpretation into the foundation of the modern historical paradigm of the Scientific Revolution. In this way, Torricelli's *esperienza* becomes a pillar supporting the general claim that the birth of modern instrumental experimental science is to be found in the work of Galileo and his followers, and that Torricelli's alleged 1644 invention of the "barometer" marks the apotheosis of this development.

At this point, we should recall again that the actual archival document that anchors this entire historiographical edifice is a single exchange of three letters between two seventeenth-century learned men. We should also remember that the inventor of the original *esperienza,* and the author of the letter that introduced it to the world, was dead before much of the canonization that would make him famous had really gotten underway. This same author also left behind not a single piece of textual evidence attesting to his participation in any of these later developments, nor any document that would indicate how he would have perceived this interpretation of his work had he lived to see it. At the same time, this author did leave behind other evidence – the Torricelli archive is thin, but not completely empty – and this material suggests that the retrospective understanding of his intentions and accomplishments as moments in the birth of modern instrumental, experimental science may not fit well with what he actually had in mind when he first started placing mercury-filled tubes in tubs and writing about what he saw. A re-reading and reinterpretation of the archive related to Torricelli's *esperienza* might open a pathway, therefore, toward a new historical understanding of Torricelli's work and the seventeenth-century Tuscan context that shaped it. Pursuing this reinterpretation is precisely the goal of the rest of this paper.

Rethinking Torricelli's Esperienza of 1644

The place to begin is upon the only firm foundation we have – Torricelli's letter to Ricci in June 1644. Working from this document and others around it in time, what can we discern about the letter's author, his work, and the agendas and motivations that may have inspired it? Knowles Middleton stresses rightly that good history grows from a rigorous attention to terminology, and especially from the elimination of modern terminological understandings that were unavailable to the historical actors we study. Terminology offers a useful entry into this case as well since the distinction between *esperienza* and *esperimenti* in the original exchange between Torricelli and Ricci offers a fruitful starting point for thinking about this history. When Ricci writes claiming to live in great anticipation of news about Torricelli's work, he uses the word *esperimenti* to describe what he is waiting to read about. Yet Torricelli, as far as my research has discerned, never uses that word in his letters, or, for that matter, in any of his other writings about empirical matters. *Esperienza* is his preferred term for all observed empirical phenomena, including those that are instrumentally produced. Assuming that this discursive tendency is in fact true of all of Torricelli's writings (more research needs to be done to confirm the point), then we have a case where our documents are marking out an important conceptual tension that is well articulated within the history of science literature.

As Peter Dear has especially shown, the seventeenth-century witnessed a transformation in the conceptual terrain captured by the Italian words *esperienza* and *esperimenti,* or, to use their English correlates, between "experience" and "experiment." (Dear 1995) In French, the single word "experience" was retained to capture both of these conceptions, and this lexicographic fact reveals the origin of each term as a reference to the same thing, namely the complex of knowledge that derives from embodied sensation. In Italian and English, however, a distinction between "experience" and "experiment" began to emerge in the seventeenth century, with experience suggesting the natural complex of sensory observations received by an embodied human subject, and then a reception of that empirical complex through the unregulated sensory channels of the human body. Experiment connoted all this as well, but it emerged as a distinct word after 1600 through an emphasis upon the intervention of instruments and other controlling devices into this natural process of sensation. Also peculiar to experiment was the suggestion that the disciplined regulation of experience by artificial, instrumental means was an appropriate means toward knowledge.

The 1612 edition of *Il Vocabolario*, a dictionary of vernacular Tuscan Italian produced by the Florentine Accademia della Crusca, articulates the common base of the two words and their emerging distinction nicely. Both Galileo and Torricelli were members of the Crusca, and while neither participated in the production of the academy's dictionary, the definitions contained in it reflect the discourse favored by learned Italians like them. *Il Vocabolario,* therefore, constitutes a rich lexicographic archive documenting the changing semantic field of learned Italian discourse, including scientific discourse, in the period (Sessa 1991).

Both "esperienza" and "esperimento" are defined in *Il Vocabolario* as knowledge of a thing acquired through an activity with it. The definitions stress the connection between experience/experiment and empirical observation, and each definition links embodied sensation with the inductive acquisition of knowledge. But whereas "esperienza" is "acquired from the particular activity (*acquistato mediante l' uso particolare*)," "esperimento" refers to knowledge achieved "by means of the use, by making attempts (*conoscer per mezzo dell' uso, far pruova*)." This added instrumental and manipulative dimension to "esperimento" distinguishing it from a simple "esperienza" is the key marker of the new understanding of experiment emergent at the time (Il Vocabolario 1612).

As Dear and others have discussed, the distinction between "experience" and "experiment" is born in the seventeenth century as part of a new epistemology of instrumental experimental science. (Dear 1995) In this new epistemology, instrumentally produced experiences, or experiments, are performed, articulated, and defended as valid representations of nature suitable for sustaining natural philosophical claims despite their contrived and artificial character. In time, the artificiality of experimentally produced experience will even become a marker of epistemological superiority, a sign that experience is linked to objective knowledge, and not simply to subjective opinion (Daston and Galison 2007). This new experimental epistemology, a cornerstone of modern science, did not arrive in one single revolutionary transformation, however. *Il Vocabolario* attests to this crooked development by recording both the emerging shift and its contested and historically contingent character in its entries on "esperienza" and "esperimento." Each entry ends with a cross-reference to a word that is not given its own entry in the dictionary, but which is offered as a neologism then in use in spoken Italian. As the entry on "esperimento" states, "we also call [*esperimento*] *cimento*." The entry on "esperienza" includes the same word: "we also use *esperienza* to signify *cimento* and *prova* (*l' usiamo anche in significato di Cimento, e di Prova.*)" The entry for "provare" completes the circle, for this term is defined as "to make a proof, *cimentare, esperimentare*, to make an attempt (*far pruova, cimentare, esperimentare, far saggio*)." Supplementary definitions further explain that *provare* means "to feel by proof, and to know the causes (*per sentir per pruova, e conoscere, essendo in causa*)" and "to confirm, to show with reason and authority (*per confermare, mostrar con ragioni, e autorità*)." A citation to Boccaccio's use of the phrase "provando, e riprovando," "try and try again," further illustrates the definition. This accidental use by the Cruscan lexicographers in 1612 of the future motto of the not accidently named *Accademia del Cimento*, the institution that after 1657 would further clarify and solidify this semantic field by building a specifically institutionalized program of experimental science upon it, illuminates the changes that were afoot in these decades in Tuscany (Il Vocabolario 1612).

When Ricci queries Torricelli about his *esperimenti,* he may, therefore, be indicating that he saw Torricelli's work in the new terms that Dear describes. However, since Torricelli never seems to speak about *esperimenti,* preferring instead to reference his *esperienze,* the question is whether he is using this term in the old sense, the new sense, or some perhaps uneasy and idiosyncratic hybrid of each? Or do Ricci and Torricelli view the terms as synonyms, indicating in their different use no distinction that warrants analytical scrutiny? Boschiero adds support to the latter view since he shows the complexities attendant to the Cimento's alleged inauguration of instrumental experimentalism in 1657. (Boschiero 2007) The traditional narrative of the Galilean experimental revolution makes the work of the Cimento a climactic institutionalization of the pioneering steps toward instrumental experimentalism initiated by Galileo and Torricelli decades earlier. But Boschiero persuasively challenges this story, and this revision should lead us to inquire further into what Torricelli is in fact up to when he stages *esperienze* with *strumenti* and then uses the phenomena so produced to draw broader conclusions.

Torricelli's letters make clear that natural philosophical theorizing is central to his motivation in producing his result. The project of the mercury *esperienza* is primarily, if not exclusively, directed toward producing a visual demonstration of the existence of the vacuum in nature, Torricelli says. He also stresses when describing his agenda that among his greatest accomplishments is his demonstration of both the presence of the vacuum in the tube, and the easy production of this alleged abhorrence through the manipulations of his *strumento*. These are ultimately natural philosophical claims, and not, as Boschiero is right to stress in a distinction that I will insist upon as well, demonstrations of empirical matters of fact as described by the historians of the new instrumental, experimental science. The history of the immediate reception of Torricelli's work also shows that he is not alone in conceiving of his work within a traditional experiential conception of natural science, albeit one comfortable with instrumental means for pursuing it. What attracts attention in Torricelli's work is the empirical evidence it offers for the existence of the vacuum, not its claim to measure natural effects. And when the event described in Torricelli's letter of July 1644 is described to others, it is usually called "the demonstration" or the "experience" regarding the vacuum. Remember, for example, that De Verdus saw no problem excising Torricelli's statements about measuring the variable weight of the air in his abridged summation of the letter that he sent to Mersenne. The precise terminology used in these descriptions suggests that the cognitive frame operative in these exchanges is one governed by traditional practices of experientially informed natural philosophy.

Knowles Middleton, who is well attuned to these historical complexities, is nevertheless persuaded that Torricelli sees his mercury tube as a new kind of modern scientific *strumento*, one conceived to perform quantitatively measured experimental tests of natural phenomena. Since his argument rests on Torricelli's own description of his device and its results, it cannot be dismissed so quickly. Torricelli makes clear in his letter to Ricci that he sees the weight of the air as the cause for the particular effect observable in the tube, and he uses the language of quantitative measurement to describe his *strumento* as a device capable of showing this variation (Torricelli 1919–1944, 3: 186–188). For Knowles Middleton, this is the precise reason to call Torricelli the inventor of the modern scientific instrument now called the barometer even though Torricelli admitted failure in his attempt to actually demonstrate the precise quantitative variation at issue with his device (Knowles Middleton 1964, 22–29).

For me, the failure of the test is less crucial than the conceptualization of it that Knowles Middleton asserts. For I am not convinced that this letter *must be* read as showing Torricelli to be a performer of instrumentally conceived experimental tests. Other contemporary contextual evidence available in the Torricelli archive also supports giving these passages a different reading. To see my point, one must distinguish, and again the precision here is crucial, between an instrumental experimental test, one where issues of precision measurement and trial and error testing are paramount, and an experimental or experiential demonstration that simply wants to establish visual proof for a natural effect. Torricelli, and the Galileians more generally, were vigorous practitioners of the second, but they were not, I contend, really concerned with the first.

In the case of Torricelli, we have textual evidence supporting this view, namely his description of his demonstration with the mercury as a "philosophical *esperienza* regarding the vacuum (*esperienza filosofica intorno al vacuo*)," and not as an *esperimento* regarding the weight of the air. The key line that Knowles Middleton builds his case upon, where Torricelli says that his goal is not simply to show a vacuum, but to "to make a *strumento* that shows the mutation of the air from more heavy and dense to more light and subtle (*far uno strumento che mostrasse le mutatzioni dell'aria, hora piu grave e grossa, et hor più leggera e sottile*)," is also less obviously a declaration of instrumental experimental intent than Knowles Middleton claims it to be. For one, Torricelli explains in the same letter why this second agenda is important, saying that he showed the connection to the weight of the air in order to convince philosophers who may need a demonstrative cause for this vacuum before they are persuaded that nature does not in fact abhor it (Torricelli 1919–1944, 3: 186–187). In short, it's the qualitative causal link that matters, not its quantitative measurement. Ricci also speaks to a philosophical reception of the *esperienza* in his response since he opens his letter with invocations of Epicurus and Lucretius on the naturalness of the vacuum, and then addresses his objections toward the validity of the physical demonstration of the vacuum and its alleged non-resistance – a major claim of Torricelli, it will be remembered – and not toward the instrumental measurements of the changing effects of air pressure (Torricelli 1919–1944, 3: 193–194). The last, it will be remembered, was also asserted as only a possibility and was not successfully achieved by Torricelli with quantitative precision. For his part, Ricci showed no concern whatsoever about Torricelli's failure to achieve this result, nor any interest in taking steps to eradicate it, a failure to see a failure in Torricelli's work that Knowles Middleton finds puzzling (Knowles Middleton 1964, 25, 29). Those who later received the report of Torricelli's *esperienza* were even more oriented in this direction – remember that De Verdus removed the measurement claim of the letter altogether – and even if Torricelli had conceived of his *esperienza* as an experimental demonstration proving a systematic correlation between the weight of the air and the height of the mercury in the tube (I do not think he did, at least in the precise sense assumed in modern notions of experimental science), his readers did not extract this lesson from his letter. What they did take from it was a visible natural philosophical provocation.

Boschiero writes that Galileo's: "use of experiments was … subservient to his mathematical, geometrical, and anti-Aristotelian natural philosophical agenda. Its most significant role was as an authoritative tool, used to persuade the reader to refute Aristotelianism and support a mechanical, Archimedean physics" (Boschiero 2007, 34). Torricelli's demonstration with the mercury can be described in similar terms, but with one crucial exception. Unlike Galileo, Torricelli displayed, at least in the documents he left behind, little affection for philosophical sparring in the manner of Galileo. Yes, his mercury *esperienza* was offered as a challenge to philosophers who refused to accept the naturalness of the vacuum. But unlike Galileo, who relished the philosophical disputation that would ensue from empirical demonstrations such as these, Torricelli had other concerns. This is one reason – and only one reason, for we cannot discern from the documents a clear hierarchy of motivations in Torricelli's work – why the mercury experiment died in his writings after it had been articulated and clarified to Ricci. Not generally inclined to fight for natural philosophical positions in the manner of Galileo, and not generally committed, at least to judge by the extant documents, to a major natural philosophical project akin to Galileo's vigorously pursued anti-Aristotelianism, Torricelli appears to have considered his mercury *esperienza* to have been a single, striking demonstration that served its purposes once it was sufficiently displayed and clarified.

Boschiero adds a further important observation when he notes the social dynamics that were often central to the choices that savants like Galileo and Torricelli made with respect to their intellectual commitments (Boschiero 2007, 18–23). The attraction to Archimedean mathematical science and empirical demonstration as an alternative to traditional natural philosophy, a trait often noted as characteristic of the Galileians, was often supported by a socially motivated effort to assert on behalf of mathematical "artisans," or *artefice* to use the Italian term (i.e. engineers, architects, instrument makers, and even painters), a claim to scientific authority in matters of natural knowledge. In its strongest form, this claim challenged the traditional monopoly on such authority held by scholastically educated university professors, and when attempting to anchor this rival authority the princely court often came to serve as an alternative institutional site suitable for securing this rival claim (Biagioli 1993). This “courtly” dimension of experimental science has dominated recent writing about the Cimento, for many Tuscan savants did use the authority provided by the court-academy nexus to establish identities that challenged traditional regimes of scientific authority (Findlen 1993; Tribby 1994, 1991; Galluzzi 1980, 2001). Torricelli, however, provides an interesting complicating case in this regard, because he was a court figure, the named successor to Galileo’s famed court position, in fact, and an explicit follower of Galileo in wedding Archimedean mathematics and mechanics together with instrumentally produced empirical demonstrations to create provocative natural philosophical claims. His mercury *esperienza* is a case in point. Yet from this court-supported position, he did not show an inclination to engage, as Galileo did, in natural philosophical disputation, or to use his court position, in the manner of Biagioli’s argument, to win battles with rival institutional authorities, including those supporting Jesuit or university-based natural philosophy.

Were it not for the way that Galileo is often taken to be the over-determining influence shaping everything in his wake, this difference would hardly need to be argued for. But since Torricelli was not Galileo, and his agendas were not exactly those of his teacher, we can gain insight by trying to find the ways that Galileo did influence Torricelli’s work while also being clear about all the ways that Torricelli might have pursued different and more personal agendas.

The role of Archimedes in the work of each can open the door to this more nuanced understanding. Much has been written about the way that Galileo sought to replace Aristotle with Archimedes as the Antique figure upon which modern natural philosophy should be built (Wallace 1984; Bertoloni Meli 2006). There is also little doubt that Galileo turned this conviction into a series of polemical battles that he waged against the traditional Aristotelian philosophical establishment. Yet while Torricelli was no less a devotee of the Archimedean legacy than Galileo, and while he was no less convinced than his teacher that it was through the Archimedean legacy that modern mathematics should be built, he stressed different aspects of this legacy than Galileo.

One difference concerns Torricelli’s singular devotion to the pure mathematical projects of Archimedean geometry, an interest that was in Galileo’s portfolio as well, but one that was not as pronounced as it was in Torricelli’s work (Segre 1991). Some of Torricelli’s geometry was of a practical and empirical orientation, and this body of work fits well with his identity as a neo-Archimedean theoretical *meccanico* in the tradition of Galileo’s *Discorsi.* Torricelli’s initial initiation into Florentine scientific culture after 1641 was primarily made through this definition of his Galileianism, namely his work to expand the arguments made in Galileo’s *Discorsi*. But Archimedes was also a pure geometrical theorist, and Torricelli is perhaps exceptional among the neo-Archimedean Galileian cohort in Tuscany in his devotion to pure mathematical problems such as those related to curves like the cycloid, a centerpiece of his intellectual work. Among his earliest and most vigorous correspondents, in fact, was Bonaventura Cavalieri, a colleague and fellow Galileian with whom he traded pure mathematical correspondence almost exclusively. Geometry, not mechanics or instrumental experimentalism, also produced Torricelli’s initial reputation in Paris prior to the mercury *esperienza,* and his geometrical work continued to serve, even after the reception of this experiment, as the mainstay of his correspondence with Mersenne (Torricelli 1919–1944; Mancosu and Vailati 1991; De Gandt 1987).

The question that all this pure mathematical work raises is how to connect it with the mechanical and empirical work that was central to Torricelli's work as well. Domenico Bertoloni Meli suggests one answer by arguing that Archimedes' Euclidean approach to geometrical problem solving presented a model of formal geometric demonstration that appealed to those Galileians longing for a systematic rigor that could give mathematics scientific authority (Bertoloni Meli 2006, ch. 3). It is certainly the case that Torricelli and all the Galileians acquired from their master a belief that geometrical demonstration was a powerful form of argument, one that could and should trump other kinds of reasoning. Yet my reading of the sources does not support Bertoloni Meli's claim that what Galileo and the Galileians most longed for was a formal presentation of mechanics as a deductive system. Far more telling of the spirit of Galileians, it seems to me, was the *Discorsi,* with its learned and witty dialogic approach to empirical problems, and its use of mathematics as an ingenious tool for providing insights into, and explanations for, the confusion of empirical phenomena.

Even when Torricelli was pursuing pure mathematics, which is to say mathematics that had no obvious empirical context or referent, one sees the spirit of the *Discorsi* at play in his work. This spirit is manifest, for example, in the way that Torricelli pursued all of his mathematics in terms of precise and concrete problems, evincing no interest in, or concern for, the kind of formal demonstrations that Bertoloni Meli says were central to the Archimedean-Galileian legacy. What one finds in the mathematical papers that Torricelli left behind are a series of ingenious mathematical solutions and *invenzioni,* not the rudiments of a formal system. His work can also be likened to a kind of collection of wondrous objects, or in this case wondrous mathematical creations. In presenting his recent discoveries and inventions to his correspondents, he also framed it on many occasions using the language of Baroque collecting. "Here is a piece of mathematics worthy of your attention," he would often say to correspondents like Ricci or Cavalieri, and if successful his act of display would be returned with praise by his interlocutor describing the work as "marvelous" or "extraordinarily ingenius." At no point, at least as evinced in the letters that Torricelli left behind, were these discussion transformed into discussions of how to further ground mathematics epistemologically, or to build from it into formal deductive systems (Torricelli 1919–1944).

The character of Torricelli's mathematics finds its historical rationale, I think, when it is seen as simply one more vehicle for producing the kind of sparkling and innovative work that he produced in numerous domains simultaneously. The vast majority of Torricelli's and Ricci's correspondence, in fact, was dominated by the exchange of mathematical work in this way. Their letters also read very often as a kind of friendly game of mathematical one-upmanship – what Leon Battista Alberti called in the quattrocento *ludi mattematici,* or "mathematical games" – with each player trying to produce a demonstration or *invenzione* more sparkling than the other (Alberti 1980). They also shared with each other the brilliant finds, and embarrassing failures, of other mathematicians they encountered. Henk Bos helps us to further understand the "geometrical game" that Ricci and Torricelli played by reminding us that geometry in this period was pursued through the fabrication of ingenious geometric constructions (Bos 2001). In its attachment to solutions achieved through rational construction, geometry therefore involved a kind of marvelous making, a fact that Torricelli emphasized in his "Profession in Praise of Mathematics," an oration delivered to the students and faculty at the Studio Fiorentino where he taught courses in mathematics. In his oration, he declared that works of geometry, "show on every page, and in every line, pure truth, through which are discovered in geometrical figures the riches of nature, and theaters of the marvelous" (Torricelli 1919–1944, II: 73). His mathematics also finds coherence when conceived in this manner. Historians have noted the apparent absence of a coherent mathematical program in Torricelli's work (De Gandt 1987), yet this incoherence disappears if one imagines Torricelli pursuing mathematics as a program of brilliant production and collecting, seeking out problems that were either daunting or elusive, and then demonstrating mathematical *sprezzatura* in the remarkable solutions found for them.

Bos encourages us to see these alien and amodern mathematical agendas as authentic historical approaches to the discipline, and if one accepts, as I do, Bos' dictum that past mathematics must be understood in terms of the historical projects of past mathematicians, and then evaluated in terms of the quality control standards that they, not us, imposed upon their work, then it follows that a collectors approach to mathematics may in fact have been the one that Ricci, Torricelli, and other seventeenth-century Italian mathematicians pursued (Bos 2004). This framework also helps to explain Torricelli's mathematical acclaim in the period, for while he was not responsible for any single innovation or advance in mathematical concepts, he was a recognized mathematical star in Italy and throughout Europe. In Paris, for example, his reputation was established in 1643 by the circulation of perhaps his most marvelous mathematical creation, a finite volume cylinder that Torricelli demonstrated was equal in volume to an unbounded and thus infinite hyperbolic solid. Cavalieri spoke for many in calling it "*mervigliosa e stravagante*" (Torricelli 1919–1944, 3: 65), and its reception, as Paolo Mancosu and Enzio Vailati have shown in an insightful article, was dominated by its status as a stupendous and even stupefying mathematical marvel (Mancosu and Vailati 1991).

Torricelli and Ricci exchanged much more ordinary geometrical fare in their many letters to one another, yet the rhetoric and content of their epistolary commerce coheres best if one sees these authors as geometrical collectors and performers, seeking to please and inspire each other through the display of their own mathematical talents. Torricelli's mercury *esperienza* appeared in June 1644 as one more marvelous episode in this continuous flow of mathematical magic, and if one situates the famous June 1644 letters in this context they start to acquire a different character. Instead of a singular epistolary event narrating an experiment of exceptional importance, Torricelli's *strumento,* and the amazing things he is able to show with it, becomes just one more mathematical marvel shared with his friend in his on-going program of brilliant mathematical production and display. Support for this view is also offered by Carlo Dati's retrospective canonization of Torricelli's genius on behalf of the Accademia del Cimento in 1663. In his *Lettera a Filaleti di Timauro Antiate*, Dati joined his account of the "true story of the famous *esperienza* with quicksilver (*vera storia della famosissima esperienza dell'argento vivo*)" with an equally true account of Torricelli's work with the cycloid, a mathematical problem that was derived from Archimedes and embroiled Torricelli in controversies with the Parisian mathematician Gilles Personne de Roberval until his death (Dati 1663). Dati's approach suggests that Torricelli's pure geometrical work on the cycloid and his experimento-mathematical work with mercury-filled tubes was understood to be two parts of a unified whole, a synergy that is occluded when Torricelli is conceived too strongly as a pure mathematician on some days and a new kind of highly disciplined experimental scientist on others.

Dati's approach, I suggest, is more consonant with the assumptions of the period in which Torricelli worked, and looking at things from his perspective we may wonder whether Bertoloni Meli is perhaps suggesting an anachronistic frame of interpretation when he reads the celebration of Archimedean mathematics in seventeenth-century Italy in terms of a longing for rigorous, formal geometrical systems of the kind that would become highly sought after in later centuries. Knowles Middleton might also be missing the actual historical motivations at play when he insists that Torricelli's *invenzione* be understood as the invention of the modern "barometer." The word barometer was never used by Torricelli, it should be remembered, and it only entered Italian in the eighteenth century as an import, it appears, from England (Battaglia 1961, II: 77). The English were the first to coin this term, and according to the *Oxford English Dictionary* it was first used in the *Philosophical Transactions of the Royal Society of London* in 1665. If Torricelli's *strumento* was not, therefore, a modern barometer either in concept or in word, then maybe we need to break away from the retrospective historical gaze that makes this invention the beacon of a newly emergent experimental science. Doing so means re-imagining how the seventeenth-century human beings who produced and contemplated this *esperienza* might have understood it in the absence of our understanding about what later thinkers would say about its significance.

Torricelli's Mercury Esperienza as Baroque Performance

One way to accomplish this reimagining is to return to the connection between science and artful craft that was central to the veneration of Archimedes in seventeenth-century Tuscany. To be "a new Archimedes," which is what Du Verdus called Torricelli in 1644, also referring to him in other letters as "the Archimedes of this century" and the "Archimedes of Florence," (Torricelli 1919–1944, 3: 172, 181, 209) meant being a mathematician and a pursuer of natural scientific explanations. But it also meant being a wondrous maker, one who not only applied mathematics in the solution of practical problems, but one who also did so with a *sprezzatura* worthy of distinction and acclaim. In his "Profession in Praise of Mathematics," Torricelli celebrated Archimedean mechanics in this way, calling it a "scene of the marvelous," and an arena where works of architecture, technology, and machinery were to be valued as much for their stunning intellectual conception as their public utility. Personifying such work was the "Glorioso Archimede," "that *famoso Mecanico* who for centuries has been known for producing more marvels than can be believed." In the battle to defend his native Syracuse, Torricelli recalled, this "single man, aging and defenseless," turned mechanics toward the service of his homeland, and was judged to be "the equivalent of a squadron of Gods" (Torricelli 1919–1944, 2: 72).

Such images of Archimedes supported an image of the *meccanico* in Italy that washed him of his associations with lowly mechanical thought and base manual labor by emphasizing instead his brilliant intellect and marvelously productive *ingegno*. Galileo, according to Biagioli, effected a similar cleansing of his earlier mechanical identity by fashioning himself as a philosopher and a courtier (Biagioli 1993), yet what Torricelli shows us is how the same result could be accomplished without the need for the philosophical clothing. In a city that reveled, as it still does today, in the miraculous *ingegno* of Brunelleschi's wondrous dome atop the Florentine Cathedral, figures like Galileo and Torricelli could achieve status and acclaim solely through the display of their Archimedean genius for mechanical fabrication.

Torricelli's mercury *strumento* takes on a different character when situated as a consequence of this precise kind of neo-Archimedeanism. The pre-1644 experiments related to the vacuum performed by Gaspar Berti can also illustrate how the characteristically Tuscan adoration of the Archimedean *artefice* served as a motivation for innovative experimental work that was not directed primarily toward natural philosophical agendas or their attendant scholastic contestations. De Waard and Knowles Middleton both view Berti's water experiment as a possible inspiration for Torricelli's later and more famous mercury *esperienza,* and like the latter, Berti's work had no reception in its day, and was only resurrected after the fact by later accounts. One such retrospective narration was the report of Magiotti, one of Torricelli's regular correspondents, to Mersenne in 1648. (Mersenne 1977, 16: 168–171) Berti's *esperienza* involved filling tubes with water rather than mercury but then using these inverted water filled tubes to produce the empty space at the closed end of the tube in the manner of Torricelli's mercury *esperienza*. The demonstration was performed at Rome sometime before 1643 and may have included Ricci among the observers (De Waard 1936, 178–81). Whoever attended, Magiotti claimed that he told Torricelli about the demonstration soon afterwards – no document confirming this report survives – and he further added that he explained to Torricelli that, "if it was sea water in the tube it would have stayed lower." Magiotti also explained to Mersenne that the motive for exploring the natural rise and fall of fluids in tubes derived from the annual Florentine ritual of cleaning the wells using siphons. This practice raised the question of why the water would elevate in the siphons, and what the sources of the attraction were. "Signor Galileo had occasion," writes Magiotti, "to observe the height of such attraction, which was always the same, about 18 ells in that Tuscan measure. … From this had their origin his speculations about the matter, which were put into his work on the resistance of solids" (Mersenne 1977, 16: 169–170).

The reference here is to Galileo's *Discorsi* of 1638, which did indeed contain a discussion of the limited height to which water can be drawn by a pump. Galileo's disciple and Torricelli's colleague Vincenzio Viviani also prepared an annotated edition of this text, allegedly with the direct support of Galileo, and he noted in the margin next to this part of the text that, "it is my belief that the same result will follow in other liquids such as quicksilver, oil, wine, etc. in which the rupture will take place at a lesser or greater height than 18 braccia according to the greater or lesser specific gravity of these liquids in relation to that of water" (Knowles Middleton 1964, 20). The empirical research supporting these quantitative speculations, performed in Tuscany by Galileian *meccanici* in the decade preceding Torricelli's work, may offer a source for Torricelli's 1644 *esperienza* with mercury.

Knowles Middleton resists any erasure of Torricelli's singularity, however, for as he states: "Berti's apparatus was not a barometer; if words mean anything, a barometer must be an instrument to measure pressure, and … Berti and his friends were interested not in the measurement of pressure, but in producing a vacuum. … [His] was [simply] a splendid physical experiment" (Knowles Middleton 1964, 18). This assessment makes sense given the teleological frame of Knowles Middleton's study, but as I have argued already, is it really self-evident that Torricelli had a new instrument to measure air pressure in mind when he dipped his tubes of mercury into the tub? Much evidence that he had other motivations in mind is in fact present, and once one accepts that Torricelli's achievement may not have been a singular, *sui generis* experimental breakthrough, a richer contextual understanding of the *esperienza* becomes possible.

Especially fruitful is treating the *strumento* and the *esperienza* it made possible as an outcome of Torricelli's Tuscan Galileian-Archimedean inclination to use geometry together with fabricated devices in the exploration and explanation of natural phenomena. This context was in fact exactly the one deployed by Magiotti in his letter to Mersenne suggesting the link between Berti and Torricelli's work. Such a frame, it should be stressed, treats modern experimental measurement as an anachronistic interpretive frame. It also downplays the role of university-based natural philosophy in motivating the work. Instead, the prime motivators for research from this perspective were empirical problems of practical significance, and the desire to show the power of geometry and mechanico-instrumental reasoning in the solution of such problems. Also important is the discovery of solutions that are striking, and ideally spectacular, in their ingenuity and applicability. This understanding, it should also be noted, also connects Galilean-Archimedean mechanics to the court nexus despite the arguments made by Biagioli and Boschiero that the court was not amenable to science of this sort. (See esp Biagioli 1993, 4). The princely court, it must be remembered, was *both* a theater for displaying the mysteries of royal majesty *and* the organizational center of the prince's sovereign state. Much of the literature on court science in Tuscany has stressed the first, looking at how savants helped to fabricate and project the glorious reign of the Medici Grand Dukes while also fashioning themselves in the process as liberal philosophical minds appropriate to inhabit this aristocratic milieu (Biagioli 1993; Findlen 1993). Yet many court mathematicians, and here Torricelli is perfect illustration, also served the court by performing utilitarian tasks of state significance, and in this context Galileian-Archimedean mechanics was crucial. Among Torricelli's extant works, in fact, is a long memo interrogating a project to transform the Val da Chiana, a tributary river valley of the Arno, into a more productive and manageable aquatic system (Torricelli 1919–1944, 2: 263–310). Torricelli did other work related to water management as well, as did Galileo and Viviani (Maffioli 1994), and if one situates the mercury *esperienza* in this context, then its motivation begins to look similar to those that produced engineering and practical mechanics rather than natural philosophizing, and to be connected to a regime of "court science" as well.

To be a new Archimedes in seventeenth-century Tuscany often meant being what we would today describe as a highly theoretical engineer and mechanical technician. Torricelli played this role well, and what is too rarely stressed about his seventeenth-century reputation is the way that artful making was crucial to this work and identity. He was in particular a highly skilled lens-maker whose products were much sought after. He gave many to Grand Duke Ferdinando II, serving in this way his role as the court mathematician. Others also looked to Torricelli for this kind of work as much if not more than for his work in mathematics and natural philosophy. The case of Balthazar de Monconys can illustrate the point. When illustrating the silence that greeted Torricelli's mercury *esperienza* in Italy, Knowles Middleton describes the visit of Monconys, a French savant and traveler, to Florence in the fall of 1646 (Knowles Middleton 1964, 31–32). Although Monconys met almost daily with Torricelli, and also with Torricelli's friend in Florence, Antonio Nardi, and with his correspondent in Pisa, Vincenzo Renieri, no mention of the mercury *esperienza* was noted in Monconys's diary, evidence, says Knowles Middleton, that it was simply not a topic of discussion in Tuscany at the time. What was mentioned, however, were the "the diverse thermometers for knowing the heat and cold all made with *eau de vie* (*divers thermometers pour connaitre le chaud et le froid, tout avec l'eau de vie*)" that the Grand Duke possessed and that Torricelli supervised (Monconys 1665, I: 130). Even more frequently cited were Torricelli's lenses. Monconys acquired several during his visit, and in letters from Renieri to Torricelli preceding Monconys' arrival in Florence, lenses were the topic of conversation (Torricelli 1919–1944, 3: 421–428). In the only letter from Monconys extant in Torricelli's surviving correspondence, a letter written from Egypt several months after he left Florence, Monconys wrote to share his enthusiasm for the lenses he had acquired and to share with Torricelli the admiration that they were receiving in Egypt (Torricelli 1919–1944, 3: 455–458). In the 1644 letter from Ricci expressing eagerness to learn of Torricelli's *esperimenti,* lenses also figure since Ricci asks not only for news of Torricelli's experiments but also about his "works with glass, and your other *invenzioni* that thousands are asking me about (*del lavoro de' vetri, et sue altre invenzioni ho mille che me ne richiedono*)" (Torricelli 1919–1944, 3: 189). It should also be noted in this context that while the mercury *esperienza* was never an explicit topic of conversation in the

29 extant letters between Torricelli and Mersenne, the Italian's *ingegno* as a lens-maker, along with his genius as a geometer, were staples of this correspondence (Torricelli 1919–1944; Mersenne 1977).

It fits well with the extant documents, therefore, to see Torricelli primarily as a wondrous *artefice* and *ingegniere* in the Tuscan neo-Archimedean tradition, and to make his mercury *strumento* just one more stunning performance to add to his glorious inventory of marvelous productions. The fact that no further discussion of the *esperienza* occurred between Torricelli and Ricci after its initial presentation and interrogation also fits well within this interpretive frame. For what correspondence like theirs was ultimately about was the sharing of sparkling results, not the systematic adjudication of scientific problems in the manner of modern scientists, or even early modern natural philosophers. The rhetoric found in the correspondence between Ricci and Torricelli also fits well with this understanding since theirs were letters that were long on sprightly discourse and spirited repartee and short on ponderous negotiation of knotty technical problems. What each aspired to do in their letters was to demonstrate their brilliance by showing some new geometrical result or mechanico-empirical solution that marked out the *sprezzatura* of its inventor.

One should also remember what Torricelli stressed about his mercury *strumento*, namely the ease with which his device produced something that others believed was difficult to achieve and even abhorrent to nature. Paula Findlen has used the Milanese "Archimedean" Manfredo Settalla to stress the magician-like conception of the Italian neo-Archimedean, a conception that linked savants oriented this way with mythic makers such as Prometheus and Daedalus (Findlen 1996, 325–334). Torricelli encouraged and received these sorts of identifications as well, and when he stressed in his epistolary report to Ricci the ease with which he produced the vacuum in his fabricated *strumento* he was emphasizing his Promethean power to use tools and artful handiwork to bring nature comfortably under his control.

To be a stunning producer of contrived wonders in this fashion also fit with Torricelli's identity as a court mathematician as well. Settala organized a museum, which served as a storehouse of his genius and a display of his wondrous art. Torricelli's productions, by contrast, largely fed the collections of the Grand Duke, but he too cultivated collecting and display as a scientific practice of self-fashioning. He also produced objects – lenses, geometrical solutions, ingenious *strumenti* – that were intended as objects to be gazed upon and admired. Visual display, in fact, was central to his mercury *strumento* in ways that are often missed. One element was mechanical, rooted in Torricelli's ability, derived from his work as a lens maker, to find or produce (it is not clear from the documents which) the glass necessary to make the tubes that made the *esperienza* successful. This was no trivial matter, for even Torricelli admitted failure in producing his desired demonstration of the variable weight of the air given the materials he had in his possession. Others, such as Mersenne and Petit in France, confirmed in their reports that getting the mercury *strumento* to perform as expected was no easy task (Knowles Middleton 1964, 25, 37. 39–40). That Torricelli succeeded as well as he did in this difficult undertaking, and that he alone was the innovator of a successful demonstration of this visual effect, was a testament to his superior skills as fabricator of glassworks and their use in brilliant displays.

Fabricating sparkling visual results was also central to Torricelli's craft in other ways, and these too played a role in the mercury *esperienza.* Among Torricelli's diverse duties was to serve as a teacher of mathematics at the *Accademia del Arte del Disegno*, the leading Florentine institution devoted to the pictorial arts. Galileo initially learned his mathematics in the orbit of this academy, and the lessons he learned here regarding the connection between *disegno,* or the union of reason and with handicraft, and natural knowledge played a key role in his scientific work (Reeves 1997; Bredekamp 2001, 2007; Edgerton 1984, 2009; Camerota 2010). A letter appointing Torricelli to be a teacher of military fortification at the academy survives among his papers (Torricelli 1919–1944, 4: 81), but also present is an unfinished treatise on pictorial perspective, a text which indicates that his job may have included teaching aspiring visual artists the principles of geometric picture making. The text is conceived as a dialogue between two learned gentleman, a fact that again speaks to the way that Galileo's texts served among his followers as a model for a dialogic, as opposed to demonstrative, mathematics and natural science. Torricelli's text also introduces the geometry of perspective not as a mathematical theory to be mastered and then applied but as a reasoned form of artistic practice that allowed logic and rationality to guide the art of visual fabrication (Torricelli 1919–1944, 2: 311–320).

This was the essence of *disegno* as a broader maker's epistemology uniting theory with practice, the rational with the physical, the mind with the hand, and mathematical conceptualization with mechanical *techné*, all in the service of natural knowing (Smith 2004; Marr 2011). That this practice was one that Torricelli actually cultivated and did not just teach is evinced by a series of drawings contained along with the manuscript pages of his treatise on perspective in the collection of his papers in the Biblioteca Nazionale Centrale in Florence (Fig. 7.1). These drawings illustrate geometric spatial constructions akin to those his manual would teach, but on other pages are sketches of a mechanical fountain and a drawing of a leg rendered using a linear form of *chiarascuro* that brings multiple curvilinear figures together in a way that aspires to capture three dimensional form. Each of these drawings are emblematic of a *meccanico* and an *artefice* trained in the practice of Florentine *disegno*.

Torricelli's mercury *strumento* takes on still another character when viewed as a result of these kinds of visual artistic practices. *Il Vocabolario* defines a "strumento" as "that by which, or by the means of which, we do something (*quello, col quale, o per mezzo del quale, noi operiamo*)" (Il Vocabolario 1612). Using this definition, we might then ask what is it exactly that Torricelli's *strumento* does? A good answer might be that the instrument makes visible to the eye things that are otherwise unseen. The unseen nature visualized by the *strumento* would include the existence of the vacuum itself, but also the connection between it and the weight of the air, a connection that makes the otherwise invisible atmospheric pressure present in nature apparent to the eyes. It should be remembered, in fact, that Torricelli ultimately presents his device as one the proves that we live at the bottom of a vast sea of air, a sea that is normally imperceptible to us (Torricelli 1919–1944, 3: 186–188). In each

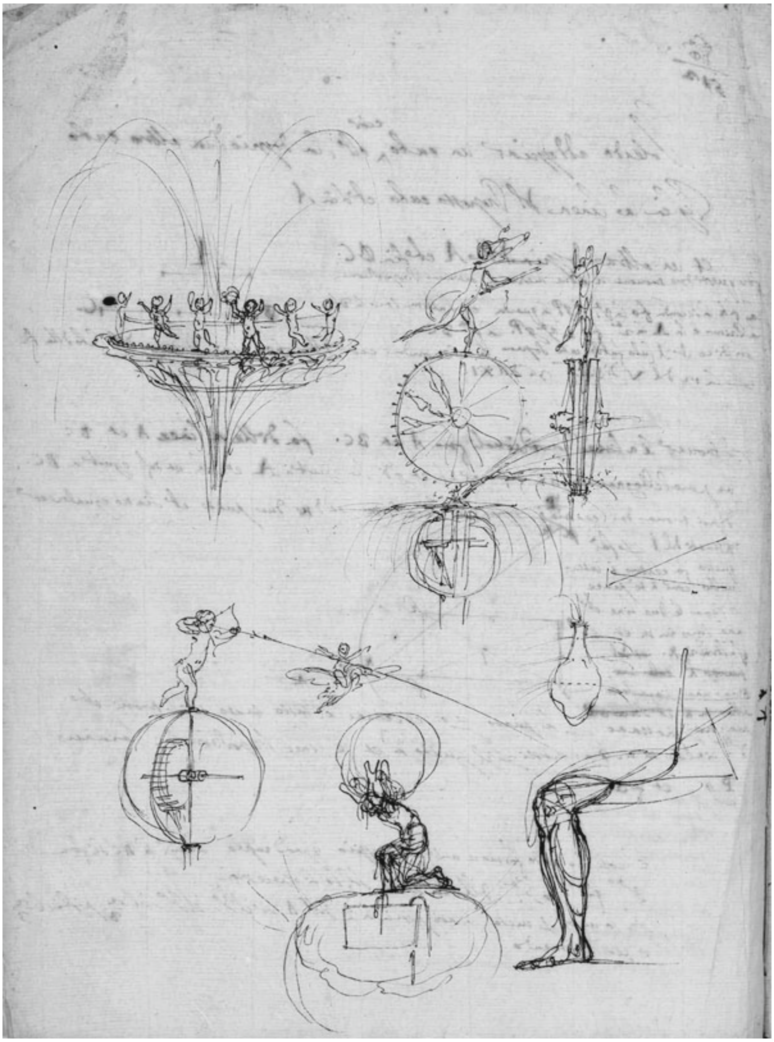


Fig. 7.1 Drawings by Evangelista Torricelli (Courtesy of the Biblioteca Nazionale Centrale Firenze)

case, what the *strumento* accomplishes or performs is a visible spectacle whereby the unseen realities of the world are made visible to human perception. An analogy with painterly perspective can also be drawn with respect to the natural veracity claimed for this artfully produced visual display. In the same way that the painter uses geometry and *disegno* to create an artificial image on the picture surface that purports to represent the realities of natural space to the observer's eye (Belting 2011), so too does the mercury *strumento* use geometry and rationally conceived manual craft to present the realities of the atmosphere and natural space to a viewer through a fabricated visual scene.

There is, in fact, an intriguing similarity between Torricelli's mercury *esperienza* and the rationally constructed paintings that Torricelli (perhaps) helped students to create as a teacher at the *Accademia del Arti del Disegno*. In each case, what is being created through a material object, be it paint on canvas or mercury in tubes, is a visual presentation that stages for an observer a fabricated re-presentation of authentic nature. This visual and artistic idea of science should not make us uncomfortable since the point is not that Torricelli was creating pictorial fictions with his *strumento* when he used it to stage natural effects visually. He was rather doing what all instrumental demonstrations of natural phenomena do – using rational, instrumental means to show us nature in ways that allow us to shake off our ordinary perception of it. Naturalistic paintings and experimental scientific demonstrations each operate in this way, for it is the very essence of instrumental experimental science to use rationally conceived and artificially staged visual effects as stand-ins for, or re-presentations of, the complexities of natural phenomena. A Renaissance painting constructed according to the rules of geometrical perspective accomplishes exactly the same result. For this reason, Torricelli's work as a teacher and theorist of painterly perspective is not without significance as a context for conceiving his work with the mercury *strumento*. The fact that Ludovico Cigoli, one of Torricelli's predecessors as a teacher of perspective at the Florentine academy, and also like him a disciple of Galileo, constructed mechanical devices that allowed students to more easily pursue geometric picture making further supports this comparison. Like Torricelli, Cigoli saw natural knowing as a manual craft that could be advanced through the use of rationally conceived machines (Camerota 2010). The perspective *strumenti* that Cigoli designed and built realized this goal by allowing painters to more easily capture and re-present for viewers the natural geometry of space. Torricelli's mercury *strumento* can also be described as a rational machine of this sort, for his device also produced for viewers a visible display of the geometric rationality of nature. Each contrivance was likewise inspired by the same Florentine program of *disegno*-conceived art making, and each marked its inventor as a wondrous *artefice* whose handiwork demonstrated a wondrous control over, and facility with, the workings of the universe.

Torricelli gives us a further reason to situate his *esperienza* within this particular context when he calls his mercury demonstration in his letter of response to Ricci "mia fantasia" (Torricelli 1919–1944, 3: 199). Few modern readers pay much attention to this phrase, treating it as little more than a bit of rhetorical fun and games. Yet by unreflectively translating this phrase into the modern term "my fantasy," these readers actually elide an important historical distinction that is crucial for understanding Torricelli's work. As Giorgio Agamben explains in a text titled "Fantasy and Experience," a dramatic reversal has occurred in the meaning of the words "imagination" and "fantasy" between the pre-modern world and our own period. "The imagination," Agamben writes, "which we now expunge from knowledge as the 'unreal,' was (for the pre-moderns) the supreme medium of knowledge." Accordingly, "the fantasy," or the union of the sensible form of the natural object with the potential intellect of the human observer, was not for pre-modern thinkers a creative fiction; it was "the supreme medium of knowledge." In this way, "far from being something unreal," the fantasy defined as the meeting of the sensible world with the human intellect in the theater of the imagination "occupies in ancient and medieval culture exactly the same role that our culture assigns to experience." Stated another way, "the homology between fantasy and experience" in premodern thought makes the fantasy the foundation of knowledge, not its antithesis (Agamben 2007, 27–8).

