EDITED BY
MARIANNE KLEMUN
AND ULRIKE SPRING

EXPEDITIONS AS EXPERIMENTS

Practising Observation and Documentation

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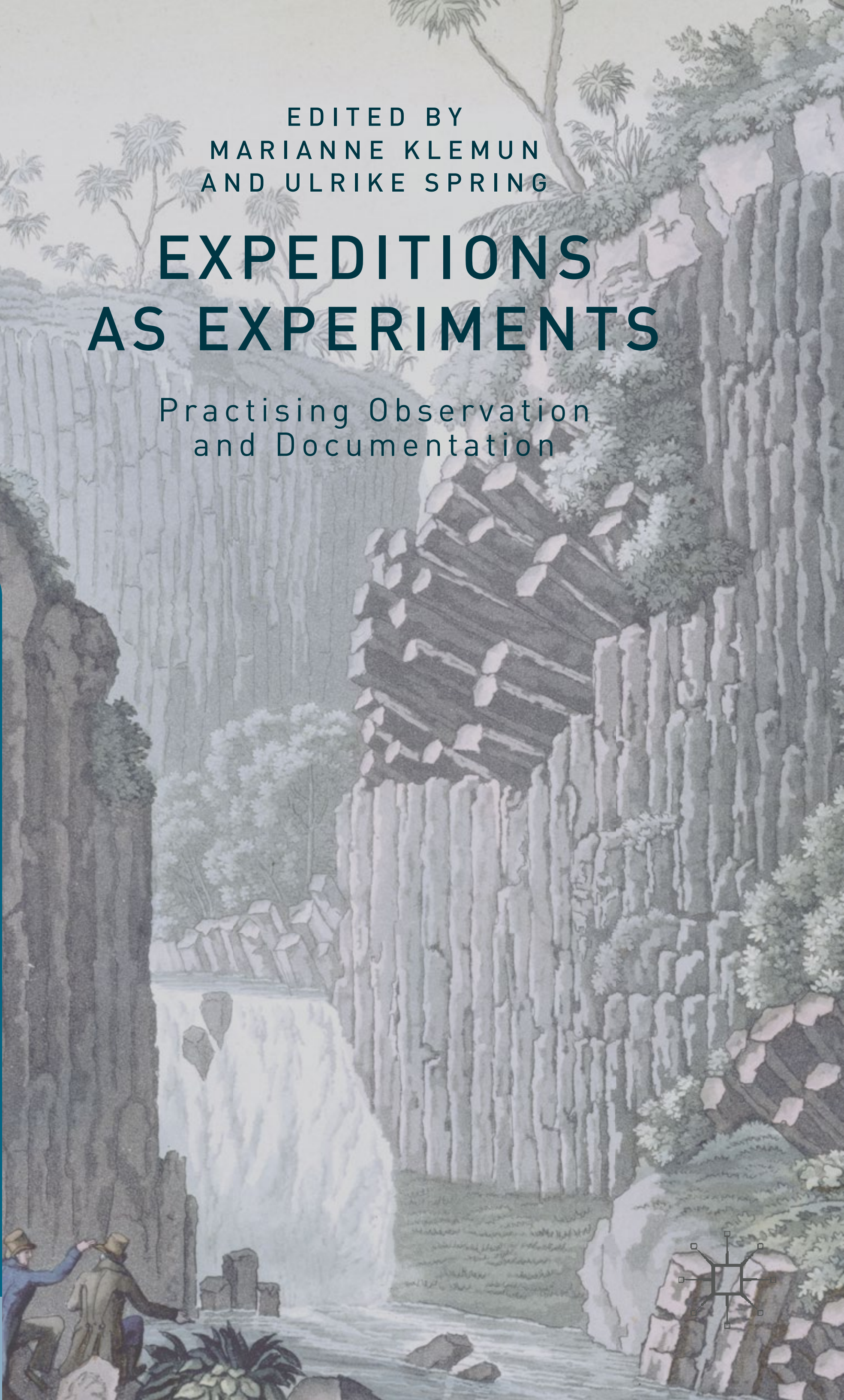

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Editors

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CHAPTER 1

Expeditions as Experiments: An Introduction

Marianne Klemun and Ulrike Spring

Introduction

For centuries exploratory expeditions have played a dominant role in acquiring knowledge about the world. With the rise of modern science they became a central feature of scientific knowledge gathering and investigation in Western culture. In the nineteenth century the number of scientific expeditions, or, to be more precise, expeditions with a focus on scientific objectives, increased rapidly as a result of growing professionalization and specialization in sciences and the onset of the so-called new imperialism aiming to bring "civilization" to the non-Western world.[1] As Roy MacLeod points out, expeditions and science became inextricably interlinked in the course of the nineteenth century.[2] The belief in scientific analysis, in its global applicability and its capacity to provide knowledge about the world, was a necessary precondition for the growing interest in expeditions; competitive imperialism between states based on growing

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national movements and increasing international competition,[3] provided an essential context. Today as well, scientific expeditions are an indispensable means of gathering knowledge.

There is an abundance of literature on expeditions, ranging from short descriptions to academic tomes. While earlier research focused on the expeditions themselves and incorporated them into national heroic narratives, in recent years, researchers have paid more attention to what happened before and after the actual expeditions, and contextualized them in a global context. The attention focused on the heroic explorer has increasingly been replaced by an interest in the communication processes among the participants, with the local people met during the expedition as well as with the scientific community. If the focus on the explorer prevails, then it tends to be from a critical perspective on his or (more rarely) her role in nation-building and on gender, class, culture and knowledge production.[4] In this collection, we take a fresh look at the events during the expeditions presented in this volume and the practices they employed to produce scientific knowledge. It continues a tradition strongly influenced by David Philip Miller's and Peter Hanns Reill's by now classic *Visions of Empire*, where scientific practices such as experiment and observation are seen as complex processes situated within a field of power, knowledge and cultural rules.[5] We go one step further, however, and regard the genre of the expedition itself as part of this process of knowledge production: experiments are not just practices executed during expeditions, the act of exploration itself functioned as such an experiment. Moreover, the expedition is a specific scientific practice in itself. As several chapters show, it is also a cultural practice and is embedded within its specific cultural, political and social contexts.

Defining Scientific Expeditions

Expeditions may be called enterprises governed by metropolitan "centres of calculation".[6] Often they are associated with heroic figures like James Cook[7] or Alexander von Humboldt, whose expeditions constituted both a "habitus"[8] and a model for future enterprises. These famous