This is how Torricelli would have understood the word *fantasia*, and in using it to describe his mercury *esperienza* he was not playing rhetorical games but rather using a technical scientific term central to early modern optical theory to capture a crucial dimension of his work. To summarize briefly this dimension of Torricelli's conceptualization, consider the relation between natural vision, optical theory and instrumental picture making bound up in the early modern term "perspective." "*Perspettivo*" originally meant sight, or the science by which vision is understood (Lindberg 1976; Edgerton 2009; Jay 1994; Elkins 1996; Belting 2011). In this respect, "perspective" and "optics" remained until the end of the medieval period largely synonymous terms that connoted the systematic science of light and vision. When, in the fifteenth century, painters and other *artefice* begin to develop the geometrical techniques for producing seemingly natural vision on a two-dimensional picture surface, a distinction between natural optics and painterly perspective began to emerge. This distinction was marked out in the early discourse by the increasing clarification of two terms, perspective tout court, which still meant natural seeing or optics, and what came to called "artificial perspective," or the fabricated production of seemingly natural seeing through geometric picture making (Anderson 2007).

What this terminological distinction indicated was a new awareness of the difference between natural seeing (perspective) and the staged, contrived, re-presentational seeing that a material object like a painting creates (artificial perspective). But since both were still perspective, what the distinction also indicated was the new integrated understanding of natural seeing and artfully contrived seeing as two entangled poles in one uniform program of natural knowing and natural re-presentation (Clark 2009). Leonardo da Vinci was one of the most vigorous students of the complexities of this dialectical relationship, and in his notebooks, and in the subsequently published *Trattato della pittura* that is found therein, Leonardo elaborated artificial perspective into numerous techniques appropriate for creating authentic natural images. These included mathematical perspective, or the ordering of pictorial space and composition using geometry, but also atmospheric perspective, or the painting of the atmosphere to replicate the misty sense of distance. Color perspective, which explored the way that certain colors appear to the eye as more or less distant, was also one of his topics of inquiry (Kemp 1992; Veltmann 1986).

Another figure who was acutely aware of the dialectical relationship between natural seeing and vision through artificial re-presentations of nature was Francis Bacon. In Bacon's scientific utopia *The New Atlantis*, the master of Salomon's House describes a room devoted to visual trickery. "We have also houses of deceits of the senses," the master explained, "where we represent all manner of feats of juggling, false apparitions, impostures, and illusions." Why include such a room of visual deceits in an institution that is otherwise devoted to "the hate of all impostures" and the avoidance of "all affection of strangeness?" (Bacon 1999, 267). The answer, I contend, involves the way that Bacon, and all early seventeenth-century natural philosophers, including Torricelli, were acutely aware of the problems involved in making seeing the anchor of believing. From their perspective, sight was the royal road to natural truth, but sight was also a highly unstable sensation, and one that had to be carefully regulated for it to become authoritative. The techniques of experimental testing and retesting that Bacon made the centerpiece of his scientific methodology, and the comparable programs that the Cimento later adopted as its epistemological ideal, were one solution to this problem since they guaranteed, or so it was claimed, a single stable vision that could lead to truth and not visual deception. But as the house of deception within Salomon's House indicates, this program was also aware of its own limitations, and anxiety persisted regarding the actual certainty achievable through visual representations of this sort.

Stuart Clark has explored in wonderful detail the instabilities prevalent in early seventeenth-century conceptions of visual knowing (Clark 2009), and Ofer Gal and Raz Chen-Morris have recently emphasized similar points in describing the way that seventeenth-century optics, in removing the embodied subject from the center of vision, both created the new possibility of a detached and objective scientific stance while at the same time posing this very position as an epistemological problem (Gal and Chen-Morris 2010). These arguments dovetail with the recent work on perspectival painting by art historians such as James Elkins, Hubert Damisch, Samuel Edgerton, and Hans Belting, and they also conform with the anxious instabilities of scientific objectivity in the modern era recently explored by Lorraine Daston and Peter Galison (Elkins 1996; Damisch 1994, 2002; Edgerton 2009; Daston and Galison 2007; Belting 2011). Against an older tradition that saw in geometrical perspectival painting a rationalization and objectification of sight brought about by the wedding of artificial representation with the natural geometry of human vision (Ivins 1938), this newer literature sees in perspective a new self-consciousness about perspectival vision itself, and a new exploration of, rather than comfort with, the natural ties that bind human vision with its artificial re-presentation.

Torricelli's use of the term "fantasia" to describe his mercury *esperienza* is an indicator that his work should be situated within these early seventeenth-century visual problematics. For this term was first and foremost a technical term designating a concrete visual perception according to the optical theory of the period. *Il Vocabolario* defined "fantasia" as a scientific term, derived from the Latin *phantasia,* which indicated the object produced by the imagination in relation to the soul (Il Vocabolario 1612). "Imagination" used this way also possessed, as we have noted already, no automatic association with creative fiction since it too was a technical term in pre-modern optical theory connoting the faculty that connected the sensory organs of the body with the cognitive and emotive – the two were not seen as distinct – activity of the soul. Summarizing this conception very briefly, a "fantasia" resulted from the bodily reception of a sensory transmission (a "species") that was passed through the appropriate medium (light understood as a material substance in the case of visual sensations) from the natural object that was its source to the sense organ of an observer. In the case of sight, this transmission produced a "fantasia" when the imagination, acting as the faculty of the soul, registered the material species and transformed it into a sensation accessible to human consciousness. Since the species that triggered this fantasia was understood to be a direct emanation from its material source, imagination and fantasy were not understood to be creative processes, but natural physiological mechanisms. And as Agamben stressed above, these terms anchored, therefore, rather than undermined, the scientific epistemology of the period (Lindberg 1976; Clark 2009).

Yet this is not to say that every *fantasia* registered in the imagination automatically translated into natural truth, or that vision was incapable of producing deception. All manner of interventions, from humorial pathologies such as melancholy to the intervention of shape-, or species-, shifting demons, could in fact intervene to disrupt the accurate imagination of naturally transmitted species. And what of objects like the rainbow that were produced in the imagination even though they did not derive from a particular material source? *Il Vocabolario* noted the possibility of visual deception, while also offering a terminological clarification designed to contain it, by defining through a different term, "fantasma," the false and deceptive product of the interaction between materially produced species, the imagination, and the soul. Summing up the distinction, the dictionary stated that "fantasia" is the imagination of that which is, and "fantasma" is the imagination of that which is not. (*Fantasia è immaginazione di quel ch' è, e Fantasma di quel che non è*)" (Il Vocabolario 1612). By calling his experience a "fantasia," therefore, Torricelli was not really mocking in a phony way its fictional potential, and we would commit an error were we to translate this word using the modern English term "fantasy." He was rather signaling that the image produced by his *strumento* was, like all visual events (or *fantasie*), a true natural phenomenon. But as a visual image of an entity, the vacuum, that by definition was not material and not capable, therefore, of transmitting a species to the eye, by calling it "my *fanatasia"* he was also saying that his was a mechanically produced image that made visible the invisible truths of nature, even if his contrivance was susceptible to all the visual instabilities attendant to such performances.

Torricelli's other work in the 1640s also supports the idea that the conundrum of visual reliability was as important to this thinking as any effort at systematic eradication of it. His initial conduit into the learned circles of Paris, for example, was not Mersenne, but Mersenne's Minim brother Jean-Francois Niçeron. It was Niçeron that carried Torricelli's first mathematical demonstrations to Paris in 1643 and introduced them to Mersenne and others (Mancosu and Vailati 1991). Afterwards Torricelli also maintained a correspondence with the Minim priest. Niçeron was also, like Torriceli, a student of mathematical perspective and the author of a 1638 treatise on the subject, although one that explored the deceptive aspects of geometric pictorial illusionism as much as its normalizing naturalism. Called *La perspective curieuse, ou La magie articielle des effets merveilleux de l'optique par la vision directe*, Nicéron's treatise taught not only standard "normalizing" Albertian perspective techniques, but also the anamorphic techniques that used geometry to create perspectival tricks and optical distortions (Niçeron 1638). The frontispiece for this work shows the connection between this mathematics of pictorial illusion and the artificial optical effects of lenses through the images of

Fig. 7.2 Frontispiece, Jean-François Niçeron, *La Perspective curieuse, ou Magie artificielle des effets merveilleux de l'optique, par la vision directe, la catoptrique, par la réflexion des miroirs plats, cylindriques et coniques, la dioptrique, par la réfraction des crystaux.* Paris: (Courtesy of the James Ford Bell Library)

putti playing with telescopes and microscopes in the observation of natural objects. In the treatise, Niçeron also described a piece of optical pictorial art of his own creation that he gave to Grand Duke Ferdinando II in 1638 along with the treatise itself. In this artwork, which is currently on permanent display in the new Museo Galileo in Florence, an optical instrument, now lost, is placed on the ledge in front of a geometrically ordered and painted panel permitting a kaleidoscopic montage of images of the Grand Duke to form in the observer's imagination (Fig. 7.2).

Fig. 7.3 Anamorphic panel painting in celebration of Ferdinando II, Grand Duke of Tuscany (Courtesy of the Museo Galileo, Florence)

The metamorphic play with the multiple personae of the sovereign – man, prince, divine manifestation, etc. – was a staple of the political theatrics of the Baroque court. In his work, Niçeron used geometrically and artistically contrived visual re-presentations to make vision at once the stable union between the natural world and the human subject and also the site where mysteries and instabilities of this very same union were displayed (Fig. 7.3).

No textual record connects Torricelli to Niçeron's treatise or to the artwork it explained, a fact that is not surprising given its creation in 1638 before Torricelli had moved to Florence and joined the Medici court nexus. Yet it is hard to imagine Torricelli anywhere else but at the center of the nexus that produced this artifact after his arrival in 1641. Even if he did not create the lenses that were in the optical instrument that brought Niçéron's pictorial machine to life, his own lens-making in the service of the Medici was very often put to uses like this even if it was also deployed by astronomers like Renieri in the production of better astronomical telescopes. Knowing what role anamorphoses and other geometrical optical deceptions would have played in his own perspectival treatise addressed to the Florentine

Accademia del Arti del Disegno is also made impossible by the fragmentary and unfinished quality of Torricelli's manuscript. Yet whether known to Torricelli or not (the archival record is again silent on the matter), a precursor to Torricelli's perspective treatise was published in Florence in 1625 by Pietro Accolti, also a teacher at the *Accademia del Arti del Disegno*. Sub-titled like the works of Cigoli and Torricelli *Prospettiva pratica,* Accolti's treatise carried as its primary title *Lo inganno de gl'occhi,* or "the deception of the eyes." The work nevertheless opened with an account of the species theory of vision and then explored the mathematical rules for generating natural illusions of vision, both normalized Albertian perspectives and anamorphic ones that played with perspectival distortions. Perspectival machines like those designed by Cigoli were also featured in the treatise (Accolti 1625).

Returning the mercury *esperienza* of 1644, Torricelli's conception of it as a "fantasia" at least confirms that he saw an optical dimension to the arguments he sought to sustain with it. His own work with optical devices and with the mathematical rules of perspective, both natural and artificial, was also a context from which his work emerged. How does this context change our interpretation of the event? One point to stress is how the strong uncertainty about vision as a path to natural knowing among seventeenth-century savants fights against conceptions of them as confident practitioners of instrumental experimental science in the modern sense. What anamorphosis taught was that visuality, and as such knowledge, was perspectively determined, and that no single vantage point, save God's, could render seeing and knowing continuously stable and veracious. This was the age, in Italy at least, when Ovid's *Metamorphoses* ruled the cultural landscape, and within this milieu images, both natural and artificial, had to be understood as simultaneously truthful and Protean in their deceptive potential even when geometry governed their production. Certainly one begins to see emerging out of the confrontation with these dilemmas new urges toward stabilizing vision through the constitution of a single, transcendent viewpoint that trumps all others. Gal and Chen-Morris persuasively read Descartes's *cogito* as one such attempt, and another is the Baconian ideal, further clarified by the Cimento and then by the experimental technologies of the Royal Society of London, to use systematic trial and error testing and rigorous discursive discipline to stabilize the objective "matter of fact" as a ground for unquestioned seeing and universal believing.

The point to emphasize with respect to Torricelli's *esperienza,* however, is that these were later developments and not ones operative, or so I contend, in the cognitive regime available to him in 1644. Accordingly, when thinking about what his experiment meant to him and his immediate audience, we need to acknowledge that it may have been as much a demonstration of a natural phenomenon with all its attendant visual conundrums as any attempt to eradicate such confusions. We also may need to recognize that rather than the first effort to use an artificial instrument to document and certify a regular fact of immutable nature – i.e. Knowles Middleton's reading of the mercury *strumento* as the first modern barometer – Torricelli's device may have been a completely early modern contrivance designed to *monstrare*, or show, a complex natural cum visual effect. It may also have been a machine designed to provoke more philosophical cogitation than quantitative clarification, and a contrivance designed to demonstrate artistic virtuosity and the wondrous power of human handiwork for revealing and manipulating nature than any mechanism for reducing nature to quantitative rule. In short, rather than calling Torricelli's *strumento* the first modern scientific instrument, we might capture its deeper historical character by seeing it as a representative example of Baroque scientific art.

Conclusion

What has been accomplished by this excursion through the many historical worlds that produced Torricelli's 1644 mercury *esperienze*? Pace Knowles Middleton and the entire tradition that sees in Torricelli's work the invention of modern, instrument-based experimental science, this account offers Torricelli as a fully early modern figure practicing various pre-modern intellectual pursuits. Viewing his work from the perspective of the individuals who shared his historical milieu, this paper also emphasizes the determinative role of these unmodern orientations in the genesis of Torricelli's work. The following attributes have been offered as exemplary of his precise historical situation. First is the neo-Archimedean strain in Torricelli's work, a tendency that allows for pure geometry, practical and theoretical mechanics, and artful making to become unified into a coherent intellectual program. Second is the way that this particular kind of neo-Archimedeanism was peculiarly supported in seventeenth-century Tuscany making it possible for a *virtuoso* like Torricelli to attain status, both at court and in the wider public, through specific mechanico-artistic endeavors. Third was the way that this particular socially supported program led to a general climate of scientific practice that emphasized the production of singular spectacular outcomes and stunning material displays. This in turn made visuality, and the artistic fabrication of it, central to Torricelli's work in exceptional ways. Together with his endeavors in the orbit of the Florentine *Accademia del Arti del disegno*, arguably the master institution of this larger scientific-artistic complex, visual display also became a central feature of Torricelli's artful science.

Out of this context emerged the mercury *esperienza* of 1644, and if we now see it as something other than a harbinger of a new and all-powerful modern experimental science coming to life in seventeenth-century Europe, we should still see it as a major event in the scientific culture that Torricelli and his work exemplified. Might this scientific culture be called Baroque? Gal and Chen-Morris end their article on early modern visuality by saying that "Descartes did not re-discover what was indeed noted since antiquity, that our vision was not to be trusted. Rather, he … reversed the epistemological role of vision. … It was a paradoxical insight: by accepting that knowing is seeing and understanding how we see, Descartes was convinced that we may not know at all. This then is the [Baroque] optical paradox" (Gal and Chen-Morris 2010). Torricelli might also be understood in terms of this same Baroque optical paradox, although one that in his day might rather have been construed as a deeper optical truth. To know that a vacuum can exist, Torricelli contends, and that the presence and weight of atmospheric air are facts of nature, a machine that makes this nature visible must be created. Yet in showing these truths through a geometrical and optical *strumento,* what we actually produce is a *fantasia*, a sensory perception of nature registered in our imagination, and not nature itself. This is what Gal and Chen note when they say: “from being [previously] the guarantor of our knowledge and a paradigm of direct acquaintance, vision became a metaphor for mediation” (Gal and Chen-Morris 2010). As a mediator, the visual *esperienza* makes knowledge possible, but as a *fantasia* it does so while also posing the limits of its own power of explanation. Gal and Chen conclude by calling this double stance Baroque, and Torricelli’s 1644 *esperienza* might also be described as a piece of Baroque science defined this way.

Yet I prefer to end with a different understanding of Baroque science as applied to this case. When Walter Benjamin deployed the category of the Baroque to make sense of the seventeenth-century German theatrical form *Trauerspiel,* he did so because the term disrupted the prevailing interpretive frames of his day, the interpretive frames that made classical rationalism the marker of aesthetic progress while relegating aberrant or “regressive” theatrical forms like *Trauerspiel* to aesthetic oblivion (Benjamin 2009; Newman 2011). The progressive assumptions of historical thinking itself therefore fought against the recuperation of *Trauerspiel,* and to accomplish his historical project Benjamin had to create a historicism detached from the normative interpretive assumptions of progressive history. The category of the “Baroque” entered at this point as both a historical category – it marked the divergent stylistic trends occluded in the traditional, progressive histories – but also, and more importantly, as a methodological category – it marked out the historical rationality that was other to the triumphant story of rational progress that kept *Trauerspiel* relegated to the historical shadows. This paper has attempted a related interpretive move using the same double meaning of the word Baroque. Wanting to liberate Torricelli’s work from the normative histories that find value in a particular presumed story of modernist scientific progress, it has returned to the archive in order to discover the history occluded by these traditional normalizing stories. The Baroque character of this history resides, therefore, in its demonstration of the unexpected historical connections and contexts within Torricelli’s work, and with its use of these occluded historical details to disrupt conventional rationalist and progressivist understandings of early science. But the Baroque was more than a disruptive period label for Benjamin; it was also a marker denoting a challenge to historical thinking itself. Baroque history from this point of view is not restricted to alternative accounts of the seventeenth century. It is also a label for history itself, a call with respect to any historical topic to bring a Baroque spirit of contingency, perspectivalism, indeterminacy, and creative play to bear in the interpretation of the past (Mali 2003). To conclude by calling this paper first and foremost a Baroque history and only secondarily a history of Torricelli’s Baroque science thus makes sense to me from a Benjaminian point of view. For in saying this, we are saying that Torricelli might have practiced a Baroque style of science in the seventeenth century, and that our understanding of his 1644 *esperienza*, like that of *Trauerspiel* for Benjamin, might benefit from a historical recovery of the unconventional Baroque rationalities that gave it meaning. But using Baroque in the second sense, we are also saying that the project of history of science itself can be aided by taking a Baroque approach to historical interpretation overall, one that follows the account of Torricelli offered here through its comfort with a Baroque perspectival and epistemological instability. In short, what this paper offers in the spirit of Benjamin is both a history of Baroque science but also an attempt to perform a more Baroque approach to the history of science itself. It is the latter, I suggest, that may be the more important contribution of this article to the project of this volume.

References

1612. *Il Vocabolario degli Accademici della Crusca.* Venice: Alberti.

Accolti, Pietro. 1625. *Lo Inganno degl'occhi, Prospettiva pratica di Pietro Accolti gentilhuomo fiorentino*. Florence: Pietro Cecconcelli.

Agamben, Giorgio. 2007. *Infancy and history: On the destruction of experience* (trans: Heron, Liz). London and New York: Verso.

Alberti, Leon Battista. 1980. *Ludi mattematici*. Parma: Guanda.

Andersen, Kirsti. 2007. *The geometry of an art: The history of the mathematical theory of perspective from Alberti to Monge*. Dordrecht: Springer.

Bacon, Francis. 1999. The New Atlantis. In *Francis Bacon: Selected philosophical works*, ed. Rose-Mary Sargent. Indianapolis: Hackett.

Battaglia, Salvatore. 1961. *Grande Dizionario della Lingua Italiana*, 20 vols. Turin: Unione Tipografico.

Belting, Hans. 2011. *Florence and Baghdad Renaissance Art and Arab Science*, (trans: Schneider, Debarah Lucas) Cambridge, MA: Belking.

Benjamin, Walter. 2009. *The origin of German tragic drama* (trans: John Osborne). New York: Verso.

Bertoloni Meli, Domenico. 2006. *Thinking with objects. The transformation of mechanics in the seventeenth century*. Baltimore: Johns Hopkins University Press.

Biagioli, Mario. 1993. *Galileo courtier. The practice of science in the culture of absolutism*. Chicago: University of Chicago Press.

Bos, Henk J.M. 2001. *Redefining geometrical exactness: Descartes' transformation of the early modern concept of construction*. Dordrecht: Springer.

Bos, Henk J.M. 2004. Philosophical challenges from history of mathematics. In *New trends in the history and philosophy of mathematics*, ed. Tinne Hoff Kjeldsen, Stig Andur Pedersen, and Lise Marianne Sonne-Hansen, 51–66. Odense: University Press of Southern Denmark.

Boschiero, Luciano. 2007. *Experiment and natural philosophy in seventeenth-century Tuscany: The history of the Accademia del Cimento*. Dordrecht: Springer.

Bredekamp, Horst. 2001. Gazing hands and blind spots: Galileo as draftsman. *Science in Context* 14: 153–192.

Bredekamp, Horst. 2007. *Galilei der Künstler: Die Zeichnung, der Mond, die Sonne*. Berlin: Akademie Verlag.

Camerota, Filippo. 2010. *Linear perspective in the age of galileo: Ludovico Cigoli's Prospettiva pratica*. Florence: Leo Olschiki.

Clark, Stuart. 2009. *Vanities of the eye: Vision in early modern European culture*. Oxford: Oxford University Press.

Cohen, H.Floris. 1994. *The scientific revolution: A historiographical inquiry*. Chicago: University of Chicago Press.

Damisch, Hubert. 1994. *The origin of perspective* (trans: Goodman, John). Cambridge: MIT Press.

Damisch, Hubert. 2002. *A theory of /Cloud/. Toward a history of painting* (trans: Lloyd, Janet). Stanford: Stanford University Press.

Daston, Lorraine, and Peter Galison. 2007. *Objectivity*. New York: Zone Books.

Dati, Carlo. 1663. *Lettere a Filaleti di Timauro Antiate, Della vera storia della Cicloide, e della famosissima esperienza dell'argento vivo, 24 Gennaio 1662*. Florence: All'Insegna della Stella.

Dear, Peter. 1995. *Discipline and experience. The mathematical way in the scientific revolution*. Chicago: University of Chicago Press.

De Gandt, François (ed.). 1987. *L'Oeuvre de Torricelli: Science Galiléenne et nouvelle géometrie*. Nice: La Lambrusque.

De Waard, Cornelis. 1936. *L'Expérience barométrique, ses antecedents et ses explications*. Thouars: Deux-Sevres.

Edgerton, Samuel Y. 1984. Galileo, Florentine "disegno," and the "strange spottednesse" of the moon. *Art Journal* 44(1): 225–232.

Edgerton, Samuel Y. 2009. *The mirror, the window, and the telescope: How Renaissance linear perspective changed our vision of the universe*. Ithaca: Cornell University Press.

Elkins, James. 1996. *The poetics of perspective*. Ithaca: Cornel University Press.

Findlen, Paula. 1993. Controlling the experiment: Rhetoric, court patronage, and the experimental method of Francesco Redi. *History of Science* 31: 35–64.

Findlen, Paula. 1996. *Possessing nature: Museums, collecting, and scientific culture in early modern Italy*. Berkeley: University of California Press.

Gal, Ofer, and Raz Chen-Morris. 2010. Baroque optics and the disappearance of the observer. *Journal of the History of Ideas* 71(2): 191–217.

Galluzzi, Paolo. 1980. Il Mecenatismo Mediceo e le Scienze. In *Idee, Istituzioni, Scienza, ed Arti nella Firenze dei Medici*, ed. C. Vasoli. Florence: Giunti Mantelli.

Galluzzi, Paolo (ed.). 2001. *Scienziati a Corte; l'arte della sperimentazione nell'Accademia Galileiana del Cimento*. Livorno: Sillabe.

Ivins, William M. 1938. *On the rationalization of sight; with an examination of three Renaissance texts on perspective*. New York: Metropolitan Museum of Art.

Jay, Martin. 1994. *Downcast eyes: The denigration of vision in twentieth-century French thought*. Berkeley: University of California Press.

Kemp, Martin. 1992. *The science of art: Optical themes in Western art from Brunelleschi to Seurat*. New Haven: Yale University Press.

Kircher, Athanasius. 1650. *Musurgia universalis sive Ars magna consoni et dissoni in X libros digesta*. Rome: Francesco Corbelletti.

Knowles Middleton, W.E. 1964. *The history of the barometer*. Baltimore: Johns Hopkins University Press.

Lindberg, David C. 1976. *Theories of vision from Al-kindi to Kepler*. Chicago: University of Chicago Press.

Maignan, Emmanuel. 1653. *Cursus philosophicus concinnatus ex notissimis cuique principiis, ac praesertim quod res physicus instauratas ex lege Naturae sensatis experimentis passim comprobata.* Toulouse: R. Bosc.

Maffioli, Cesare S. 1994. *Out of Galileo: The science of waters, 1628–1718*. Rotterdam: Erasmus Publishing.

Mali, Joseph. 2003. *Mythistory: The making of a modern historiography*. Chicago: University of Chicago Press.

Mancosu, Paolo, and Enzio Vailati. 1991. Torricelli's infinitely long solid and its philosophical reception in the seventeenth century. *Isis* 82: 50–70.

Marr, Alex. 2011. *Between Raphael and Galileo: Mutio Oddi and the mathematical culture of late Renaissance Italy*. Chicago: University of Chicago Press.

Mazauric, Simone. 1998. *Pascal, Gassendi, et le problème du vide*. Paris: Presses Universitaires de France.

Mersenne, Marin. 1647. *Novarum observatorum physico-matheticarum*. Paris: Bertier.

Mersenne, Marin. 1977. In *Correspondance du P. Marin Mersenne, religieux minime*, 16 vols, ed. Mme Paul Tannery, Cornélis de Waard, and René Pintard. Paris: Centre National Scientifique.

Monconys, Balthazar de. 1665. *Journal des voyages de M. de Monconys*, 3 vols. Lyon: Boissat & Remeus.

Newman, Jane O. 2011. *Benjamin's Library: Modernity, Nation and the Baroque*. Ithaca, NY: Cornell University Press.

Niçeron, Jean-François. 1638. *La perspective curieuse, ou La magie articielle des effets merveilleux de l'optique par la vision directe*. Paris: P. Billiane.

Petit, Pierre. 1647. *Observation touchant le vuide faite pour la première fois en France, contenue en une letter écrite à Monsieur Chanut … par Monsieur Petit le 10 novembre 1646. Avec le discours qui en esté imprimé Pologne sur le mesme sujet en juillet 1647.* Paris: Sebastien et Gabriel Cramoisy.

Reeves, Eileen. 1997. *Painting the heavens: Art and science in the age of Galileo*. Princeton: Princeton University Press.

Thirion, Jean. 1907–1908. "Pascal, l'horreur du vide et la pression atmospherique." *Revue des questions scientifiques.* XII: 383–450, XIII: 149–251.

Tribby, Jay. 1991. Dante's restaurant: The cultural work of experiment in early Tuscany. In *The consumption of culture: 1600–1800*, ed. Ann Bermingham and John Brewer. London: Routledge.

Tribby, Jay. 1994. Club medici: Natural experiment and the imagineering of 'Tuscany'. *Configurations* 2(2): 215–235.

Rougier, Louis. 2010. In *De Torricelli à Pascal*, ed. Simone Mazauric. *Philosophiae Scientiae* 14: 2.

Segre, Michael. 1991. *In the wake of Galileo*. New Brunswick: Rutgers University Press.

Sessa, Mirella. 1991. *La Crusca e le crusche: il vocabolario e la lessicografia italiana del Sette-Ottocento*. Florence: Accademia della Crusca.

Shapin, Steven. 1994. *The social history of truth: Civility and science in seventeenth-century England*. Chicago: University of Chicago Press.

Smith, Pamela. 2004. *The body of the Artisan: Art and experience in the scientific revolution*. Chicago: University of Chicago Press.

Torricelli, Evangelista. 1919–1944. In *Opere di Evangelista Torricelli*, 4 vols. in 5, ed. Gino Loria and Giuseppe Vassura. Faenza: G. Montaeri.

Torricelli's Barometric Experiment. Virtual Museum of the Museo Galileo. http://catalogue.museogalileo.it/multimedia/TorricellisBarometricExperimentText.html

Veltmann, Kim. 1986. *Leonardo da Vinci studies I: Linear perspective and the visual dimensions of science and art*. Munich: Deutscher Kunstverlag.

Wallace, William. 1984. *Galileo and his sources: The heritage of the Collegio Romano in Galileo's science*. Princeton: Princeton University Press.

Zucchi, Niccoló. 1648. *Experimenta vulgata non vacuum probare, sed plenum et antiperistasim instabilire.* Rome.

Chapter 8
William Harvey and the Way of the Artisan

Alan Salter

Abstract The scientific discoveries of William Harvey are usually studied in the context of the Aristotelian natural philosophy of late Renaissance Europe but it is my contention that they are more profitably understood by reference to what I term the craft empiricism of the period. Harvey's actual way of inquiry – quite distinct to the claims of his formal works – is to be found in the artisanal techniques of midwives, shepherds, huntsmen and such like who acquired the skills to comprehend their respective object worlds – the birthing mother, the sheep and the lamb, the herd of deer – through rigorous apprenticeship and constant practice. Chief of all their skills, unlearned and tacit, was one of intuitive recognition which enabled them to accommodate variance and disorder in the conduct of their craft. By an exact reading of Harvey's works, especially his late study on generation, De Generatione Animalium, I show that he too was an artisan, demonstrating the same techniques that he observed and borrowed from the artisans he clearly admired.

Introduction

When William Harvey delivered the Lumleian lectures to the London College of Physicians it was not surprising that he selected as his authorities the anatomical texts of the Aristotelians Caspar Bauhin and Andreas Laurentius. He had been educated in the Aristotelian tradition, firstly as an undergraduate at Cambridge and subsequently at Padua, at the time the home to the only Aristotelian medical faculty in Europe; he had a deep understanding of the nature works and was familiar with many of the philosophy texts. His research program was based on the Aristotle of Fabricius, his anatomy Professor at Padua; he chose the chick as the subject of his

---

A. Salter (✉)
University of Sydney, Sydney, Australia
e-mail: alan.salter@sydney.edu.au

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_8,

inquiries into generation, the animal Aristotle chose, and relied on Aristotle's own studies into animals and on his technique of comparative anatomy.

Aristotelian ideas pervade Harvey's work: the teleology, the centrality of the heart as the sun in the microcosm, the appeal to circularity to explain the movement of the blood and the eternal cycle of generation, the language of universals and the analysis of the structure and function of the part. Harvey adopted scholastic formats for his two major works: the *disputatio* for *De Motu Cordis* and the *quaestio* for *De Generatione Animalium*. In commenting on Harvey's demonstration of circulation one writer claims that it was "conceived and executed entirely in the tradition of philosophical anatomy" and that his research program was "philosophical rather than medical", that is it had no explanatory force (French 1999, 255, 232).

Thus reads the scholarly justification of the 'William Harvey as Aristotelian' thesis that has long dominated Harveian studies. But this scholarship is not secure. Literary structure is not an investigative technique; at the time it was a post hoc device to ensure clarity of argument and gain assent to a disputed proposition. Citations of Aristotle may be evidence of reading but Harvey's *De Generatione Animalium* contains several references to Galen and Pliny as well. Examples of Aristotelian concepts are unquestionably to be found, but not always in the chiefest discoveries. A critical argument in demonstrating the circulation of the blood was quantity, but quantity was a Galenic concept as Nutton has pointed out (Nutton 1999, 290). And as Wear has recognized, Harvey's inquiries into circulation were not directed at the structure of the part but at what he calls the complex whole (Wear 1983, 229). To read the preface to *De Generatione Animalium* as conceptually Aristotelian is to misread it. Although it is a difficult text bearing little relationship to the work itself, the preface displays a distinctly sceptical view of the Aristotelian conception of sensory perception and its consequent universals. Harvey stumbles when he attempts to enlist material and efficient cause in his discussion of the relative importance of the male and the female in generation and deliberately avoids Aristotle's solution to the problem of conception. But it is in his emphatic rejection of the necessity of final cause as the determinant of proper knowledge that we see a departure from Aristotelianism. Although he admits to Boyle and others that it was the function of the venous valves that suggested the possibility of circulation and although he acknowledges that he had failed to discover its purpose he nonetheless decisively dismisses the criticisms of his opponents who insisted that the absence of final cause in his notion of circulation was sufficient reason to reject his theory.

Scholars themselves are divided in their interpretations of Harvey's Aristotelianism: French sees it as unequivocal; Wear wants to blend it with observation to form a hybrid; and Schmitt points out that even Harvey's contemporaries could not agree on what it meant to be an Aristotelian (French 1994, 3–17; Wear 1983, 223; Schmitt 1989, 136). Much of the apparatus used to identify Harvey with Aristotle–the circle, the microcosm, comparative anatomy and even monism – is not unique to Aristotle, but present in other philosophical traditions.

It is my contention in this paper that Harvey is not to be understood by reference to an idea of late Renaissance Aristotelianism but is more properly located in the craft empiricism of early modern England. Harvey grounds his way of research in the practices of craftsmen and women in trades as diverse as midwifery, butchery, huntsmanship and shepherding. To these craftsmen knowledge was indistinguishable from practice; it was acquired by the application of skills learnt in apprenticeship and perfected over many years in the world of everyday experience. It is this same equivalence we find in Harvey: an investigative technique that, though rarely articulated, defines him as an artisan, skilled and experienced in the craft of inquiry into the physiology and generation of living animals.

Harvey's Way of Inquiry

The Problem of Inquiry

Harvey's inquiries attend to the part: the heart, the embryo, the blood, the muscles and so forth. The part however is not determinate for it is present in a context that is conscious of other parts. "No man" he says to Ent in a supposed conversation recorded in the Preface to *De Generatione Animalium* "has ever rightly determined of the use or office of any part who has not diligently considered with himself the structure, situation, annexed vessels and other accidents thereof" (Harvey 1651b, 4). These adjacencies in space and time problematize the part and its putative status. His examination of the *punctum saliens* in the egg is complicated by its uncertain status as a part, for as he writes in *De Generatione Animalium* "(A)bout the end of the fourth and beginning of the fifth day, the blood-red point, being now enlarged, seems to be changed into a very small and extremely thin bladder containing blood" and later in the same passage "I could not distinguish any difference in the vessels, for the arteries do not differ from the veins either in their coat or in their pulsation" (ibid., 99). And his investigations into the motion of the heart are complicated by the presence and action of the arteries and veins and the proximity of the chest wall (Harvey 1616, 265). The part and its context are indeterminate; what is a part in one inquiry is a context in another and what is a context in one inquiry is a part in another. Nor can the part be made subject to an a priori determination for Nature cannot be ordered. In discussing the generation of animals and "the common error of those…who spin philosophies" he ridicules those "who make all things out of atoms…As if, indeed, generation were nothing other than a mere separation, or assemblage, or ordering of things." Making sense of generation called for more than a knowledge or acquaintance with its component parts or processes, for generation "is a thing quite distinct from all of them" (Harvey 1651b, 65).

Inquiry concerns the integrity of the part. Is it to be considered as something distinct from its constituents, as an actual collection comprising several subordinate parts or as an invented class of numerous similar parts observed over a period of time? To Harvey there is no doubt. The whole is not to be properly comprehended by reference to its parts; as he claims in *De Generatione Animalium* "there is a mystery greater and more divine than the bare assemblage, alteration and composition of the whole out of the parts, for the whole is constituted and is to be seen before its parts, the mixed body before its elements" (ibid., 208). A preoccupation with the part limits and may destroy our conception of the whole. This is not to deny the value of the part nor that it exists; rather that as a means of discovery attention to observing the part is inferior to a grasp of the whole. Proper inquiry to Harvey is not to be restricted to the observation and description of parts but by some treatment of the whole that he only ever acknowledges tacitly and which we only discover by reference to his actual practice.

The problematic existence of parts as independent and identifiable objects is further complicated by their lack of uniformity. Eggs come in different shapes and sizes and the circulation of the blood "is not everywhere and always the same" (Harvey 1649, 46). Although these variances can sometimes be explained, for example by the differences in hens or by the constitution of the blood, Nature sometimes changes things just "for fun" (Harvey 1651b, 56). To the philosopher this caprice must be eliminated by theory, but for Harvey it must be accommodated. In the second essay to Jean Riolan, his fiercest opponent in the debate on circulation, Harvey rejects the views of some physicians who do not "think it satisfactory…to draw up new systems unless they solve all the phenomena" (Harvey 1649, 46). Although he acknowledges that there may be a "true course of Nature" (Harvey 1651b, 81) and that a collection of variant parts may contain at least one part that can be designated its norm this norm cannot be determined according to some rule. There is no given proportion in a collection of parts that constitutes a normal proportion, at all times and in all circumstances. Things are rarely certain; they are typically no more than mostly so.

The essence of inquiry for Harvey is to be found in the observation and identification of the normal whole in the face of an uncertainty of presence and an ambiguity of recognition. He grounds this inquiry in multiple, separate and momentary acts of observation and the active attention of the individual mind given to them over the course of many years. Writing of his observations of the four day old egg he writes that "I have many times observed the punctum saliens" and "I am quite certain from many experiments I have made" and "this has been done by me and by others again and again" (ibid., 98–100). He does not define observation but seems to mean a written description either in note or published form of the effect on the senses of the object of inquiry. His descriptions are detailed and comprehensive and he was justly proud of them, as no small simple thing but as the result of a skill that he had perfected over many years. But what is it that these numerous observations purport to describe? How can an accumulation of observations be interpreted as something coherent, for either they are identical, in which case there is no need to accumulate a multitude of them, or they are different in which case the inquirer forms them into some whole that is more than addition or accumulation yet masquerades as the description of an actual object? Since his own words discount the first possibility we must conclude that when he describes a particular thing his description is usually drawn from notes committed to writing at the time of observation. When, in Chapter 17 of *De Generatione Animalium*, writing of the "cloudy substance (that) sometimes obscured the pulsating vesicles", he comments that "by the help of a brighter light and a magnifying glass, and by making comparison with my observations of the following days" we can infer a practice of note-taking at the time of observation (ibid., 102). It seems clear therefore that what Harvey actually does is to distil all such observations into what we might call a universalized observed particular, either as a final summing up of all accumulated observations of similar particulars into one single description or as a rolling synthesis performed in each act of comprehension, wittingly or unwittingly removing each variant part, however small, and constructing a whole part as it were in the creation of a hypothesis. Every observation in his writings involves a judgment as to the normal whole and which variances to discard or which contexts or background conditions to invoke to justify the rejection of other variant parts or processes. No described observation is objective; there is either a contextual judgment that permits the exclusion of variants and the fashioning of wholes or an intellectual one that involves a synthesis of all prior observations in our mind either in the act of writing or in the act of comprehension, so that each observation of a particular becomes a new particular derived as a function of all prior particulars in its class. Yet though Harvey admits the existence of variants and devotes some passages to them in his major texts he nowhere admits that his observations of claimed particulars are anything but equivalent. Unless specifically mentioned, all observations are presented as though uniform.

It is not therefore in his ability to identify and describe a particular part that Harvey's skill of observation lies, but in his understanding of what constitutes a whole, given the presence of often numerous subordinate parts in it, and what can be discarded from a collection of sometimes variant parts. This is a skill that operates in the absence of evidence and for which he offers no determination either quantitative or qualitative. It is a capacity for apprehending the relevant and important things directly, without recourse to reason and it is this capacity that I refer to as intuition or intuitive recognition. Of the contractions and dilations of the vessels in the four day old egg, Harvey writes "(H)aving observed these things with much caution and circumspection in a great number of eggs, I stood for a while in suspense wondering what opinion I should hold concerning them…The beginnings of the very greatest things are always exceeding small and very obscure because of their extreme smallness" (ibid., 110). To Harvey the skill of intuition is a necessary pre-condition of inquiry since there is no guarantee that multiple observations carried out on a great number of objects over many years will lead to knowledge. Harvey himself is uncertain about what to conclude even with the benefit of a sufficient quantity of empirical data, and reason has no part to play. Even the choice about what we observe is intuitive for there is no objective evidence contained in the part or the process that calls for special attention nor can Aristotelian abstractions specify what is to be explained. The experienced inquirer knows what to look for: "no-one will notice them unless he be highly experienced" Harvey writes of the differences in the eggs laid by one hen in *De Generatione Animalium* (ibid., 76).

The Priority of Experience

Harvey's skill of intuitive recognition is grounded in an experience formed in sensory perception and in the numerous and frequent acts of observation that accompany it. Experience is acquired in the world of the everyday, not in the formality of the laboratory or in the scholarly disputation. It is not advantageous he writes "to decide something about the works of Nature from the meaning of words, or to summon anatomical disputes before the grammatical tribunal" nor should the inquirer rely on "petty reasonings borrowed from mechanics" nor on "their own private opinions previously made up (and) the dogmas (erected in accordance with them)" (Harvey 1651a, 76; 1651b, 18, 85). Nor is experimentation privileged as a distinct and superior form of experience, for all the actions of the inquirer are experimental; all provide instantaneous evidence of the attributes of the object.[1] Experience properly acquired is infallible; in a short passage on whether eggs harden in the womb or on first coming into contact with the air he notes that he learnt "by infallible experience" that they harden in the womb and later in the same work that "we know from experience" how quickly the new-born babe loses its ability to suck (ibid., 64, 66, 306). Experience not only surpasses theory but is diluted by it; ancient doctrine may guide the inquirer but is not a substitute for it. Experience is a necessary condition of judgment for as he writes in the Preface to *De Generatione Animalium* "without the right verdict of the senses… and valid experience, we make judgments entirely on phantoms" (ibid., 13).

Experience requires multiple observations; they are mistaken he writes in criticism of a certain physician of Paris "who think that they have revealed all on the basis of a very few observations only" (Harvey 1655, 93) but it was not because Harvey was correct that 'they' were mistaken, but because all conclusions in inquiry depend on a great number of observations. This is no well worked out theory of induction as though more, similar observations are a sufficient endorsement of a prior judgment, nor that the number of observations though large can be limited, but rather a belief, itself grounded in experience, that numerous observations are necessary for valid experience and the number of these observations cannot be prescribed. Variants, whether "chance peculiarities, such as character" (Harvey 1653, 89) or those abnormal effects whose cause can be determined must also be eliminated so that the norm can be judged and this elimination requires a sufficient number of observations. Observations must also be frequent (Harvey 1651b, 13). Time alters the parts and processes of Nature; things grow and decay, as in the egg with the jump-like alterations of its early days and hours (ibid., 106–114).

Experience originates in observation, which is an act of perception. In the Preface to *De Generatione Animalium* Harvey recounts how a painter distinguishes between the 1,000 sketches of a face he is about to portray by placing them together, side by side, so as to determine by active observation their various similarities and differences. The experienced individual inquirer knows what to observe and what to notice because he attends to the thing and the context of the thing; the object that is observed is thus the consequence of an active choice. In a passage of prodigious description in the Lumleian lectures which provides us with clues to the immense effort of attentiveness demanded by proper observation Harvey explains to his audience how difficult it is to discover the correct motion of the heart:

> I have observed these things for hours together and I have not been able easily to distinguish between them…. either what they call diastole is the contraction of the heart…or else X it is what they say it is; or at least that in diastole the fleshy wall of the heart is thickened while the ventricles are in truth compressed…. See how hard and difficult it is to distinguish… between dilatation and contraction and to say of what nature is systole and of what diastole[2] (Harvey 1616, 265).

The assiduous inquirer inspects multiple similar parts in different contexts, frequently over many years; inquiry mandates industry and diligence. In a letter to Caspar Hoffman, a forceful opponent of his views on circulation, Harvey writes "(I)f … (you are) averse to deciding for yourself by means of dissections,…I adjure you, refrain from despising the industry of others or turning it into a fault,…in respect of something he has tested so often over so many years" and in the same letter he claims that Nature herself possesses the attributes of industry and skill for "I have always been full of admiration for Nature's skill, wisdom and industry" (Harvey 1636, 3). Industry seems to have been such a rare characteristic amongst physicians and inquirers that it deserved special praise; in a letter to Johann Horst, Chief Physician in Hesse-Darmstadt, Harvey writes: "I praise highly the singular industry of Pequet and of others in searching into the truth" (Harvey 1654–5, 92). You cannot discover unless you do the work for yourself, unless you take the time and the trouble.

We must rely on our own experiences: "I would now lay them before you that you may see with your own eyes and judge for yourselves" he wrote in his lecture notes (Harvey 1616, 265). These experiences are private; in the second essay to Riolan he asks: "Who will persuade those who have never tasted it that wine is sweet and far surpasses a drink of water? With what proofs will he convince those who are blind from birth that the sun is bright and more splendid than all the stars?" (Harvey 1649, 54). Experience cannot be acquired from the testimony of others nor from ancient texts and nor can it be learnt. Like the sweetness of wine or the brightness of the sun it exists only for the individual who possesses it. "Per me" Harvey announces on the title page of his Lumleian lectures (Harvey 1616, 2). These are my lectures, he seems to be saying, and they are based on my own experience, and those who wish to acquire experience must, like me, acquire it for themselves.

In her recent study of science in the revolutionary period, Smith describes artisanal experience as a "process of struggle", a phrase that neatly captures the industry, diligence and tenacity necessary to acquire a knowledge of nature (Smith 2004, 149). The craftsmen and women of early modern England endured their own particular struggle too, but their goal was not knowledge but judgment and it is their attainment of this goal that I next discuss.

The Way of the Artisan

The Particular

The best surviving record of how artisans learnt their trade comes from the instructional books of midwifery and the popular improvement texts of husbandry. These texts speak of an attention to the particular and the lessons of practice. Midwives were taught that their patients displayed particular and specific signs and should be diagnosed according to these actual signs and not the exigencies of Aristotelian categories. Diagnoses implied particular treatments and remedies to be applied to the woman in childbirth according to the specific history of her condition. The author of *The English Midwife Enlarged* provides an example:

> And do we not know not only in the same Country and Field but also on the same Vine, grapes sometimes six weeks ripe before their ordinary Season; and others not till a month after?....So do we see Women brought to bed six weeks and 2 months before, and sometimes as long after their ordinary term;…there have been Women as Hippocrates acknowledgeth who have gone 10 or 11 whole Months with Child, which is so much the rarer, by how much it exceeds its limits (Anon. 1682, 22)

The apprentice midwife absorbed herself in the craft and spent time watching and copying, learning to judge the circumstances of individual patients and their cases, ordering herbals and making up recipes, cleaning equipment and so on. Midwifery texts offered maxims, tips, signs and stories handed down by earlier generations of midwives; they provided practical advice and simple diagnoses and therapies (Lane 1988, 12: 18). In her compendium of midwifery, *The Midwifes Book*, Jane Sharp listed 14 common rules laid down by "divers physicians" to test for conception; she had rules for women coming into labour and clues to detect the imminence of birth (Sharp 1671, 82–83, 159–162). Midwives were encouraged to tell their own stories of the birthing-room and thus confer on them the legitimacy of experience as diagnostic and therapeutic remedies.

For the typical English midwife the acquisition and perfection of competence was laborious. There was no intellectual system to govern the craft, no clinical foundation to justify treatment and no topics such as physiology, anatomy or apothetick to suggest diagnostic pathways. Success was only possible by continuous and disciplined application over several years. Jane Sharp said she was a "Practitioner in the Art of Midwifry above 30 years" (ibid., Frontispiece); Dorothy Chambers and Ann Ramsey, two midwives in Restoration London, had 13 and 20 years experience respectively (Evenden 2000, 51); and many midwives possessed several years informal experience as mothers, sisters and gossips before taking up the craft.[3] In practice the range of cases that midwives came across presented few complications; historians of the period have estimated that 98 % of all births were normal and midwives with average case loads of 8–20 births a year rarely if ever came across difficult births (Wilson 1995, 18, 34–35). Nonetheless the range of cases a midwife might encounter in her career was potentially vast, and she was expected to be familiar with the way of dealing with each case, in the manner acknowledged by the custom and practice laid down by earlier midwives.

We see the same catalogue of rules, the same priority accorded to personal experience and the same attention paid to particulars in the husbandry works of the period. Speaking of the times when stallions and mares should be together, the popular time being the middle of March, Gervase Markham, author of several hugely popular works on animal husbandry and household management, has a different point of view: "in the strictnesse of mine owne opinion and experience, I have ever found from the beginning of March till the end of Aprill, the very best time of all" (Markham 1617, 31). Only the word of the experienced and competent man prevails; in criticism of the opinion of the inexpert concerning the early covering of mares he writes: "Divers horsmen heere in England (but not any expert breeders) I have heard, holde strong argument against this opinion of mine" and on the same topic "all men of experience know" of the correctness of Markham's opinion (ibid., 31, 32). Knowledge is grounded in personal experience; the mind of man creates only speculation and imagery. There is no theory, no set of universal precepts; all rules can be gainsaid by the actual and experienced occurrence. Horse husbandry, like midwifery, is governed by the particular circumstance.

Apprenticeship and Experience

Apprenticeship grounded the skills of the artisan. The master would share his secrets and show the apprentice how to perform his tasks; in the words of the author of *The Apprentices Companion* "The eyes of Servants look to the hand of their master" (Burton 1681, 36). But the reality seems to have been somewhat different. Instruction was haphazard; craftsmen could not typically articulate what had to be done and few apprentices could afterwards explain what they had actually learnt (Long 2001, 74; Smith, 59–93). The single example of instruction cited by Edward Barlow, apprenticed to the sea, is of a senior officer offering a tip on how to turn the capstan to raise the anchor, his account suggesting little more than a casual encounter: "one of the officers of the ship, calling me away, bid me come near to the bar's end and there I should do the more good" (Barlow 1659–1703, 33). Yet Barlow's subsequent confident command of seamanship seems to have developed nonetheless, as he describes in this short passage on the ship's departure for Brazil:

> And being under sail, we directed our course southerly, having a fair wind;…for after you come as far southerly as the North "Tropick" or 24 or 25 degrees northerly, you seldom miss of a fair wind…and then you meet with continual rains,…and many times strong gusts of wind, waving about upon all points of the compass…And being come into (the equatorial latitudes) we had many cross winds…and sometimes we did catch…dolphins… and many times sharks which are thereabout in abundance. (ibid, 81, 82)

This detailed understanding of current, wind, water condition and marine life seems to have accrued through some passive absorption in the world of the vessel and the sea. Barlow has acquired a skill, if not without formal training then with training that at the time may have seemed so casual or to be of so little consequence that he did not record how he learnt and may not even have been aware of the act of learning. Much of what happened in the early modern apprenticeship (and by inference, in subsequent craft practice) seems to mirror Barlow's experience, of merely being present in the craft, absorbing its practices and dwelling in its processes of production or activity. As Turner points out, the transmission of skill or practice is known only by inference, as though by a causal connection evident only through time (Turner 1994, 47). Barlow has learnt to do something but does not seem to be aware of his progress nor does he consider it sufficiently important to describe it or to explain it.

Technical skill and experience alone however offered no more than a grounding, for the world of the craft was governed by chance and uncertainty. Wood shaped by the carpenter was rarely uniform and stone cut by masons varied from quarry to quarry and sometimes within the same quarry. The artisan had to develop a skill of accommodation to this world. Sharp for example notes that although there are several rules to test for conception mothers often complicate diagnosis for "(N)ot one of twenty (women) almost keep a just account (of their menstrual cycle)" and although there are signs for false conception, she notes that "(I)t is very hard to know a false conception from a true until four moneths be past" (Sharp 1671, 81, 86). Guillemeaue relates a story of a noblewoman who went several months ignorant of her pregnancy, thinking her condition to be a consequence of colic:

> Being come to Paris, her Colique was somewhat mitigated, and a little while after she voided two or three gallons of water, without any paine, thinking verily then, that she was not with child: yet five days after she delivered very happily, and with little paine, of a faire daughter, there following very little water, or none at all. (Guillemeaue 1612, 97)

In a short account of how to tell the age of a horse, Markham provides some signs: "(F)irst by the pride, fulnesse, and cheerefulnesse…(then) if his eies be round…and his countenance smooth and free from sadnesse… then we gesse and knowe that such a Horse is young" (Markham 1617, 27). Although these signs are no more than clues and although he goes on to list several more reliable signs, he is not being ambiguous when he writes that guessing, which is a sensible conjecture based on incomplete information, is an acceptable basis for knowledge.

This skill of accommodation is an acquired ability to judge in the face of uncertain signs in ambiguous contexts and it is the skill that Harvey most admires amongst artisans. In an extensive passage in *De Generatione Animalium*, the longest sustained piece of writing on artisans in his corpus, he discusses the differences amongst eggs laid by different chickens and the ease or difficulty in matching eggs to laying hens. Harvey insists that he "could easily recognize without much difficulty in a basketful of eggs which egg belonged to which hen" and goes on to point out that acquiring such a skill is no more than a matter and time and trouble (Harvey 1651b, 76). But it is the passage that follows which is of greater importance, for in it he discusses the existence of the skill of recognition that is to be found amongst shepherds and keepers. He writes:

> What we experience daily among keepers is far more worthy of our admiration. The more diligent sort of park-keepers that have the care of a great number of bucks and does, if by chance they find in the woods or rides the antlers that are cast off each year, they recognize infallibly to which buck they belong. (ibid, 76–7)

This skill of intuitive recognition is "far more worthy of our admiration." "We," that is Harvey and others (probably his physician or natural philosophical colleagues), might believe ourselves to be skilled but these skills are little compared to what we observe daily among craftsmen such as keepers. It is these men that we should admire, not ourselves, for they possess a skill of infallible recognition that identifies the particular and grasps its significance.

As if to emphasize this point Harvey provides another example:

> There was a simple, ignorant shepherd who had the care of a great flock of sheep and he grew so well acquainted with each one of them that if any one were missing from the flock, although he did not know how to count, yet he could straightway tell which one it was, from whom it was bought or whence it came. (ibid, 77)

Acquisition of this skill of recognition is not a matter of learning or education therefore, for it can be acquired by a simple ignorant and innumerate shepherd, though judging from the tenor of the passage as a whole it is apparently not often observed amongst the learned. It is a skill that comes from a long acquaintance with the object of the shepherd's attention, the individual sheep in its flock. The shepherd comes to an immediate judgment; he could "straightway tell". There is no careful ordering of facts, no rules to be followed and no argument leading to a deductive conclusion. Nor is this skill to be minimized merely because the shepherd observes an absence rather than a presence, for Harvey deals with this objection in the next sentence: "(O)nce, for a trial, he singled out from among 40 lambs penned together in the same fold the one his master chose and carried it to his mother as she was grazing with the flock" (ibid., 77). There is no doubt that Harvey tells this story in a mood of admiration and respect, as though looking on in wonder as another practitioner, equal in status and achievement to himself, accomplishes some exceptional feat of craft or learning. There is no suggestion here that Harvey the learned physician and philosopher stoops to conquer as it were, of the Fellow of the London College and graduate of Padua sneering with disdain as some mechanical performs an amusing, trivial trick.

Harvey continues his tale of intuition and its possessors with the case of a huntsman who could "positively distinguish" a stag from its horns or its footprints once seen, and even judge its size, fatness and vigour "having seen merely its footprints," whether it be worn out by the chase, and whether a stag or a hind. The passage reaches its climactic conclusion with an account of a feat of recognition which seems scarcely credible:

> I would say further that there are some who, in a hunt, when some forty hounds are pursuing the quarry and all are in full cry, can distinguish each hound even a long way off and know by their ear alone which is first and which behind, which is following the right scent and which is running off at a loss, whether the quarry be still in flight, or standing at bay and engaging the hounds in battle with its horns, whether the stag has run for a long time or whether it be newly roused from its lair. And all this in the very midst of the noise and cry of hounds and men and horns, even in a dark and unknown wood. (ibid, 77)

There is a deliberate attempt here to create narrative complexity and an impression of chaos equal to the environment being described. The pack is large, the noise is loud and distant, the woodland may be dark and unknown, the posture of the stag uncertain and its engagement in the chase lost to view. Yet in all of this there are some huntsmen whose faculties of recognition are tuned to such a high pitch that they can distinguish the details of the hunt and its progress as if present at the chase. Harvey cannot explain this skill. It cannot be acquired by text or learning, only by a persistent acquaintance with the object and an absorption in its world. But for the shepherd or keeper these skills are not tricks; they go unremarked because they are necessary, a prior condition of craft, a part of what it is to be an artisan. Harvey's intent of course is to sharpen our appreciation by providing an extreme example, yet there is nothing magical about this skill; its acquisition comes with time and diligent practice, every day. Though "we" as readers or as colleagues may be sceptical, he is not, for this pinnacle of skill only serves to highlight what can be attained by those who practice.

Artisans and Trust

It was this competence gained from long exposure to the object of their craft that engendered public trust in the artisan. Women had confidence in midwives because other patients, often neighbours or relations, recommended them. Over three-fifths of London deliveries were repeat business and clients continued to use the same midwife throughout their child bearing years. Trust was not grounded in civil status for patient-midwife relationships were typically classless; there is no evidence of preference for man-midwives among higher class women and there are frequent testimonials on behalf of low class midwives in London or the country. Even surgeons' wives used midwives rather than surgeons, who typically lacked the experience of midwives (Evenden 1993, 19–20). Louise Bourgeois, midwife to the court of France, was cited by physicians in their learned texts (Broomhall 2004, 203) and Fabricius consulted midwives on matters of delivery and abnormalities. Tradesmen accepted the authority of midwives. In a passage in *Cavalarice* Gervase Markham deals with the case of a mare having difficulty foaling. After suggesting various possibilities for easing her pain, he writes that "if it shall faile, then I would have you take the helpe of some discreet woman, whose better experience knowes best how to handle such an occasion" and from earlier admiration in the book we can infer that he means a midwife (Markham 1617, 51).

Added to the trust accorded by women to their midwives and the endorsement of physicians and other artisans was the exogenous legitimation of the church with its system of ecclesiastical licensing. Although initially intended to provide for the possible administration of the sacrament of baptism in the absence of a priest, the practice continued after the Reformation as a means to ensure proof of skill, effectiveness of practice and integrity of character. Bishops of the Anglican Church issued licenses on the evidence of competent persons; although surgeons and physicians sometimes provided evidence of technical ability witness was in the main provided by women, for they had sole command of the birthing process and placed much importance on childbed experience (Guy 1982, 56:528–542). Midwives were expected to be able and expert. Women of the parish would know of this expertise and had an interest in ensuring that only the competent were granted approval to practice. The process was rigorous: midwives were usually required to demonstrate long experience and other midwives and sometimes patients were called on to witness the competence of the applicant licensee.

Trust depended on experience but this trust only carried to the boundaries of the trade. Midwives were not entrusted to carry out difficult births nor did they trust themselves to do so. As the author or authors of *The Compleat Midwifes Practice* noted "let the midwife be very skilful, that she may decline as much as in her lies, all the impediments that may be avoided" (Anon. 1656, 105). But if midwives could not carry out difficult births, nor could physicians or surgeons be trusted to perform simple births, for the two skill domains were distinct. Their respective birthing circumstances and conditions bore no relationship to one another; diagnosis and treatment were specific to each domain. In 1633 Elizabeth Whipp and Hester Shaw attacked Peter Chamberlen's request to incorporate a College of Midwives under his personal direction because, they said, he had no experience in normal deliveries only in difficult ones, thus distinguishing between the two: the skill required for a normal birth was not that required for a difficult one and skill in the latter domain did not imply skill in the former, notwithstanding the hierarchy implied by the two descriptions. It was not the case, they argued, that difficult cases included normal cases and the following year a bishop's inquiry upheld their petition (Evenden 2000, 106). Midwives emphatically rejected the view that learning overcame experience and some physicians concurred. Ambrose Parey, the celebrated French physician, wrote: "(N)o man becomes a workeman by booke…I must needs liken them… To Pilots by booke onely: to whose care, I thinke, none of us would commit his safety at Sea" (Parey 1579, To The Reader). Midwives insisted that their craft was a practice and were proud of it. Jane Sharp dismissed learning as no more than the concealment of truth: "(I)t is not hard words that perform the work, as if none understood the Art that cannot understand Greek. Words are but the shell, that we ofttimes break our Teeth with them to come at the kernel, I mean our brains to know what is the meaning of them" (Sharp 1671, 12). For Louise Bourgeois words contributed nothing to the competence and judgment of the midwife: "(M)y practice is not a language" she wrote, "these are true effects" (Broomhall 2004, 133).

Even Scripture could be denied by experience. In *Cavalarice*, Gervase Markham writes of the strange opinions of philosophers and horsemen on breeding: "(H)ow ever these opinions are mayntained by the Scriptures, or by *Laban*sheepe, for mine owne part I holde neither trust nor truth in them" (Markham 1617, 43). This is the story of Laban's sheep in Genesis 30: 25–43 and tells of animal breeding founded on the notion that what is in the mind of the animal at the moment of conception is impressed upon the foetus. It is a similar practice Markham has in mind, of horsemen painting a horse a certain colour in order to ensure the same colour in the foal, and which he derides in the text. This explicit and militant rejection of the authority of Scripture tells not only of a belief that experience was superior to doctrine but of a belief held with such resolution and self confidence that it permitted a trust in the uncivil and unlearned artisan in their specific domain in preference to the word of the learned or the teaching of the church. Artisans were worthy of our trust because they possessed experience. Women had confidence in their midwives because other patients, often neighbours or relations, and midwives trusted and recommended them (Wilson 1995, 31–33). The husbandman trusts his own judgment because it is grounded in experience; in Markham's words "(my) Philosophie is my owne experience" (Markham 1617, 29).

Harvey's trust in artisans was based on this same relatedness: knowledge was acquired by practice and was thus authentic. The knowledge of the scholar, philosopher and anatomist was mostly acquired from the word of others and thus lacked the endorsement of time and personal experience. In chapter 9 of *De Motu Cordis*, in the argument from quantity, he relies in part on the evidence of butchers who "can testify to all these things well enough, when in slaughtering an ox they cut the carotid arteries and in less than a quarter of an hour drain out the whole mass of blood and empty all the vessels" (Harvey 1628, 81). And in a short passage in *De Generatione*

Animalium on human childbirth he compares the knowledge of the experienced midwife to that of the learned anatomist:

> Again, midwives that are experienced in these matters know well that if all the waters come away before the womb be opened as it should be, the woman with child will continue longer in labour and her travail be more difficult, whereas the reverse should be the case were it, as Fabricius would have it, that the waters going out first did so much conduce to the softening and lubricating of the parts. (Harvey 1651b, 304)

Now Fabricius was no mere anatomist but was described by Harvey in *De Generatione Animalium* as his guide. Yet he was not to be trusted on matters of which he had no experience, notwithstanding his reputation and learning. It was the midwives that knew well for they possessed an experience that was actual and personal and which therefore justified a common trust. Harvey's trust existed independently of status; he would have acknowledged the artisan's lack of learning and would have been aware of the low repute of midwives, butchers, shepherds and keepers. He trusted the judgment of artisans, in spite of their civil status and the privacy of experience on which their judgment was based, because judgment is an observable fact, like the actual thing in Nature, and is thus capable of empirical endorsement. Just as sense data validates our belief in the existence of the thing, so actual experience validates our trust in the judgment of the artisan.

William Harvey and the Way of the Artisan

The way of the artisan was not studied, nor was it the subject of the learned treatise; it is apparent today only in the slightest of references, the merest of hints scattered throughout a vast assortment of texts. But such an absence of formal exposition need not hinder our understanding. An analysis of craft by a close examination of its four elements, discernible in these texts, can reveal and help us grasp its immanent properties. These elements I will call definition, values, process and skill.

By *definition* I mean the craft as it was actually practiced. Craft was often limited by statute or church or by guild or livery company regulation; its domains were thus narrow and specific, and vigorously defended. By *values* I mean the system of beliefs and opinions held concerning its conduct and performance. Values were irreducible, first encountered during induction into the craft. Such values could be separated into two sub-categories, those applicable to the object-world, the flock and its pasture for example, and those to its practice, such as the necessity of an experience that is privately acquired. By *process* I mean the approach typically taken to the acquisition and maintenance of craft skill. Artisans acquired skill through an initial apprenticeship, followed by continual learning and practice. By *skill* I mean the small number of techniques essential to practice. Some skills were public, observable by the ordinary onlooker; some were private, the probable cause of effective practice. The most common private skill was intuition. It was the essence and pinnacle of the artisan's craft; only an acute intuition could make sense of the instability of the object-world. Textual authority absorbed in the classroom was futile; its rigidity and claimed certainty confounded the efforts of craftsmen and women in the exercise of their private judgment. Of this incompatibility, John Maubray, a prominent man-midwife of the period writing of his art, observed that it is "attended with so many complicated circumstances of *accidental Difficulties,* that it is almost impossible for any Persons, who never apply'd themselves this way, to believe how much it differs from all the *THEORY,* that the most ingenious *MAN* can make himself *Master* of" (Maubray 1724, 176–7).

Harvey's way of inquiry is a craft too, grounded in the practices of English artisans. Though the actualities of his technique lie unuttered in his published works, they are undeniable: his insistence on examining actual things and contexts in space and time, on the activity involved in inquiry, on the importance of time in building experience and on the necessity of exercising a private intuition in the face of uncertain and ambiguous observations. His practices mirror those of midwives, shepherds, keepers and butchers. Midwifery dealt with the individual birthing mother, her particular history and circumstance; it developed in the midwife a skill of judgment in the care of mother and child that was grounded in years of experience as apprentice, junior midwife and midwife. The crafts of keeper, huntsman, shepherd and butcher, though less well documented, display the same characteristics that we observe in Harvey: the narrowness of the skill domain, the necessity of dwelling in the object-world, the emphasis on the individual animal in its herd or flock and the same insistence on long years of learning to build technical and intuitive skills and to perfect practice. Artisans had no interest in the prejudicial systems of the philosophers; they looked to experience, personally acquired and privately held, to validate their craft and to justify their standing. Harvey sought this same greatness of experience and the skill of intuition that it offered; what he learnt from artisans was the pathway to its possession.

Harvey as Baroque Inquirer, or Not?

Against the background of a Baroque science characterized as a high cultural artifact, grounded in art but discovered in order, setting paradox, sensualism and violence against rigor and logic, an artifact with conflict at its heart, Harvey's way of inquiry seems strikingly out of place. His practice was rooted in the English vernacular tradition of the artisan, experiential, intimate and dismissive of the abstract. In part this anomaly is to be expected: the historiography of the New Science has been written largely with reference to the so-called exact sciences, optics, astronomy, mechanics and the like, which admit the explanatory and the theoretical. The life sciences, however, are not so accommodating; the inquirer confronts an objectively messy world and must approach this messiness with a way of inquiry that is fitting to it.

Harvey is less strikingly out of place, however, if we consider him to represent a world of inquiry obverse to the Baroque, in which art is found not in the public depiction of Nature but in the private productions of the inquirer's own imagination, and order is found not in Nature's patterns and behaviors but in the performance of a craft of inquiry. We might call this other world the world of the variant Baroque.

Harvey considered Nature to be a mystery, a seemingly formless landscape, not an assemblage of parts but something prior and quite distinct, so that in understanding Nature and its composition the inquirer must first grasp its wholeness. In the face of such a belief reductionism and systematic observation fail conspicuously; inquiry must rely on a personal insight that can penetrate to the essence of Nature's parts and its processes. Such an insight, let us call it intuition, is grounded in the gradual accumulation of non-uniform sensory experiences and is the necessary condition for the application of judgment. And it was in the English craft tradition with its apprenticeship and its life of continual perfection that Harvey found the most secure formation of this intuition and judgment.

Such judgment may be grounded in the knowledge that derives from sensory experience, but the senses do not speak; only the inquirer speaks and then only with the mediation of an intellect. Baroque art mediates the production of knowledge too and in this respect Harvey's mediating intellect may also be called Baroque. But Harvey's way of inquiry requires no forced synthesis of art and order, no deliberate borrowing of the one to bolster the other. Harvey's art and order act together. The order that is part of practice offers the possibility of an art but only because such an art relies on a prior order; order as a separate domain cannot create an art of intuition. If there is a Baroque-ness to Harvey then, it is not the Baroque of Gal and Chen-Morris, but a variant Baroque, a more resistant strain less amenable to reduction, like Harvey's nature.

Notes

1. A notable exception is the suite of ligature experiments in De Motu Cordis to demonstrate the outward and inward flow of arterial and venous blood.
2. The letter X in his lecture notes means that he does not agree with an adjacent proposition, in this case that it is the diastole that is contractionary of the heart.
3. The word 'gossip' is a corruption of the words 'God's sibling' and in this context meant a woman, typically a friend or relative, who assisted at the birth.

References

Anon. 1656. *The compleat midwifes practice*. London: Nathaniel Brooke.

Anon. 1682. *The English midwife enlarged*. London: Thomas Sawbridge.

Barlow, Edward. [1659–1703] 1934. *Barlow's journal*, vol. I (trans: Lubbock, Basil). London: Hurst & Blackett.

Broomhall, Susan. 2004. *Women's medical work in early modern France*. Manchester: Manchester University Press.

Burton, Richard. 1681. *The apprentices companion.* London: Thomas Mercer.

Evenden, Doreen. 1993. Mothers and their midwives in 17th century London. In *The art of midwifery: Early modern midwives in Europe*, ed. Hilary Marland. London: Routledge.

Evenden, Doreen. 2000. *The midwives of seventeenth century London*. Cambridge: Cambridge University Press.

French, Roger. 1994. *William Harvey's natural philosophy*. Cambridge: Cambridge University Press.

French, Roger. 1999. *Dissection and vivisection in the European Renaissance*. Aldershot: Ashgate.

Guillemeaue, James. 1612. *The happy Deliverie of women*. London: A Hatfield.

Guy, John. 1982. The Episcopal licensing of physicians, surgeons and midwives. *Bulletin of the History of Medicine* 56: 528–542.

Harvey, William. [1616 as amended] 1964. *Prelectiones Anatomie Universalis lectures on the whole of anatomy* (trans. and ed. from Harvey's original manuscript by Gweneth Whitteridge). Edinburgh: E. & S. Livingstone.

Harvey, William. [1628] 1976. *An anatomical disputation concerning The movement of the heart and blood in living creatures* (trans: Whitteridge, Gweneth). Oxford: Blackwell Scientific Publications.

Harvey, William. [1636] 1958. A letter to Caspar Hoffman, in a selection of distinguished ones sent to him, and published in Nuremburg. In *De Circulatione Sanguinis* (trans: Franklin, Kenneth). Oxford: Blackwell Scientific Publications.

Harvey, William. [1649] 1958. A second essay to Jean Riolan. In *De Circulatione Sanguinis* (trans: Franklin, Kenneth). Oxford: Blackwell Scientific Publications.

Harvey, William. [1651a] 1958. To Paul Marquard Schlegel, of Hamburg. In *De Circulatione Sanguinis* (trans: Franklin, Kenneth). Oxford: Blackwell Scientific Publications.

Harvey, William. [1651b] 1981. *Disputations touching the generation of animals* (trans: Whitteridge, Gweneth). Oxford: Blackwell Scientific Publications.

Harvey, William. [1653] 1958. To the excellent and learned Doctor Giovanni Nardi, of Florence. In *De Circulatione Sanguinis* (trans: Franklin, Kenneth). Oxford: Blackwell Scientific Publications.

Harvey, William. [1654–5] 1958. To Johann Daniel Horst, Chief Physician of Hesse-Darmstadt. In *De Circulatione Sanguinis* (trans: Franklin, Kenneth). Oxford: Blackwell Scientific Publications.

Harvey, William. [1655] 1958. To the distinguished and eminent gentleman, Johann Daniel Horst, Chief Physician of Hesse-Darmstadt. In *De Circulatione Sanguinis* (trans: Franklin, Kenneth). Oxford: Blackwell Scientific Publications.

Lane, Joan. 1988. Provincial medical apprentices and masters in early modern England. *Eighteenth Century Life* 12: 14–27.

Long, Pamela. 2001. *Openness, secrecy, authorship*. Baltimore: Johns Hopkins University Press.

Markham, Gervase. 1617. *Cavalarice*. London: Edward Allde for Edward White.

Maubray, John. 1724. *The female physician*. London: James Holland.

Nutton, Vivien. 1999. A diet for barbarians. In *Natural particulars*, ed. Anthony Grafton and Nancy Siraisi. Cambridge: MIT Press.

Parey, Ambrose. [1579] 1634. *The workes of that famous Chirurgion Ambrose Parey.* (trans. from the Latin by Thomas Johnson). London: Th. Cotes and R. Young.

Schmitt, Charles B. 1989. William Harvey and Renaissance Aristotelianism a consideration of the Praefatio to *De Generatione Animalium* (1651). In *Reappraisals in Renaissance thought*, ed. Charles Webster. London: Variorum Reprints.

Sharp, Jane. [1671] 1999. In *The midwives book*, ed. Elaine Hobby. Oxford: Oxford University Press.

Smith, Pamela. 2004. *The body of the Artisan*. Chicago: University of Chicago Press.

Turner, Stephen. 1994. *The social theory of practice*. Cambridge: Polity Press.

Wear, Andrew. 1983. The way of the anatomists. *History of Science* 21: 223–249.

Wilson, Adrian. 1995. *The making of man-midwifery: Childbirth in England 1660–1770*. Cambridge: Harvard University Press.

Part III
Excess

Chapter 9
Crossing the Pillars of Hercules: Francis Bacon, the Scientific Revolution and the New World

John Gascoigne

FRANCISCI
DE VERULAMIO,
Summi Angliæ
CANCELLARII,
Instauratio
magna.

Multi pertransibunt & augebitur scientia.

Anno 1620.

LONDINI
Apud Joannem Billium
Typographum
Regium.

Sim: Pass: sculp

J. Gascoigne (✉)
School of Humanities, University of New South Wales, Sydney, NSW, Australia
e-mail: j.gascoigne@unsw.edu.au

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_9,

Abstract The significance of the connection between the discovery of the New World and scientific discovery has been one that has been remarked on since the time of Francis Bacon. The article assesses such claims made by Bacon and his contemporaries in the light of the recent historiography of the subject. In doing so it analyses a number of the notable features of the Scientific Revolution: the scientific role of the pioneering exploring nations, Spain and Portugal; the place of natural history in the formulation of early modern science; and the interconnections between the sciences associated with navigation and the theoretical developments that were most central to our accepted understanding of the Scientific Revolution. The article concludes with an analysis of the limitations of the term "Scientific Revolution" in accommodating the scientific discoveries associated with the discovery of the New World particularly in the realm of natural history with its accumulation of detail which was so much a part of the Baroque mentality.

Introduction

Few illustrations capture better the high hopes held for the infant scientific movement of the seventeenth century than the frontispiece to Francis Bacon's *Great Instauration* (1620). After long centuries in awe of the achievement of the Ancients, Europe now moves beyond their realm symbolised by the Pillars of Hercules, the promontories that flank the entrance to the Strait of Gibraltar. The pillars which once marked the end of the known world now massively frame an instance of the ingenuity and might of the Moderns in the form of a top-of-the-line vessel with its sails swelled out returning, one assumes, from a successful and lucrative voyage to the New World. In the distance is another vessel beginning to make its way in the opposite direction, another assertion of the Moderns' command of oceans and whole tracts of the globe virtually unknown to the Ancients. Beneath the illustration is the motto, "Multi pertransibunt & augebitur scientia" (Daniel 12:4)—"Many shall go to and fro, and knowledge will be increased," a highly charged reference to the chiliastic text from the Book of Daniel with its reference to the "last ages of the world" and to mystical revelations.

The fact that Bacon chose such an illustration for a work dedicated to the advance of the scientific principles which he saw as replacing those derived from antiquity makes apparent the extent to which Bacon discerned a parallel between the discovery of the New World and the advance of the new science. Both involved going beyond the boundaries set by Antiquity and both seemed to offer the possibility of advances in human (or, at least, European) control over the natural realm. It was a parallel that Bacon spelt out clearly and repeatedly in his voluminous works. The opening address to the king of the second book of his *The Advancement of Learning* (1605) takes up the same image by asking "For why should a fewe received Authors stand vp like *Hercules Columnes*, beyond which, there should be no sayling, or discovering" (Bacon 2000a, 55). The *New Organon* (published in conjunction with the *Great Instauration* in 1620)—with its aspirations of creating a new form of logic to replace what Bacon considered to be the barren one of the scholastics based on the work of Aristotle—compared his laying out of a new method with the way in which Columbus "before his epic voyage across the Atlantic … gave reasons why he believed he could discover new lands and continents beyond those known then." The work expresses the hope that the advance of science will be encouraged by "the breath of hope blowing from *that new continent*" and, echoing the millennial hopes embodied in the motto to his *Great Instauration,* Bacon again referred to the same quotation from the book of Daniel while suggesting that "it was fated (i.e. Providence so arranged it), that thorough explorations of the world … and the growth of the sciences would meet in the same age" (Bacon 2004, 151, 173, 151).

Such a fascination with the discovery of the New World ran very deep in early modern society. The voyages of Columbus and their aftermath were for many a shaping narrative of their age. When López de Gómara dedicated his *General History of the Indies* to Charles V in 1552 he wrote excitedly: "Except for the incarnation and death of the Creator, the greatest event since the creation of the World was the discovery of the Indies; and thus they are called the New World" (cited in De Asúa and French 2005, 74). The idea of discovery seeped into the fabric of intellectual discourse and became one of its informing ideas. Indeed, as Reiss argues (2005), for Bacon the notion of discovery as a form of scientific advance was in many ways an extrapolation from the Columbian experience. Columbus had enlarged the realms of the king of Spain and, so, too, the philosopher should enlarge the realm of knowledge to the benefit of his king and humanity more generally. As Bacon was well aware, discovery, whether geographical or scientific, brought with it an expansion of power and particularly the power of the realm which it was one of the functions of Baroque culture to celebrate and demonstrate. For Bacon royal patronage of science was part of a broader Baroque emphasis on using royal power to hold together an increasing complex polity which was expanding its sway across the globe—a development which the Iberian nations had pioneered.

The resonance of the term "discovery" was manifest not only in the writings of Bacon but also in that of many other scientific authors of the early modern period. When Bacon's near contemporary, the Dutch diplomat, Constantijn Huyghens, father of the great Christiaan, sought to convey the wonder and possibilities of the magnifying glass, he naturally reached for an analogy with geographical discovery: "we wander through a world of tiny creatures, till now unknown, as if it were a newly discovered continent of our globe" (cited in Dear 2001, 129)—a metaphor also later employed by Hooke in 1665 when he similarly described the possibilities of the microscope which enabled "a new visible World, [to be] discovered to the understanding" (cited in Sorrenson 1996, 221). In the Restoration period Joseph Glanvill in his *Vanity of Dogmatizing* (1661) emphasized the advantages to be obtained by following the method of the new science (as largely laid out by Bacon) by again drawing an analogy with the riches of the New World: "there is an *America* of secrets, and an unknown *Peru* of Nature, whose discovery would richly advance the [Arts and Professions]" (cited in Alexander 2001, 18). John Ray in his *Wisdom of God Manifested in the Works of Creation* (1691) directly echoed Bacon in contending: " Let us not think that the Bounds of Science are fixed like Hercules's

Pillars and inscribed with a Ne Plus ultra" (cited in Parrish 2006, 71). This contemporary preoccupation with the accord between the discovery of the New World of the Americas and the New World of science has received some attention in the literature on the Scientific Revolution though not as much as it merits. Taking as its focus Bacon and the Baconian heritage, the object of this paper, then, is to draw on the existing historiography of the subject in order to analyze some of the key issues and to provide a foundation for further discussion of the linkages between geographical and scientific discovery in early modern Europe.

The Scientific Revolution and the "Iberian Question"

Though the humanistic study of the Ancients and their texts could in some contexts be a stimulus for scientific innovation its emphasis on traditional authority and text-based learning could also detract from that first-hand empirical enquiry on which the Bacon placed such emphasis—though he himself owed much to humanism. One theme which emerges from some of the existing literature, then, is the importance of the discovery of America as a force for undermining that veneration and deference to the Ancients to which Bacon had alluded in his call for the Moderns to go beyond the Pillars of Hercules. Hooykaas (1987), for example, argues that the innovation which geographical discovery required in the realm of navigation and the rich treasure trove of new specimens of natural history that were brought back from the New World diverted some currents within humanism which had the tendency to sweep intellectual life back, behind the Pillars of Hercules, into the realm of the Ancients. The encounter with the New World, he insists, prompted a greater emphasis on first-hand experience thus weakening the traditional reliance on textual authority which was chiefly the fruit of the scholastic tradition but also was confirmed by some forms of humanism. Such reliance on texts was later to be challenged by the Royal Society's motto, "Nullius in verba" ("On the word [or text] of no-one"). Thus Hooykaas cites the Portuguese explorer, João de Castro, who, against the speculation of the Ancients on the non-existence of the Antipodes, argued that the experience of navigators now showed that it had become "a thing most conformable to reason"—a moral later pointed by Ramus in his anti-Aristotelian writings on logic in which he argued that scholars "of so many ages did not know what navigators, merchants, uneducated people learned not by arguments but through experience" (cited in 1987, 459–60). To similar effect Eamon quotes the French explorer, Jacques Cartier, remarking that "The simple sailors of today have learned the opposite of the opinion of the philosophers by true experience" (cited in 1994, 272). Such considerations have prompted Goodman and Russell to make the claim that the shock of the discovery of the New World and the scientific as well as material riches which this yielded was the factor which "most stimulated the rise of modern science in Europe" (1991, 417).

Such strong statements of the view that the Spanish "discovery" of America and the near contemporary passage beyond the Cape of Good Hope by the

Portuguese were one of the major catalysts in generating the scientific advances to which we retrospectively give the name the Scientific Revolution have, as we have seen, considerable support within the early modern world. Certainly, Bacon thought there was a close connection. But the emphasis on the maritime achievements of the Iberian nations immediately raises an obvious difficulty. Why was it that the Iberian peninsula, which was most involved in the early expansion of Europe, was also one of the areas of Europe traditionally thought to be least involved in the Scientific Revolution? Recently a number of works have sought to counter this scientific version of the "Black Legend." Both Barrera-Osorio and Cañizares-Esguerra, for example, have emphasized the importance of the early Iberian scientific encounter with the New World arguing that this provided a model for subsequent European scientific activity especially in the value accorded to empirical observation, the willingness to break with the authority of the Ancients and the development of scientific institutions to deal with this flood of new information. Indeed, they go so far as claiming that the origins of the Scientific Revolution should be sought in the Spain of the 1520s (Barrera-Osorio 2006, 2, 2007; Cañizares-Esguerra 2004, 2006, 14).

However, revisionism is here perhaps running too fast. While the stimulus of navigational innovation there and contact with the natural history of the New World did prompt some scientific activity in the Iberian world it did not create a strong continuing scientific tradition. More persuasive is Goodman's summation of the trajectory of scientific activity in both Spain and Portugal as being one of "sixteenth-century activity, seventeenth-century stagnation followed by a campaign to rescue the countries from the darkness of scientific backwardness" (1992, 171). Such an overview is confirmed by prosopographical studies of scientists included in the *Dictionary of Scientific Biography*: of the cohort born between 1450 and 1550 10 % derived from Spain and Portugal but this drops to 2 % both for the period 1551–1650 and 1660–1760 (Gascoigne 1990, 250, 1995, 579).

Discussions of this scientific decline have traditionally centred around the impact of religious reaction. This, however, has to be qualified by the fact that Italy remained of considerable scientific importance even though subject to similar (if less pronounced) ideological pressures: the comparable percentage figures for Italy for the same three periods are 35, 16 and 8—evidence of decline (albeit from a very high base) but nonetheless of a still vital scientific culture especially given the fact that Italy was under increasing foreign domination. Moreover, if the impact of the New World was so important as a stimulus for scientific culture why was Italy very much to the fore in the early stages of the Scientific Revolution—even though it did not directly sponsor voyages of discovery beyond the Mediterranean nonetheless? Italy's subsequent relative scientific decline can also be explained by a more general economic decline since it lost much of its traditional Mediterranean-based trade to the Atlantic.

Yet Spain had the advantage of having both Atlantic and Mediterranean ports which also raises another important feature of early modern Spain which perhaps explains why it did not sustain the scientific momentum which early contact with the New World brought in its train. As Cook's recent major work on Dutch science in the seventeenth century has emphasized there were important points of congruity between trade and science: both flourished through the establishment of networks of information and exchange and both valued accurate information and practical advantage (2007, 57, 58, 225, 410). Though Spain's incorporation of the New World brought great riches it did little to promote trade at home since regional variations and aristocratic and clerical privilege stood in the way of the growth of a national and, still more, an international market leaving much of Spain's economy in foreign hands. Spain, contemporaries commonly acidly remarked, "has become an Indies for the foreigner" (cited in Kamen 1978, 43). The sluggishness of Spanish science and Spanish trade—despite the initial impetus occasioned by contact by the New World—were remarkably parallel and perhaps reflect similar factors as work. The astonishing expansion of the Spanish Empire brought with it the opportunity to sustain traditional aristocratic and clerical values which were antithetical to both trade and science. Similarly it has been argued that the new realms acquired by Spain and Portugal actually helped retard their political and intellectual development since the need to rule such vast territories and bring them within the fold of Catholic Christendom prompted an expansion of state and bureaucratic power and an increasing preoccupation with religious orthodoxy (Scammell 1969, 406, 410).

The Iberian World and the Growth of Natural History

Such an assessment of the scientific role of the Iberian nations does reflect, however, a view of the Scientific Revolution which views it, in the post- Koyréan manner, as being at root a conceptual shift in the way in which the cosmos was understood. It is a view of scientific change which accords well with the notion of a "revolution" which brought with it a fundamental shift in world view and it gives particular prominence to the mathematical sciences where such a transformation is most apparent. Such a conception of the Scientific Revolution has been greatly reinforced by Kuhn's enormously influential notion of a paradigm shift which, appropriately, emerged from his study of Copernicus. Yet, Kuhn (1997) also reminds us in his important article, "Mathematical versus Experimental Traditions in the Development of Physical Science," that the sciences are not necessarily one and, particularly in the early modern period, natural history (or what he calls the "Baconian sciences") followed a different trajectory from the mathematical sciences which had been transmitted from the Ancient World. Our understanding of the impact of the discovery of the New World needs to take account of such a distinction which helps to go some way to explaining why there was no neat correlation between the advance of exploring nations and the advance of science within them.

The limitations on the growth of the networks and habits of mind promoted by trade—together with the force of religious and political orthodoxy—may have made Spanish intellectual life relatively impervious to the theoretical advance of the mathematical and astronomical sciences which we have traditionally put at the forefront of our view of the Scientific Revolution. Nonetheless, the Spanish empire supported a considerable degree of activity devoted to natural history. It thus played a significant part in the accumulation of scientific knowledge—an important aspect of the early modern scientific movement which Cook has underlined in his call for more studies which, as with his work on the connections between Dutch trade and science, balance the existing preoccupation with the production of knowledge (2007, 411). He also underlines the importance of exchange of knowledge in which trade played a major part. Here the obstacles to the growth of more buoyant commercial networks within the Iberian empires, together with their strong tradition of state secrecy, lessened the impact of the information they acquired from the New World. However, other forms of exchange such as the international networks created by religious orders such as the Jesuits or the Franciscans did help to bring aspects of the natural history of the New World to the attention of the European republic of letters more generally. In doing so they contributed to a Baroque imagination which gloried in the profusion of detail while at the same time seeking to subdue it with organizing principles such as classificatory systems. Such ordering did not, however, require the degree of philosophical revision that the advances in the mathematical and physical sciences brought in their wake—a further consideration which explains the strength of natural history in the Iberian world.

Natural history could also be readily justified both in religious and imperial terms. Spanish students of natural history could argue (as Bacon and his disciples later frequently did) that such pursuits brought them closer to the Creator through a study of his Creation. Another Baconian theme was that natural history brought with it the possibility of economic advance by enabling a more effective exploitation of Nature. This was a familiar *topos* in the Spanish empire: when, for example, in 1570, Philip II sent his royal physician Francisco Hernández to New Spain on the first major European natural history expedition it was with a view to obtaining useful medical information as well as forming part of a scientific consolidation of Spanish claims to this territory (Barrera-Osorio 2006, 17, 134). As recent scholarship has emphasized, Bacon himself owed considerable debts to the Spanish among them the model for his Pillars of Hercules frontispiece (based on Andrés Garcia de Céspedes' *Regumento de Navegación* (Madrid 1606)) and possible parallels between *The New Atlantis* and the 1606 memorial of Pedro Fernando Quirós (Cañizares-Esguerra 2006, 12, 22).

The extent of common ground between Bacon and the Spanish naturalists is brought out by the work of the Spanish Jesuit, José de Acosta (1539–1600), whose *Natural and Moral History of the Indies* (1590) was one of the most influential accounts of the natural history of the New World. Acosta saw his work as an indication of the limits of the wisdom of the Ancients and the importance of first-hand experience (Ford 1998; De Asuá and French 2005, 88–9). Whereas the Ancients considered the Antipodes to be "the burning Zone" Acosta affirmed in his prefactory "Advertisement to the Reader" that "contrarie to the antient and received Philosophy" they were in fact "very moist, and in many places very temperate." Later, he again sided with the Moderns by stressing that the new continent "aboundes in grasse, pastures, and forests, contrary unto what *Aristotle* and the Auntients did holde." He stressed, too, the consonance between his form of science and religion since "having knowledge of the works of nature, which the wise Author of all nature made, we may praise and glorifie the high God who is wonderfull in all things and all places." The theme of the achievements of the Moderns in surpassing the Ancients—an important part of the linkage between geographical and scientific discovery—is one to which Acosta frequently returns: the Ancients had no conception that "this new found world was peopled by any Nation" nor could they since "the Ancients had never knowledge in the arte of Navigation, without the which they could not runne so far into the sea." For Acosta contact with the New World brought with it that greater openness to observation and experience about which Bacon later wrote so eloquently: "If in naturall and phisciall things, we must not seeke out infallible and mathematicall rules, but that which is ordinarie and tried by experience, which is the most perfect rule. " Like Bacon, too, he regarded his work as a contribution to natural history since it provided a factual basis on which others could build since "it is a Historie, and no fables" (Acosta 1604, 1, 183, 44, 99, 496). For Acosta, as for Bacon, the term "history" brought with it the connotations of reliable knowledge—part of the accumulation of data on which theory could be erected—since its etymology went back to the Greek world for "learning by inquiry" (Cook 2007, 21). Appropriately, Bacon was to draw on Acosta's work in his own writings on natural history (Irving 2008, 42–3). In his *History of the Winds* (part of his *Great Instauration*) Bacon noted, for example, that "In Peru, which is a very windy place, Acosta observes that the winds blow most when the Moon is full" while in his *New Organon* he cited Acosta on the behaviour of the tides (Bacon 2004, 321, 2007, 71).

Spain might have continued to fall behind in the promotion of what we commonly see as the central aspects of the Scientific Revolution—above all the heliocentric universe, the replacement of Aristotelian physics and the promotion of mathematisation of Nature—but the sort of natural history tradition exemplified by Acosta remained part of the fabric of Spanish imperial rule. Possessing the New World meant effectively describing and cataloguing its natural and human resources (Smith and Findlen 2002, 17–18; Pimental 2000, 19). Francisco Hernández's great naturalizing expedition which began in 1570, 20 years before the appearance of Acosta's work, set the tone for future scientific expeditions around the globe by bringing with him painters and engravers (Pimental 2000, 22). In the seventeenth century the decline of Spanish power was reflected in a decline in state-sponsored naturalizing but some of this work continued locally in a variety of settings including universities and religious houses. The eighteenth-century Bourbon revitalization of the Spanish Empire brought with it greater attention to science. Among the reforming measures of Charles III was to be the foundation in Madrid of a Museum of Natural Science in 1771 (Engstrand 2000, 173) and a Royal Botanical Garden (Cañizares-Esguerra 2003) which received exhibits from a growing number of scientific expeditions within the Spanish empire—some 57 expeditions being dispatched by the Crown from 1760 to 1808 (Lafuente and Volverde 2004, 136). When naturalizing in South America between 1799 and 1804 Alexander von Humboldt was to express his admiration for the scientific patronage of the Spanish Crown by remarking that "No European government has laid out greater sums to advance the knowledge of plants than the Spanish government" (Steele 1964, vii).

The Spanish Crown was often reluctant to share such information with others (Puig Semper 1994, 602)—significantly, the journals of Hernández's pioneering expedition were kept under lock and key (Ogilvie 2006, 210). A drastically abridged and very belated version of his vast survey of the natural history of Mexico was published outside Spain in 1651 in Rome thanks to members of the Academia dei Lincei (Cañizares-Esguerra 2004, 99). Despite such secretiveness on the part of the Spanish state, enough data about this new quarter of the globe became public to weaken traditional attitudes to the study of Nature. From the time of Columbus's first voyage reports of new animals made their way back to Europe (George 1980, 80). The growing number of plants and animals unknown to the Ancients further weakened the hold of ancient authorities (Barrera-Osorio 2006, 102; Rossi 1991, 164). It also weakened traditional ways of understanding Nature in which much stress was placed on what Ashworth has termed the emblematical significance of a particular object. The flora and fauna that arrived from the New World came without the long associations and symbolic overtones that had been built up around their European equivalents (Ashworth 1990). The experience of contact with the vast array of natural objects that the New World showered upon an entranced European audience promoted closer scrutiny of individual objects, rather than their more generalized significance, thus fostering a more empirical approach to the study of the natural world (Barrera-Osorio 2002, 175)—an approach that was to be exemplified in Bacon's work.

The rich stores of data from the New World helped shape, for example, the outlook of Bacon who drew on American material in compiling natural histories with a near global reach. In his *The History of Dense and Rare* (one of a number of natural histories which formed part of his *Great Instauration*), for example, he alluded to types of plant from the West Indies and the Azores which had in common the fact that they could store water while in *Sylva Sylvarum* he refers to the effects on human digestion of mechoacham, a root from Mexico (Bacon 1857–74, II, 355, 2000b, 151). As we have seen, he drew on Acosta in his account of the winds in South America and it is to him and other Iberian authors to whom he was indebted in his *On the Ebb and Flow of the Sea* for his account of the pattern of the tides in Florida, Peru and the way in which "the flow of waters" was "stronger towards the Straits of Magellan, where there is a way out to the west" (Bacon 2000b, 73, 81).

More fundamentally, Bacon was also responding to the fact that the sheer volume of data that the study of natural history was generating at home and abroad required an approach to learning that required different ways of sharing knowledge than those employed by the humanist scholar or the scholastic philosopher. The ever-growing tide of specimens also required institutions which could play a part in authenticating not readily accessible data—particularly if they came from foreign lands (Dear 2001, 124). There was so much to be done that the only effective approach was a communally-based assault on the secrets of Nature. True science, as Bacon realized, required co-operative endeavour (Sargent 1996) and there had been some moves in that direction within the Spanish empire. As an enthusiastic advocate for the role of the state in the promotion of science Bacon seems to have had some sympathy for the innovative measures that the Spanish empire took in building up a bureaucratic machine to deal with its vast domains. In his *Valerius Terminus or The Interpretation of Nature* (written some time before the

Advancement was published in 1605), for example, when discussing the ways in which the specialist labour of all the different branches of learning might nonetheless be drawn together, he turns to the Spanish example as an instance. Thus he contends for "an administration of knowledge in some such order and policy as the king of Spain in regard to his great dominions." For this ruler was able to keep together the different spheres of activity since "though he hath particular councils for several countries and affairs, yet, hath one council of State or last resort, that receiveth the advertisements and certificates from all the rest" (Bacon 1857–74, III, 231). In his depiction of a model scientific community in *The New Atlantis*, Bacon describes something very like a religious order such as the Jesuits with the scientists working together and having an almost priestly role. The Spanish overtones of the book are strengthened, too, since the voyage to the lost island begins with a departure from Peru into the Pacific and the inhabitants of the New Atlantis speak Spanish (Cañizares-Esguerra 2004).

The Spanish encounter with the New World, then, brought with it European access to a vast array of new natural history specimens and information which prompted a reconsideration of traditional modes of understanding the significance of the natural world. Possibly, too, the swelling volume of such specimens prompted figures such as Bacon to contemplate new ways of organizing the scientific endeavour the better to master the ever-growing extent of natural history. There remains the question, however, of the extent to which this New World encounter can have been said to have stimulated the currents of thought we associate with the Scientific Revolution. The fact that the Spanish themselves remained active proponents of natural history in their vast territories without any strong move to break with traditional forms of scientific explanation does suggest that natural history was not a strong solvent of tradition. True, by calling attention to so much that was unknown to the Ancients, it did call into question the hold of traditional authorities. But in early modern times natural history was too descriptive and lacking in a widely shared body of theory to provide an alternative world view. It was not until the eighteenth century that the great systems of classification were developed and articulated and, with these, natural history achieved a greater degree of system and a mastery over its multifarious materials.

Some have disputed, too, whether the impact of the New World specimens fundamentally changed the practice and modes of explanation employed by European naturalists. Both George (1980: 100) and Ogilvie (2006, 143), for example, argue that the exotic examples from outside Europe were integrated into forms of exposition and organization that had already been built up around the study of European flora and fauna. This was highly important in imperial terms as an instance of the way in which European knowledge systems were taking on a universalizing aspect as local forms of knowledge from around the globe were translated into terms familiar to Europeans and amenable to European exploitation (Cook 2004, 100–101, 118). But how far can it be said that data for the New World was a necessary stimulus for scientific change? Recently Cooper has presented a persuasive case for the view that one of the major impacts of the Columbian voyages was to stimulate the study of natural history at home in Europe. Knowledge of the exotic led to a growing preoccupation with the study of the diversity of the natural world which could be studied nearby. This brought with it an increasing emphasis on the strange and unusual and on the importance of particular instances, attitudes of mind in tune with the Baroque imagination. This could promote a fascination with the bizarre and exceptional which could be corrosive of existing systems of natural philosophy (Cooper 2007, 3, 10).

Daston has also argued similarly for the importance of the study of prodigies and portents as a way of provoking reconsideration of accepted scientific orthodoxies and a system of natural history which was concerned with the norm rather than the exception. As she points out, Bacon was particularly important in promoting the study of such materials arguing in the *New Organon* that it was important to collect "*Deviating* instances such as the errors of nature, or strange and monstrous objects" (cited in Daston 1994, 261). No doubt the arrival of exotic specimens strengthened such a preoccupation with the bizarre and the unusual but one could find plenty of such instances without going abroad. In sum, the New World might have added to the breadth of the domain of natural history but it did not necessarily add to its theoretical depth. The strangeness of these novel specimens did raise issues about the way in which they should be classified which had larger ramifications for systems of classification more generally but they did not, of themselves, prompt the conceptual shifts which we have come to associate with the Scientific Revolution (Sloan 1996, 296).

The view that it was not the task of natural history to enter into the territory of the philosopher and theoretician was one which Bacon himself held strongly. In his *Advancement of Learning* he had argued that natural history was the essential but in some senses subordinate pre-requisite for "physics" (in its seventeenth-century sense as a synonym for natural philosophy). For him the appropriate division of labour was that "NATVRAL HISTORY describeth the *varietie of things*; PHISICKE [physics], the CAVSES, but VARIABLE or RESPECTIVE CAVSES" (Bacon 2000a, 82). Such views were further developed in the *Description of the Intellectual Globe* (1612) in which he viewed natural history as "the primary matter of philosophy" contending that "the noblest end of natural history is this; to be the basic stuff and raw material of the true and legitimate induction" (Bacon 1996, 105). In the *New Organon* he again presented a crucial but nonetheless subordinate role for the natural historian in promoting the endeavours of the natural philosopher: "So we should have good hopes of natural philosophy once natural history (which is its basis and foundation) has been better organized, but none at all before." In developing such a soundly based natural history as the foundation for true philosophy he accorded considerable importance to the fruits of exploration: "Nor should this fact count for nothing: that by prolonged voyages and journeys (which have become prevalent in our times) many things in nature have been disclosed and found out which shed new light on philosophy" (Bacon 2004, 157, 133).

Such a handmaiden view of natural history was to some extent challenged in the physical sciences—ironically particularly in the Royal Society where Bacon was so greatly revered. Robert Boyle, for example, went beyond Bacon's simple divide between the natural historian and the natural philosopher since he regarded his experimental natural history as informing his theory and, reciprocally, theory as posing questions for the natural historian (Anstey and Hunter 2008, 107, 110, 126).

As the mathematical and physical sciences became more and more equated with natural philosophy thanks to their growing theoretical sophistication the term "natural history" became closely identified with what later became the life-sciences. Within this scientific realm Bacon's influence did much to perpetuate the role of the natural historian as a collector rather than a philosopher. Indeed this ancillary role was not challenged fundamentally until the late eighteenth-century when natural historians acquired greater self-confidence with the development of wide-ranging systems of classification. By the time of Buffon natural historians were beginning to claim a greater role in the actual formulation of scientific knowledge and its underlying causes. In the "Initial Discourse" to his vastly influential *Histoire Naturelle* (1759), for example, Buffon sketched a nobler role for the natural historian than being a mere compiler of facts: as a natural historian one should not "limit oneself solely to the making of exact descriptions and the ascertaining of particular facts." Rather the natural historian had the right to venture into the territory once reserved for the philosopher by "rais[ing] ourselves to something greater and still more worthy of our efforts, namely: the combination of observations, the generalization of facts, linking them together by the power of analogies, and the effort to arrive at high degree of knowledge." By so doing, he continued, "we are able to open new routes for the further perfection of the various branches of natural philosophy" (Lyon and Sloan 1981, 121).

Until natural history was so reconceptualised its role as a promoter of scientific change was limited. The Baconian view of the natural historian as the handmaiden of the philosopher meant that natural history as such could only play a limited role in the transformations we associate with the Scientific Revolution. The impact of the Columbian voyages greatly extended the range of examples on which the natural historian could draw and prompted reconsideration of existing patterns of classification and the authority of the Ancients. It also prompted greater preoccupation with first hand observation and with the empirically-based study of particular specimens including the bizarre and exceptional—preoccupations which meshed well with the mentality of the Baroque. The need to cope with the mounting piles of evidence thus accumulated was also a stimulus to reorganize science along more communal and co-operative lines. Such changes helped to stimulate scientific enquiry which was less in awe of tradition but the limited role accorded to natural history meant that it was more a part of the chorus than a soloist in the grand opera of the Scientific Revolution.

The Impact of the Navigational Sciences

Along with its importance as a stimulus for the study of natural history the great voyages of exploration also impinged on the mathematical sciences especially those concerned with navigation. The outcome was another branch of natural history in Bacon's sense—meshing well with his hopes for a history of trades—with much detailed empirical data ready to be crushed in the wine press of philosophy to make a more heady (and theoretical) brew. The fact, however, that navigation and the mathematical sciences (including astronomy) were close cousins did, however, make for a closer connection with the scientific advances which shaped the cognitive shift to which we give the name, the "Scientific Revolution."

Of course, navigation of an increasingly sophisticated kind existed in Europe before Columbus. Undoubtedly, however, the Portuguese long-distance sailing around Africa and that of the Spanish across the Atlantic provided an enormous stimulus in the development of navigation skills and ship design (culminating in the late sixteenth-century galleon, chiefly a Spanish invention (Edwards 1992)). Without the mathematical skills needed to calculate latitude the Portuguese long-distance voyages would have been impossible (Bennett 1991, 179). The need for accurate observations on which lives were dependent was a great incentive to subject factual information to the scrutiny of direct observation unclouded by traditional theoretical assumptions. The great Portuguese navigator, Duarte Pereira, wrote in his treatise on the subject, *Esmeraldo de Situ Orbis* (1505–8), for example, that "Because experience is the mother of things by it we discovered the very root of truth" (cited in Reiss 2005, 233). Moreover, the Iberian nations grasped the importance of collective endeavour in this branch of knowledge and, to that extent, prefigured the role of later scientific societies. The Portuguese Casa da Mina in Lisbon and the Spanish Casa de la Contratación (established at Seville in 1503) both attempted to draw together navigational information from across their wide realms and to provide maps and other data to assist their pilots (Turnbull 1996). Importantly, the chief official at the Casa de la Contratación, the Piloto-Mayor, was (at least for the first 50 years of the institution's existence) himself an experienced navigator (Waters 1967, 213) rather than being drawn from the sort of textual training offered by the expanding Spanish universities.

We return again, then, to the "Iberian question" —why if Portugal and Spain were at the forefront of this scientific aspect of the expansion of Europe did not they not make more of an impact on the developments we associate with the Scientific Revolution? One obvious response was the compulsive secrecy of Iberian officials anxious to keep for themselves any navigational data which might gain advantages for their country. Without greater public dissemination of the fruits of their exploration, it could be argued, such material was unlikely to fertilize scientific enquiry. This plausible hypothesis does, however, need to be qualified by the fact that, though the Iberian nations were certainly not willing to share any information that might help their commercial and strategic competitors, they did produce many of the best textbooks on navigation more generally—from the 1560s these began to spread beyond Spain and, having been translated into a range of European languages, remained in common usage for the next century (Waters 1967, 216).

But for navigational treatises to yield scientific fruit they needed not only the undoubtedly important contribution of mariners who could observe at first hand but also they required to be integrated into scientific theory. Such a development did not come readily to the Spanish or Portuguese universities and intellectual culture more generally with its strong commitment to preserving scholastic orthodoxy—the better to defend religious orthodoxy. Even within the Casa de la Contratación, which was largely staffed by men who had first-hand experience of the sea, the pressure to conform to prevailing theoretical canons was strong. The attempt to impose standard "pattern" charts which conformed to generalized geographical theory led to a protest in 1545 by 50 pilots about the imposition of charts "made by people who have not sailed, nor understood the art of navigation, nor have experience of it, nor have seen the lands or the coasts, bays and islands" (Sandman 2002, 90)— but the protest was in vain and thereafter only the official charts were permitted to be used.

While Spain may not have promoted a strong linkage between navigation and the sciences Bacon had high hopes for the scientific dividend to be obtained from the very considerable navigational advances which were occurring in his age. Indeed, in his *Advancement of Learning* he saw the conjunction of the voyages of exploration and the early stirrings of the scientific movement as Providentially ordained: "And this Proficience in Nauigation, and discoueries, may plant also an expectation of the furder proficience, and augmentation of all Scyences, because it may seeme they are ordained by God to be *Coevalls*, that is, to meete in one Age." Later in the work he returned to the association between the advances in navigation and of science with an extended parallel between the way in which the West Indies would "had not been first discovered, if the vse of the Mariners needle, had not been first discovered" and how science could not move into new realms "if the Art it selfe of *Inuention* and *Discouerie*, had not been passed ouer" (Bacon 2000a, 71, 107–8).

Bacon's hope that the progress of exploration and, with it, advances in navigation would provide material on which the sciences could build came some way to realization—largely because mathematics offered a ready bridge between navigation and the emerging physical sciences. As early as Copernicus's *On the Revolutions of the Heavenly Spheres* (1543) navigational advance provided material on which science pondered. In considering the size of the earth, and especially its habitable portion, Copernicus was moved to reject the authority of the Ancient, Ptolemy, since "moderns have added Cathay and other vast regions as far as 60° longitude." But more significant still were "the islands discovered in our time under the princes of Spain and Portugal and especially America … which they consider a second *orbis terrarum* on account of her so far unmeasured magnitude." Drawing together mathematics and geography he continued on to argue that "reasons of geometry compel us to believe that America is situated diametrically opposite to the India of the Ganges" (Copernicus 1952, 513).

The problems of navigation and the variation of the compass provided an impulse for the early study of magnetism—a subject about which William Gilbert's *On the Loadstone and Magnetic Bodies* (1600) was the major text. Naturally, where possible, Gilbert drew on the experience of the Iberian navigators. He valued the observations on the variation of the compass made by the Portuguese on the voyages to the East Indies, inexact though they were, but was unenthusiastic about the scientific use to which they had put such data: "whoever reads what the Portuguese have written will quickly see that in very many respects they are mistaken" (Gilbert 1952, 89). Gilbert formed part of a larger community of late sixteenth and seventeenth century English theoreticians preoccupied with the scientific problems posed by navigation who have been the subject of studies by Alexander (2001, 2002). Alexander points, for example, to the links between the development of more accurate navigational tables by figures such as William Briggs and the publication of John Napier's system of logarithms. Within this world the connections between voyaging and science were direct: the mathematician and astronomer, John Dee, studied under notable navigators such as the Portuguese Pedro Nuñez and was an active promoter of voyages such as that to the Northwest Passage as, too, was William Briggs, some decades later. Such a nexus helped to make Gresham College, London a site for both the teaching of navigation and a centre for moves for reform of natural philosophy in line with the advances being made in practical mathematics. In 1633 members of that institution urged the University of Cambridge to consider reforming their scholastic curriculum so "that the same improvement may by this meanes accrew vnto our Physicks, that hath advanced our Geography, our Mathematicks, and our Mechanicks" (cited in Bennett 1991, 189).

This tradition of linking navigation and science was to continue in England and in the late seventeenth and early eighteenth centuries reached its acme with Edmond Halley. Indeed, in the *Mathematical Principles of Natural Philosophy* (1687), Newton cites Halley's observations on the pendulum clock on his voyage to St Helena in 1677 where he found it "to go slower there than at London." These he contrasted with experiments with the pendulum clock sponsored by the Académie des Sciences on the West Indian islands of Cayenne and Granada intended to show that the force of gravitational attraction was more marked at the equator than at the poles (Reidy et al. 2007,17). Newton was also indebted to Halley for passing on observations of tides by seamen especially "in the kingdom of Tunquin [Vietnam]." In discussing the movement of tides and the associated gravitational attraction of the sun and the moon Newton could draw on the vast store of navigational data that had accumulated since the Portuguese first ventured around Africa in making statements such as "in those tracts of the Atlantic and Ethiopic seas which lie without the tropics, the waters commonly rise to 6, 9, 12, or 15 ft; but in the Pacific sea, which is of a greater depth, as well as of greater extent, the tides are said to be greater than in the Atlantic and Ethiopic seas" (Newton 1952, 293, 296, 299, 326). Such venturing into seas unknown by the Ancients reinforced in Newton's contemporaries the familiar but fundamental message that the Moderns had surpassed the Ancients. "[T]he ancients," wrote Halley in his paper to the Royal Society of 1700/1 on "The Geography of the Ancients and Moderns," "did believe the Globe to be much less than our more accurate dimensions thereof have since determined it" (MacPike 1975, 167).

The connecting thread between navigation and science lay both with astronomy and, more particularly, with mathematics. Navigation, however, was governed by practical imperatives which imposed limits on the quest for scientific information for its own sake (Alexander 2001, 2). Speculative mathematics which had no immediate and obvious practical advantage was not likely to be directly encouraged by navigation though it might use the fruits of further mathematical and astronomical advance once their utilitarian benefits became apparent. Navigation, then, was a useful stimulus to the advances we associate with the Scientific Revolution though rarely a direct patron of the pursuit of science for its own sake. The achievements of the navigators did, however, serve as one of the great master narratives of the age informing the mentality of scientists as it did other sorts and conditions of men—in doing so it promoted a degree of self-confidence and intellectual excitement and daring that found its literary representation in the Faust myth so familiar to the contemporaries of the dramatist, Christopher Marlowe. The Columbian voyages seemed to provide proof positive that the Moderns could go further than the Ancients and that it was possible in science as in geography to go beyond the Pillars of Hercules.

The reverberations of such views can be found in the writings of some of the canonical figures of the Scientific Revolution even though it is difficult to connect their science directly to the impact of the New World. When, for example, Kepler wrote to Galileo about their mutual quest to find astronomical support for the Copernican system he did so in terms drawn from the language of exploration: "How great a difference there is between theoretical speculation and visual experience, between Ptolemy's discussion of the Antipodes and Columbus's discovery of the New World." In reaching for some way of expressing the importance of the scientific advances being made contemporaries likened natural philosophers to explorers: the Neopolitan man of letters, Giambattista Manso, wrote in 1610 that "Ptolemy had been judged to be a new Hercules beyond whose limits it was impossible to go" but, as he told Galileo, "you may count yourself almost a new Columbus" (cited in Pagden 1993, 91, 98).

Conclusion

As the eighteenth century progressed the mathematical forms to which the navigational sciences lent themselves began to spread to natural history—in part in response to voyaging both within Europe and abroad. Bourguet's work has underlined the importance of the way in which natural history took on an increasingly pronounced quantitative character with the growing use of instruments such as the barometer and thermometer. Particularly important was the work of Réné Réaumur (1683–1757) whose refinement of the thermometer made precise numerical readings more feasible. This in turn made it possible for him to commission a team of naturalists (drawing particularly on French colonial officials and the Compagnie des Indes) to record temperatures around the globe (Bourguet and Licoppe 1997). This data could in turn be correlated with patterns of plant and animal distribution (Bourguet 2002, 2004)—an instance of the growing convergence of the two strands of the classical mathematical and the Baconian sciences which Kuhn has argued largely followed different trajectories in the age of the Scientific Revolution.

In other ways, too, natural history was growing scientifically more assertive. Its sense of its importance and status was given an enormous boost with the rise of effective systems of classification in the period from 1690 to the 1740s (Sloan 2006, 904). The need for such systems became ever more acute with the ever mounting number of specimens being brought back from distant lands—especially with the rise of the Dutch, English and French sea-borne empires. The worth of these classificatory models and, particularly, the increasingly dominant Linnaean system was to be further reinforced as the century progressed and Europe came to terms with another New World, that of the Pacific. The growing self-confidence and scope of natural history was reflected in the increasing attention paid to natural history in the narratives of exploration —the order and precision that natural history could now offer being perhaps seen as a way of legitimizing the growing sway of European empires (Pratt 1992, 33; Currie 2005, 63). The anonymous preface to the translation of *A Voyage to the Cape of Good Hope* (1785) by the Swedish naturalist, Anders Sparrman makes the high claim that "Now every authentic and well-written book of voyages and travels is, in fact, a treatise of experimental philosophy. From these sources natural history derives its most copious streams" (Sparrman 1785, iv). Very likely this was written by George Forster who had accompanied Sparrman on Cook's second voyage—a voyage on which Sparrman had been employed as a naturalist by George's father, Johann Reinhold Forster, who, to Cook's often intense irritation though much to the benefit of the world of learning, served as chief naturalist on board. The growing determination of naturalists such as the Forsters to be accorded greater scientific recognition was apparent in George Forster's preface to his account of Cook's second voyage—*A Voyage Round the World* (1777)—where he insisted that his father was sent to the Pacific to do more than "being a naturalist who was merely to bring home a collection of butterflies and dried plants"—rather it was his goal to write "a philosophical history" (Forster 2000, 9). It was to be a goal later shared by another, later, Pacific naturalist, Charles Darwin, for whom the experience of exploration and discovery acted as a stimulus to both natural history in the more traditional sense of collection of data but also to the post-Buffonian view of natural history as a science which could share with philosophy the search for causes.

Discovery around the globe and discovery within the sciences thus continued to be linked. Importantly, there was the psychological catalyst to search for the new and abandon accustomed ways of thinking in the face of the discovery of the New World or more accurately the New Worlds of not only America but parts of Asia and Africa—an experience revived in new ways by the eighteenth century European discovery of the Pacific and, perhaps, in our own time by space exploration. The navigational skills required to reach these New Worlds were a ready stimulus to the mathematical and astronomical science though the intersection between practical application and theorizing was often far from straight forward. More problematical was the intersection between the Scientific Revolution as traditionally understood and the vast increase in the scope of natural history as it came to terms with whole new quarters of the globe. Part of the difficulty was the Baconian view of the natural historian as a distinct and, in some ways, subordinate agent to the philosopher to whom the task of assigning causes was assigned. Another was the sheer scale and number of the natural history specimens collected which, until the rise of classificatory systems in the early eighteenth century, threatened to bury the natural historian in fascinating but unruly detail.

The diversity that natural history could display certainly made manifest the extent of travel and the widening reach of the European understanding of the globe. The fact that so many plants and animals were added to the lists compiled by the Ancients gave confidence to the Moderns in their quest to arrive at a new understanding of nature. Perhaps, however, as Kuhn suggests the natural and mathematical sciences were marching to a different rhythm with the theoretical flowering of the Baconian sciences coming in the nineteenth century. This does not accord readily with our traditional Copernicus to Newton view of the Scientific Revolution but perhaps these markers are but new Pillars of Hercules beyond which we need to sail.

References

Acosta, Joseph. ([1590]1604). *The Naturall and Morall Historie of the East and West Indies …* (trans. into English by E[dward]G[rimestone]). London: Edward Blount and William Aspley.

Alexander, Amir. 2001. Exploration mathematics: The rhetoric of discovery and the rise of infinitesimal methods. *Configurations* 9: 1–36.

Alexander, Amir. 2002. *Geometrical landscape: The voyages of discovery and the transformation of mathematical practice*. Stanford: Stanford University Press.

Anstey, Peter, and Michael Hunter. 2008. Robert Boyle's 'Designe about natural history'. *Early Science and Medicine* 13: 83–126.

Ashworth Jr., William B. 1990. Natural history and the emblematic world view. In *Reappraisals of the scientific revolution*, ed. D.C. Lindberg, R.S. Lindberg, and R.S. Westman, 303–332. Cambridge: Cambridge University Press.

Bacon, Francis. 1857–74. In *The works of Francis Bacon*, ed. James Spedding, Robert L. Ellis, and Douglas D. Heath, 14 vols. London: Longman.

Bacon, Francis. 1996. *The Oxford Francis Bacon*. In *Philosophical studies*, vol. VI, ed. Graham Rees, c. 1611–c.1619. Oxford: Oxford University Press.

Bacon, Francis. 2000a. *The Oxford Francis Bacon,* vol. IV, *The advancement of learning*, ed. Michael Kiernan. Oxford: Oxford University Press.

Bacon, Francis. 2000b. *The Oxford Francis Bacon,* vol. XIII, *The Instauratio Magna: Last writings*, ed. Graham Rees. Oxford: Oxford University Press.

Bacon, Francis. 2004. *The Oxford Francis Bacon,* Vol. XI, *The Instauratio Magna* Part II: *Novum Organum and Associated Texts*, ed. Graham Rees. Oxford: Oxford University Press.

Bacon, Francis. 2007. *The Oxford Francis Bacon,* vol. XII, *The Instauratio Magna*: Part III: *Historia Naturalis and Historia Vitae*, ed. Graham Rees. Oxford: Oxford University Press.

Barrera-Osorio, Antonio. 2002. Local herbs, global medicines. Commerce, knowledge, and commodities in Spanish America. In *Merchants and marvel: Commerce, science and art in early modern Europe*, ed. Pamela H. Smith and Paula Findlen, 163–181. New York: Routlege.

Barrera-Osorio, Antonio. 2006. *Experiencing nature: The Spanish American empire and the early scientific revolution*. Austin: University of Texas Press.

Barrera-Osorio, Antonio. 2007. Empiricism in the Spanish Atlantic world. In *Science and empire in the Atlantic world*, ed. James Delbourgo and Nicholas Dew. New York/London: Routledge.

Bennett, J.A. 1991. The challenge of practical mathematics. In *Science, culture and popular belief in Renaissance Europe*, ed. Stephen Pumfrey, Paolo Rossi, and Maurice Slavinski, 176–190. Manchester: Manchester University Press.

Bourguet, Marie-Noëlle. 2002. Landscape with numbers: Natural history, travel and instruments in the late eighteenth and early nineteenth centuries. In *Instruments, travel and science: Itineraries of precision seventeenth to the twentieth centuries*, ed. Marie-Noëlle Bourguet, Christian Licoppe, and H. Otto Sibum, 96–125. London: Routledge.

Bourguet, Marie-Noëlle. 2004. Measurable difference: Botany, climate, and the gardener's thermometer in eighteenth-century France. In *Colonial botany: Science, commerce, and politics in the early modern world*, ed. Londa Schiebinger and Claudia Swan, 270–286. Philadelphia: University of Pennsylvania Press.

Bourguet, Marie-Noëlle, and C. Licoppe. 1997. Voyages, Mesures et Instruments: Une Nouvelle Expérience du Monde au Siècle des Lumières. *Annales. Histoire, Sciences Sociales* 52: 1115–1151.

Cañizares-Esguerra, Jorge. 2003. Spanish America: From Baroque to modern colonial science. In *The Cambridge history of science, iv, eighteenth-century science*, ed. Roy Porter, 718–740. Cambridge: Cambridge University Press.

Cañizares-Esguerra, Jorge. 2004. Iberian science in the Renaissance: Ignored how much longer? *Perspectives on Science* 12: 86–124.

Cañizares-Esguerra, Jorge. 2006. *Nature, empire, and nation: Explorations of the history of science in the Iberian world*. Stanford: Stanford University Press.

Cook, Harold J. 2004. Global economies and local knowledge in the East Indies: Jacobus Bontius learns the facts of nature. In *Colonial botany: Science, commerce, and politics in the early modern world*, ed. Londa Schiebinger and Claudia Swan, 100–118. Philadelphia: University of Pennsylvania Press.

Cook, Harold J. 2007. *Matters of exchange: Commerce, medicine, and science in the Dutch golden age*. New Haven: Yale University Press.

Cooper, Alix. 2007. *Inventing the indigenous: Local knowledge and natural history in early modern Europe*. Cambridge: Cambridge University Press.

Copernicus, Nicholas. [1543]1952. *On the revolutions of the heavenly spheres* (trans: Wallis, Charles). Chicago: Great Books.

Currie, Noel Elizabeth. 2005. *Constructing colonial discourse: Captain Cook at Nootka sound*. Montreal: Mc Gill-Queens' University Press.

Daston, Lorraine. 1994. Marvellous facts and miraculous evidence in early modern Europe. In *Questions of evidence. Proof, practice and persuasion across the discipline*, ed. James Chandler, Arnold I. Davidson, and Harry Harootunion, 243–289. Chicago: University of Chicago Press.

De Asúa, Miguel, and Roger French. 2005. *A new world of animals: Early modern Europeans on the creatures of Iberian America*. Aldershot: Ashgate.

Dear, Peter. 2001. *Revolutionizing the sciences: European knowledge and its ambitions, 1500–1700*. Houndmills: Palgrave.

Eamon, William. 1994. *Science and the secrets of nature: Books of secrets in medieval and early modern culture*. Princeton: Princeton University Press.

Edwards, Clinton R. 1992. The impact of European overseas discoveries on ship design and construction during the sixteenth century. *GeoJournal* 26: 443–452.

Engstrand, Iris H.Wilson. 2000. Of fish and men: Spanish marine science during the late eighteenth century. *Pacific Historical Review* 69: 3–30.

Ford, Thayne R. 1998. Stranger in a strange land: José de Acosta's scientific realizations in sixteenth-century Peru. *The Sixteenth Century Journal* 29: 19–33.

Forster, George. [1777] 2000. In *A voyage round the world in his Britannic Majesty's sloop, Resolution*, ed. Nicholas Thomas and Oliver Berghof, 2 vols. Honolulu: University of Hawaii Press.

Gascoigne, John. 1990. A reappraisal of the role of the universities in the scientific revolution. In *Reappraisals of the scientific revolution*, ed. D.C. Lindberg, R.S. Lindberg, and R.S. Westman, 207–260. Cambridge: Cambridge University Press.

Gascoigne, John. 1995. The eighteenth-century scientific community: A prosopographical study. *Social Studies of Science* 25: 575–581.

George, Wilma. 1980. Sources and background to discoveries of new animals in the sixteenth and seventeenth centuries. *History of Science* 18: 79–104.

Gilbert, William. [1600]1952. *On the loadstone and magnetic bodies and on the great magnet of the earth* (trans: Mottelay, Fleury). Chicago: Great Books.

Goodman, David. 1992. The scientific revolution in Spain and Portugal. In *The scientific revolution in national context*, ed. Roy Porter and Mikulás Teich, 158–177. Cambridge: Cambridge University Press.

Goodman, David, and Colin Russell (eds.). 1991. *The rise of scientific Europe 1500–1800*. Milton Keynes: Open University Press.

Hooykaas, R. 1987. The rise of modern science: When and why. *British Journal for the History of Science* 20: 453–473.

Irving, Sarah. 2008. *Natural science and the origins of the British empire*. London: Pickering and Chatto.

Kamen, Henry. 1978. The decline of Spain: A historical myth. *Past and Present no.* 81: 24–50.

Kuhn, Thomas. 1997. Mathematical versus experimental traditions in the development of physical science. In *The essential tension. Selected studies in scientific tradition and change*, 31–65. Chicago: University of Chicago Press.

Lafuente, Antonio, and Nuria Valverde. 2004. Linnaean botany and Spanish imperial biopolitics. In *Colonial botany: Science, commerce, and politics in the early modern world*, ed. Londa Schiebinger and Claudia Swan, 134–147. Philadelphia: University of Pennsylvania Press.

Lyon, J., and P.R. Sloan. 1981. *From natural history to the history of nature: Readings from Buffon and his critics*. Notre Dame: Notre Dame University Press.

MacPike, Eugene (ed.). 1975. *Correspondence and papers of Edmond Halley*. New York: Arno Press.

Newton, Isaac. [1687]1952. *Mathematical principles of natural philosophy* (trans: Motte, Andrew and revised by Florian Cajori). Chicago: Great Books.

Ogilvie, Brian W. 2006. *The science of describing: Natural history in Renaissance Europe*. Chicago: University of Chicago Press.

Pagden, Anthony. 1993. *European encounters with the new world. From Renaissance to romanticism*. New Haven: Yale University Press.

Parrish, Susan. 2006. *American curiosity. Cultures of natural history in the colonial British Atlantic world*. Chapel Hill: University of North Carolina Press.

Pimental, Juan. 2000. The Iberian vision: Science and empire in the framework of a universal monarchy, 1500–1800. In *Nature and empire: Science and the colonial enterprise*, ed. Roy MacLeod, *Osiris*, second series 15: 17–30.

Pratt, Mary Louise. 1992. *Imperial eyes: Travel writing and transculturation*. London: Routledge.

Puig-Samper, Miguel. 1994. Botanical exploration in the new world in the eighteenth century: Zarogoza symposium report. *Physis* 31, n.s, fasc. 2: 602–604.

Reidy, Michael S., Gary Kroll, and Erik W. Conway. 2007. *Exploration and science: Social impact and interaction*. Santa Barbara: ABC Clio.

Reiss, Timothy. 2005. 'Seated between the old world and the new': Geopolitics, natural philosophy and proficient method. In *Francis Bacon and the refiguring of early modern thought. Essays to commemorate the Advancement of learning (1605–2005)*, ed. Julie Solomon and Catherine Martin, 223–246. Aldershot: Ashgate.

Rossi, Paolo. 1991. Society, culture and the dissemination of learning. In *Science, culture and popular belief in Renaissance Europe*, ed. Stephen Pumfrey, Paolo Rossi, and Maurice Slavinski, 143–175. Manchester: Manchester University Press.

Sandman, Alison. 2002. Mirroring the world: Sea charts, navigation, and territorial claims. In *Merchants and marvels: Commerce, science and art in early modern Europe*, ed. Pamela H. Smith and Paula Findlen, 83–108. New York: Routledge.

Sargent, Rose-Mary. 1996. Bacon as an advocate for cooperative scientific research. In *The Cambridge companion to Bacon*, ed. Markku Peltonen, 146–171. Cambridge: Cambridge University Press.

Scammell, G.V. 1969. The new worlds and Europe in the sixteenth century. *The Historical Journal* 12: 389–412.

Sloan, Phillip. 1996. Natural history, 1670–1802. In *Companion to the history of science*, ed. R.C. Olby, G.N. Cantor, J.R.R. Christie, and M.J.S. Hodge, 295–313. London: Routledge.

Sloan, Phillip. 2006. Natural History. In *The Cambridge history of eighteenth-century philosophy*, ed. Knud Haakonssen, 2 vols, II, 903–938. Cambridge: Cambridge University Press.

Smith, Pamela H., and Paula Findlen. 2002. Commerce and the representation of nature in art and science. In *Merchants and marvels. Commerce, science and art in early modern Europe*, ed. Pamela H. Smith and Paula Findlen, 1–25. New York: Routledge.

Sorrenson, Richard. 1996. The ship as a scientific instrument in the eighteenth century. *Osiris*, second series 11: 221–236.

Sparrman, Anders. 1785. *A voyage to the Cape of Good Hope…* (translated from the Swedish original), 2 vols, Dublin: Messrs White, Cash, and Byrne.

Steele, Arthur. 1964. *Flowers for the king: The expedition of Ruiz and Pàvon and the Flora of Peru*. Durham: Duke University Press.

Turnbull, David. 1996. Cartography and science in early modern Europe: Mapping the construction of knowledge spaces. *Imago Mundi* 48: 5–24.

Waters, D.W. 1967. Science and the techniques of navigation in the Renaissance. In *Art, science, and history in the Renaissance*, ed. Charles S. Singleton. Baltimore: Johns Hopkins Press.

Chapter 10
The Hive and the Pendulum: Universal Metrology and Baroque Science

Nicholas Dew

Abstract Early modern scholars and statesmen were acutely aware of the need for improved standards of measurement, albeit for differing reasons. The variety of man-made units across territories and histories was, by the seventeenth century, already a sceptical commonplace, and was understood in terms of the mutability of human institutions. The late seventeenth century saw many scholars advance possible candidates for a universal standard. The most promising of these was the use of a seconds pendulum as a standard for length, a project which was actively pursued by the French Académie Royale des Sciences in the 1670s and 1680s, and remained a goal cherished by savants through the eighteenth century. This paper's first section places the Académie's early metrological projects in the context of the scholarly community's ideal of a universal measurement standard, which was often expressed in ways combining political, theological, and humanistic concerns. Melchisédech Thévenot's ludic proposal that honeycombs might be a length standard is explored as one example. The second section examines the Académie's attempts to test the seconds pendulum as a universal length standard, by taking the missions to Uraniborg (1671) and to London (1679) as case studies in the practice of metrological work.

The Hive: Universal Measurement in Baroque Theory

Towards the end of May 1680, London was hit by a hailstorm. Even in the "little ice age" of the seventeenth century, this was uncommon for the time of year. The curious *virtuosi* rushed into the streets to measure the dimensions of the hailstones. One of these virtuosi was John Locke, who sent news of this strange event to his French

---

N. Dew (✉)
Department of History and Classical Studies, McGill University, Montreal, QC, Canada
e-mail: nicholas.dew@mcgill.ca

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_10,

friend, Nicolas Toinard: "Last Tuesday hailstones of enormous size fell all over the city here. I myself measured one lump of ice … which had a circumference of 420 *grys*…" (Locke 1976–1989, 2: 175–6 [Locke to Toinard, 20 May 1680]).[1] Toinard read the letter to the group of Locke's friends in Paris, a group which included François Bernier, Jean Picard, Eusèbe Renaudot, Henri Justel, and Melchisédech Thévenot, among others. The French *curieux* marvelled at Locke's news—after all, a hailstorm in late May was a strange fact—but were more concerned about interpreting the measurement. The hailstones, Locke said, had a circumference of 420 *grys*—which sounded rather large—but none of the French knew what this strange English unit, the *gry*, was. Toinard had asked his friends who knew some English (like Thévenot and Adrien Auzout), but none of them were familiar with the term. Toinard therefore begged Locke to explain the mystery (Locke 1976–1989, 2: 183 [Toinard to Locke, 27 May 1680]).

Locke replied, apologetically. The *gry*, as he thought he had already explained to Toinard, was a unit of his own invention. A few years earlier, while on his travels around France, he had devised his own measurement system, which was designed to be both rational (being partly decimal) and universal (being based on a naturally-occurring constant). The *gry* was one thousandth of the "philosophical foot". The philosophical foot was to be divided into 10 inches, each inch into 10 lines, and each line into 10 grys (Locke 1976–1989, 2: 194 [Locke to Toinard, 10 June 1680]).[2] The philosophical foot—also known as the "universal foot"—was one third of the philosophical (or universal) yard. The philosophical yard was the length of a pendulum beating seconds, which was at this time a popular candidate for a universal standard of measurement, not least because it was conveniently close to most existing yard lengths. The *gry* was, then, roughly a third of a modern millimetre, so Locke's hailstones with their circumference of 420 *grys* were about 4 1/2 cm in diameter.

Locke's news may have been about meteorology, but what matters for my purposes is the *metrology*. This minor episode of miscommunication between Locke and the French *savants* encapsulates, in many ways, the metrological problem that faced the scientific community of the late seventeenth century. Two things are important. The first is the fact that Locke has developed a rational measurement system derived from a supposed natural constant: in this, he is representative of the ambitions of the *savant* community at large. The second is the very *untranslatability* of his reported data ("420 grys"), which is representative of the acute problems inherent in the communication of measurements in this period. Locke's system was still only a private one, although he hoped it would one day be adopted. This only underlines for us that measurements could only be communicated if a *shared* system existed—but in order to establish such a system, special objects, techniques, and individuals had to travel from place to place. The chaotic diversity of weights and measures in *ancien régime* Europe was, of course, a familiar problem (Kula 1985, 161–184; Zupko 1978; Haustein 2001; Alder 1995). For instance, in the very same exchange of letters, Locke had also asked Toinard if he could translate some measurement terms from Montpellier—because a friend there had sent him a recipe for baking bread, and he wanted to know what the measures in the recipe meant (they were: "une *truquette* d'eau", "une *piche* d'eau", and "une *hemine* de farine"). These units from Lower

Languedoc were unknown in Paris, so Toinard had promised to send for accurate information on the spot ("sur les lieux"), adding that all *he* knew was "that their pound is 4 ounces less than ours" (Locke 1976–1989, 2: 175, 182–83).[3]

Both of these instances, the Montpellier bread-making recipe and the hailstones of London, remind us that problems of metrology had spatial as well as historical dimensions. To put this another way: the first thing to do, when faced with a measurement-translation problem in the seventeenth century, was to write to friends in other places, to ask for a unit's value in relation to some known unit; if that failed, to ask for specimens of the units to be sent; and then, if that also failed, the only thing left to do was to travel, to remove all mediation, and to directly measure the reference objects. First letters circulated; then metal rules; then people. Existing measurement systems were commonly defined by their territorial extent, standards usually being named after the city or province that defined them. So *universal* measurement schemes were, literally, *utopian*. In reply to Locke's wish that "that people might some day agree upon the philosophic foot" (175), Toinard agreed heartily with the principle, but was sceptical about its practicality, adding, only half jokingly, that it would perhaps only be possible to institute Locke's system in America—specifically in the colony of Carolina, for which Locke had helped draft the constitution—since there, things could be "cut from a fresh cloth". Toinard adds, tantalisingly, that he has heard a rumour that "a country" is considering adopting the universal yard, but he doesn't dare say which one (Locke 1976–1989, 2: 182–183 [Toinard to Locke, 27 May 1680]).[4] Meanwhile, with such schemes still pending general adoption, *savants* like Locke travelled around, continually noting the various values of the coins, weights and measures they found as they toured from one town to the next. Locke, when he was in Paris in 1677, had paid the English-born instrument maker, Michael Butterfield, to make him a brass rule, upon which were inscribed the units of London, Paris, Leiden, Copenhagen, and Rome, along with the philosophical foot for comparison. This he used to take measurements when visiting the Roman ruins in Nîmes, and the Châteaux of the Loire (Locke 1953).

Like universal language schemes, projects for a universal system of measurement were widespread at this time, and were usually discussed in the rhetoric of the "Republic of Letters". For example, Locke, when introducing his scheme in his *Essay Concerning Human Understanding*, says a decimal system would be of "general convenience" in the "Commonwealth of Letters" (Locke 1975, 624 [IV.10.10. note a]; cf. Locke 1976–1989, 2: 39). At the same time, seventeenth-century *savants* all knew that measurement standards were tied to local forms of authority: political theory in the period conventionally identified the authority over weights and measures as one of the "marks of sovereignty" (e.g. Bodin 1583, 244; Bodin 1992, 80–1 [book 1, ch. 10]). This meant that a legally instituted universal system might only see the light under a "universal monarchy". So *savants* were aware of the distinction between a metrological system conceived as a convention to be voluntarily adopted by a scientific community, and one to be imposed upon a really existing economy (as was to be attempted by the French Revolutionary governments: Alder 1995; Heilbron 1993, 243–77; Baker 1990, 156–159). Even if it still seemed unlikely, to late seventeenth-century thinkers, to be something that any European state might actually impose (despite Toinard's rumour), a "philosophical" measurement system could at least be set up as a convention among scholars, and it could also allow for past and present measures to be passed on to posterity. There remained, nonetheless, a tension between the value that might be attached to a standard owing to its widespread use and its convenience, and the value attached to those measurement standards that were thought to have the moral authority of either God, Nature, or of the Ancients (or a combination of these).

Humanists had long been troubled by their ignorance of the true values for the Roman foot or Hebrew cubit. In the 1640s, John Greaves had provided one of the most thorough investigations of the problem of Roman weights and lengths, based on his antiquarian travels in the eastern Mediterranean (Shalev 2002). Greaves concluded his book by suggesting that the most reliable way to provide posterity with standards of conversion between ancient and contemporary metrics was to use long-standing monuments, like the pyramids, as physical standards (Greaves 1647, 123–8). In a more ecclesiastical register, though, it was not uncommon to associate the ancient Hebrew values with divine (and therefore also natural) authority (Bennett and Mandelbrote 1998). It should not be surprising that the English churchman Richard Cumberland, best known as a theorist of natural law, also wrote a treatise on the values of the ancient Hebrew measures, which was printed by the Royal Society's printer in 1686. Cumberland, in his dedicatory letter to Samuel Pepys (then the Society's president), cast his metrological researches as both eirenic and commercial, calling it "the peaceable Doctrine of Measures and Weights, which in their General Nature, are the Common Concern of all Mankind; as being the necessary Instruments of just Dealing, and fair Commerce between all Nations". Cumberland went on to argue that the ancient Hebrew measures were likely to contribute to peaceful commerce because they were "the Rules of that Righteousness, whereof Noah, the Father of all Men now living, was a Preacher". He concluded the book by suggesting the seconds pendulum be used as a universal measure (Cumberland 1686, sig. A6r-7r, 124–27).

Two decades earlier, John Wilkins, another prominent English divine, and also closely linked to the Royal Society, had already made explicit the connection between reforming language and reforming metrology, in his *Essay Towards a Real Character, and Philosophical Language*, probably the best-known language-reform scheme to emerge from England (Lewis 2007). In the second part of the *Essay*, Wilkins discussed the problem of a "natural standard, or universal Measure" (he identifies the two), noting that it was "esteemed by Learned men as one of the *desiderata* in Philosophy" (Wilkins 1668, 191–2). Ancient measures had once been derived from natural objects, such as the width of a grain of barley, or the various anthropometric measures (the inch, palm, span, cubit, foot, pace, and so on), but none of these were suitably invariant. The current candidates for a length standard included a division of a meridian arc, which had been suggested by Gabriel Mouton, a Lyon cleric (Mouton 1670), and which was later to be revived in the French Revolutionary metric system, as well as a proposal using "the *Quick-silver experiment*" (i.e. a column of mercury in a Torricellian apparatus). The first Wilkins thought too difficult to achieve with any certainty, and the second obviously too subject to variations in the “gravity and thickness of the *Atmosphere*, together with the various tempers of the Air in several places and seasons”. He therefore proposed (citing Wren, Brouncker, and Huygens) the length of a seconds pendulum, which was presumed to be less subject to local and temporal variation, and went on, just as Locke did later, to divide the resulting unit in decimal fashion, complete with derived units of capacity and weight (Wilkins 1668, 191–2).[5]

French scholars were engaged in a similar range of antiquarian, theological, and natural-philosophical discussions of metrology. Claude Lancelot, who had written, with Antoine Arnauld, the *Port-Royal Grammar* of 1660, which reflected on the basis for translation between languages, wrote an erudite treatise on the antique capacity unit, the “hémine”, largely in order to resolve debates among religious communities over the precise daily ration of wine allowed by the rule of Saint Benedict (Lancelot 1667). Meanwhile, one of Locke’s friends, the collector and scientific academy host Melchisédech Thévenot, was also interested in the problem of a universal measure, and proposed a rather striking solution.

In a “Discourse on the Art of Navigation”, published as an appendix to a collection of travel accounts which was itself an annex to his larger travel collection (Dew 2006), Thévenot discussed the problem of transmitting measurement standards across time and space. The passage is worth quoting in full:

> In an enterprise in which so many projects have failed that it has come to seem almost hopeless, it occurred to me that perhaps we would have more success by using one of those creations that we say animals make by instinct; we could, it seems to me, reasonably suppose that this instinct, being based in an eternal cause, must always be the same, and exempt from the varieties which distinguish everything that comes from men. Among other examples, I found that the cells made by bees of the same species, measured at the time that the bees build them, are equal among themselves, and having since measured those near to Paris, Leiden, and Florence, I found no difference; and if one follows the lines according to which the bottoms or bases of these cells are arranged, one will find that the same number of cells always comes to the same measurement. Thus, if all of the measures that are currently used in the world were to be reduced to that of the bees, posterity would by this means be able to know them all: and this measure, which I here propose, would be all the more universal [*générale*], since there are bees in every part of the world, in polar regions just as in places near the equator. And even though I build it on wax, nothing stops me from believing that this [unit] could last as long as the world, and that it is more apt for this design than the jasper [*diaspre*][6] of the tomb upon which Gravius [John Greaves] marked the English foot, and easier to understand and to put into practice than the measure based on the oscillations of a pendulum combined with astronomical observations, as has been proposed in France and in Poland. But, before being able to establish it, I would like to be able to compare the works [*ouvrages*] of bees in distant places, those from the Cape of Good Hope and from Egypt, for example, with those from Muscovy and from Mexico, etc. And if they are found to be equal everywhere, this measure could be made common to all nations, and by this means we could transmit the knowledge of the measurement systems of our age to posterity—which is what we are seeking to do (Thévenot 1681, separately paginated, 23–25).[7]

The passage is typical of that ludic style in natural philosophical writing which Paula Findlen has identified as common currency in the “culture of curiosity”, from Kepler down to at least Leibniz (Findlen 1990, 1998). That Thévenot’s suggestion was playful does not mean that the idea lacked any seriousness. Thévenot notes that honeybees made their cells by “instinct”, and that this guarantees their constancy: since animal instinct could reasonably be supposed to come from an unchanging “eternal cause” (or by the “hand of God”, as he puts it further on), honey-bees must be exempt from human mutability. Thévenot also specifies, as if to add plausibility to his claim, that the honeycombs must be freshly made, and that one must only compare honeycombs made by bees of the same species, though he does not say which. The regularity of the cells was something, he goes to add, that Aldrovandi and Muffet, and all those other “*personnages de grande lecture*, who believed themselves to have got to the bottom of bee-research simply by collecting everything that the Ancients and Moderns had written about them”, had failed to notice (Thévenot 1681, 25–26). Thévenot also notes that of the three most common tessellating shapes (the square, the triangle and hexagon) the hexagon contains the largest area. Bees have managed, through animal instinct alone, to construct their cells according to the optimum shape, something that only the most able geometers might have calculated.

Thévenot goes on: “Thus, one might apply to these workers the verses that the Poet applied to himself, and say, *in tenui labor, at tenuis non gloria* [‘little the scale to work on, yet not little the glory’], or indeed allow a Persian Poet to exclaim, with the license common to the poets of his country, that if Archimedes had examined such a surprising structure (*ouvrage*), he would have ‘bitten the fingers of admiration with the teeth of envy’” (Thévenot 1681, 27; cf. Virgil 1982, 124).[8] Alongside this nod to the Orientalist erudition for which he was known (Dew 2009), Thévenot here made what was, for his readers, the obvious allusion to the fourth book of Virgil’s *Georgics* (IV.6), reminding readers of the long tradition in which bees’ labour could be compared to human labour, and the bee hive used as a metaphor for the human polity (Virgil 1982, 124–43; cf. Pliny 1991, 149–157 [book 11]; Burke 1997; Allen 2004; Woolfson 2010).[9]

Although published in 1681, Thévenot had been working on his apian metrology at least 10 years earlier. From his country home at Issy (outside Paris), he had been able to support the work of both Jan Swammerdam and Niels Steno, both of whom collected and dissected insects during their time with him. Thévenot had announced his measurement idea in a letter to Henry Oldenburg in 1671 (28 October 1671; Oldenburg 1965–1986, 8: 310–11), which uses language almost identical to that of the version he later published. Around the same time, Thévenot had built a glass hive with which to observe bee behaviour. Thévenot’s friend, the Gassendist philosopher and traveller, François Bernier, in a satirical edict mocking the Sorbonne’s motions against the new science, mentions Thévenot’s use of a glass hive, and casts him as a spy working maliciously against the Republic of Bees, out of disregard for the teachings of Aristotle (Bernier 1992 [1671], 235).

Bees were a common rhetorical resource for natural philosophers in the mid-century, and interpreting them was, thanks to Virgil, always tied up with emblematic significance. The idea of using a glass hive to observe bee life was something that Thévenot could have learned from his contemporaries in the culture of curiosity. In the Hartlib circle, around 1650, there was discussion of a glass hive made by the Gloucestershire parson William Mewe, which inspired Hartlib to pursue bee research over several years, inspiring others (including Wilkins and Wren) to design glass hives and to write about the "republic of bees", pointing economic lessons for the English interregnum Commonwealth (Hartlib 1655, 52; Raylor 1992; Johns 1998, 266–71; Bennett and Mandelbrote 1998, 162–3). Earlier still, bee research had been a part of the Lincean academy's natural historical work in Rome in the 1620s, not least because the bee was the emblem of the Barberini family, under whose patronage the Linceans worked (Freedberg 2002, 151–94; Findlen 1994, 214–6, 378–80). Bee research frequently brought forth political commentary, either playful or serious, in the scientific culture of the period. But if bees had long been endowed by humans with the power to suggest solutions to the problem of social order, Thévenot was now endowing them with the power to provide a solution to a problem of knowledge.[10]

The Pendulum: Establishing a Metrological Network in Practice

Thévenot seems to have been the only scholar to suggest that honeycomb cells were sufficiently regular to become the basis of a universal length standard. The length of a pendulum beating seconds, however, was more widely accepted as a *potential* candidate, and had been discussed in these terms, as we have seen, by several English savants, but also on the continent by Mersenne, by Huygens, and by the Poland-based Italian Jesuit, Tito-Livio Burattini (Koyré 1953; Blamont 2001; Armogathe 2001; Giustini 1992). Even while it was being advanced as a candidate, though, there were always concerns about possible problems with the seconds pendulum. As early as 1620, Bacon, in the *New Organon*, had already speculated that weight might vary with altitude (Bacon 2000, 163–4 [book 2, aphorism 36]), and in the 1660s there was a common concern that the pendulum's motion would vary with differing climates, atmospheric conditions, and with latitude. (Boyle and Brouncker in 1661 had proposed that someone take a pendulum clock up the Pico Tenerife, to test the effects of varying atmospheric pressure on a pendulum). Christiaan Huygens, who had done more work on pendulums than most, argued in the 1660s for the seconds pendulum as a length standard, but was also concerned, as early as 1666, about possible variations in weight with latitude, since he thought that the earth's rotation would produce a centrifugal force in the atmospheric vortex, which would cause bodies to lose weight when close to the equator (Huygens 1986, 167–70; Defossez 1946, 153–67; Costabel 1987; cf. Matthews 2000).

The seconds pendulum was therefore both a leading candidate for a length standard, and yet, at the same time, its candidacy was being challenged by theoretical objections, even before any experimental data from diverse locations had been gathered. What made the data available was the mapping expeditions organized by the French Académie Royale des Sciences and centred on the Paris Observatoire. From its very foundation (1666) the Académie was planning expeditions to advance astronomy, geodesy, and cartography. The interest of the *savants* in using new techniques to improve their figures for fundamental units like the size of the earth, and the distance from the earth to the sun, was cannily married to the interests of the king and his ministers, with projects like the remapping of France, the establishment of the Paris meridian, and the project to map the whole world from the Paris Observatoire. To these ends, the Paris academy organized a series of expeditions around France—but also further afield—from around 1670 onwards (Olmsted 1942, 1960). Since it was already a seemingly good candidate for a universal measure, and yet still shrouded in theoretical doubt, the measurement of a seconds pendulum was added—even in the earliest proposals for voyages—to the list of instructions for the Académie's envoys (albeit as a secondary task, supplementing their astronomical work).[11] The abbé Jean Picard, in his 1671 *Mesure de la Terre*, effectively announced the Académie's commitment to the seconds pendulum as a length standard, and also gave one of the fullest discussions of how the measurement should be done (Picard 1671, 3–5). Through the 1670s, figures were collected from a range of locations across Europe. Two occasions will be taken as examples here: Picard's measurement made in 1671 at Uraniborg, and Rømer's in London in 1679.

Picard in Uraniborg, 1671

Picard's mission to Uraniborg in 1671 was among the earliest of the Académie des sciences's overseas astronomical expeditions. Initially, the Académie had hoped to send a mission to Madagascar (McKeon 1965, 246–57; Olmsted 1942, 118–9). The target was then revised to a mission to Alexandria, in Egypt. This also proved too ambitious, and the Académie had to settle for a cheaper alternative: the Baltic. The aim was to use modern instruments and techniques—telescopes fitted with micrometers, pendulum clocks, and the concerted observation of Jupiter's satellites—to find the difference in longitude between Paris and Uraniborg (since the available figures differed), so that the observations of Tycho Brahe, made there almost a century earlier, could be reduced to the Paris meridian. The Uraniborg mission was an exercise in translation, in several senses. Locating Uraniborg precisely in relation to Paris would allow the French to translate Tycho's figures onto a Parisian standard. At the same time, the French were interested in appropriating an existing project to produce a new edition of Tycho's papers, to improve the error-prone text of Kepler's Rudolphine Tables. The intended publication of the corrected Tycho at the Imprimerie royale under the patronage of Louis XIV would effect a symbolic translation of the prestige of Uraniborg to Paris (Pedersen 1987; Cassini 1693, 40–1; Picard 1693).

Picard left Paris in July 1671, with a battery of instruments and a young trainee named Etienne Villiard. They visited Leiden en route, where Picard was able to converse with the great cartographer Blaeu about geodesy, and to purchase a piece of luminous Icelandic spar.[12] He was also able to measure a standard for the Rhenish foot.[13] After visiting Hamburg en route, they arrived in Copenhagen, where they were received by the local *savants*. The French were surprised to learn that the island of Hven, on which Uraniborg was built, was no longer a Danish possession, but was under Swedish control (as it had been since 1660). Such details of Baltic diplomacy had not reached the Académie. The operations on Hven were organized from the round tower of the Copenhagen Observatory, where Picard's host was Erasmus Bartholin, professor of mathematics and medicine there. Bartholin introduced Picard to a young and gifted student of his, Ole Rømer, and the four of them (Picard, Villiard, Bartholin and Rømer) sailed over to Hven together. Bartholin was already working on the new edition of Tycho, and his cooperation was essential both for the Uraniborg mission and for the publication project. For his part, Picard seemed concerned to make sure that news of his visit did not reach England—it seems because he feared the Royal Society would be keen to get hold of Bartholin's Tycho papers and produce their own edition.[14] The astronomical work went on into November, when the two senior scholars decided to avoid spending winter on Hven, and headed back to the relative warmth of the Copenhagen Observatory, leaving Villiard and Rømer on the island. The measurement of the seconds pendulum was carried out on Hven, and Picard records in his account of the mission that it was witnessed by both Bartholin, and Andreas Spole, professor of mathematics from the University of Lund. In a letter to Colbert, Picard reported that the agreement of both these witnesses (and the concurrence of both a Dane and a Swede to boot) made the observations all the more "authentic". He also noted, for Colbert's benefit, that the Baltic *savants* acknowledged that France had now become "the mother of the arts and sciences", and that this was due to Colbert (Picolet 1979). The result, Picard was happy to report, was that the seconds pendulum was found to have exactly the same length in Uraniborg as in Paris: 36 inches 8 ½ lines (twelfths of an inch), Paris measure (Picard 1693, 12).

Rømer in London, 1679

Picard was so impressed with the work of the young Ole Rømer that he brought him back to Paris with him. Rømer spent the next 10 years based in Paris (1672–82) where he engaged in a variety of projects, building spectacular instruments for the education of the Dauphin, and working at the Observatoire on the eclipses of Jupiter's satellites (calculations which led him to argue for the finite velocity of light). At the very time of Picard's return (in 1672), another expedition of the Académie was just setting off. Giandomenico Cassini's trainee Jean Richer was leaving La Rochelle on a Senegal Company ship bound for Cayenne, where he was to conduct astronomical observations, and also to measure the seconds pendulum. From Cayenne, just under 5 degrees north of the equator, Richer was to report that the pendulum needed shortening, by a Paris line and a quarter (2.81 mm). This was such a small difference that most of Richer's superiors back in Paris suspected that he had made a mistake (Olmsted 1942; Dew 2008; Schaffer 2009, 261–263).

Before leaving for Denmark, Picard had asked the Royal Society to provide a pendulum measurement for London (Oldenburg 1965–1986, 7: 496–500 [Vernon to Oldenburg, 8 March 1671]). The English reported a figure of 36 inches and 4 tenths of an English foot, which—according to the conventional rates of conversion—seemed to give 36 inches, 11 and 13/20 lines in Paris measures. This seemed considerably longer than the Paris length (now replicated at Uraniborg), which made Picard suspect either an error on the part of the English, or an error in the conversion from English to French units, or both. For this reason, Picard stepped up his requests for an accurate copy of the English foot standard to be made and sent to Paris. In 1679, an opportunity arose to settle the doubts over the question, by sending Ole Rømer to London. Rømer's task was to carry out the pendulum measurement—effectively to show the English how it had to be done—and to verify the exact value of the London foot.[15]

Rømer made the journey from Paris to London with Locke, who had met him in France and was now on his way back to England. They arrived in London in late April 1679. Rømer and Locke spent a few weeks enjoying London together—Rømer seems to have fallen for a pretty woman who ran a hardware shop ("*pulchra mercatrix*"), and so bought a lot of pliers and knives (Locke 1976–1989, 2: 26, 52). In late May, finally getting down to his task, Rømer went to the Greenwich observatory, where under Flamsteed's eye he began the pendulum work. Flamsteed reported that they found the length to be the same as in Paris, although he noted that Rømer had left him a pendulum ball, so that he could repeat the experiment himself later on.[16] By this date, the pendulum experiment was coming under scrutiny, and the attention to both the material apparatus and to technique is reflected by the fact that Rømer left one of his pendulum bobs with Flamsteed, and by the fact that Robert Hooke and Denis Papin (Robert Boyle's assistant) visited Rømer and examined his instruments: the brass ball for the pendulum bob, his sliding steel ruler, and even the pendulum cord, made of silkgrass, an exotic hemp which the French had found to be the best material for the purpose (Hooke 1935, 412; cf. Dew 2008, 63, 70 n. 32).

By June 1679, Rømer was back in Paris, supposedly having brought the Royal Society into line with the measurements that the Académie had found in both Uraniborg and Paris. Nonetheless some doubts still remained: there were rumours that the English had changed their minds, and by September, Rømer was allowing that their might be a measurable difference between the London and Paris lengths after all (Locke 1976–1989, 2: 35 [Justel to Locke, 11 June 1679]; 91–2 [Rømer to Locke, 5 Sept. 1679]). In the following months, metal rules and pendulum balls continued to be sent between Paris and London. In the next couple of years, Picard and Philippe La Hire went on mapping missions to the South-West of France (to Bayonne and Sète), which appeared to provide new evidence of the non-variation of the pendulum. The only outlying figure, by this date, was Richer's from Cayenne. The next French expedition beyond Europe, to Gorée (Senegal) and the Antilles, produced a more unsettling result, since it reported an even greater shortening than that found by Richer, and at a more northerly latitude than Cayenne. However, this result failed to convince the Academicians for several years (Dew 2010).

Across the 1670s, then, Picard's project to establish the invariance of the seconds pendulum *seemed* to be successful. The Académie had gathered the experimental data from a range of locations which could resolve the theoretical doubt that had long existed as to the viability of the seconds pendulum as a universal length standard. (The theoretical doubts had more to do with the Copernican diurnal rotation of the earth rather than the shape of the earth, at this stage.) The process of replicating the pendulum measurement, and making the numbers cohere from a range of locations in Europe, was troubled—even though the actual variation in the acceleration due to gravity between Paris and London was probably too small to be measured—by the fact that this apparently simple experiment was actually difficult to do. The success of the measurement depended on knowing the correct procedure (such as making sure you set the pendulum to very small vibrations), but also on the accuracy of the timekeeping (which required a large and accurate clock as well as daily solar observations), and on material details like the proper kind of thread for the cord, the correct dimensions for the bob, or a properly-shaped metal clip from which to hang the thread.[17] It was only through dogged correspondence, and the circulation of highly skilled people (Picard and Rømer), and their special apparatus, that the replications were achieved at all, and a consensus established—even while such expeditions touched upon rivalries within the supposedly cooperative Republic of Letters. A few years later, Isaac Newton's argument for the earth's having an equatorial bulge, which entailed the re-classification of Richer's figure from Cayenne as an extremely accurate measurement (Schaffer 2009), was to challenge the notion that the seconds pendulum could function as a universal measure; although the idea that it could provide a locally-specified standard was to survive throughout the eighteenth century.

Conclusion

How scientific cultures frame their most ambitious metrological projects reveals a great deal about such cultures' values.[18] The dream of deriving a universal standard of measurement from a natural constant was by no means new in the seventeenth century, and it was destined to survive much later. In the seventeenth century, though, it resonated with the ideals of the scholarly community, in which appeals were made to a range of theological, humanistic, antiquarian, and "natural" forms of authority. The metrological projects of the seventeenth century can strike us as strange, as much for their references to Solomon's Temple or the Egyptian pyramids, as for their explicitly articulated connections between metrics and political sovereignty. The connection between shared standards and social order was a truism for Thévenot and his contemporaries. Thévenot's proposed honeycomb standard may or may not have been a joke—the ambiguity is itself telling—but the playful register conceals a more serious paradox. The honeycomb itself hovers on the border between art and nature, as a technical feat produced by bees. Human art must be instructed by nature's art. But by offering as a natural standard the craftwork of bees, especially with a nod to Virgil, Thévenot's fable of the bees also hints at the relationships between natural regularities and social organization, and between social orders and technical prowess. The project to make the seconds pendulum a universal unit was an attempt to use artificial means (clocks, pendulum apparatus, astronomical timekeeping techniques) to represent a supposed natural constant (the acceleration of falling bodies), which was assumed to be the optimal basis for a system of standards. It may at first sight appear to have had much more chance of success, and it was connected at the practical level to the newest techniques and institutions; but it was nonetheless a project that was conceived within the same scholarly culture, and endowed with some of the same values, as Thévenot's hive.

Notes

1. Dates are given in the calendar used by the source (Old Style for letters sent in England; New Style in France), except where needed for clarity. The passage continues (in de Beer's translation from Locke's Latin): "… it was rounded in shape and slightly flattened on both sides, so that it was not perfectly spherical. I hear that others were measured by various people and found to have twice as great a circumference; but the middling specimen that I handled myself sufficiently astonished me, and I should be glad to know from your philosophers up to what weight solid bodies of such bulk can be suspended in the air. I doubt whether the Cartesians can have any contrivances to help in this matter, and whether the Occult Qualities of the Peripatetics may not break down under such a load". Locke here turns from a report of a rare phenomenon to a point about natural philosophy, in a fashion typical of his letters in this period.
2. Locke explains: "When I used grys in giving the measurement of our hailstones I did so in the belief that I had once told you, when enjoying your delightful company, that this is the name I have given to 1/1000 of the universal foot, so that 420 grys signifies 4 pouces 2 lines or 420/1000 of that foot; but the globule that I handled myself was a very small one". For Locke's invented universal system (which incidentally happens to be decimal), see Locke to Boyle, 16 June 1679 (Locke 1976–1989, 2: 38–39 and notes), de Beer's long note on metrology at ibid., 14–16, and at 39 n. 1. See also Locke (1953), 161 (entry for 7 August 1677) and 185 (29 Jan 1678). In his travel journal, Locke frequently measured buildings and expressed the measurements in his "universal" system. For contemporary projects for decimal metric systems, see also Sarton (1935), 188–194. For background on Locke's French correspondence, see Bonno (1955).
3. Both "truquette" and "piche" remain unidentified; confusingly, an "hémine" was an ancient unit of about half a pint (Lancelot 1667) whereas an "émine" (sic) was a Montpellier unit of volume, approximating 26 litres (Zupko 1978, 62–3). The Paris pound (*livre*) comprised 16 oz (*onces*), but various provincial pounds had fewer ounces.
4. Toinard writes: "Il est tres a souhaiter que l'on convient d'une mezure et d'un poids, mails il n'y a pas lieu d'esperer cela que dans la Caroline, ou lon taille en plein drap. Je n'oserois vous mander ce que j'ay apris depuis peu sur ce sujet a legard de ceux qui pouroient et devroient l'introduire dans un etat qui inviteroit peut-etre le reste de l'Europe a cete conformité et uniformité universele". Locke included the system in his Carolina scheme (Woolhouse 2007, 156; cf. Arneil 1996, 118–31).
5. Wilkins here gives a reasonably detailed account of how to perform the pendulum measurement, with the important exception of how to establish a reference for seconds of mean solar time.
6. Thévenot's term "diaspre du tombeau" is unclear: "diaspre" might mean a diaphanous shroud, or a kind of jasper; the stone seems more likely in this context.
7. In French, the passage reads: "Dans une entreprise que tant d'efforts inutils ont renduë comme desesperée, il m'est venu dans l'esprit que peut-estre l'on y réüssiroit mieux en se servant de quelqu'un de ces ouvrages que nous disons que les bestes font par instinct; nous pouvons ce me semble supposer avec raison que cet instinct leur venant d'une cause eternelle, il doit estre

toûjours le mesme [et] exempt de toutes ces varietez qui distinguent tout ce qui vient des hommes. Entr'autres exemples je trouvay que les cellules des abeilles de mesme espece, mesurées dans le temps que les abeilles les bâtissent, sont égales entre elles, [et] ayant depuis mesuré celles des environs de Paris, de la Ville de Leyden, de Florence, je n'y trouvay aucune difference; [et] que si l'on suit les rangs selon lesquels les fonds ou bases de ces cellules sont disposées, l'on trouvera qu'un mesme nombre de cellules donne toûjours la mesme mesure. Ainsi rapportant toutes les mesures dont on se sert maintenant dans le monde, à celle des cellules des abeilles, la posterité pourra par ce moyen les connoistre toutes: Et cette mesure que je propose icy sera d'autant plus generale, qu'il y a des abeilles dans tous les endroits de la terre, aussibien aux lieux qui approchent des Poles, qu'en ceux qui sont plus avancez vers la ligne: Et quoy-que je l'établisse sur de la cire, rien ne m'empéche de croire qu'elle ne puisse durer autant que le monde, [et] qu'elle ne soit plus propre à ce dessein que le diaspre du tombeau sur lequel Gravius a marqué le pied Anglois, [et] plus aisée à entendre [et] à pratiquer que celle qui se peut tirer des vibrations du pendule, jointes à une observation celeste, comme on l'a voulu faire en France [et] en Pologne. Mais auparavant que de l'établir, je voudrois avoir pû comparer les ouvrages des abeilles de lieux éloignez, du Cap de Bonne Esperance [et] d'Egypte; par exemple, avec celles de la Moscovie [et] du Mexique, [etc.]. Et si elles [se] trouvent par tout égales, cette mesure se pourra rendre commune à toutes les nations, [et] par son moyen l'on pourra transmettre la connoissance des mesures de nostre siecle, à la posterité, qui est ce que l'on cherche".

8. "Ainsi l'on peut appliquer à ces ouvrieres les vers que le Poëte s'appliquoit à luy-mesme, [et] dire à leur honneur, In tenui labor, at tenuis non gloria. Ou bien souffrir qu'un Poëte Persan s'écrie avec une licence ordinaire aux Poëtes de son païs, Que si Archimede avoit examiné un ouvrage si surprenant, il se seroit mordu les doigts d'admiration avec les dents de l'envie".
9. See also the special issue of *Studies in Eighteenth-Century Culture*, 18 (1988), with essays by Carol Blum, Jeffrey Merrick, Ann Fairfax Withington, and Roseanne Runte.
10. Later, in the mid-eighteenth century, the geometry of the form of honeycomb cells was to be studied by Réaumur, Bazin, and Maraldi (although without the suggestion of a length standard): Fleck (1979), 32–33, and Spary (1999), 272–306.
11. As Olmsted notes (1942, 119), Auzout in his 1667 proposal for an expedition to Madagascar included the pendulum: Archives de l'Académie des Sciences (Paris), Registre des Procès-Verbaux, 2: ff. 43–50, at f. 49.
12. Picard to Cassini, 11/21 August 1671, Bibliothèque de l'Observatoire, ms B.4.11*bis*, bundle "Picard", letter 1 (sent from Hamburg).
13. Picard found the ratio of the Rhenish foot to the Paris foot to be 696:720, rather than 695:720, as had previously been thought, which implies that his measuring instruments were capable of distinguishing between sixtieths of an inch (Picard 1693, 2–3; reprint *MARS*, 7 (1), 194–5).
14. Bibliothèque de l'Observatoire (Paris), ms B.4.11*bis*, bundle "Picard", letter 7 (Picard to Cassini, 13 Feb. 1672): "les Anglois ont fait leur possible pour auoir les originaux, mais enfin nous sommes maitres".
15. These twin aims are made clear in Archives de l'Académie des Sciences (Paris), Registre des Procès-Verbaux, 7: f. 240v (8 April 1679): "Mr Roemer a fait voir les instruments qu'il porte en Angleterre pour obseruer la longueur de la pendule, et verifier la longueur du pied de Londres."
16. Flamsteed (1995–2002), 1: 690–92, Flamsteed to Towneley, 3 and 22 May 1679. Flamsteed reports (692, 22 May): "wee tried here the length of a pendulum that vibrates seconds and found it 39 1/8 inches English Measure, or of the Paris 36 71/100 hee has left a ball of the same weight with mee wherewith I intend to repeate the Experiment at my first leasure"; 36.71 inches is an approximation of the value that the Académie des Sciences was now using as its usual value for Paris (usually expressed as 36 inches, 8 1/2 lines).
17. For remarks on replication and craft skill in a contemporary context, see (among others) Collins (1992), and Collins (2001).
18. For metrology in the sociology of science, more generally, see Latour 1987, 247–57; O'Connell 1993; Mallard 1998; Schaffer 2000.

Acknowledgement I would like to thank all the participants at the Baroque Science workshop at the University of Sydney for their help with this essay, and especially Ofer Gal and Raz Chen-Morris.

References

Alder, Ken. 1995. A revolution to measure: The political economy of the metric system in France. In *The values of precision*, ed. M. Norton Wise, 39–71. Princeton: Princeton University Press.

Allen, Danielle. 2004. Burning the fable of the bees: The incendiary authority of nature. In *The moral authority of nature*, ed. Lorraine Daston and Fernando Vidal, 74–99. Chicago: University of Chicago Press.

Armogathe, Jean-Robert. 2001. Un seul poids, une seule mesure: le concept de mesure universelle. *XVIIe siècle* 213: 631–640.

Arneil, Barbara. 1996. *John Locke and America: The defence of English colonialism*. Oxford: Clarendon Press.

Bacon, Francis. 2000. *The new organon*, ed. Lisa Jardine (trans: Silverthorne, Michael). Cambridge: Cambridge University Press.

Baker, Keith M. 1990. *Inventing the French revolution: Essays on French political culture in the eighteenth century*. Cambridge: Cambridge University Press.

Bennett, Jim, and Scott Mandelbrote. 1998. *The garden, the ark, the tower, the temple: Biblical metaphors of knowledge in early modern Europe*. Oxford: Museum of the History of Science/ Bodleian Library.

Bernier, François. 1992 [1671]. Requeste des Maistres ès Arts, Professeurs et Regens de l'Université de Paris, présentée à la cour souveraine de Parnasse. In *Corpus: revue de philosophie*, ed. Sylvia Murr, 20–21: 231–239.

Blamont, Jacques. 2001. La mesure du temps et de l'espace au XVIIe siècle. *XVIIe siècle* 213: 579–611.

Bodin, Jean. 1583 [1576]. *Six Livres de la République*. Paris: Jacques Dupuis.

Bodin, Jean. 1992. In *On sovereignty*, ed. Julian H. Franklin. Cambridge: Cambridge University Press.

Bonno, Gabriel D. 1955. *Les Relations intellectuelles de Locke avec la France, d'après des documents inédits*. Berkeley/Los Angeles: University of California Press.

Burke, Peter. 1997. Fables of the bees: A case-study in views of nature and society. In *Nature and society in historical context*, ed. Mikuláš Teich, Roy Porter, and Bo Gustafsson, 112–123. Cambridge: Cambridge University Press.

Cassini, Giandomenico. 1693. De l'origine et du progrès de l'Astronomie, et de son usage dans la Geographie et dans la Navigation. In [Académie des Sciences], *Recueil d'Observations faites en plusieurs voyages par ordre de Sa Majesté, pour perfectionner l'astronomie et la géographie*. Separately paginated, 43 pages. Paris: Imprimerie royale. Reprinted in *Memoires de l'Académie Royale des Sciences*, *1666–99*, 8: 1–52.

Collins, Harry. 1992. *Changing order: Replication and induction in scientific practice*. 2nd ed. Chicago: University of Chicago Press. (Orig. ed. 1985.)

Collins, Harry. 2001. Tacit knowledge, trust and the Q of sapphire. *Social Studies of Science* 31(1): 71–85.

Costabel, Pierre. 1987. Picard et l'étalon universel de longueur fondé sur le pendule. In *Jean Picard et les débuts de l'astronomie de précision au XVIIe siècle*, ed. Guy Picolet, 315–328. Paris: CNRS.

Cumberland, Richard. 1686. *An essay towards the recovery of the Jewish measures and weights, comprehending their monies, by help of ancient standards, compared with ours of England*. London: Richard Chiswell, printer to the Royal Society.

Defossez, Léopold. 1946. *Les Savants du XVIIe siècle et la mesure du temps*. Lausanne: Edition [sic] du Journal Suisse d'Horlogerie et de Bijouterie.

Dew, Nicholas. 2006. Reading travels in the culture of curiosity: Thévenot's collection of voyages. *Journal of Early Modern History* 10(1–2): 39–59.

Dew, Nicholas. 2008. *Vers la ligne*: circulating measurements around the French Atlantic. In *Science and empire in the Atlantic world*, ed. James Delbourgo and Nicholas Dew, 53–72. New York: Routledge.

Dew, Nicholas. 2009. *Orientalism in Louis XIV's France*. Oxford: Oxford University Press.

Dew, Nicholas. 2010. Scientific travel in the Atlantic world: The French expedition to Gorée and the Antilles, 1681–1683. *British Journal for the History of Science* 43: 1–17.

Findlen, Paula. 1990. Jokes of nature and jokes of knowledge: The playfulness of scientific discourse in early modern Europe. *Renaissance Quarterly* 43: 292–331.

Findlen, Paula. 1994. *Possessing nature: Museums, collecting and scientific culture in early modern Italy*. Berkeley/Los Angeles: University of California Press.

Findlen, Paula. 1998. Between carnival and lent: The scientific revolution at the margins of culture. *Configurations* 6(2): 243–267.

Flamsteed, John. 1995–2002. *The correspondence of John Flamsteed, the first astronomer royal*, ed. E.G. Forbes, L. Murdin, and F. H. Willmoth, 3 vols. Bristol: Institute of Physics.

Fleck, Ludwik. 1979. *Genesis and development of a scientific fact* (trans: Bradley, Fred and Thaddeus J. Trenn). Chicago: University of Chicago Press.

Freedberg, David. 2002. *The Eye of the Lynx: Galileo, his friends, and the beginnings of modern natural history*. Chicago: University of Chicago Press.

Giustini, Pietro Alessandro. 1992. Tito Livio Burattini e la nascita della metrologia scientifica. In *La matematizzazione dell'universo: momenti della cultura matematica tra '500 e '600*, ed. Lino Conti, 360–362. Assisi: Porziuncola.

Greaves, John. 1647. *A discourse of the Romane foot, and denarius: from whence, as from two principles, the measures and weights, used by the ancients, may be deduced*. London: Printed by M. F. for William Lee.

Hartlib, Samuel. 1655. *The reformed common-wealth of bees: Presented in severall letters and observations to Samuel Hartlib, Esq*. London: Printed for Giles Calvert.

Haustein, Heinz-Dieter. 2001. *Weltchronik des Messens: Universalgeschichte von Mass und Zahl, Geld und Gewicht*. Berlin: De Gruyter.

Heilbron, John L. 1993. *Weighing imponderables and other quantitative science around 1800*. Historical studies in the physical and biological sciences: Supplement to vol. 24, part 1. Berkeley: University of California Press.

Hooke, Robert. 1935. In *The diary of Robert Hooke, M.A., M.D., F.R.S., 1672–1680*, ed. H.W. Robinson and W. Adams. London: Taylor & Francis.

Huygens, Christiaan. 1986 [1673]. *The pendulum clock, or geometrical demonstrations concerning the motion of pendula as applied to clocks* (trans: Blackwell, Richard J., with an introd. by H. J. M. Bos). Ames: Iowa State University Press, 1986.

Johns, Adrian. 1998. *The nature of the book: Print and knowledge in the making*. Chicago: University of Chicago Press.

Koyré, Alexandre. 1953. An experiment in measurement. *Proceedings of the American Philosophical Society* 97(2): 222–237.

Kula, Witold. 1985 [1970]. *Measures and men* (trans: Szreter, R.). Princeton: Princeton University Press.

Lancelot, Claude. 1667. *Dissertation sur l'Hemine de Vin, et sur la Livre de Pain de S. Benoist, et des autres anciens Religieux*. Paris: Charles Savreux.

Latour, Bruno. 1987. *Science in action: How to follow scientists and engineers through society*. Cambridge: Harvard University Press.

Lewis, Rhodri. 2007. *Language, mind, and nature: Artificial languages in England from Bacon to Locke*. Cambridge: Cambridge University Press.

Locke, John. 1953. In *Locke's travels in France, 1675–1679: As related in his journals, correspondence and other papers*, ed. John Lough. Cambridge: Cambridge University Press.

Locke, John. 1975. In *An essay concerning human understanding*, ed. P.H. Nidditch. Oxford: Clarendon.

Locke, John. 1976–1989. *The correspondence of John Locke*, ed. E.S. de Beer, 8 vols. Oxford: Clarendon Press. Also available from Intelex Past Masters, at: http://library.nlx.com/

Mallard, Alexandre. 1998. Compare, standardize and settle agreement: On some usual metrological problems. *Social Studies of Science* 28(4): 571–601.

Matthews, Michael R. 2000. *Time for science education: How teaching the history and philosophy of pendulum motion can contribute to science literacy*. New York/Boston/Dordrecht: Kluwer Academic/Plenum Publishers.

McKeon, Robert M. 1965. Etablissement de l'astronomie de précision et œuvre d'Adrien Auzout. Doctoral thesis, University of Paris.

Mouton, Gabriel. 1670. Nova mensurarum geometricarum Idea: Et nova methodus eas, et quascumque alias mensuras communicandi, et conservandi in posterùm absque alteratione. In Mouton, *Observationes diametrorum solis et lunæ apparentium, meridianarúmque aliquot altitudinum Solis et paucarum fixarum*, 427–48. Lyon: Matthæi Liberal.

O'Connell, Joseph. 1993. Metrology: The creation of universality by the circulation of particulars. *Social Studies of Science* 23(1): 129–173.

Oldenburg, Henry. 1965–1986. *The correspondence of Henry Oldenburg*, ed. A.R. Hall and M.B. Hall, 13 vols. Madison, Milwaukee, and London: University of Wisconsin Press.

Olmsted, John W. 1942. The scientific expedition of Jean Richer to Cayenne (1672–1673). *Isis* 34: 117–128.

Olmsted, John W. 1960. The voyage of Jean Richer to Acadia in 1670: A study in the relations of science and navigation under Colbert. *Proceedings of the American Philosophical Society* 104(6): 612–634.

Pedersen, Kurt Møller. 1987. Une mission astronomique de Jean Picard: le voyage d'Uraniborg. In *Jean Picard et les débuts de l'astronomie de précision au XVIIe siècle*, ed. Guy Picolet, 175–203. Paris: CNRS.

Picard, Jean. 1671. *Mesure de la Terre*. Paris: Imprimerie royale. Reprinted in *Memoires de l'Académie Royale des Sciences*, *1666–99*, 7 (1): 133–192.

Picard, Jean. 1693 [1680]. Voyage d'Uranibourg, ou Observations astronomiques faites en Dannemarck par Monsieur Picard de l'Académie Royale des Sciences. In [Académie des Sciences], *Recueil d'Observations faites en plusieurs voyages par ordre de Sa Majesté, pour perfectionner l'astronomie et la géographie*. Separately paginated fascicule (29 pp). Paris: Imprimerie royale. Reprinted in *Memoires de l'Académie Royale des Sciences*, *1666–99*, 7(1): 193–232.

Picolet, Guy. 1979. Une lettre inédite de l'abbé Picard à Colbert (13 octobre 1671). *XVIIe siècle* 125: 389–395.

Pliny, the elder. 1991. *Natural history: A selection* (trans: Healy, John F.). Harmondsworth: Penguin

Raylor, Timothy J. 1992. Samuel Hartlib and the commonwealth of bees. In *Culture and cultivation in early modern England: Writing and the land*, ed. Michael Leslie and Timothy J. Raylor, 91–129. Leicester: Leicester University Press.

Sarton, George. 1935. The first explanation of decimal fractions and measures (1585). Together with a history of the decimal idea and a facsimile (no. XVII) of Stevin's Disme. *Isis* 23(1): 153–244.

Schaffer, Simon. 2000. Modernity and metrology. In *Science and power: The historical foundations of research policies in Europe*, ed. L. Guzzetti, 71–91. Luxembourg: European Communities.

Schaffer, Simon. 2009. Newton on the beach: The information order of *Principia mathematica*. *History of Science* 47: 243–276.

Shalev, Zur. 2002. Measurer of all things: John Greaves (1602–1652), the great pyramid, and early modern metrology. *Journal of the History of Ideas* 63(4): 555–575.

Spary, Emma. 1999. The nature of enlightenment. In *The sciences in enlightened Europe*, ed. William Clark, Jan Golinski, and Simon Schaffer, 272–306. Chicago: University of Chicago Press.

Thévenot, Melchisédech. 1681. Discours sur l'art de la navigation. In Thévenot, *Recueil de voyages*. Paris: Estienne Michallet. Separately paginated.

Virgil. 1982. *The Georgics* (trans: Wilkinson, L.P., Penguin Classics). Harmondsworth: Penguin
Wilkins, John. 1668. *An essay towards a real character, and philosophical language.* London: S. Gellibrand, and John Martyn, printer to the Royal Society.
Woolfson, Jonathan. 2010. The renaissance of bees. *Renaissance Studies* 24(2): 281–300.
Woolhouse, Roger. 2007. *Locke: A biography*. Cambridge: Cambridge University Press.
Zupko, Ronald Edward. 1978. *French weights and measures before the revolution*. Bloomington: Indiana University Press.

Chapter 11
Chymical Philosophy and Boyle's Incongruous Philosophical Chymistry

Victor D. Boantza

Abstract During the 1660s and 1670s Boyle proposed a reformation of traditional and contemporary chymical philosophy and practice by reinterpreting them along mechanical principles. By submitting chymical phenomena to the laws of matter and motion, coupled with a systematic experimentalism, Boyle looked to introduce intelligibility and simplicity into an allegedly ambiguous chymical discourse. During the late 1660s, Samuel Cottereau Duclos (1598–1685), leading chymist of the early French Royal Academy of Sciences, provided a critical assessment of Boyle's "physico-chymical" creation. Attentive in part to Boyle's critique and dismissive of certain aspects of traditional chemical philosophies, Duclos favored a clarification and re-contextualization of chymistry that would not dissociate it from its historical roots, assuming a different stand within the 'ancients' versus 'moderns' debate. By perceiving Boyle qua reformer (as a member of the very scientific community Boyle sought to reform), Duclos exposed Boyle's lack of experimental proficiency and acquaintance with the chymical realm. Reading Boyle's new "physico-chymical" science from a distinctly chymical perspective, Duclos revealed its paradoxical and incongruous nature, rendering it a discordant solution: a baroque middle ground which ultimately compromised chymistry's status as the ultimate science of matter and material change.

Abbreviations

AdS, PV	= Académie Royale des Sciences, Procès-Verbal de séance, Paris, France.
SC	= Boyle Robert. *The Sceptical Chemist*. (in *Works*, vol. II)
CPE	= Boyle Robert. *Certain Physiological Essays*. (in *Works*, vol. II)
OFQ	= Boyle Robert. *The Origin of Forms and Qualities*. (in *Works*, vol. V)

V.D. Boantza (✉)
Unit for History and Philosophy of Science, The University of Sydney, Sydney, Australia
e-mail: boantza@gmail.com

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_11,

"Precise Speculations" and "Sensible Operations"

A little known figure in the history of science, Samuel Cottereau Duclos (1598–1685) was the establisher of the chemical laboratory and research program at the newly inaugurated Parisian Royal Academy of Sciences, founded in 1666 (Metzger 1969, 266–272; Partington 1961–70, III, 11–13; Debus 1991, 151; Principe 1998, 40). Duclos has been receiving lately increasing scholarly attention (Stroup 1990, 2002; Clericuzio 2000, 178–180; Kim 2003, esp. 48–52; Holmes 2003; Jacob 2006, esp. 52–65; Boantza 2007, 2010, 2012; Franckowiak 2008, 2009), yet we still know little about his life and work. Nearly half a century after his death, Bernard de Fontenelle, the perpetual secretary to the Academy, remarked that Boyle had "ventured to explain all chemical phenomena according to the corpuscular philosophy, that is, by the sole movement and configuration of small particles. Mr. du Clos… being perhaps more chemically minded, found it unnecessary and impossible to reduce [chemistry] to such clear principles as shapes and motions, and had subsequently subscribed to a misleading obscurity" (Fontenelle 1733, I, 79).[1]

A devout Cartesian, Fontenelle offered an evocative distinction between chemistry and physics, suggesting that chemistry "resolves bodies by sensible operations into certain gross and tangible principles such as salts, sulfurs, etc. Physics, however, by the power of its precise speculations, acts upon these principles, like chemistry does on bodies, by resolving them into yet finer and simpler principles, that is, the motion and infinite configurations of small particles. Herein, then, lies the principal difference between physics and chemistry, akin to the difference between Mr. Boyle and Mr. Duclos." "The spirit of chemistry," Fontenelle concluded, "is highly confused and shrouded in mystery; it resembles the mixts, the principles [constituents] of which are entangled within each other"; by contrast, "the spirit of physics is very clear, simple and unobstructed" (ibid., 80).[2]

Given his mechanistic tendencies, and considered in retrospect, Fontenelle's evaluation is not surprising. It squares well with numerous other contemporary depictions of Boyle, praising his association with "physics," the "corpuscular philosophy," and clarity, as opposed to a chemistry of "principles" hopelessly plagued by "a misleading obscurity." Nor is it substantially different from the first modern assessments of Boyle's science.[3] My main interest in Fontenelle's words has to do with their particular origin and context. The words comprise Fontenelle's assessment—in the first volume of his *Histoire de l'Académie Royale des Sciences*, covering the Academy's activities during its first two decades—of Duclos' scrutiny of Boyle's *Certain Physiological Essays*, conducted during the winter of 1668–69.[4] From September to February, the weekly Saturday meetings of the philosophical section (*physique*)[5] were dedicated to a close reading of Boyle's *Essays*, followed by Duclos' theoretical and experimental evaluation of their various dimensions.

Although the examination of the *Essays* marks Duclos' most systematic and comprehensive treatment of Boyle, it was not the only one. From 1666 to 1669, Duclos was the most active and vocal member among founder-academicians and referred to Boyle's work on numerous occasions (Sturdy 1995, 107–108).[6] Awaiting the establishment of the chymical laboratory, Duclos used the opportunity to raise chymical issues and spark discussions over matter theories, the nature of chymical analysis and its corresponding elemental perceptions, chymical experimentation and textual authorship, the nature of fluidity, cohesion, and material change, as well as the application of the corpuscular philosophy to chymical theory and practice.[7] Even a cursory inspection of the variety of chymical subjects explored by Duclos (mostly by way of lecture-demonstrations) reveals a close kinship to the issues that preoccupied Boyle, especially during the 1660s, his most active and fruitful period of chymical inquiry. Having immersed himself in natural philosophy and chymical studies during the late 1640s and 1650s, in the following decade Boyle published his influential *Sceptical Chymist* (1661), the *Certain Physiological Essays* (1661), *The Usefulness of Natural Philosophy* (pt. 1; 1663), *The Origin of Forms and Qualities* (1666), among other works. In what follows, Duclos' references to the *Origin of Forms of Forms and Qualities* (hereafter *OFQ*) and to the *Certain Physiological Essays* (hereafter *CPE*) are most prominent. Throughout his memoirs, Duclos mentioned Boyle's "Chymista Scepticus"[8] only once, pointing out the Englishman's favorable mention of Van Helmont's alkahest (AdS, PV 4: 144v). There is little doubt, however, that many of the chymical subjects Duclos invoked in front of the assembly, especially during 1666–68, were closely related to the critique of traditional chymistry, prominently exemplified by *The Sceptical Chymist* (hereafter *SC*).

The Crisis of Chymical Principles

Recent scholarship has pointed to the previously overlooked complex character of the *SC*, a multi-layered and polemical manifesto that announced and created a crisis in seventeenth-century chymistry (Clericuzio 1994; Principe 1998). Although the precise categorization of the subjects of Boyle's skepticism has many ramifications, bearing as much on Boyle as on the identity of the seventeenth-century chymical community, two groups stand out as clear targets: the Paracelsian systematizers and the iatrochemical textbook writers. The former, exemplified for instance by the work of Joseph Duchesne (*Quercetanus*; discussed below) (Kahn 2007, *passim*; Hirai 2001), are condemned for advancing grand cosmologies on frail experimental grounds; the latter, represented for instance by Jean Beguin (Metzger 1969, 35–51; Partington 1961–70, III, 2–4; Clericuzio 2006), are reprimanded for reducing chymistry to mere technical practice, manual operations, and pharmaceutical pursuits. Both groups have their origins in the Paracelsian movement. At the core of Boyle's critique stood his claim for a discrepancy between chymical theory and practice—the two domains have drifted apart and gained a disadvantageous independence (Cf. Joly 1992). This undesired effect was most vividly captured by the mismatch between chymical analysis and elemental theories. Boyle honed in on this weakness to present a lengthy critique of fire analysis (distillation) and its corresponding products, regarded by Paracelsians, spagyrists, and vulgar chymists as the elementary constituents of bodies (Debus 1967; Holmes 1971).

While for Fontenelle chymistry was "confused" and "shrouded in mystery," Boyle complained about the chymists' "obscure, ambiguous, and almost

Aenigmatical Way of expressing what they pretend to Teach," a practice and discourse arising from "their Dark and Smoakie Laboratories," awaiting to be exposed and "brought into the open light" (*SC*, 209–211). A problem of still graver import, however, can be gleaned from Fontenelle's (incidental) equivocal use of "principles": to denote Boyle's adoption of "clear *principles* [such] as shapes and movements" of minute particles on the one hand, and to refer to Duclos' advocacy of dubious "*principles* such as salts, sulfurs, etc." on the other. On this account, whereas Boyle subscribed to the purportedly clear notions of the mechanico-corpuscular program, Duclos is associated with a matter theory predicated on the Paracelsian *Tria Prima*, the three principles-elements of salt, sulfur, and mercury. In both cases, the main allusion is to a particular matter theory closely associated with a research program—the physical and the chymical, respectively.

In the prefatory passages to the *SC* Boyle declared his "unsatisfyedness not only with the Peripatetick, but with the Chymical Doctrine of the Primitive Ingredients of Bodies" (*SC*, 215). The former represents matter theories and chymical explanations based on the Four Aristotelian Elements (earth, water, fire, air), which Boyle faulted for being rationally rather then empirically deduced. Since the "Assertors of the four Elements value Reason so highly," they have considered it "much more high and Philosophical to discover things *a priore*, then *a posteriore*. And therefore the Peripateticks have not been very sollicitous to gather Experiments to prove their Doctrines." Nevertheless, Boyle granted that the peripatetic doctrine was, to a certain extent, "clear and intelligible to the Understanding as obvious to the sense"; after all it originated with Aristotle who drew upon "Theories of former Philosophers, which are now with great applause revived" (ibid., 221–222). In discussing the advocates of the "Chymical Doctrine," however, Boyle employed a most vitriolic language, depicting "*Paracelsus* and some few other sooty Empiricks" as philosophers who

> having their eyes darken'd, and their Brains troubl'd with the smoke of their own Furnaces, began to rail at the Peripatetick Doctrine, which they were too illiterate to understand, and to tell the credulous World, that they could see but three Ingredients in mixt Bodies; which to gain themselves the repute of Inventors, they endeavoured to disguise by calling them, instead of Earth, and Fire, and Vapour, Salt, Sulphur, and Mercury, to which they gave the canting title of Hypostatical Principles (ibid., 223).

Boyle rejected the peripatetic Four Elements, the chymical *Tria Prima*, as well various contemporary combination thereof (mostly operative elemental pentads). In fact, Boyle denied the possibility of a definite number of elements conforming to chymical analysis. What Boyle was ultimately after, however, were not the experiments adduced by chymists, but their interpretations of these experiments: "It is one thing to be able to help Nature to produce things, and another thing to Understand well the Nature of the things produc'd" (ibid., 278). More specifically, he proclaimed that, "There is a big Difference betwixt the being able to make Experiments, and the being able to give a Philosophical Account of them" (ibid., 294; see Sargent 1995; Cf. Franckowiak 2009).

The tenor of Boyle's discussion in the *SC* is mostly critical, condemning various contemporary matter theories, chymical cosmologies, and analytical chymical perceptions. Yet his own suggestions and remedies are only mentioned hesitatingly and sporadically. He charged the spagyrists for failing to provide causal explanations; for performing experiments while being prejudiced by theory; for experimentally identifying the *Tria Prima* because it was what they had expected to find. Boyle mostly criticized the "Suppositions which Chymists as well as Periateticks, without proving, take for granted; and upon which Depends the Validity of the Inference they draw from their Experiments" (*SC*, 277). The main thrust of Boyle's critique can be gleaned from the various descriptions he used to convey his reservations about the state of contemporary chymistry. Chymical discourse was "Obscure, Ambiguous and Aenigmatical"; chymists failed to write "intelligibly enough" and exhibited an "over great-reservedness"; "without proving" their arguments, the "validity" of their interpretations was questionable; given the "unreasonable liberty they give themselves of playing with Names at pleasure," chymical entities lacked fixed referents; finally, Boyle admonished chymists for not having "Clear and Distinct Notions" concerning elements, and for being "Un-Philosophical" (*SC*, 209, 211, 213, 277, 291–292).

Striving for clarity, intelligibility, open discourse, causal proof, "validity," and "Clear and Distinct Notions," in works following the *SC*, Boyle advanced a reformulation of chymical discourse along what he aptly designated as "physico-chymical" or "chymico-physical" lines.[9] The traditional claim, revolving around the association of Boyle's chymical pursuits with "the spirit of physics" (Hall 1958), in the sense conveyed by Fontenelle, has been challenged and qualified (Clericuzio 1990; Kim 1991; Chalmers 1993; Principe 1994, 1998). Yet even in the opening lines of the *SC* Boyle highlighted the link between chymical experiments, mechanical perceptions, and causality:

> though I am a great Lover of *Chymical Experiments* … I distinguish these from their Notions about the *Causes of things*, and their manner of Generation. And for ought I can hitherto discern, there are a thousand *Phaenommena* in Nature … which will scarcely be clearly & satisfactorily made out by them that confine themselves to deduce things from Salt, Sulphur and Mercury, and the other Notions peculiar to the Chymists, without taking much more Notice than they are wont to do, of the *Motions and Figures, of the small Parts of Matter*. (*SC*, 208; *italics* added)

This passage represents, in an abstract sense, the outline of Boyle's *reformative* program—encompassing his vision for a solution to the aforementioned difficulties—on which he would further elaborate in several works, and particularly in the *CPE*, which drew the close attention of Duclos and the Academy.

"Out of the Strong Came Something Sweet": Secrecy and Double Standards

Duclos' first substantial discussion of Boyle came in a memoir entitled "Observations on certain salts, effectively sweet, drawn from highly acrid materials."[10] The memoir, "proposed to the assembly" on 26 March 1667, was delivered the following week. The opening statement reads: "The first of these observations is by Mr. Boyle who, like Samson, has presented an enigma" (AdS, PV, 1: 93–94). The reference is to the fourth experiment in the second section of the "historical part" of Boyle's then recently published *OFQ* (1666) (*OFQ*, xxviii).[11]

Duclos' allusion to Samson is instructive. Having chosen Delilah for wife from among the philistines at Timnah Samson threw a weeklong wedding feast. To the celebrating philistines he said: "let me now put a riddle to you; if you can tell me what it is, within the 7 days of the feast, and find it out, then I will give you 30 linen garments and 30 festal garments; but if you cannot tell me what it is, then you shall give me 30 linen garments and 30 festal garments." Samson's riddle—"Out of the eater came something to eat. Out of the strong came something sweet"—refers to an incident where he was attacked by a lion. Samson tore the lion apart with his bare hands and went on his way. Some time later, he came back by the place where he had slain the lion and observed that inside the carcass a swarm of bees had made a hive; inside the hive was a honeycomb full of honey. Thus the phrases "out of the eater" and "out of the strong" refer to the lion; the phrases "something to eat" and "something sweet" refer to the honey. Unable to respond to the challenge, the philistines threatened and pressured Delilah, who enticed her husband and ultimately divulged the answer: "What is sweeter than honey? What is stronger than a lion?" To this, an enraged Samson replied, accusing the philistines, "If you had not plowed with my heifer, you would not have found out my riddle," the "heifer" standing for Delilah (Judges 14: 12–18; Darby Bible Translation).[12]

Duclos used the fable as a metaphor to point to Boyle's condescending view of chymists, for if Boyle was likened to Samson the chymists were but lowly philistines. Duclos mocked Boyle for not abiding by his own standards. Having condemned the chymists for their "over great-reservedness" and for writing "enigmatically"—which he deemed as detrimental to scientific and philosophical discourse—Boyle presented, according to Duclos, an "enigma." Unlike Samson, Duclos added, he did not "promise a reward" (AdS, PV, 1: 93–94). This cynical tone, as we shall see, forms a prominent and recurrent theme in Duclos' reading of Boyle, especially in suggesting that the Englishman often presented various excuses and justifications in order to cover up for his lack of chymical knowledge and experimental proficiency.

Addressing Pyrophilus,[13] Boyle excused himself for discoursing "upon the *Phaenomena* of an Experiment, which I do not teach you to make … since I cannot as yet … plainly disclose to you what I must now conceal" (*OFQ*, 407). Duclos was hardly impressed with Boyle's apology, which he deemed as unacceptable and duplicitous. Duclos reformulated Samson's riddle—"Out of the strong came something sweet"—and recast it as a chymical query—"what is the sweetness that proceeds from acrimony"—relating it directly to Boyle's "enigma." Presenting the issue before the assembly, Duclos stated:

> Mr. Boyle, having extracted a sweet salt out of some very acrid materials, refuses to elaborate. He only describes a few [of its] singular qualities, in virtue of which he had designated it as anomalous. After having excused himself for acting against his own custom and inclination, he proposes to uncover a curious experiment, which he is committed to keep secret and not divulge, either the materials [involved], or the method. He describes some properties of an extraordinary salt that he first produced following his own plan and then remade, while adding something following the advice of a learned and well traveled chymist, who recommended it to him as a highly special and precious salt. (AdS, PV, 1: 94)[14]

These words set the stage for the rest of Duclos' memoir concerning "Observations on certain salts … drawn from highly acrid materials," which is dedicated to a detailed inquiry into the origins and nature of this "precious salt." The account presents, among other things, an exposition of numerous weaknesses on Boyle's part, as perceived from a distinctly chymical standpoint, and as opposed to Boyle's "physico-chymical" middle grounds.

Boyle's excuse for not disclosing "the way of making this Salt" was that he found it "so nice and intricate a thing … [that he] could scarce easily describe it, so as to enable most men to practice it." Duclos noted Boyle's statement and proceeded to render a detailed list of various other properties of the salt, as depicted by Boyle, to which Duclos would later return. For instance, according to Boyle, all the ingredients composing the salt were "far more salt then Brine, or more sowr than the strongest Vinegar," yet the salt itself was "rather sweet." Boyle stressed this was the "onely instance" he had encountered in which salts "compose a substance *really sweet*" (*OFQ*, 407–408). In his closing remark, Duclos critically emphasized those "properties of the sweet salt that Mr. Boyle drew from certain salty, acidic and acrid materials, which he refuses to otherwise specify." (AdS, PV, 1: 97).

Duclos drew unmistakable parallels between Boyle's mysterious salt and a similar substance and extraction reported by the German pharmacist Johan Schröder, in his *Quercetanus redivivus, hoc est, Ars medica dogmatico-hermetica* (1638). The experiment in question, Duclos explained, is attributed to no other than *Quercetanus*, the Latinized namesake of Joseph Duchesne (d. 1609; also known as Sieur de la Violette), commonly considered as a Paracelsian systematizer *par excellence*, in particular due to the metaphysical chymical cosmology expounded in his major work *Le grand miroir du monde* of 1587 (Debus 1991, 51–59).

Displaying his supreme knowledge of the chymical literary corpus, Duclos set out to solve Boyle's "enigma." He informed the assembly that much like Boyle, Duchesne described the production of a sweet salt out of salty, acidic, and acrid substances. He then proceeded to deliver sections of Duchesne's recipe *in extenso*, partly due to Duclos' didactic style of presentation (lecture-demonstration style) and partly because he found Duchesne's detailed description (recipe style) suggestive of the importance accorded to the practitioner's experimental proficiency:

> If we commence [our discussion] with the crystals of the marine salt, which are obtained from such an acrid and salty material, there is little doubt that the illiterate [uninformed, uninitiated] will mock us, claiming this impossible … But we do not submit to such judgments. It is of the True Philosophers that we write here, those in possession of vast knowledge, who know the truth. It is because of them, however, that we can be understood only by those initiated in this art, who know the terms, and who are imbued with the True Philosophy … having dissolved the marine salt in its proper and natural menstruum, then filtering and coagulating the solution according to the rules of the art, and repeating this until the salt became very pure and clear, one should take six pounds and pour them onto a vitriolic and mellifluous solvent, consisting of vegetal and animal matters; the quantity should enable a good fermentation; after the required digestion has been achieved, [the solvent] acts as a vehicle to elevate with greater ease the phlegmatic, sulphureous, and vitriolic spirits, both sweet and acidic, which are strongly attached to the salt. This extraction should be conducted in an earth retort, which can sustain the fire and yield it in degrees in a most precise and accurate manner, for this is the principal thing in this whole affair. This is why the

> operation should be performed by an artist who knows well how to manipulate the furnace and regulate the fire … for the spirits to be given off properly, this equal degree of fire must be maintained for eight days … the insipid phlegm should then be distilled on a very gentle fire using a vapor bath, the alembic placed in a cold place, where very distinct, sweet tasting crystals will form (AdS, PV, 1: 97–99).[15]

According to Duchesne, this sweet salt had several remarkable characteristics. It is capable of dissolving gold "radically" and of "heightening" its vital virtues, which suggests Duchesne's association of the salt with the practice of metallic transmutations. Moreover, should withered and dry flowers be drenched in a solution of this salt in *aqua fortis* (nitric acid), they would recover their previous color and vivacity. Finally, Duchesne reported his preparation of a "universal medicine" out of this salt, which he had successfully administered (ibid., 99). Boyle made do with mysterious allusions, stating that the salt is "exceedingly noble" and that "besides some of the things I had been told [by the anonymous traveler] it would perform, I could do divers other things with it"; but since some related phenomena are "not so proper for this place, [they] are reserv'd for another" (*OFQ*, 407). It appears that Boyle too acknowledged some of the salt's powerful properties and at least associated it with some kind of heightened activity, the precise nature of which he refused to disclose.

Before returning to Duclos' discussion of Duchesne, a few contextualizing remarks concerning Boyle are in place. In the *OFQ* Boyle declared that he "deliver[s] Experiments, not so much as parts of Natural History, [but] as instances to confirm the *Hypotheses*, and Discourses they are annexed to." And it is the "Particularian Philosophy" that Boyle sought to "Confirm and Illustrate" by presenting various chymical phenomena. He urged corpuscularians to "endeavour to illustrate and promote the New Philosophy, by addicting themselves to Experiments, and perusing the Books of Chymists," which would help "make the *Corpuscularian Philosophy, assisted by Chymistry*, preferred to that which has so long obtained in the Schools" (*OFQ*, 392–393).

To this end, Boyle set out to present in the *OFQ*, partly in a Baconian vein, a multitude of "Notes and Experiments concerning the Productions and Changes of Particular Qualities." Seeking to advance the "Principles of the Corpuscularian Philosophy," he undertook to "subjoyn some such Natural *Phaenomena*, as either induce me to take up such Notions, or which I was directed to find out by the Notions I had imbrac'd." This is the backdrop against which the report concerning the "anomalous salt" should be read. Even though the experimental observations expounded in the *OFQ* are explicitly interpreted in a corpuscularian framework, some, like the ones pertaining to the sweet salt, comprise an exposition of various "Productions and Changes of [its] Particular Qualities." Yet Boyle clearly trusted that "Nature"—whether "Master'd by Art" or left "to disclose her Self freely"—will not fail to "attest the Truth of our [corpuscularian] Doctrine" (*OFQ*, 381).

In Duclos' final opinion, Boyle's sweet anomalous salt and Duchesne's sweet crystals "[were] one and the same," an identification that was further supported by the nature of "the substances from which the salt is extracted, its qualities and its virtues" (AdS, PV, 1: 100). Duclos' first comment, following the lengthy description of Duchesne's procedure,[16] referred directly to the interpretation of the salt's

"Particular Qualities." Boyle's primary reason for discussing the salt in the context of "qualities" arose from the stark contrast between the taste of the components and of the compound:

> several Ingredients, that compos'd this Salt, were all of them such, as Vulgar Chymists must according to their Principles, look upon as purely Saline, and were each of them far more salt then Brine, or more sowr then the strongest Vinegar, or more strongly tasted then either of those two Liquors; yet the Compound, made up of onely such Bodies, is so far from being eminently salt, or sowr, or insipid, that the Stranger being ask'd, what Tast it had, would not scruple to judge it rather sweet, than of any other Tast (*OFQ*, 407).

Boyle presupposed that any "Vulgar" explanation of this phenomenon would require recourse to substantial qualities (possibly their multiplication) for representing the three distinct tastes (Cf. Henry 1986; Anstey 2000; Hutchison 1982; Alexander 1985; Emerton 1984), corresponding to "several Ingredients" or constituents of the salt, the identity of which Boyle, once more, refused to reveal. Following Duchesne's procedure—as well as his own experience—for Duclos, there was little mystery involved.

The "three different tastes," he explained, "mark three kinds of different materials which correspond closely to three substances which compete materially in the production of the crystals of the sweet salt." Duclos identified these as the marine salt (salty), the honey vinegar (sour)[17] and the acid (spirit), which is distilled and extracted from the two (acrid; or in effect the one Boyle thought "more strongly tasted then either of those two Liquors"). Whereas Boyle wrote of a salt that is "rather sweet, than of any other Tast," a "*really sweet*" salt, Duclos proclaimed:

> the anomalous salt of Mr. Boyle is sweet, it is endowed with a real sweetness, and [Boyle] is astonished as to how this sweetness can proceed from salty materials. Mr. Du Chesne's crystals are possessed of a *real and manifest* sweetness (AdS, PV, 1: 101).

Herein, then, to paraphrase Fontenelle, lies one of the principal differences between Boyle and Duclos, who added:

> If this astonishes a learned and great chymist like Mr. Boyle, Mr. Du Chesne is well justified in his fear of the judgment of the ignorant and of being ridiculed for proposing the extraction of sweet crystals from marine salt (ibid., 103).

We might wonder who is the 'vulgar' chymist and who is the 'philosophical' one. *Prima facie*, Duclos presented to the assembly a chymical problem, which he proceeded to resolve. Yet both the context and the text suggest more. Duclos' reading of Boyle is intricate; we can learn as much from the critique leveled against Boyle as we can from Duclos' discussion. Ultimately, it forms a testimony concerning the ways by which a seventeenth-century 'chymical philosopher' read an aspiring 'philosophical chymist'.[18] In particular, it marks a traditional chymical practitioner's reactionary statement against an innovative and self-proclaimed revisionist chymical program.

Throughout the memoir Duclos displayed his authority in both his critical as well constructive assessments. Duclos deconstructed Boyle's account while employing it as a vehicle to both expose Boyle—and what Boyle stood for—and to advance his own agenda. While Boyle was interested in refashioning chymistry according to new "physico-chymical" principles, in the spirit of the New Science, and especially in the context of the mechanical philosophy, Duclos was intent on reforming chymistry by clarifying, possibly recasting, its status vis-à-vis its complex traditional origins. These were two radically different endeavors, represented by two different strategies to transform chymistry into a "modern" pursuit, as Duclos referred to it numerous times, setting it apart without severing it from its ancient, alchemical, and Hermetical origins. Duclos distinguished repeatedly between "philosophes hermetiques" and "chimistes vulgaires," differentiating them from "les chymiste[s] modernes" (AdS, PV, 1: 6–7).

Duclos faulted Boyle's lack of chymical knowledge while insisting on aspects of traditional chymistry that he thought could not be glossed over without compromising essential and distinct dimensions of chymical knowledge and practice, as part of a fundamental and complete interpretation of natural phenomena. In defending traditional chymistry, Duclos upheld high metaphysical and epistemological standards, in which regard he might be seen, as Frankowiack has recently suggested, as a sort of "post-*Sceptical Chymist* chymist" (Frankowiack 2009).[19] Boyle's relationship to chymistry's roots, tradition, and past had always been convoluted and ambivalent. Given his inconsistent acknowledgment of his sources (see, e.g., Newman 2006), it is hardly surprising that we lack clear evidence of his knowledge or recognition of Duclos. Duclos' abundant references to Boyle,[20] accentuated by his lengthy and systematic scrutiny of the *CPE*, signal the importance Duclos had ascribed to Boyle's work. Reflecting upon this critique—be it a virtual debate—from a stylistic perspective, Duclos' reading of Boyle embodies a tension which came to be recognized as markedly baroque. "In a restricted sense," Levine argues, this interplay of contrasts, polarities, and disparities "mean[s] simply the tension that occurred between modernity and *ancienneté*," a self-conscious intellectual and cultural rivalry, which both produced and were subsumed by a set of "tensions of an uneasy baroque" (Levine 1999, ix–x).[21] Typically, both Boyle and Duclos owed much to "*ancienneté*." Boyle, however, sought to publically replace it, eliminate its problems, and accordingly modernize it. Duclos read Boyle's attempt as wrong-headed, inconsistent, and incongruous.

Although Duclos condemned Boyle's practice of secrecy this concern did not comprise the core of his criticism.[22] Duclos used the analogy between Samson-versus-philistines on the one hand and Boyle-versus-vulgar-chymists on the other, as a springboard into a discussion on the nature of matter and chymical knowledge. He questioned at once Boyle's mastership of the written chymical corpus and the validity of his arguments, while exposing his weaknesses as a practitioner. Establishing beyond experimental doubt that Boyle's "anomalous salt" was the same as Duchesne's (conceivably described well over half a century earlier), Duclos concluded that since Boyle admitted that this was the "onley [such] instance" that he had "hitherto met with of Salts," it followed that Boyle "either failed to read Duchesne or that his anomalous salt is not different from the abovementioned sweet crystals of marine salt" (*OFQ*, 408; AdS, PV, 1: 103). Boyle was accordingly either unfamiliar with Duchesne—in which case the level of his erudition was questionable, as were his claims to originality—or, possibly worse, he simply failed to acknowledge his sources; so much for Boyle's advocacy of "perusing the Books of Chymists" (Cf. Newman 1996; Principe 1998; Newman and Principe 2002, esp. 6–34).

Duclos' cynicism climaxed in his description of "a learned and great chymist such as Mr. Boyle," who "is astonished as to how this sweetness can proceed from such salty materials." Even if Boyle's astonishment was at least partly rhetorical—merely suggesting the possibility of explaining the transformation of qualities in corpuscular terms—Duclos was well justified in doubting the Englishman's understanding of the issues at stake. Boyle remarked that "Another thing considerable in our Anoumalous Salt is, That though its Odour be not either strong or offensive … yet if it be a little urg'd with heat" it will give off a strong and offensive smell, which squares well with the reports of "some, that have been us'd to the powerful stink of *Aqua fortis*, distill'd Urine, and even spirit of Sal Armoniack its self." Nonetheless, Boyle added, "when these Fumes settle again into a Salt, their Odour will again prove mild and inoffensive, if not pleasant" (*OFQ*, 408). Duclos conceded that,

> Mr. Du Chesne has not mentioned this with respect to the salt crystals; but those who have *seen and smelled*, as I have, can assure that these salt crystals, when cold, do not have a disagreeable smell; but those [odors] rendered by the fire are not a bit pleasant (AdS, PV, 1: 101; *italics* added).[23]

Duclos' account revolved around Duchesne's procedure, with which he was personally and experientially acquainted. Towards the end of the memoir he mentioned Boyle's complaint that since the procedure is "so elusive and so encumbering," he (Boyle) had found it difficult to describe and teach (*OFQ*, 407). Even though Duclos' memoir comprised in itself a refutation of any such statement, he focused on the fact that

> Mr. Du Chesne has made it very clear in the procedure he had described … it requires much *industry* and *accuracy*, for which a highly *skilled* and *conscientious* artist [practitioner] is needed, as well as vessels of high quality, and a *furnace* in which the *fire can be accurately adjusted* (AdS, PV, 1: 103; *italics* added).[24]

Duchesne was equally explicit: the distillation is the most crucial part of the procedure. The fire should be manipulated judiciously, its "degrees" controlled "in a most careful and accurate manner, for this is the principal thing in the whole affair." The operation could succeed only if performed by "an artist who knows well how to manipulate the furnace and control the fire"—its degree and intensity—which should be steadily "maintained for 8 days." Duchesne emphasized that the vessel employed should "not redden too much but acquire only a faint shade of red" (ibid., 99); Duclos confessed to have "seen and smelled" these "crystals of Mr. Du Chesne [which] are possessed of a *real and manifest* sweetness." The procedure, then, was both attainable and describable. Its success, however, depended crucially on the proficiency and aptitude of the chymist, in which context, the fire, the furnace quality, and the skill level of the distiller are of prime significance.

The "philistine" chymist, we might add, has answered Samson's riddle. For Duclos there was no riddle to begin with, nothing to be "astonished" about; hence the implicit rejection of Boyle's underlying message, presenting the case of the "Anoumalous

Salt" as a momentous experimental instance, allegedly evincing the need for "Chymists to learn and relish the Notions of the Corpuscular Philosophy." In the preface to the section discussed, Boyle depicted the lamentable alternative in vivid terms:

> To ascribe all *Phaenomena*, that seem any thing Difficult … to *substantial Forms*, and, but *nominally understood*, *Qualities*, is so general and easie a way of resolving Difficulties, that it allows Naturalists, without Disparagement, to be very Careless and Lazy … where as the Cultivators of the Particularian Philosophy, being obliged by the nature of their *Hypothesis*, and their way of Reasoning, to give the particular Accounts and Explications of particular *Phaenomena* of Nature, are also obliged, not only to know the general Laws and Course of Nature, but to enquire into the particular Structure of the Bodies they are conversant with, as that wherein, for the most part, their Power of acting, and Disposition to be acted on, does depend (*OFQ*, 393).

Fontenelle thought Duclos had "subscribed to a misleading obscurity," not unlike the one Boyle railed against in this passage. But as we have seen, Duclos can hardly be considered as "Careless and Lazy" or as advocating easy "way[s] of resolving Difficulties." Nor was he unfamiliar with the "Laws and Course of Nature." If anything, Duclos—the chymist who had "seen and smelled," who stressed the "real and manifest," who highlighted the skill and industry of the operator, and whose discussion revolved around the "furnace" and those "substances which compete materially"—stands in contradistinction to any "general and easie a way of resolving Difficulties." Duchesne nonetheless certainly drew upon the "three principles" and Fontenelle chided Duclos for his recourse to "gross and tangible principles such as salts, sulfurs, etc." These references to the *Tria Prima* need to be further contextualized and set against the background of Duclos' chymical operative epistemology.

Duclos' Principles: Matter, Vitalism, and the "Terms of the Art"

On the last day of 1666, Duclos delivered the inaugural communication of the philosophical group in a memoir entitled "projet d'exercitations physiques". Early on he made a statement much in line with the spirit of Boyle's *SC*: "those that have proceeded inaccurately in their search for the principles of natural mixts, by way of chymical analysis, have adopted mercury, salt, and sulfur as principles and primary constituents." The *Tria Prima*, Duclos proclaimed, "being neither simple nor primary, cannot be principles." In fact, "a more accurate resolution" would demonstrate that these three principles could be further resolved (decomposed): sulfur into water, salt, and earth; mercury into salt and phlegm; salt into phlegmatic water and earth (AdS, PV, 1: 2). But whereas Boyle associated his reformative ambitions with the likes of Bacon, Descartes, and Gassendi, Duclos drew inspiration from traditional chemical philosophy, especially Van Helmont's, who even Boyle held in fairly high esteem (Pagel 1982; Newman and Principe 2002, esp. 56–89 and chapter 6). Duclos suggested two methods for studying the principles of natural mixts: either by chymical resolution and inspection of the received components or by observing their generations and corruptions. His Helmontian slant is revealed by his claim that "in the *extreme resolution* of natural mixts, nothing *apparent* remains but water. By observing the generation of the mixts, water is recognized as their primary matter" (AdS, PV, 1: 2; *italics* added; see Pagel 1982, esp. 49–60).[25]

In discussing distillation Duclos explained that as an inaccurate chymical analysis of natural mixts, it is often understood as yielding five different substances: phlegm, spirit, oil, salt, and earth. This referred to the common proliferation of elemental pentads, various combinations of the peripatetic and Paracelsian elements and principles, which corresponded qualitatively to the products of distillations. These pentads arose from the addition of two of the Aristotelian elements—usually considered passive or inactive—to the *Tria Prima*, which represented the active constituents, especially the mercury and sulfur. Duclos added that "three of these substances are considered by [the vulgar chymists] to be essential, the spirit, the oil, and the salt," explaining how "the spirit is likened to mercury, the oil to sulfur, and the salt, which does not assume a different denomination, is considered either fixed or active. The phlegm and the earth, by contrast, are considered as "purely material, lacking all virtue … vain and inefficient" (AdS, PV, 1: 4).

Much like Boyle, Duclos was critical of fire analysis and advocated solution chymistry. Unlike Boyle, however, he was much less dismissive of contemporary and traditional chymical knowledge, most likely due to his superior understanding of it. Rather, he looked to separate himself from certain perceptions and traditions, which he either rejected or aimed to revise. In his ensuing discussion on the principles of natural mixts, Duclos clarified to the members of the assembly several key misconceptions concerning chymistry while advancing his own ideas on the nature of matter and chymical analysis. As seen, he spelled out the origin of terms like the common pentads and their relation to traditional systems of elements. Likewise, he framed the "vulgar chymists" as those who advocated and performed inaccurate chymical analyses. These chymists, he remarked,

> consider phlegm as a basis or an elementary draft … lacking all essence, that the dead and damned earth (*these are the terms of the art*) is an excrement devoid of energy … that these two weaken the virtue of the other three [principles], which is why they advocate the practice of their separation and removal (AdS, PV, 1: 4–5; *italics* added, parentheses original).[26]

Duclos attributed the source of the *Tria Prima* to the "hermetical philosophers, who wished that their sacred philosophical stone be composed of mercury, salt, and sulfur." For Duclos these "mysterious philosophers," alongside the "vulgar chymists," accorded the *Tria Prima* an elementary status in virtue of "some analogy between the three matters which compose the grand arcana of the Hermetics, and the three energetic substances … separated from several mixts by way of imperfect analysis" (AdS, PV, 1: 4–5).[27]

Duclos set himself apart from both groups by displaying allegiance to a most distinguished "modern chymist," who possessed the knowledge of a "resolutive liquor, capable of penetrating and resolving radically all the mixed bodies." The allusion is to Van Helmont's utopian universal solvent—the "alcahaliest" (alkahest) (see Reti 1968; Joly 1996; Boantza 2010). As if responding to Boyle's concern over the "origin of forms and qualities," Duclos insisted that "we cannot be accused of having introduced by this practice new forms, since nothing apparently remains but water" (AdS, PV, 1: 6–7).[28] Reasoning along Helmontian lines, Duclos suggested that an extreme resolution—such as the one brought about by the alkahest—would leave nothing behind except for water. Water, then, was the "primary matter" and the "primary component in the composition of natural mixts" (ibid., 1: 2, 4). But even in the case of a radical resolution, one could still not be certain

> that water is simple and absolutely elementary, although nothing else *appears* to be present, and we do not *detect* any signs of previous fermentations, [the water] can still be imprinted with some invisible efficient, capable of reproducing in it the new forms of salt, mercury, and sulfur, etc. We can only conclude that these primary forms are accidental; that they resulted from the action of some internal agent in the water (ibid., 1: 7; *italics* added).[29]

Thus *materially* the radical resolution is conclusive, and the ultimate proof is *empirical*, since nothing "appears" to be present or can be otherwise sensually "detect[ed]" in it. Yet the water might still retain some insensible—hence *immaterial*—activity. An "invisible efficient" might still transform the water into salt, sulfur, or mercury.[30]

Duclos' distinction between perfect and imperfect mixts elucidates the "accidental" character he ascribed the *Tria Prima*. When an "intangible and spiritual efficient" acts upon water *qua* prime matter it produces the *Tria Prima*, the multiple combinations of which can produce only "imperfect mixts." "Perfect mixts," on the other hand, are considered as "partaking of life" or of being vivified (to a certain degree) and could not be produced by this "alterative efficient of water." To account for their occurrence "it is necessary to assume a third type of principles of mixts, which modern chymists designate as arcana par excellence for being the most perfect and the most noble of all." Therefore, Duclos surmised, "the principles of natural mixts are the material bodies, the alterative spirit, and the vivifying soul, or the arcanum" (AdS, PV, 1: 3).[31] Duclos' depiction of perfect mixts as partaking of life is reminiscent of the increased complexity attributed to organic matter, for these mixts are unique in that "their mercuries, their salts, and their sulfurs are so diversified that they cannot originate from the alterative spirits alone"; hence the need to assume the existence of a "3rd principle more energetic and less corporeal than the alterative spirit" (AdS, PV, 1: 8).[32]

The significance of the vitalistic, animistic, and Neoplatonic dimensions of Duclos' cosmology notwithstanding, it is his definition of the "corporeal" that is most revealing in the present context:

> We name corporeal, not that which is extended in three dimensions geometrically; but that which is *tangible*. And we name incorporeal and spiritual that which in this sense is not corporeal, and cannot be *handled* or *touched sensibly* (AdS, PV, 1: 8; *italics* added).[33]

Recalling Fontenelle's Cartesian oriented remark, it would seem that he had raised (inadvertently) the most consequential issue in recognizing that chymistry "resolves bodies by sensible operations into certain gross and tangible principles such as salts, sulfurs, etc." The tone of Fontenelle's remark was of course derisive, pitting Duclos' "gross" chymistry against Boyle's "precise" and intelligible mechanical physics.

Yet Duclos' message embodies a different outlook. For Duclos, not all the "principles" of natural mixts were "gross and tangible," and while the *Tria Prima* might indeed be accessed empirically it was far from being elementary in the sense meant by either Fontenelle or Boyle. From a metaphysical standpoint, out of three types of principles assumed by Duclos—body, spirit, and soul—only the first was "tangible" and solely recognizable in chymical operations by virtue of the fact that it could be "*handled* or *touched sensibly*."

The corporeal for Duclos was not merely sensible but also manipulable. The chymist worked with matter, which assumed the "imperfect" forms of substances; it was the knowledge of these substances and the complex art of their manipulation and identification that was unique to the chymist. Duclos' chymical "principles" were tightly linked to experimental demonstration. This is a far cry from the chymists Boyle had denigrated as those who without proof "take [suppositions] for granted; and upon which Depends the Validity of the Inference they draw from their Experiments." Drawing on his outline for the research of the "principles of natural mixts" and his vision of chymical experimentation, Duclos challenged "the Validity of the Inference" Boyle had drawn from his own experiments, most notably those dealing with saltpetre , a substance of central significance to both natural philosophers (Cf. Debus 1964; Newman 2006, 208–215). Duclos' reading of Boyle's *Essay on Niter* offers a deeper understanding of the Frenchman's chymistry, with particular reference to Boyle's most radical reformative endeavor: the submission of chymical phenomena to the principles of the mechanical-corpuscularian philosophy. The discussion discloses Duclos' relation to particulate theories of matter and to the application of mechanistic principles to chymistry.

Particles of Saltpetre: Mechanism and Chymical Causality

As part of his examination of the *CPE*, during the assembly's inaugural Saturday meeting of 1669, Duclos announced having reached

> the second part of Mr. Boyle's book, which contains two treatises, in which the author attempts to reconcile the principles of the corpuscular philosophy with the experiments of the chymists and to account for these experiments by the doctrine of atoms (AdS, PV, 6: 1r).[34]

This refers to Boyle's *Specimens of an Attempt to Make Chymical Experiments Useful to Illustrate the Notions of the Corpuscular Philosophy.* Advancing chymistry's usefulness for making "some Meliorations … of Mineral and Metalline Bodies, and many excellent Medicines for the Health of Men, besides divers other Preparations of good use in particular Trades" might suffice, Boyle suggested, to persuade some people that chymical pursuits were not an utter waste of time; "yet this would scarcely suffice to manifest it to be useful to Philosophy." To validate the latter point—and hence promote chymistry as an inseparable part of natural philosophy—Boyle set out to prove that "Chymical Experiments might be very assistant even to the speculative Naturalist of his Contemplations and Enquiries" (*CPE*, 86). Accordingly, Duclos noted, "out of all the notable practices of chemistry" Boyle "had chosen the resolution and reintegration of saltpetre " (AdS, PV, 6: 1r–v). Duclos dedicated two consecutive memoirs (January 7th/12th) to an evaluation of Boyle's essay on saltpetre : the *Physico-Chymical Essay Containing An Experiment with some Considerations touching the differing Parts and Redintegration of Salt-Petre*.

In the spirit of his two preceding *Physiological Essays*, Boyle began by complaining that saltpetre , "in that form wherein it is sold in Shops, it be no very obvious concrete." Yet its significance could hardly be overstated:

> it is to be found in so great a number of Compound Bodies, Vegetable, Animal, and even Mineral, that it seems to us to be not only one of the most Catholick of Salts, but so considerable an Ingredient of many sublunary Concretes, that we may justly suppose it may well deserve our serious enquiries. (*CPE*, 93)

Duclos remarked, once again with irony, that despite deeming "saltpetre … as worthy of the most exact study," Boyle excused himself for not having taken the time to pursue such inquiries due to his "grand affairs" (AdS, PV, 6: 1v).[35]

Boyle first attended to the resolutive procedure. He took four ounces of purified saltpetre , melted it "into a limpid Liquor" and added a "small live Coal," which "presently kindled it, and made it boil and hiss, and flash for a pretty while." He repeated the procedure several times, and kept adding "glowing" coals until the niter had ceased to fulminate. He then divided the "remaining fix'd Nitre" into two equal parts. One part he dissolved in water to which he added "Spirit of Salt-petre" until the effervescence had died down, then he filtered it and exposed it to open air. The second part, which had not been dissolved, was likewise mixed with the "same Spirit till the hissing and ebullition were altogether ceas'd," after which Boyle exposed the sample "in an open glass Jar to the air in the same window with the former." Judging by "their manner of burning, as their shape," Boyle concluded that in both instances the resulting "Crystals" were of saltpetre. He finally couched his qualitative observations within a quantitative explanation: "the weight of the Spirit of Nitre requisite to be drop'd on, till all the ebullition made betwixt that Liquor and the Solution of fix'd Nitre were ceas'd" was nearly identical to the weight that the "Salt-Petre [had] lost in its detonation" (*CPE*, 94–96).

Boyle was clear about the import and significance of this experiment:

> it appears that the whole body of the Salt-Petre, after it's having been sever'd into very differing parts by distillation, may be adequately re-united into Salt-Petre equiponderant to it's first self; this Experiment will afford us a noble… single instance to make it probable that that which is commonly called the Form of a Concrete… whence all it's qualities are in the vulgar Philosophy, by I know not what inexplicable wayes, supposed to flow, may be in some bodies but a Modification of the matter they consist of, whose parts by being so and so disposed in relation to each other, constitute such a determinate kind of body, endowed with such and such properties; whereas if the same parts were otherwise disposed, they would constitute other bodies of very differing natures form that of the Concretes whose parts they formerly were, and which may again result or be produc'd after it's dissipation and seeming destruction, by the re-union of the same component particles, associated according to their former disposition. (*CPE*, 107–108)

Boyle looked to advance an explanation based on the experimental consequences of the reintegration experiment, according to which the "Form of a Concrete… [and] all it's qualities"—poorly explained by the "vulgar Philosophy"—would be deduced from stronger premises, namely, the "Modification[s] of the matter" and of the substance at hand. This "Modification," moreover, which governed the substance's "properties" and "being," proceeded from the relative disposition of its particles.

Duclos was skeptical about Boyle's prediction of such "noble" reinterpretations of a theme as pivotal as the "Form of a Concrete" on the basis of the findings afforded by this experiment. For Duclos, saltpetre resulted from "the condensation of air in a sulfurous salt," and it was this condensed air that caused its fulmination. "It will suffice," he argued, "to elucidate the composition of saltpetre for examining the way by which Mr. Boyle had reasoned concerning some changes in this substance," referring to its resolution and reintegration (AdS, PV, 6: 3r–v).

Like Duclos, Boyle's account of the various "changes" observed in the saltpetre owed to his view of its composition. "This Experiment," Boyle explained, "seems to afford us an instance by which we may discern that Motion, Figure, and Disposition of parts, and such like primary and mechanical Affections … of Matter, may suffice to produce those more secondary Affections of Bodies which are wont to be called Sensible Qualities" (*CPE*, 98). He first discussed the "Tangible Qualities," such as "Heat and Cold," explaining that saltpetre is commonly taken to be, in essence, a very cold body. "When the Parts of this so cold Body" are combined, however, they "immediately agitate each other with great vehemency," resulting in the production of a great amount of heat. Since this "agitation lasted, so long the heat endur'd [yet] … when the motion ceas'd, the heat also vanish'd," Boyle concluded that heat was "nothing but a various and nimble motion of the minute particles of Bodies" (*CPE*, 99). Duclos found this interpretation wanting. "It is indeed motion that brings about the effervescence, but Mr. Boyle has not indicated the *cause* of this tumultuous motion," which, Duclos added, "he probably could not ascribe to the figure and disposition of particles" (AdS, PV, 6: 4v; *italics* added).[36]

From Duclos' standpoint, Boyle's corpuscular interpretation—associating the effervescence with the great "agitation" between the "Parts" of the saltpetre —was not a proper causal explanation precisely because any reliance on the speculative, indeed imaginary "figure and disposition of particles" could not comprise a valid chymical explanation. Duclos did not merely challenge the explanatory prowess of Boyle's mechanical-corpuscular argument: he squarely denied its plausibility. Duclos further indicated that whereas the mixture of iron with the spirit of saltpetre "excites a very violent motion and a great heat," the dissolution of camphor in the same acid did not exhibit any such effects. Whereas the camphor, Duclos clarified, was entirely oily and hence lacked any salt, the iron was rich in sulfurous salt. It follows that the tumultuous motion excited during the dissolution of iron in the spirit of saltpetre was *caused* by the "mutual and reciprocal action of salts of different qualities" (AdS, PV, 6: 4v–5r).[37] In a similar fashion, Duclos proceeded to expose the rest of Boyle's corpuscular explanations concerning various qualities of niter, while providing chymical explanations drawing on the myriad patterns of interrelated behaviors and actions of various substances and constituents, with which he was intimately and experimentally familiar.

Reflections on Chymistry and Baroque

Duclos' critique of Boyle forms a complex and multilayered historical document that can be read on several levels, within numerous contexts. It can be read as a testimony of how one seventeenth-century savant read the work of another; as a unique source of information about the way Boyle's natural philosophy was received across the Channel; or as a platform for examining how traditional views were treated and accommodated within a Royal Academy whose members tried to keep debate to a minimum (see Stroup 1990) while affected by the agendas of the budding New Science. Most significantly, Duclos' depiction of Boyle bears special witness to how Boyle's endeavor—its critical and constructive dimensions alike—was perceived and received by a member of the very scientific community Boyle sought to reform.

Boyle looked to establish a new chymical order by reconciling the chymical with the physical, by submitting chymical phenomena to the physically governed laws of matter and motion. Recent scholarship has successfully demonstrated and stressed Boyle's far-reaching interest and *private* beliefs in alchemical and other spiritual pursuits. Such arguments are concerned with substantiating the continuity between alchemy and chymistry during the scientific revolution by underscoring the New Science's debts to alchemy and other traditional sciences (Debus 1990; Newman 1994, 1996, 2006; Newman and Principe 2002; Principe 1998; Cf. Vickers 2008). This type of scholarship is largely interested in correcting the enduring, indeed biased, portrayal of Boyle as a mere physically minded chemist, a view that can be traced back to Fontenelle and the early eighteenth century. Duclos' reading of Boyle—*qua* reformer, representative of the New Science, and proponent of a novel physico-chymical discourse—provides significant information about what constituted *distinctly chymical* knowledge and the ways of its production and assimilation during the scientific revolution.

In line with the New Science's self-ascribed advancement of a value-free rhetoric, Boyle highlighted empirical standards and experimental agendas. Yet it is on these very grounds that Duclos had repeatedly exposed the Englishman. Duclos faulted Boyle for his lack of experimental proficiency, and more importantly, for his lack of experimental knowledge of substances, procedures, and practices, outside of which chymistry did not fully exist as an operative science. Boyle's program, founded on abstract interpretive categories, was not only useless from a practical standpoint, but its reductive dimension threatened to hinder the progress of chymical discoveries. It would therefore run against Boyle's goal to show the usefulness of chymistry to natural philosophy. For Duclos, chymical knowledge was a knowledge of particulars, derived from and akin in a sense to an vast accumulation of experiences and experimental phenomena, which was why he observed that a chymist must be closely acquainted with the literary (al)chemical corpus. Given the irreducible vastness of the realm of chymical occurrences and their corresponding means of manipulation and production, a skilled chymical experimenter must be able to distinguish between reliable and unreliable authorities. Not unlike George Starkey's practice, for instance, Duclos regarded certain chymical authors as authorities, to whose writings and findings he deferred (Newman and Principe 2002, 174–197). The underlying principle was that in face of the irreducible extent and complexity of the realm of material appearances and transformations, chymical practices, reactions, substances, and corresponding methods, a fichymist must crucially draw on the authority of others. In this sense, the authoritative chymical written corpus was considered as a sort of communal dynamic natural history in progress, an ever-evolving repository of legitimized chymical knowledge that could be, and should be, applied and relied upon.

Duclos was mindful of Boyle's critique of "vulgar" chymistry and its related elemental theories. Like Boyle, he agreed that neither the *Tria Prima* nor the peripatetic elements (or any of their combinations) comprised the true chymical elements or the ultimate constituents of mixts. Boyle, however, offered little in their stead and proceeded to introduce a new way of explaining chymical phenomena by invoking the properties and motions of corpuscles, their sizes, shapes, configurations, and textures. At times equally dismissive of the same elemental theories, Duclos looked to clarify them and situate them within a broader context of chymical analysis. As the only entities the chymist could know—even if incompletely—Duclos refused to relinquish the "tangible" substances, the material and "corporeal" entities, and their empirical manifestations with which the chymist works and through which he approaches and studies natural phenomena (Cf. Klein and Lefebvre 2007). Instead, Duclos proclaimed the irrelevance, and more importantly, the practical uselessness of the quantitative universal definition of matter by three-dimensional spatial extension.

According to Duclos' chymistry, corporeal matter is by definition tangible, manipulable, experimentally demonstrable, and empirically accessible. Unlike Boyle's imaginary particles, substances can be "handled or touched sensibly." Yet at the same time, particles, or "parts," were not lost on Duclos' chymistry. In fact, they were employed in a particular and forceful epistemological manner, based on Duclos' restrictive perception of the role of motion in chymical explanations. Epistemological recourse to particles and their motion, for Duclos, designated the realm of physical interactions, which are superficial and hence reversible (that is, not transmutational processes or essential changes). In explaining distillation, Duclos allowed that, "the heat of the fire excites a motion in the mobile parts [of the mixt], according to their degree of mobility." The respective "parts" would rise within the retort in succession and according to their degrees of mobility: "those that share the same degree of mobility cannot separate at the same degree of heat … and those that are unequally mobile separate from each other." The most mobile are "most agitated" and hence rise first, followed by the less mobile (AdS, PV, 4: 58v).[38] This explains at once how distillation works to decompose mixts but also signals the limitations of fire as a physical analytical tool. After all, two constituents can be *chymically* different yet by virtue of sharing the same degree of mobility (excitability), will not be separated during distillation, as the fire will cause "them [to] rise together." Elsewhere, Duclos indicated that "fire can occasion … not only separation, but also union" (ibid., 60r), once more pointing to the complexity of chymical practice. For Duclos the motion of "parts" was a physical category; he thus deemed Boyle's employment of particles in motion for explaining chymical phenomena, as descriptive rather than causative.

The central role assigned to motion by both natural philosophers reflects a leading concern of the baroque, the age of motion *par excellence*, during which movement became a dominant theme in fields as varied as mechanics, metronomy, architecture, literature, music, and art (Fleming 1946; Skrine 1978; Landes 1983; Martin 1977; Mumford 1934; Maravall 1986, esp. 175–176). Baroque is commonly identified, in an important sense, with the seventeenth-century all-encompassing move away from the immutability, eternality, and static symmetry of the Renaissance towards an increasing emphasis on and expression of dynamics, movement, intensity, tension, contrast, force, and sensuality. Duclos and Boyle, however, had fundamentally different approaches to motion and to its role in chymical explanations. Boyle stressed the *intelligibility* of explaining chymical phenomena according to mechanical laws, considering matter and motion as universal and unifying principles. Duclos, by contrast, found such abstractions solely heuristic and devoid of explanatory potential: chymical reality was for him irreducibly complex and found within an eternal state of change and flux. This is why Duclos emphasized "tangible," empirically recognizable substances, and the *dynamics* of the chymical *processes* in which these substances were perpetually embedded. Instead of changes in the "imaginary" configurations of particles, Duclos spoke of "different materials" and of "substances which compete materially." In a sense, the relations between substances also defined those substances by situating them on the expanding grid of perpetual material change and transformation.

From Duclos' standpoint, Boyle stood to lose chymistry to the very categories he pertained to save it from, as Boyle attempted to free chymistry from the metaphysical shackles of a "darken'd" and "troubl'd" alchemical heritage by reducing it to the metaphysics of abstract matter and motion. For Duclos, the subordination of chymical phenomena to mechanical principles was not only incongruous but positively misleading since it blurred the line between the physical and the chymical programs, which were based on diverging methods of controlling and studying nature. Boyle's motion and corpuscular interactions were descriptive; Duclos' motion was empirical, demonstrable, and explanatory. This distinction followed from their respective definitions of corporeality and bodies. Duclos defined a body as "tangible"; only that which could "be handled or touched sensibly" could qualify as a body. For Boyle, bodies were material spatial extensions, unobservable minute parts of one single homogenous and catholic matter. This metaphysical disparity embodies a distinctly baroque concern with the interplay between the role of the senses and the changing perceptions of the natural world.

Boyle's notion of corporeality was meant to restrict the role of the senses. In good Cartesian spirit, geometrical abstractions provided objective clarity whereas sensual experiences were subjective and flawed. The idea involved a definition of body and matter which were independent from experience and empirical reality. Boyle deemed the old traditional attempts to establish the "principles of natural mixts," whether Aristotelian or Paracelsian, on empirical or observational foundations as erroneous and scientifically unsound.[39] Boyle, of course, had a prominent empirical agenda but it was chiefly related to his experimental philosophy and not to his matter theory. It is, moreover, his advocacy of systematic experimentalism and the collection of empirical instances (in a Baconian vein) that betrays his attitude toward the sensual. Whereas the senses belong to the realm meant to "help Nature to produce things," and systematically arrange them, the intellect—reasoning by way of simple and distinctly intelligible mechanical principles—"understand[s] well the Nature of the things produc'd." It is also quite evident which of the two domains Boyle privileged when speaking of the "big Difference betwixt the being able to make Experiments, and the being able to give a Philosophical Account of them." In a baroque age of growing uncertainty and increasing epistemological anxieties (Friedrich 1965), Duclos' very definition of corporeality turned on the senses and on chymical practice; what was intangible or impossible to manipulate was to be considered as immaterial. In the face of such anxieties, alongside the rise of confidence in the human powers to control nature, Boyle looked to simplify nature while Duclos acknowledged its irreducible complexity.

In Duclos' final judgment, Boyle's natural philosophy, in the context of chymistry, was essentially paradoxical. He criticized Boyle for compromising and enfeebling the status of chymistry as the science of matter, viewing Boyle's self-proclaimed greatest achievement—the "physico-chymical" reconciliation—as discordant and inconsistent, an incongruous baroquely middle ground. This incongruence is multifaceted. While Boyle called for openness, Duclos exposed his practice of secrecy; whereas Boyle derided chymists for being "Aenigmatical" and "shrouded in mystery," Duclos uncovered Boyle's deceptive use of riddles; Boyle taxed those who "discover things *a priore*, [rather] then *a posteriore*" while for Duclos "the figure and disposition of particles" could not comprise a chymical cause. On a most fundamental level, baroque science derives from "the discrepancy between practical acknowledgement of irreducible complexity and the insistent public avowal of discoverable, fundamental simplicity."[40] Boyle, then, especially as viewed and exposed by a contemporary, his 'chymical counterpart', is an exemplar of baroque science. Viewed in this light, Fontenelle's caricature seems less baroque—in the pejorative sense—aptly capturing "the discrepancy" between utopian "precise speculations" and mundane "sensible operations."

Notes

1. "[Boyle] avoit entrepris de rendre raison de tous les Phénomènes Chimiques par la Philosophie corpusculaire, c'est-a-dire, par les seuls mouvemens & les seuls configurations des petits corps. M. du Clos, grand Chimiste, aussi-bien que M. Boyle, mais ayant peut-être un tour d'esprit plus chimiste, ne trouvoit pas qu'il fut nécessaire, ni meme possible, de réduire cette Science a des principes aussi clairs que les figures & les mouvemens, & il s'accommodoit sans peine d'une certaine obscurité spécieuse." All translations are mine unless specified otherwise."
2. "La Chimie par des opérations visibles résout les corps en certains principes grossiers & tangibles, sels, souffres, &c. Mais la Phisique par des spéculations délicates agit sur ces principes, comme la Chimie a fait sur les corps, elle les résout eux-mêmes en d'autres principes encore plus

simples, en petits corps mus & figures d'une infinité de façons : voila la principale différence de la Phisique & de la Chimie, & presque la même qui etoit entre M. Boyle, & M. du Clos"; "L'esprit de chimie est plus confus, plus envelopé; il ressemble plus au mixtes où les principes sont embarrasses kes uns avec les autres, l'esprit de Physique est plus net, plus simple, plus dégagé."

3. The view of Boyle as mechanical philosopher and 'physicist' goes back to his contemporaries and immediate successors (Leibniz, Peter Shaw, etc.); similar depictions appeared in the eighteenth century. Venel, for instance, "in his article 'Chymie' for the *Encyelopédie*, in which he aimed at discriminating between chemistry and physics, complained that Boyle 'est trop exactement physicien corpusculaire-mechaniciéne, ou physicien proprement dit' and suggested placing him among the physicists rather than among the chemists." Clericuzio 1990, 562. Hall 1958, the first twentieth-century influential historical study of Boyle's science, echoes similar sentiments. For more nuanced studies, beside Clericuzio's, see Cook 2001; Kim 1991; Principe 1998.
4. Duclos used the 1667 Latin edition *Tentamina Quædam Physiologica*, a translation of the 1661 English edition.
5. The distinction is between the 'mathematical' and the 'philosophical' factions. Among the founding members of the Academy were seven mathematicians, responsible for research into geometry and astronomy, and seven philosophers, in charge of physics, zoology, chymistry, anatomy, medicine and botany. During the early period, Duclos dominated the research agenda of the philosophical group, presenting memoirs on topics that were key in both institutional as well as philosophical contexts, such as research into the principles of mixts, botany and plant analysis (as part of the ambitious project on the *Histoire des Plantes*), mineral water analysis, etc.
6. The chronicles of the early academy—and its minutes, the *procès-verbaux* for the period 1666–69 in particular—bear witness to an exceptionally high level of activity and influence on Duclos' part.
7. I use 'chymistry' (and 'chymical') to denote the transitional phase (especially the sixteenth and seventeenth centuries) bridging classical and medieval alchemy with modern chemistry. This terminology has become commonplace in recent studies of early modern chemistry. For a discussion of this see Newman and Principe 1998.
8. The 1662 Latin edition of the Sceptical Chymist, the Chymista scepticus, vel, Dubia et paradoxa chymico-physica circa spagyricorum principia.
9. The phrase is taken from the title of Boyle's 'Essay on Niter': *A Physico-Chymical Essay… Redintegration of Salt-Petre* (*CPE*, 93). The subtitle of the *SC* is *chymico-physical doubts & paradoxes, touching the experiments whereby vulgar spargirists are wont to endeavour to evince their salt, sulphur and mercury, to be the true principles of things.*
10. Duclos mentions Boyle twice beforehand: with respect to his experiments with the air-pump and in a letter, reproduced by Duclos, of his former student and colleague, Nicaise Lefebvre, Fellow of the Royal Society since 1663 and chymist and apothecary in the court of Charles II. Cf. Franckowiak 2009.
11. Published about a year prior to Duclos memoir: March-April 1666.
12. The "heifer" represents Delilah. Retrieved 05 October 2008 from http://darbybible.com/judges/14.htm
13. This was Boyle's pseudonym for Mr. Richard Jones, son of the Lord Viscount Ranelagh. Boyle, *Works*, II, 6.
14. "Monsieur Boele ayant tiré un sel doux de quelques matières fort acres, ne veut pas dire ce que c'est. Il en marque seulement quelques qualitez singulières qui luy ont donne sujet de l'appeler anomal. Apres c'estre excusé de ce que contre sa coustume et son inclination, il propose a couvert une expérience, que sa parole le donnée a quelque curieux l'engage détenir secrète et de rien divulguer, ny la matière, ny la méthode, il descrit quelques proprietez d'un sel extraordinaire qu'il dit premièrement fait, suivant sa pensée et depuis refait en y adjoustant quelque chose par le conseil d'un chimiste, qui avoit appris beaucoup de choses en voyageant, et qui luy recommanda ce sel comme fort singulier et pretieux."

15. "Si nous commençons par les cristaux du sel marin qui se tirent d'un matière si acre et si salée, il ny a point de doute que les ignorans se mocqueront de nous, ne jugeant pas que cela se puisse faire… Mais nous ne la soumettons par a leur jugement. C'est aux vray philosophes que nous escrivons cecy, comme a ceux qui en sçauront mieux connoistre la vérité. C'est en leur faveur néanmoins que nous ne puissions estre entendus que de ceux qui sont initiez en cet art, qui en sçavent les termes, et qui sont imbus de la vraye philosophie… ayant fait dissoudre le sel marin en son menstrue propre et naturel, puis filtré et coagulé la solution, selon les regles de l'art, et réitéré tant de fois que ce sel soit devenu très pur, et très clair, il en faut prendre six livres, et y surverser d'un certain dissolvant vitriolé et melliflué fait d'un matière végétale, et animale, y mettant de cette liqueur, en telle quantité qu'elle suffise pour procurer une meilleure fermentation, afin qu'apres en avoir fair une digestion convenable, il serve de véhicule pour faire élever plus facilement les esprits phlegmatiques, les sulphurez et les vitriolez les doux et ceux qui sont acides, lequels sont tout fortement liez au corps du sel, cette extraction de ces différents esprits se doit faire dans des cornues de terre qui soustiennent le feu comme font celles de Beauvais, donnant le feu par degrez avec soin et addresse ; car on cecy consiste le principal de l'affaire, et pour ce il faut y commettre un artiste qui l'entende bien il faut aussy que le fourneau de réverbère soit propre a y pouvoir bien régler le feu, qui doit estre tel que la cornue ne rougisse pas trop, mais quelle prenne seulement couleur de rouge obscur, et tanné, a ce que le sel qui est dedans ne fonde pas ; car les esprits ne s'en pourroient bien dégager, ce degré de feu doit estre continué également durant huict jours… il faut faire distiller le phlegme insipide à très douce chaleur du bain vaporeux ; puis mettre l'alembic en lieu froid, ou il se formera des cristaux très clairs de saveur douce."
16. Duclos also drew parallels between Boyle's account and a report taken from Hartman's notes on Croll, which I shall not discuss here. The reference is probably to the 1635 *Oswaldi Crolli Basilica chymica, pluribus selectis & secretissimis propria manuali experientia approbatis descriptionibus, & usu remediorum chymicorum selectissimorum aucta a Ioan Hartmanno.*
17. Duclos used the term "vinaigre fait de miel." This 'honey vinegar' refers to what Duchesne described as the "mellifluous solvent, consisting of vegetal and animal matters."
18. I take this wording from Kim, who contrasts 'chemical philosophy' with 'philosophical chemistry' to highlight the difference between traditional early modern chemistry and Boyle's program, striving to 'philosophize' chemistry. Kim 2001, 379.
19. Franckowiak treats aspects of Duclos' critique of Boyle (see Franckowiak 2003; Franckowiak 2009) and I thank him for sharing with me some time ago a draft of a paper on Duclos' reading of Boyle. The notion of "post-*Sceptical Chymist* chymist" is suggestive of certain dimensions of Duclos' critique; it also represents Franckowiak's view concerning Duclos' reading of Boyle, and as such it deserves particular attention. By examining segments of Duclos' memoirs, Franckowiak argues for the pivotal role of Boyle's *SC* in the history seventeenth-century French chemistry. His argument sets Duclos' critique of Boyle against the background of Boyle's own *devastating* critique of "chemical principles." On this account, during the first half of the seventeenth century, French chymistry emerged triumphantly out of debates against Aristotelianism and Galenic medicine and it was perceived as epistemologically superior, as an *alternative* to these scholastic views. It enjoyed a strong institutional standing in pharmacy and within the French textbook tradition, or as seen through the increasing popularity of chymical courses and teaching (Clericuzio 2006). This led to its gaining an unprecedented epistemological authority. On Franckowiak's account, the publication of the *SC* is claimed to have exposed the *inherent* frailty of chymical theory, sounding the death knell of the principalist approach, which served Boyle's purpose to epistemologically reduce chymistry to a mere set of practices that could then be comfortably subjected to mechanical interpretations. It is against this theoretical void, generated by Boyle, that Duclos' contribution is measured. In face of this supposedly irreparable rupture, Duclos chose to give up chymical theoretical ambitions and turned to reestablish chymistry on radically empirical grounds, those allowed by Boyle's reduction. Franckowiak points out chymistry's general post-1661 shift from epistemological 'certainty' towards 'plausibility',

emerging as a science of the "vraisemblable" (Franckowiak 2008, 15–19). This view of the relation between chymical practice, the principalist approach, and chymical thought is crucially informed by a challenging but questionable analysis by Joly, who argues for a *fundamental* epistemological dissociation between alchemical thought and practice: "le laboratoire alchimique est le lieu ou la théorie se manifeste. Dans l'alchimie, la théorie et la pratique ne constituent pas deux moment successifs qui s'enrichiraient et s'instruiraient mutuellement dans une progression dialectique. Ce sont plutôt deux manifestations particulières de la doctrine, dont le modèle est donné par ce qui supposé être l'ensemble des processus naturels. C'est la raison pour laquelle l'alchimiste n'attend de ses pratiques aucune confirmation ni vérification de ses thèses" (Joly 1992, 92). Frackowiak interprets principalist (alchemically-oriented) chymistry precisely along such lines: "Loin d'être intégralement spéculative, cette chimie orientée vers l'obtention d'un objet qui est en fait une conséquence de la théorie chimique – et non l'inverse –, s'appuie très fortement sur une pratique particulière des principe paracelsiens, dans laquelle le passage au laboratoire représente alors une autre manière d'exprimer une théorie foncièrement vraie" (Franckowiak 2008, 11). My argument is concerned with aspects of the dialectical texture of the Scientific Revolution, in which context I examine how Boyle, a leading proponent of the New Science, was interpreted by a contemporary savant who belonged to the very community Boyle sought to reform. Boyle's *SC* is indeed dedicated to criticism of contemporary chymistry. However, in various other works from the 1660s Boyle displays a consistent attempt to reform chymistry—even if mainly along physicalist principles—and thus *rehabilitate* it. Any judgment of Boyle's intentions concerning the general direction of chymistry outside the greater context of the Scientific Revolution and related agendas seems speculative at best. This is why I highlight Boyle's self-portrayal—through his *skeptical* criticism—as a proponent of a purportedly innovative, intelligible, value-free, and open discourse. Moreover, my interest is in demonstrating Duclos' chymical agendas as arising from his reactionary reading of Boyle's quantitative physicalist reduction of chymistry. In this respect I am particularly concerned with *matter* theory and its relations to *method*—both experimental and rhetorical—and in Duclos' view of mechanism (and Cartesianism), especially in the context of the Academy, as an unwarranted alternative to vitalist-transformative trends, exclusively represented by chymistry (from a matter theoretical standpoint). As part of a distinct baroque sensibility—pertinent to the seventeenth-century Scientific Revolution—I examine Duclos' and Boyle's approaches to 'modernity', as it was being cast. Although writing in the immediate post *SC* era, Duclos seems to have been much more concerned with the potentially devastating impact of mechanism on chymical theory and practice than with the critique of elemental theories. The preoccupation of contemporary chymists with the "principles" of substances (mixts) was a common theme, which persisted well into the eighteenth century and which can be vividly substantiated by tracing the dynamics of disciplinary demarcation between the 'physical' and the 'chymical' (Boantza 2012). Boyle's "physico-chymical" creation was for a time a potent alternative that drew much of its influence from the specific architecture and dynamics of the Scientific Revolution (as much as it reflects upon it). Far from trying to repopulate conceptual grounds vacated by Boyle, Duclos cautioned against the blurring of the disciplinary (ontological and epistemological) boundaries between chymistry and the emergent natural philosophy.

20. Duclos refers equally to Paracelsus and Van Helmont; however, Boyle is treated particularly and predominantly in a critical fashion.
21. On this subject consider also the classic study Jones 1965. Contrasting and constantly fluctuating interpretive stands within the ancients-versus-modern 'debate' is, of course, endemic to the seventeenth-century; the same shifting epistemological patterns that have been readily identified as 'baroque' in art, architecture, literature, and politics, are dominant in contemporary natural philosophy. See Friedrich 1965, esp. 38–43.
22. For a recent study on Boyle and secrecy see Hunter 2011.
23. "Le S^r du Chesne n'apoint remarque cela en ses cristaux de sel ; mais ceux qui les ont veus et flairez comme j'ay fait, peuvent asseurer que ces cristaux doux de sel estant froids, n'ont point d'odeur désagréable ; mais que celles qu'ils rendent au feu n'est gueres plaisante."

24. "Le S^r du Chesne a bien fait voir en la procédure qu'il a descripte… qu'il y faut beaucoup d'industrie, et bien de l'exactitude, et désire pour cela un artiste bien soigneux et bien expert, de bons vaisseaux et un fourneau bien propre a reigler le feu."
25. "ceux qui ont procédé moin exactement a cette recherche des principes des mixtes naturels, par l'analyse chymique, ont pris pour principes et premières pièces constitutives le mercure, le sel, et le soulphre."
26. "Ils jugent que le phlegme est un rudiment ou ébauche élémentaire… ny essensifie, que la terre morte et damnée (ce sont les termes de l'art) est un excrément sans énergie… que ces deux affoiblissent la vertu des trois autres, et pour ce ils veulent qu'on travaille a les séparer et rejeter."
27. "quelque analogie entre les trois matières qui composent le grand arcane des Hermétiques, et ce trois substances énergiques qu'ils scavent séparer de quelques mixtes par une analyse imparfaite."
28. "Quelqu'un des plus renommez d'entre les chymiste modernes s'est vante de la connoissance d'un moyen fort expéditif de résoudre tous les corps mixtes… On nous accusera point d'avoir par ces travaux introduit de nouvelles formes, s'il ne reste en apparence que celle de l'eau."
29. "nous n'asseurerons pas que cette eau soit simple et seule, quoyqu'il n'y paroisse autre chose, et qu'on n'y voye plus les effets de sa fermentation précédente. Elle pourroit ester encore empreinte de quelque efficient invisible, capable de reproduire en elle de nouvelles formes de sel, de mercure, et de soulphre, etc. Nous pouvons seulement conclure que ces premières formes estoient accidentelles ; que'elle estoient l'effect de l'action de quelque agent interne dans la matière de l'eau."
30. Pagel argued that Van Helmont could not "agree with those who assume a gap between things corporeal and spiritual." Duclos seems to have assumed such a "gap" and in this respect could be seen as differing from Van Helmont, despite having been influenced by Van Helmont's theory of alkahest, which Duclos had applied in a possibly idiosyncratic fashion to distinguish between radical (chymical) resolutions and partial (physical) ones.
31. "mixtes parfaicts" differ from "mixtes imparfaictes" in that they "ont quelque participation de la vie"; "il est nécessaire de supposer un troisième genre de principes de ces mixtes, que les chymists modernes appellent archée par excellence, comme estant le plus parfaict, et le plus noble des autres. Et qu'ainsy les principes des mixtes naturels, sont le corps matériel, l'esprit altératif, et l'âme vivifiante, ou l'archée."
32. "il est nécessaire qu'il y ait en ces mixtes pairfaicts un 3e principe plus énergique et encore moins corporel que l'esprit altératif."
33. "Nous dison icy corporel, non pas ce qui est estendu en triple dimension géométrique; mais ce qui est tangible. Et nous appellons incorporel, et spirituel ce qui en ce sens n'est pas corporel, et ne peut etre manie ou touché sensiblement."
34. "la seconde parties de livre de m. Boyle, laquelle contient deux traittez, ou cet autheur a tasché d'accommoder les principes de la philosophie corpusculaire aux expériences des chimistes et de rendre raison de ses expériences par la doctrine des atomes."
35. "salpestre qu'il estime digne d'estre exactement recherchée."
36. "c'est bien le mouvement qui fait l'effervescence, mais M^r Boyle n'assigne pas la cause de ce mouvement tumultueux, que peut estre il n'a pû trouver dans la figure & dispositions des particules."
37. "C'est donc par l'action mutuelle et réciproque des Sels de diverse qualité, qu ce mouvement est excité."
38. "La chaleur du feu excite du mouvement dans les matières mobiles, selon le degré de leur mobilité, de sorte que celle qui sont mobiles en mesme degré ne se séparent point les unes des autres par un mesme degré de chaleur, qui les agitant également les fait monter ensemble, et les sépare seulement de celles qui sont moins mobiles. Et celles qui sont inégalement mobiles se séparent les unes des autres, car les plus faciles a se mouvoir estant plus agitées et plus tost raréfiées par la chaleur s'eslevent les premières et quittent celles qui sont moins moniles mais qui les peuvent suivre estant pressées d'une chaleur plus forte."
39. Although Boyle considered the Peripatetic Elements as being rationally rather then empirically deduced, the Aristotelian elements have ultimately empirical origins.
40. See Gal's chapter in this book and the Introduction.

References

Alexander, Peter. 1985. *Ideas, qualities, and corpuscles: Locke and Boyle on the external world*. Cambridge: Cambridge University Press.

Anstey, Peter R. 2000. *The philosophy of Robert Boyle*. London: Routledge.

Boantza, Victor D. 2007. Reflections on matter and manner: Duclos reads Boyle, 1668–69. In *Chymists and chymistry: Studies in the history of alchemy and early modern chemistry*, ed. Lawrence Principe, 181–192. New York: Science History Publications.

Boantza, Victor D. 2010. Alkahest and fire: Debating matter, chymistry, and natural history at the early Parisian academy of sciences. In *The body as object and instrument of knowledge: Embodied empiricism in early modern science*, ed. O. Gal and C. Wolfe, 75–92. Dordrecht: Springer Publishing.

Boantza, Victor D. 2012. *Matter and method in the long chemical revolution: Laws of another order*. Burlington: Ashgate. forthcoming.

Boyle, Robert. 1999–2000. *The works of Robert Boyle*, ed. M. Hunter and E.B. Davis, 14 Vols. London: Pickering & Chatto.

Chalmers, Alan. 1993. The lack of excellency of Boyle's mechanical philosophy. *Studies in History and Philosophy of Science* 24: 541–564.

Clericuzio, Antonio. 1990. A redefinition of Boyle's chemistry and corpuscular philosophy. *Annals of Science* 47: 561–589.

Clericuzio, Antonio. 1994. Carneades and the chemists: A study of *The Sceptical Chymist* and its impact on seventeenth-century chemistry. In *Robert Boyle reconsidered*, ed. Michael Hunter, 79–90. Cambridge: Cambridge University Press.

Clericuzio, Antonio. 2000. *Elements, principles and corpuscles: A study of atomism and chemistry in the seventeenth century*. Dordrecht: Kluwer Academic Publishers.

Clericuzio, Antonio. 2006. Teaching chemistry and chemical textbooks in France. From Beguin to Lemery. *Science Education* 15: 335–355.

Cook, Margaret G. 2001. Divine artifice and natural mechanism: Robert Boyle's mechanical philosophy of nature. In *Osiris, 2nd series, Science in theistic contexts: Cognitive dimensions*, ed. John Brooke, Margaret Osler and J. M. van der Meer 16: 133–150. The History of Science Society.

Debus, Allan G. 1964. The Paracelsian aerial niter. *Isis* 55: 43–61.

Debus, Allan G. 1967. Fire analysis and the elements in the sixteenth and the seventeenth centuries. *Annals of Science* 23: 127–147.

Debus, Allan G. 1991. *The French Paracelsians: The chemical challenge to medical and scientific tradition in early modern France*. Cambridge: Cambridge University Press.

Debus, Allan G. 1990. Iatrochemistry and the chemical revolution. In *Alchemy revisited: Proceedings of the international conference on the history of alchemy at the university of Groningen, 17–19 April 1989*, ed. Z.R.W.M. von Martels, 51–66. Leiden/New York: Brill.

Emerton, Norma. 1984. *The scientific reinterpretation of form*. Ithaca: Cornell University Press.

Fleming, William. 1946. The element of motion in baroque art and music. *The Journal of Aesthetics and Art Criticism* 5(2): 121–128 [Special Issue on Baroque Style in Various Arts].

Fontenelle, Bernard le Bovier de. 1733. Histoire de l'Académie Royale des Sciences, depuis son établissement en 1666 jusqu'en 1686, 2 vols. Paris: Martin, Coignard, Guerin.

Franckowiak, Rémi. 2003. Le développement des théories du Sel dans la chimie française de la fin du XVIe à celle du XVIIIe siècle. Université Charles de Gaulle, Lille III. Unpublished PhD thesis.

Franckowiak, Rémi. 2008. La Chimie du XVIIe Siècle: Une Question de Principes. *Methodos*, 8 [Chimie et mécanisme à l'âge classique]. Retrieved 5 Oct 2008, from http://methodos.revues.org/document1823.html

Franckowiak, Rémi. 2009. Du Clos, un Chimiste Post-*Sceptical Chemist*. In *La Philosophie Naturelle de Robert Boyle*, ed. C. Ramond and M. Dennehy, 361–377. Paris: Vrin.

Friedrich, Carl J. 1965. *The age of the Baroque: 1610–1660*. New York: Harper & Row.

Hall, Marie B. 1958. *Robert Boyle and Seventeenth-Century Chemistry*. Cambridge: Cambridge University Press.

Henry, John. 1986. Occult qualities and the experimental philosophy: Active principles in pre-Newtonian matter theory. *History of Science* 24: 335–381.

Hirai, Hiro. 2001. Paracelsisme, néoplatonisme et médecine hermétique dans la théorie de la matière de Joseph Du Chesne à travers son Ad veritatem hermeticae medicinae (1604). *Archives Internationales d'Histoire des Sciences* 51: 9–37.

Holmes, Frederic L. 1971. Analysis by fire and solvent extractions: The metamorphosis of a tradition. *Isis* 62: 128–148.

Holmes, Frederic L. 2003. Chemistry in the Académie Royale des Sciences. *Historical Studies in the Physical and Biological Sciences* 34: 41–68.

Hunter, Michael. 2011. Robert Boyle and secrecy. In *Secrets and knowledge in medicine and science, 1500–1800*, ed. E. Leong and A. Rankin, 87–104. Burlington: Ashgate.

Hutchison, Keith. 1982. What happened to occult qualities in the scientific revolution? *Isis* 73: 233–253.

Jacob, Margaret C. 2006. *Strangers nowhere in the world: The rise of cosmopolitanism in early modern Europe*. Philadelphia: University of Pennsylvania Press.

Joly, Bernard. 1992. Qu'est-ce Qu'un Laboratoire Alchimique? *Cahiers d'Historie et de Philosophie des Sciences* 40: 87–102.

Joly, Bernard. 1996. L'alkahest, dissolvant universel ou quand la théorie rend pensable une pratique impossible. *Revue d'Histoire des Sciences* 49: 305–344.

Jones, Richard F. 1965. *Ancients and moderns. A study of the rise of the scientific movement in seventeenth-century England*. Berkeley: University of California Press.

Kahn, Didier. 2007. *Alchimie et paracelsisme en France à la fin de la Renaissance (1567–1625)*. Genève: Librarie Droz.

Kim, Young S. 1991. Another look at Robert Boyle's acceptance of the mechanical philosophy: Its limits and its chemical social contexts. *Ambix* 38: 1–10.

Kim, Mi G. 2001. The analytical ideal of chemical elements: Robert Boyle and French didactic tradition of chemistry. *Science in Context* 14: 361–395.

Kim, Mi G. 2003. *Affinity, that elusive dream: A genealogy of the chemical revolution*. Cambridge: MIT Press.

Klein, Ursula, and Lefèvre. W. 2007. *Materials in Eighteenth-Century Science: A Historical Ontology*. Cambridge: MIT Press.

Landes, David S. 1983. *Revolution in time: Clocks and the making of the modern world*. Cambridge: Harvard University Press.

Levine, Joseph M. 1999. *Between the ancients and the moderns: Baroque culture in restoration England*. New Haven: Yale University Press.

Maravall, Antonio J. 1986. *Culture of the Baroque: Analysis of a historical structure*. Minneapolis: University of Minnesota Press.

Martin, John R. 1977. *Baroque*. New York: Harper & Row.

Metzger, Hélène. [1923] 1969. *Les doctrines chimiques en France du début du XVIIe à la fin du XVIIIe siècle*. Paris: A. Blanchard.

Mumford, Lewis. 1934. *Technics and civilization*. New York: Harcourt, Brace. Newman.

Newman, William R. 1994. *Gehennical fire: The lives of George Starkey, an American Alchemist in the scientific revolution*. Chicago: University of Chicago Press.

Newman, William R. 1996. The alchemical sources of Robert Boyle's corpuscular philosophy. *Annals of Science* 53: 567–585.

Newman, William R. 2006. *Atoms and Alchemy: Chymistry and the experimental origins of the scientific revolution*. Chicago: University of Chicago Press.

Newman, William R., and Lawrence M. Principe. 1998. Alchemy vs. chemistry: The etymological origins of a historiographic mistake. *Early Science and Medicine* 3: 32–65.

Newman, William R., and Lawrence M. Principe. 2002. *Alchemy tried in the fire: Starkey, Boyle, and the fate of Helmontian chymistry*. Chicago: University of Chicago Press.

Pagel, Walter. 1982. *Joan Baptista Van Helmont: Reformer of science and medicine*. Cambridge: Cambridge University Press.

Pagel, Walter. 1985. The religious and philosophical aspects of van Helmont's science and medicine. In: *Religion and neoplatonism in Renaissance medicine*, ed. Marianne Winder, part III. London: Variorum.

Partington, James R. 1961–70. *A history of chemistry*, 4 Vols. London: Macmillan.

Principe, Lawrence M. 1994. Boyle's alchemical pursuits. In *Robert Boyle reconsidered*, ed. Michael Hunter, 92–102. Cambridge: Cambridge University Press.

Principe, Lawrence M. 1998. *The aspiring adept: Robert Boyle and his alchemical quest*. Princeton: Princeton University Press.

Reti, Ladislao. 1968. Van Helmont, Boyle and the alkahest. In *Some aspects of seventeenth-century medicine and science: Papers read at a Clark Library Seminar*, ed. Ladislao Reti and W.C. Gibson, 3–19. Los Angeles: William Andrews Clark Memorial Library.

Sargent, R.-M. 1995. *The diffident naturalist: Robert Boyle and the philosophy of experiment*. Chicago: University of Chicago Press.

Skrine, Peter N. 1978. *The Baroque: Literature and Culture in Seventeenth-Century Europe*. New York: Holmes & Meier.

Stroup, Alice. 1990. *A company of scientists: Botany, patronage, and community at the seventeenth-century Parisian academy of science*. Berkeley: University of California Press.

Stroup, Alice. 2002. Censure ou querelles scientifiques: l'affaire Duclos (1675–1685). In *Règlement, usages et science dans la France de l'absolutisme*, ed. Cristian Demeulenaere-Douyère and Eric Brian, 435–452. Paris: Tec & Doc.

Sturdy, David. 1995. *Science and social status: The members of the Académie des sciences: 1666–1750*. Woodbridge: Boydell Press.

Vickers, Brian. 2008. The 'new historiography' and the limits of alchemy. *Annals of Science* 65: 127–156.

Chapter 12
The Simulation of Nature and the Dissimulation of the Law on a Baroque Stage: Galileo and the Church Revisited

Rivka Feldhay

Abstract This paper presents the way Galileo's telescopic observations were woven into a new kind of astronomical discourse that provoked extreme reactions by the Catholic establishment of those days. Galileo, I shall argue, invented his own strategies for dealing with the gap opened up by the telescope between appearances and being – between what seemed to be the case and what actually was out there. In doing so, Galileo added a dimension to the practices of signification common in the political, theological and theatrical arenas of Baroque culture. In each of these areas, the gap between "seem and be" was haunting political actors and courtiers, theologians and playwrights. Obviously, the need to cope with such gap gave birth to new or modified cultural forms, among them new forms of representation and allegory. Following Louis Marin's work on the discourse of representation around the King's portrait (Marin 1988), as well as Walter Benjamin's argument on the centrality of allegorical practices in Baroque theatre (Benjamin 1977), I shall isolate two additional arenas in which Baroque forms of representation and allegory were used: in Galileo's attempt to cope with the visual evidence about the heavens on the one hand; and in the Inquisition trial of the Medici mathematician-philosopher on the other. My aim is to show that the constitution of a new kind of scientific discourse and the challenge it posed to Catholic hegemony took active part in Baroque rituals of representation and allegory, and that those should not be read as mere literary techniques. Rather, Galileo's use of representation and allegory involved him in a highly sophisticated system of communication relevant for understanding different dimensions of a baroque scholar's life. In the last part of the paper I will point out how, during the trial of Galileo, allegorical practices were stretched beyond the limit of signification and transformed into a mode of dissimulation – a recognized practice defined by contemporaries as "dissimulazione onesta".

R. Feldhay (✉)
The Cohn Institute for the History and Philosophy of Science
and Ideas, Tel Aviv University, Tel Aviv, Israel
e-mail: feldhay@post.tau.ac.il

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1_12,

Representation and Allegory as a Way of Life

The first chapter of the *Portrait of the King* has been dedicated by Louis Marin to the problem of representation in the seventeenth century (Marin 1988, pp. 3–15). While probing into the structure of representation and the sign Marin discovers the unique relationship holding between the representation of power and the power of representation. Representation, he claims, has two meanings: presentation of something absent and its substitution by something else; and a doubling of someone or something that is present. Such doubling empowers the thing represented, as in the case of a passport that doubles the presence of the presenter, presenting him as a citizen of a state, and empowering the state by the fact of its representation: "…to represent," says Marin, "will always be to present oneself representing something" (Marin 1988, 5). What is the effect of such model of representation, Marin asks, and answers: the very act of presenting something absent already has a much more powerful effect than mere reflection is supposed to have. That is the effect of framing, Marin seems to say, as he quotes Leon Battista Alberti speaking of the divine power of representation. Framing transforms the substitute into something better than the source (as in the case of the dead person who looks much better in the image than in real life); and doubling acts in the same way as machines do, by accumulating mechanical advantage.[1] The machine of representation can be understood in terms of a lever that increases the power in relation to the location of the weight on the lever's arm. In this way Marin deduces the relationship of representation to power. The act of representation puts power into signs, and the representation by means of signs doubles the power. By means of representation, that which is represented, as well as the presenter gain authority and legitimization.

In what follows Galileo's strategies of representation of celestial phenomena, his acts of framing and the way he gained authority through representation will be described and analyzed. But there is more to his writing on celestial phenomena than just representation, for all his strategies aimed at constituting one analogy in terms of which celestial phenomena had to be understood, namely the analogy between celestial and terrestrial matter. Establishing this analogy meant the negation of the fundamental premise of traditional cosmology anchored in the great divide between the heavens made of crystalline, transparent and eternal matter, and changing terrestrial matter given to generation and corruption. Establishing this analogy meant annihilation of the hierarchy between heaven and earth. It actually meant that the earth was but another planet, neither the center of the universe nor the crown of Divine creation. Galileo did not dare to claim all these in a direct manner in his *Sidereus nuncius*, the object of my analysis. Rather, he preferred to establish his analogy first. Thus representation was ultimately used as a means for achieving an allegorical interpretation of the "book of nature" – mountains on the moon – much as an allegorical reading of the sacred book of scripture became necessary in order to legitimize the new story about the physical nature of the heavens. This turn to allegory puts Galileo's *Sidereus nuncius* at the center of the Baroque stage that privileged allegory as a form of expression.

But what do we mean by “allegory” when we choose to draw attention to the “allegorical” practices of Galileo, and more generally to the allegorical way of life that became so popular in the age of Baroque?

“Allegory” according to Angus Fletcher “says one thing and means another” (Fletcher 1964, 2). While destroying the simple expectation that language means what it says, it is still “a fundamental process of encoding our speech” (Fletcher 1964, 3). Rainer Nägele further examines the cultural conditions in the context of which allegories are usually produced: “Allegory”, he remarks, “is the product of a crisis” (Nägele 1991, 82). It expresses dissatisfaction with an existing authoritative reading of a text but attempts to still save it by declaring its “appearance” a “mere surface”. Thus a gap between signifier and signified opens up, a gap that requires filling up with deliberate action (or manipulation). The result is appropriation by negation – negation of what seems “natural” and “proper” and appropriation by some kind of arbitrary or even violent tearing from the natural context and re-planting in a new “artificial” one.

Walter Benjamin invokes the power of Baroque allegory by pointing out its immersion in a dialectics between extreme opposites that express the complexities, or rather inconsistencies, inherent in the intellectual and cultural order of the period. One aspect of such dialectics occurs, according to Benjamin, between what he calls “convention” and “expression”. The first denotes consensus-based practices – such as conventions of perspectival paintings in the Renaissance; the second denotes unmediated insight into the truth of cultural artifacts beyond contemporary conventions. “…Allegory,” Benjamin writes, “is both: convention and expression; and both are inherently contradictory…The allegory of the seventeenth century is not convention of expression, but expression of convention. At the same time expression of authority, which is secret in accordance with the dignity of its origin, but public in accordance with the extent of its validity” (Benjamin 1977, 175). Thus Benjamin leads us to see how the artificiality and arbitrariness of allegorical practices reach beyond convention and force the audience to accept the authority of the allegorist: “The voluptuousness with which significance rules, like a stern sultan in the harem of objects, is without equal in giving expression to nature” (Benjamin 1977, 184); “If an object becomes allegorical…then, it is exposed to the allegorist, it is unconditionally in his power. That is to say it is now quite incapable of emanating any meaning or significance of its own; such significance as it has, it acquires from the allegorist. He places it within it, and stands behind it; not in a psychological but in an ontological sense. In his hands the object becomes something different; through it he speaks of something different and for him it becomes a key to the realm of hidden knowledge… This is what determines the character of allegory as a form of writing. It is a schema; and as a schema it is an object of knowledge, but it is not securely possessed until it becomes a fixed schema: at one and the same time a fixed image and a fixing sign” (Benjamin 1977, 183–4).

Galileo’s *Sidereus nuncius* is full of examples manifesting Galileo’s peculiar combinations between convention and expression, between the traditional language of astronomy – namely mathematical representation – and an ingenious allegorical reading of material nature. Galileo was undoubtedly dissatisfied with the reading of the "book of nature" common among his contemporaries. In an effort to convince his readers of an alternative Pythagorean reading, he could only resort to the fleeting moment of his experience as an observer. That moment is depicted in his text through a variety of images intended to convey to his readers his intuition about the "aesthetic" truth emanating from the scene rather than by any proof of the Copernican system unavailable to him in that moment.

Broadening the Scope of the Visible[2]

The little booklet entitled *Sidereus nuncius* (*Starry Messenger*) was published in March 1610 in Venice. The text contained Galileo's first systematic account of some crucial celestial observations made through the telescope. Galileo's telescopic observations bore different kinds of fruits: from new objects hitherto unseen – such as many fixed stars, or the four moons of Jupiter – to the representation and interpretation of the rough surface of moon, the new explanation of its secondary light and Galileo's speculations about the nature of the Milky Way. The text also transformed its author from a respectable but rather poorly paid professor of mathematics at the University of Padua, compelled to complete his income by tutoring and hostelling private students as well as by producing and selling mathematical instruments – into a sort of courtier of Cosimo de Medici, serving as the Duke's Mathematician and Philosopher (Biagioli 1993).

How did Galileo present his discoveries? How did he recreate the experience of observing entirely unknown phenomena in the minds and feelings of his readers steeped in a commonly accepted view that the heavens are unchangeable?

The association between the human capacity to see and sense on the one hand, and the human capacity to know on the other hand dates back to Aristotle. In this spirit Aristotle made a distinction between celestial bodies – perfect but less knowable – and imperfect terrestrial things such as animal and plants. Thus in his *De partibus animalium*, he stated:

> Of things constituted by nature some are ungenerated, imperishable, and eternal, while others are subject to generation and decay. The former are excellent beyond compare and divine, *but less accessible to knowledge*. The evidence that might throw light on them, and on the problems which we long to solve respecting them, *is furnished but scantily by sensation* (*De partibus animalium* I, 5, 645a21-645b6; my emphasis).

Because for Aristotle knowledge depends so much on the senses, and because the primary sense for him is vision, he judges the eternal heavenly bodies that are physically remote as less knowable. This becomes more plausible while reading a passage from *On the Heavens*, where he claims that: "We cannot reasonably attribute to anything any characteristics but those which *observation detects* in many or all instances" (*De caelo* I. 279b18-20, my emphasis)

The idea that the scope of the visible could and should be broadened beyond Aristotle's boundaries was especially popular among Renaissance humanist artists, first and foremost Leon Battista Alberti (1404–1472). In his treatise on perspective, *De pictura*, he first articulated the thought that by embedding mathematical knowledge in the material world human visibility field could be enlarged and intensified:

> Mathematicians measure the shapes and forms of things in the mind alone and divorced entirely from matter. We, on the other hand, ***wish to set this thing up as visible***, and will therefore use, as is said, a cruder Minerva for writing (Alberti 1972, 36; my emphasis).

The thing Alberti wished to "set up as visible" was obviously the perspectival grid, a geometrical entity which Alberti imagined as a veil that intersects the visual pyramid and has the advantage that "it always presents the same surfaces unchanged, for once you have fixed the position of the outlines, you can immediately find the apex of the pyramid you have started with…" (Alberti 1972, 68). Developing and teaching the techniques of perspective – setting mathematical things visible, in Alberti's terms – was at the heart of his project. Simultaneously, he developed a special "ethos" for humanist painters, differentiating between the simple act of representing beautiful things, and the representation of the idea of beauty in things. Alberti used this distinction to justify the painter's active encroaching upon the boundaries between the natural and the artificial that perspective techniques embodied for him and his contemporaries.

But Alberti was certainly not alone in his quest to broaden the scope of the visible. Another outstanding example was Nicholas of Cusa (1401–1464) who made an extraordinary attempt to include in his project of broadening the visible not only mathematical entities, but also the ultimate invisible, namely God. He accepted the challenge set by Alberti's system of perspective, and in a bold attempt suggested a new theology based on this desire to see the invisible. "However, we want to see the indivisible beginning itself" (Nicholas of Cusa 1962, cap. vii, f. CLXXXIIII,r).[3] In one of his later small treatises – *De beryllo* – Cusanus suggested an experiment with a beryl, as a sort of a lens: "Beryl stones are bright, white, and clear", he wrote. "To them are given both concave and convex forms. *And someone who looks out through them apprehends that which previously was invisible*" (Nicholas of Cusa 1962, cap. ii, CLXXXIIII, v.; my emphasis).[4]

Alberti's discourse on the visible, no less than Cusanus' on the invisible indicate that the cultural background relevant for understanding Galileo's problematization of the visible was anchored in the material culture of practitioners, but also in the discourse of humanist artists such as Alberti and Leonardo da Vinci as well as in that of humanist theologians such as Nicholas of Cusa. Galileo was certainly quick to discern the possibilities offered by the telescope to destroy the Aristotelian hierarchy between invisible or less visible celestial objects versus visible and knowable terrestrial ones. Indeed, the telescope seemed to shrink the gap in visibility between heavenly and earthly objects by enlarging the size and by decreasing the vagueness of distant things. However the technical possibilities opened up by the new instrument did not necessarily lead to a new astronomical discourse.

Materializing and Sensualizing the Visible

The *Sidereus nuncius* provides us with a wonderful example of the transformation of observed phenomena into objects in a new kind of astronomical discourse. Focusing on the moon for the moment, one may closely follow Galileo's strategies of identifying and classifying the spots; their mapping; their representation by verbal and pictorial techniques, as well as their interpretation as mountains and valleys analogous to those existing on earth by means of a theory on the diffusion of light.

First Galileo classified two parts on the surface that faces us: "a lighter and a darker; the lighter part seems to surround and to pervade the whole hemisphere, while the darker part discolors the moon's surface like a kind of cloud, and makes it appear covered with spots". Then he classified two kinds of spots on that surface: "…those spots which are fairly dark and rather large are plain to everyone and have been seen throughout the ages; these I shall call the 'large' or 'ancient' spots, distinguishing them from others that are smaller in size but so numerous as to occur all over the lunar surface, and especially the lighter part".

The act of framing begins with pointing out to the reader the uneven line that Galileo saw separating the dark part of the moon from its enlightened part while the moon was 4 or 5 days old: "…the boundary which divides the dark part from the light does not extend uniformly in an oval line as would happen on a perfectly spherical solid, but traces out an uneven, rough, and very wavy line as shown in the figure below" (Galilei 1610, 32).

This uneven line serves as a key image to the following understanding of the rough nature of the surface of the moon established through analogy with common experiences on earth. Protrusions of light, Galileo continues, cross the dividing line and penetrate the dark part. Moreover, the lighted part is scattered with many small dark spots that are wholly separated from the dark part. Galileo observes that the darkest parts of those spots are directed towards the sun, while those parts directed away from the sun appear to be surrounded with glorious contours which he imagines as the top of earthly mountains flooded by sunshine:

> There is a similar sight on earth about sunrise, when we behold the valleys not yet flooded with light though the mountains surrounding them are already ablaze with glowing splendor on the side opposite the sun. And just as the shadows in the hollows on earth diminish in size as the sun rises higher, so these spots on the moon lose their blackness as the illuminated region grows larger and larger (Galilei 1610, 32).

This analogy – or allegorical reading – is at the heart of the new and revolutionary meaning endowed by Galileo to the dark spots perceived through the telescope on the moon. Gradually, he argues, the spots are losing their darkish hew and the lighted parts are growing. Galileo then presents what he sees in terms of light gradually diffusing from the top of the moon's mountains towards the valleys. Gradually the patches are broadened, become brighter and brighter, until they unite with the rest of the lighted part. This description is then given a further allegorical reading:

> And on earth, before the rising of the sun, are not the highest peaks of the mountains illuminated by the sun's rays while the plains remain in shadow? Does not the light go on spreading while the larger central parts of those mountains are becoming illuminated? And

> when the sun has finally risen, does not the illumination of plains and hills finally become one? But on the moon the variety of elevations and depressions appears to surpass in every way the roughness of the terrestrial surface, as we shall demonstrate further on (Galilei 1610, 33).

Thus, the elaboration of the analogy is the very process by which the moon is being transformed from a celestial body made of perfect, crystalline celestial matter into an object that is not essentially different from the earth upon which we live, a legitimate object of sensual knowledge.

There are three reasons for considering Galileo's interpretation an allegorical reading of the book of nature at this point. First, there was no way for him to really experience celestial matter either by touching or by any other physical means; second, even seeing was extremely limited by the mediation of an instrument that was hard to operate in the very first stage of its career; third and foremost, Galileo completely detached himself from the metaphysical discourse that allowed one to speak of the material nature of the heavens in the philosophical tradition. Representation and allegorical reading, however, were interestingly intertwined with plastic representation in the form of a series of drawings of the phases of the moon that accompanied the text. In recent years the significance of artistic know-how for the understanding of natural phenomena has gained the wide attention of scholars, among them Eileen Reeves (1997) and Horst Bredekamp (2007) who wrote on Galileo's moon drawings. They have drawn our attention to Galileo's superb mastery of the painters' brush in endowing his moon drawings with real material quality, to his ability to vary the shadowing of the dark areas and to finely apply chiaroscuro techniques for embodying the explosion of sunlight on the surface of the moon as well as for distinguishing the reflection of the secondary light from the earth to the moon. Some have even argued that Galileo's technical training in drawing was behind ability to imagine and present celestial phenomena in terms of well known terrestrial experience of light diffusion. What should be stressed, however, is the impact of this "theatre of light" on the relatively wide audiences who were able to read the text. Coming into contact with the phenomena through the wooden prints of the moon drawings and Galileo's comments on them, they could even start to feel that they were actually sharing with him the experience of watching closely those mountains on the surface of the moon.

Embedding the Visible in Traditional Astronomical Elocution

A comparison between how celestial bodies were imagined in Copernicus' astronomical discourse with the role played by the moon in Galileo's new astronomical discourse suggests two diametrically opposed strategies of object construction. The accumulating impression from reading Copernicus is that the objects are being systematically emptied of their sensible, physical contents. For in the Copernican discourse *what is seen is not what there is, while what there is differs from what is seen*. Thus, the observed retrograde motion of the planets is perceived to reflect the invisible motion of the earth around the sun. Such motion, however, leaves no sensual traces: it did not result in any observed parallax (in the telescopes that served Galileo and his contemporaries), no strong wind on the surface of the earth was felt, no clouds or birds were deflected from their routes, nor was it observed that a stone falling from the top of the tower was left behind. Galileo's telescope, on the contrary was able to fill the rather sterile Copernican universe with sensible realities. His discourse provided some practices for regulating the new space of visibility in order to render Copernicus' theoretical move meaningful, first and foremost in visual terms. At the same time, he had to incorporate the new objects into the traditional mode of astronomical elocution, whose distinctive characteristic was its use of geometrical, arithmetical and trigonometric language. Traditional astronomy was essentially mathematical. Its primary objects were the geometrical models serving as the most appropriate tools for representing and predicting the motions of the planets. Galileo was well aware that his innovations had to be cast in the same language. He could not just describe in plain language what he saw and represent it graphically. He had to speak the mathematical language of the tradition, which he later on presented as the language of Mother Nature. His attempt to measure the height of the mountains of the moon is a perfect example illustrating how he meant to fuse his innovations with the established astronomical discourse. Thus, the text embodies the striking combination – full of tension – of convention and expression – the old and the new – so well articulated by Benjamin as a typical Baroque strategy (see above). Reversing the previous practices of rendering the abstract concrete, he now abstracted from the concreteness of the moon's body, in order to measure it by means of a geometrical model. Thus he came to the pretty good estimation of 4 miles for the height of a mountain on the moon. His estimation of 1 mile for the maximum height of mountains on earth was quite wrong, however.

A New Type of Authority

The *Sidereus nuncius* may be viewed as a textual event embodying the crossroad between different traditions of knowledge production and heralded the new kind of sponsors they acquired in the sixteenth and seventeenth centuries, namely princes and kings. The construction and use of the telescope to investigate the true nature of celestial bodies strengthened, in a dramatic fashion, early modern tendencies to melt together technological and artistic know-how in such fields as lens polishing and painting with new answers to traditional and new questions in optics, astronomy and natural philosophy such as the problem of celestial light, or the periodic times of Jupiter's satellites (Bredekamp 2007; Valleriani 2010). However, Galileo rightly sensed that limiting his message to mathematical practitioners, painters, natural philosophers or professional astronomers was not enough. His involvement in contemporary court culture deepened his awareness that without spreading his discoveries to widening circles of consumers of the new Renaissance culture, and even more importantly without acquiring the support of a strong patron his ambitious projects could not be realized. Thus by simultaneously playing on different intellectual grounds Galileo assumed an important role in the emergence of a field that was, in fact, both scientific and political.

The elaborate title of his book states that this "starry messenger" is meant to reveal "great, unusual, and remarkable spectacles opening these to the consideration of every man" (Title of *Sidereus nuncius*, Galilei 1610, 21). As many scholars have already noted, the reference of "starry messenger" is not unambiguous. While the term could refer to the stars "revealing themselves" to every man via the telescope, it could also refer to the constructor of the telescope, the one revealing and representing nature's secrets and giving them meaning. The title also serves for a preliminary self presentation of Galileo as a Florentine gentleman, professional mathematician and inventor: "observed by Galileo Galilei gentleman of Florence Professor of mathematics in the University of Padua with the aid of a spyglass lately invented by him." No less important, Galileo presented himself as an "author", a title that endowed him with the authority to name the Jupiter moons which he discovered as the "Medicean Stars":

> In the surface of the Moon, in innumerable Fixed Stars, in Nebulae, and above all in Four Planets swiftly revolving about Jupiter at differing distances and periods, and known to no one before the Author recently perceived them and decided that they should be named The Medicean Stars. (Title of *Sidereus nuncius*, Galilei 1610, 21)

Naturally, the text was dedicated to Galileo's patron, Cosimo de Medici. In the dedicatory letter to the prince, Galileo built analogies and a hierarchy between artists who served princes with artifacts that are subject to decay over time, writers presenting their patrons with texts, perceived by Galileo as "literary monuments", and natural philosophers like himself, who could offer their princes their "natural gifts" such as Jupiter's satellites. Thus, in the hierarchy built by Galileo he saw natural philosophers as superior, for he deemed discoveries related to nature the most enduring. However, immediately following the argument about the nobleness of his gift emanating from its eternity Galileo reversed the direction of emanation. Invoking the relationship between Jupiter and his moons in analogy to the relationship between the prince and his virtues, he claimed that as power emanates from the planet to its satellites, so does eternity emanate from the prince to the stars: "For scarce have the immortal graces of your spirit begun to shine on earth when in the heavens bright stars appear as tongues to tell and celebrate your exceeding virtues to all time" (Title of *Sidereus nuncius*, Galilei 1610, 21). In Galileo's time, Baroque courtly discourse was an allegorical discourse. Galileo, who perfectly mastered its rules used this mastery to deliver a complex, equivocal message to his Prince and to his readers. On the one hand the title of the text referred to the new stars sending their message to the world. On another level, the rules of allegorical discourse allowed the author to choose the right analogy and its direction. Thus, Galileo's *Starry Messenger* may have referred not simply to the message from the stars to the readers, but rather to himself as a bearer, mediator and perhaps even creator of the glory carried to Cosimo through the Medicean stars discovered by his philosopher, namely by Galileo.

Framing Philosophical Astronomy

The implications of Galileo's strategies in building his new discourse became clearer in two additional texts written between the publication of the *Sidereus nuncius* (1610) and the first inquisitorial sanctions against Copernicanism (1616).

In his *Istoria e dimostrazioni intorno alle macchie solari e loro accidenti* (History and Demonstrations concerning Sunspots and their Phenomena, 1613) Galileo explicated his goal to unify natural philosophy and mathematical astronomy into one new science which he named "philosophical astronomy," aiming to investigate the "system of the world" terrestrial and celestial. On the one hand he aimed at redefining the traditional role of the astronomer. Proponents of "philosophical astronomy", he argued, "…going beyond the demand that they somehow save the appearances, seek to investigate the true constitution of the universe—the most important and most admirable problem that there is" (Galilei 1613, 97). On the other hand Galileo recasted the traditional goals of natural philosophy which, he claimed, should not seek to penetrate the essence of things, but should concentrate on the geometrical properties of objects (Galilei 1613, 123–4).

Galileo's project to unify natural philosophy and mixed mathematics (namely those sciences that study natural phenomena using mathematical methods, and especially astronomy) alienated university philosophers, whose subject matter and methodology he had sharply criticized and to whose philosophy he had presented an alternative. It was, however, his ideas about the two books—the book of nature and the Bible—that most boldly defied the authority of theologians. The two books, he contended in his famous *Lettera a Madama Cristina di Lorena Granduchessa di Toscana (Letter to the Grand Duchess Christina*, 1615), differed in their subject matter, their audiences, and their goals.

> The authority of Holy Scripture aims chiefly at persuading men about those articles and propositions which, surpassing all human reason, could not be discovered by scientific research or by any other means than through the mouth of the Holy Spirit himself….the writers of Holy Scriptures not only did not pretend to teach us about the structure and motions of the heavens and the stars, and their shape, size, and distance, but they deliberately refrained from doing so (Finocchiaro 1989, 93–4).

Not only was the Bible concerned with problems of faith and salvation, which Galileo separated from problems of understanding nature and its truths, but the difference was as deep as language itself:

> [I]t is appropriate for Scripture to say many things that are different (in appearance and in regard to the literal meaning of the words) from absolute truth… [N]ot every scriptural assertion is bound to obligations as severe as every natural phenomenon (Finocchiaro 1989, 93).

Nature, on the other hand, in whose actions God is not "any less excellently revealed," and which "proceed[s] alike from the divine Word" is "inexorable and immutable; she never transgresses the laws imposed upon her, or cares a whit whether her abstruse reasons and methods of operation are understandable to men" (Finocchiaro 1989, 93–4). Thus philosophy, as Galileo stated in *Il Saggiatore (The Assayer,* 1623), "is written in this grand book, the universe, which stands continually open to our gaze…It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures" (Galilei 1623, pp. 237–8). This is the reason that the book of nature can only be read by experts: "But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed" (Galilei 1623, pp. 237–8).

"The book of nature is written in the language of mathematics" has become Galileo's most well known dictum throughout the ages. In Baroque terms it may mean that nature indeed "reveals herself" to all those ready to learn its language. It presupposes that nature is transparent to the competent readers of its language. However, such a message stands in deep contradiction to Galileo's attempts to induce nature to talk not only in the *Sidereus nuncius*, but also in the letters on sunspots and other texts that constituted his philosophy of nature. There, nature expresses itself through metaphor, analogy and other figures of speech which Galileo used with no smaller competence than he used the language of mathematics. It is through these two extreme, even contradictory means – convention (mathematical language) and expression (striking analogies) – that his discourse was actually constructed.

Galileo drew daring conclusions from the differences between the subject matter, language, and aims of the books of nature and scripture. In these differences he anchored his radical claim about the necessity to separate the authority of philosophers, acquainted with the mathematical language of nature, from that of theologians, trained to interpret the Bible. Moreover, in matters of natural philosophy, he asserted the priority of philosophers over theologians: Galileo petitioned the theologians whom he held to be of supreme authority of interpretation, to suspend their judgment about the motion or rest of the earth, for "it would be proper to ascertain the facts first, so that they could guide us in finding the true meaning of Scripture; this would be found to agree absolutely with demonstrated facts, even though prima facie the words would sound otherwise, since two truths can never contradict each other" ("Letter" in Finocchiaro 1989, 104). Quoting the witty words of a contemporary ecclesiastic, Galileo summed up by pointing out not only the different contents, language, and audiences of the two books, but also their different intentions: "The intention of the Holy Spirit is to teach us how one goes to heaven, not how heaven goes" (Finocchiaro 1989, 93).

A close reading of those Galilean texts that accompanied his telescopic discoveries shows that from the beginning no essential connection existed between his commitment to Copernicanism and his practices of celestial observations. Also, on the surface of these texts there was hardly any reference to the controversial issue of the motion of the earth. Indeed, in the *Sidereus nuncius* Galileo promised that: "…we shall prove the earth to be a wandering body surpassing the moon in splendor, and not the sink of all dull refuse of the universe; this we shall support by an infinitude of arguments drawn from nature" (Galilei 1610, 45). However, no ultimate proof that could fulfill the contemporary, apodictic canons was found during his lifetime. Nevertheless, in his writing Galileo started to draw the possibilities for the new rules that would guide the discourse on celestial phenomena. By verbally articulating the right analogies between the sun and its satellites (including the earth) and Jupiter surrounded by the Medicean stars, and by plastically representing what should be seen in spite of being invisible, namely the mountains on the moon and the light and motion of the earth; and furthermore by explaining away what was seen but should be neglected – like the perfectly even contours surrounding the moon – Galileo appropriated the realm of the visible from commonsense in favor of the professional observer. Thus, Galileo's philosophical astronomer became the ultimate authority on what was visible, and what could not be seen in the heavens. Simultaneously, Galileo framed his discourse by redrawing the boundaries between traditional mathematical astronomy, natural philosophy and theology and the claim that the book of nature is written in mathematical language. Therefore it was not transparent to those who had no proficiency in that language, meaning the theologians. Last, Galileo attempted to mobilize political power to support and defend himself, claiming that his discoveries were relevant for the visibility of the Prince's power. No doubt the new discourse challenged not only the interpretation of the Holy Scriptures and the theologian's authority. By monopolizing the field of visibility as well as the language of nature, and by tying up the authority of the philosophical astronomer to the power of princes he challenged the very quest for a scientific-theological synthesis in the name of the Church's best interests.

The Church Reacting to Galileo's Challenge

On the 24th of February 1616, a group of 11 theologians, consultants of the Holy Office, convened to discuss the Copernican theory. This theory – according to which the sun rests in the center of the world, and the earth moves around it, and also rotates daily – was found by the theologians to be philosophically foolish and absurd, and formally heretical – or at least erroneous in faith – since it contradicts the Holy Scriptures ("Consultants Report on Copernicanism" (24 February 1616) in Finocchiaro 1989, 146).

This decision of the Inquisition's consultants had two immediate consequences. First, the Inquisition passed the decision to the Congregation of the Index, which decided to suspend Copernicus' book until corrected, and to prohibit completely two other books that attempted to accommodate the interpretation of the Scriptures to the new astronomical theory. Second, according to the explicit direction of the Pope, the Inquisition also appointed one of its cardinals – the Jesuit Robert Bellarmine (1542–1621) – to deliver the theologians' decision to Galileo Galilei (then courtly mathematician and philosopher of Cosimo de Medici visiting Rome) and to admonish him. The meeting between Bellarmine and Galileo took place in the cardinal's palace, just 2 days after the theologians pronounced their verdict. Galileo was told of their decision and was admonished. However, in spite of the oath of secrecy that bounded anyone involved with the Inquisitions' acts, a wave of rumors about a presumed trial led Galileo to ask Bellarmine for a document that would testify to the "true" course of the events. And indeed, the Inquisition files do contain such a document – Bellarmine's certificate – signed by the cardinal, and witnessing to the admonition not to *defend* or *hold* the doctrine that the earth moves around the sun and the sun stands at the center of the world (Finocchiaro 1989, 153). But the certificate denied the occurrence of a trial.

Nevertheless, the trial that never happened did enter into most histories of the Galileo affair that have memorized it as “the first trial of Galileo”.

Although all of these facts are recorded in the Inquisition files and make up a seemingly coherent, comprehensible sequence, historians dealing with Galileo’s trials have been haunted by them for ages. The kinds of problems they raise bedevil any attempt to tell a true historical story on their basis. It seems that there is no way to know the intention of the Church in the condemnation of Copernicanism as the decision of the theologians was liable to at least three different interpretations, all of which were used by the Church itself in different circumstances. Also, there is no way to know what really took place in Bellarmine’s meeting with Galileo on the 26th of February, 1616, since the Inquisition documents testifying to this event contain internal contradictions.

The first difficulty arises from the tension that exists between the Theologians’ verdict of the Copernican theory as heretic, or at least erroneous in faith, and the decision of the Congregation of the Index to suspend Copernicus’ book until corrected.

How Can a Heretic Theory Be Corrected?

A literal, commonsensical reading of the condemnation leads one to believe that a condemned theory should not be discussed, represented, taught or used in any other way, and many historians believe that that was the intention of the Church. However, this was not the only interpretation possible for contemporaries. In a 1615 letter to Foscarini – the Carmelite theologian and mathematician – which explicitly mentions Galileo’s name as an implied addressee – Bellarmine developed the idea that using the Copernican theory as a mathematical hypothesis – namely as a means for the computation of celestial motions – had no harm in it. But Bellarmine also emphasized that philosophical assertion of the Copernican theses as a true, physical description of the universe was dangerous, since it contradicted the Holy Scriptures (“Cardinal Bellarmine to Foscarini” (12 April 1615), Finocchiaro 1989, 67–69). In 1620 the Congregation of the Index issued another decree that stated the legitimacy of using Copernicanism for practical purposes of computation, and suggested a few minor corrections of Copernicus’ book in order to secure its reading in this spirit (“Correction of Copernicus’ *On the Revolutions*” (15 May 1620), Finocchiaro 1989, 200–202). Finally, in a letter written to his friend and patron Federico Cesi, Galileo mentioned a conversation between Pope Urban VIII and one of his cardinals, in which the Pope himself interpreted the decree as allowing *to hold* the Copernican theory as a mathematical hypothesis (“Galileo to Cesi”, (8 June 1624), Favaro 1968, 182–3). However, even the interpretation of the Church policy in these terms – namely, allowing it as a mathematical hypothesis without claiming it as philosophically true – does not eliminate the ambivalence inherent in the condemnation. For the question still arises, whether treating Copernicanism as a mathematical hypothesis actually meant that it could, one day, be proved – namely that it had the status of a possible, though not yet proven theory – or whether the very possibility for such proof to exist was excluded by the Church. Thus, the true intention of the condemnation remains obscure for us, as it had been for contemporaries and for generations to come. But any attempt to give a credible, well-based historical account of the affair depends on the answer given to this question.

This is not the only example of a manipulative use of the acute gaps between a signifier and its signified during the affair. Even more puzzling is the evidence we have about the Galileo-Bellarmine encounter, still remembered as the first trial. We have three Inquisition documents testifying to the fact that Galileo was admonished not to hold or defend the Copernican theory. All three are connected with the name of Bellarmine, who was to carry out the admonition under the explicit direction of the Pope, confirmed by the Congregation of the Inquisition. They include Bellarmine's report to the Congregation that the admonition had been performed, and his testimony of the event left in the hands of Galileo – Bellarmine's certificate quoted above ("Inquisition Minutes" (25 February 1616), Finocchiaro 1989, 147; "Inquisition Minutes" (3 March 1616), 148; "Cardinal Bellarmine's Certificate" (26 May 1616), 153). However, the one document that directly relates what had really happened in the palace of the cardinal – not as reported by Bellarmine, but as written by the Inquisition officials – this document actually tells a different story than the one told by Bellarmine. For it turns out that according to this document, right after the admonition by Bellarmine, in the same place, and in presence of the Cardinal, another Inquisitorial procedure took place, this time carried out by the Dominican Commissary, in presence of a notary, and under the gaze of two witnesses. In this procedure, Galileo was warned not simply to avoid *holding* or *defending* the Copernican theory, but *not to teach it in any way whatever*, (my emphases) either orally or in writing ("Special Injunction" (26 February 1616), Finocchiaro 1989, 147). This document, however, was found in the files unsigned.

Some historians, sensitive to the gross contradictions between those documents attempted to eliminate them by assuming that the unsigned document was forged, apparently during the trial of 1633 – the only real trial of Galileo, as a result of which his *Dialogue Concerning the Two Chief World Systems* (1632) was condemned, and he was sentenced to home arrest for the rest of his life (De Santillana 1976). Such assumption of forgery, however, is arbitrary. It seems too harsh an intervention on the part of the historian, an attempt to mould history, or re-create it rather than represent what the documents actually tell us. Other historians aspire to reduce the contradiction by an act of interpretation that minimizes the differences between the procedure of Bellarmine, and the one that followed immediately afterwards (see, for example, Fantoli 1996). These differences, however, seem irreducible. They signal a limit that is also a point of a new departure.

An Allegorical Reading of the Inquisitorial Law

One may attempt to relate the condemnation of the Copernican books to the admonition of Galileo by seeing the last as an application of the first. But the attempt soon transpires as a vicious circle. Once the condemnation is interpreted as a strong prohibition to deal with Copernicanism in any way whatsoever, the bulk of evidence – three documents related with the name of Bellarmine, together with the external evidence of Bellarmine's letter from 1615 – becomes irrelevant. This strategy leaves the historian with one unsigned document to support the case. But if one chooses to believe that dealing with Copernicanism as a mathematical hypothesis was actually allowed to Galileo by the Inquisition – as Bellarmine's documents seem to imply – how would we interpret the main accusation of the trial of 1633? How should one understand the wording of the sentence, according to which "there is no way an opinion declared and defined contrary to divine Scripture may be probable"? ("Sentence" (22 June 1633), in Finocchiaro 1989, p. 289). Perhaps the context should be broadened. One may start a new project of reading about the Holy Scriptures and their interpretation during the Counter-Reformation. However, here too the hope of getting rid of the contradictions is soon frustrated. True, the Counter-Reformation Church became increasingly defensive about its authority to read and interpret the Scriptures, vis-à-vis the Protestant stubborn assault on this authority. It did consider its traditions holy, and did emphasize its monopoly in this regard. One of the main decrees of the Council of Trent (1545-1563) – the decree on the Scriptures – maintains that:

> no one, relying on his own judgment and distorting the Sacred Scriptures according to his own conception, shall dare to interpret them contrary to the sense which Holy Mother Church, to whom it belongs to judge of their true sense and meaning, has held and does hold, or even contrary to the unanimous agreement of the Fathers. ("Decrees of the Council of Trent Session IV" (8 April 1546), in Blackwell 1991, 183).

It is also true that preference for literal interpretation of the Scriptures can be discerned in that period among Catholics. Still, literal interpretation did not mean fundamentalism. Many of the outstanding biblical interpreters of the Counter Reformation still believed that "Scripture is clearly very broad by its very nature and is open to various readings and interpretations", as the Jesuit Benedictus Perera wrote in his *Commentariorum et disputationum in Genesim* (Rome 1591–5, quoted Blackwell 1991, 20). In the bulky volumes of his *Controversies*, Bellarmine himself testified to the general preference among both Protestants and Catholics for literal interpretation of the Scriptures: "…we and our adversaries agree that effective arguments ought to be sought in the literal meaning alone" (Blackwell 1991, 188). This preference, however, did not exclude interpretive use of the figurative dimensions inherent in the language of the Holy Text, since for Bellarmine literal meaning included full recognition of the polysemic potential of every text. Thus he wrote that: "there are two types of literal meanings: simple which consists of the proper meanings of words; and figurative, in which words are transferred from their natural signification to another. There are as many types of the latter as there are types of figures" (Blackwell 1991, 188).

The space of signification drawn by Bellarmine about 20–30 years before the Galileo affair well represents the conventions of his time. These conventions operated not only in the realm of exegesis, but also in the construction and interpretation of the natural world – as shown in my discussion of the *Sidereus nuncius*, as well as in the political space of the court: whether that of the Medici, the Pope or the Inquisition.

We have seen how Galileo maneuvered his presentation of Jupiter and its satellites to signify for his readers Cosimo and his virtues. In a similar way the legal documents related to the Galileo affair clearly testify to the encoded language used by the different actors to disclose, but also to mask, their positions concerning the status of the Copernican theory in relation to the Holy Scriptures. No one among them truly considered giving up the heliocentric hypothesis for all practical purposes. In fact, the transition from the Julian calendar to the Gregorian one initiated by Pope Gregory XIII was anchored in the Prutenic tables based on Copernican computations. The boundaries between the licit and the illicit, however, were left ambiguous. In the space of possibilities that stretched between absolute and possible truth, between the hypothetical and the probable, and between ***hold***, ***defend*** and ***teach*** the new cosmological picture, actors made full use of the gap opened up between signifiers and signified in an allegorical discourse recognized as legitimate in so many areas of life. Their judgments remained ambiguous, polyphonic, and suggestive of the richness of intellectual and religious approaches typical of Catholic tradition. In the context of Baroque culture such ambiguities, inherent in human systems of communication, developed into conventions that enabled people to suspend their judgments and live simultaneously on more than one level of meaning. Such practice was known as "honest dissimulation" – an expression coined by The Italian Renaissance writer Torquato Accetto in a book published in 1641 under the title *Della dissimulazione honesta* (Accetto 1641).[5]

Immediately at the opening to his text Accetto states his goal: to defend and teach a "cautious way of life" at the center of which he plants the concept of "prudence". Prudence was a major value for early modern humanists and Accetto explains it in terms of inclination towards truth, but a truth that is accommodated to particular time and place. Prudence is necessary, he argues, for dealing with the human affairs in which unexpected accidents and cunning dangers inhere. Such a world necessitates deep understanding and competence in using two strategies of communication which he calls "simulation" and "dissimulation". Simulation is the art of making the absent present. Galileo's strategies of representing the new celestial phenomena provide an excellent example. Dissimulation is the opposite, namely the art of hiding or masquerading that which is present. Bellarmine's terminology in speaking about the licit and illicit in dealing with Copernicanism is a good example of "honest dissimulation" – implying that Galileo may *teach* Copernicanism but not *hold* or *defend* it. It is essential to emphasize that Accetto keeps warning against identifying dissimulation with deception, hence his recurrent emphasis of "honesty" in the act of dissimulation. Honest dissimulation is anchored in the knowledge of finding the right moment in which to disclose or articulate a message and the right moment for keeping silent. Dissimulating is closely connected to a good sense of timing for which temperance and balance are major assets. Furthermore, Accetto imagines dissimulation as a special space existing outside the self in which the self is capable of "accommodating" his beliefs to the needs of the moment. With this he recognizes that dissimulation is a kind of splitting of the self in order to be able to live simultaneously on two seemingly incompatible levels of meaning: more than blind is the one, he writes, who believes that in order to enjoy earthly life – it is necessary to abandon heaven. And he continues:

> The lover of peace is the one who dissimulates honestly in his suffering, silence and expectation while he accommodates himself to the stream of occurrences and enjoys even things which he does not possess, while violent people are unable to enjoy even those things that they do have. (Accetto 1641, “L’autora chi legge”).[6]

The inner space for dissimulation is further imagined as a “grey area,” a tenebrous space separated from the light of truth by a veil but not identified with the darkness of falsehood. It is within this space that truth is suspended in order to be demonstrated and shine out at the right time. Accetto believes that the play of light and shadow is necessary for conducting prudent human affairs just as nature legislated the change between day and night. Both manifest and secretive administration of one’s affairs conforms with reason that is the law of life but also with contingent conditions that occur along the way, he claims.

The gaps, ambiguities, and tensions latent in the Inquisition files of the “Galileo affair” have left very broad margins for historians to invent their hypotheses and create their narratives. The most influential among those narratives has been a polarized, dichotomous one that treats the conflict between Galileo and the Church as inevitable (Feldhay 2001). This structure has served different purposes for different groups of people throughout the centuries. For nineteenth century Italian secularists it served as a myth that encouraged their quest for national unity, which they thought would promote their individual political freedom and guarantee their intellectual freedom. For some adherents of the Enlightenment concepts of reason and faith today, casting the story in terms of an inevitable conflict directly leads to a more general statement of the inherent antagonism between Catholic authoritarianism and modern science not only in the past but also in the present.

But there is another option. No doubt, the Galileo affair has been uncritically exploited for the construction of Western intellectuals’ identity story as a moral drama of our liberation from religion by means of science. In contradistinction, a critical return to the sources should allow us to reflect upon the Galileo affair in a more complex way, which may be closer to historical truth. I deem this way more relevant for the complex, multi-cultural, multi-religious reality of the twenty-first century, in which it is finally dawning upon us that science has not gained a victory over religion, since it cannot – and probably even should not – aspire to replace it.

After the long years of reading and re-reading those documents, and many others I could lay my hand on, it is my opinion that the end of the affair – Galileo’s trial, the sentence and the verdict – was not inevitable. I have come to see the history of the Catholic reform movement as a struggle between two or more alternative – sometimes contradictory – strategies of coping with the modern world of the seventeenth century in general, and with Galileo’s astronomical discourse in particular

(Feldhay 1995). Thus, the contradictory documents actually signal to me a true contradiction in the socio-political and cultural realities of the seventeenth century. One strategy – taken by the Jesuits, including Cardinal Bellarmine – was to concentrate on education and the missions. It required a tremendous effort to assimilate the new knowledge of the sixteenth century which they called physico-mathematics, without giving up the Aristotelian conceptual framework to which they were committed. To do that they used "honest dissimulation" – reading Galilean texts in terms of commentaries to Aristotle, for example. Their efforts resulted in an impressive transmission of modern physico-mathematical and mechanical knowledge to wide audiences through the system of colleges they built up in Europe and outside it. Another way was through the traditional structures of Catholicism, including the Inquisition. I interpret the gap between Bellarmine's admonition – not to *hold* or *defend* Copernicanism – and the Inquisition's injunction – *not to hold, teach, or defend it in any way whatever, either orally or in writing* – as an aspect of the struggle over the cultural policy of the Church between traditional modernists and conservatives. The Jesuits' interest was to attempt a cautious and gradual modernization. Indeed they were extremely sensitive to the options made available by the new techniques of representation and allegory in their science classes and their theatres. When necessary they stretched such techniques in the direction of "dissimulazione honesta." This form of expression testifies to their difficulties not only with more traditional circles within the Church, but also within the society itself that experienced harsh debates over the status of physico-mathematics. With the publication of the *Dialogue* in 1632, it became clear that Galileo himself exploited allegorical strategies declaring his discourse "hypothetical" and at the same time not hiding his Copernican sympathies. However, just as Accetto warned his readers, what had seemed prudent in 1616 became a disaster in 1633. The Jesuits, who did not refrain from inquisitorial means when they served their purposes, obviously cooperated in the attempt, in 1633, to re-interpret the previous events of 1616 and incriminate Galileo. In those changing circumstances his figure transformed from a most popular philosophical mathematician and courtier to a Baroque hero of a mourning play.

Notes

1. This is my metaphor, but I think it fits Marin's spirit.
2. This part of the paper is based on a paper written with Raz Chen for a volume entitled *Before Copernicus* (forthcoming).
3. "Uolumus autem ipsum vt principium indivisibile videre." Translated in Nicholas of Cusa, "De Beryllo (On [Intellectual] Eyeglasses)"; in *Nicholas of Cusa* (1998), p. 771.
4. "Uolumus autem ipsum vt principium indivisibile videre." Translated in Nicholas of Cusa, "De Beryllo (On [Intellectual] Eyeglasses)"; in *Nicholas of Cusa* (1998), p. 710.
5. The book can be found online: http://it.wikisource.org/wiki/Della_dissimulazione_onesta
6. http://it.wikisource.org/wiki/Della_dissimulazione_onesta/L%27autor_a_chi_legge

References

Accetto, Torquato. 1641. *Della dissimulazione onesta*. a cura di Salvatore Silvano Nigro, Collana Biblioteca Einaudi n. 4 (Torino, 1997). http://it.wikisource.org/wiki/Della_dissimulazione_onesta. Accessed 2 Oct 2011.

Alberti, Leon Battista. 1972. *On painting and on sculpture: The latin texts of De picture and De Statua*, ed., trans. and intro. C. Grayson. London: Phaidon.

Aristotle. 1984. *De partibus animalium* (On the parts of animals) (trans: Ogle, W.). In *The complete works of Aristotle*, ed. Jonathan Barnes, Princeton NJ: Princeton University Press vol. 1, 994–1086.

Benjamin, Walter. 1977. *The origin of German tragic drama* (trans: Osborne, John). London: Verso.

Biagioli, Mario. 1993. *Galileo, courtier: The practice of science in the culture of absolutism*. Chicago: University of Chicago Press.

Blackwell, R.J. 1991. *Galileo, Bellarmine, and the Bible*. Notre Dame: University of Notre Dame Press.

Bredekamp, Horst. 2007. *Galilei der Künstler: der Mond, die Sonne, die Hand*. Berlin: Akademie Verlag.

De Santillana, Giorgio. 1976 (reprint). *The crime of Galileo*. Chicago: University of Chicago Press.

Fantoli, Annibale. 1996 (2nd English ed.). *Galileo: For Copernicanism and for the church* (trans: G.V. Coyne, S.J.). Vatican: Vatican Observatory Publications.

Favaro, Antonio (ed.). 1968 (reprint). *Le opere di Galileo Galilei*. 20 vols. Firenze: G. Barbèra.

Feldhay, Rivka. 1995. *Galileo and the church: Political inquisition or critical dialogue?* Cambridge: Cambridge University Press.

Feldhay, Rivka. 2001. Recent narratives of Galileo and the church or: The three dogmas of the counter-reformation. In *Galileo in context*, ed. Jürgen Renn, 219–239. Cambridge: Cambridge University Press.

Finocchiaro, Maurice A. (ed. and trans.). 1989. *The Galileo affair: A documentary history*. Berkeley : University of California.

Fletcher, Angus. 1964. *The theory of a symbolic mode*. Ithaca: Cornell University Press.

Galilei, Galileo. 1610. *The starry messenger* (trans: Drake, Stillman). In *Discoveries and opinions of Galileo*, ed. and trans. Stillman Drake, 1957, 21–58. Garden City: Doubleday Anchor Books.

Galilei, Galileo. 1613. *Letters on sunspots* (trans: Drake, Stillman). In *Discoveries and opinions of Galileo*, ed. and trans. Stillman Drake, 1957, 87–144. Garden City: Doubleday Anchor Books.

Galilei, Galileo. 1623. *Letters on sunspots* (trans: Drake, Stillman). In *Discoveries and opinions of Galileo*, ed. and trans. Stillman Drake, 1957, 229–280. Garden City: Doubleday Anchor Books.

Marin, Louis. 1988. *Portrait of the king* (trans: Houle, Martha M.). Houndmills: Macmillan Press.

Nägele, R. 1991. *Theater, theory, speculation: Walter Benjamin and the scenes of modernity*. Baltimore: Johns Hopkins University Press.

Nicholas of Cusa. 1962. *Nicolai Cusae Cardinalis Opera* (Paris, 1514); Unveränderter Nachdruck, Frankfurt/Main: Minerva G.m.b.H.

Nicholas of Cusa. 1998. *Nicholas of Cusa: Metaphysical speculation*: *Six latin texts* (Translated into English by Jasper Hopkins). Minneapolis: The Arthur J. Banning Press.

Reeves, Eileen. 1997. *Painting the heavens: Art and science in the age of Galileo*. Princeton: Princeton University Press.

Valleriani, Matteo. 2010. *Galileo engineer*. Dordrecht: Springer.

Name Index

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1,

Subject Index

O. Gal and R. Chen-Morris (eds.), *Science in the Age of Baroque*, International Archives of the History of Ideas 208, DOI 10.1007/978-94-007-4807-1,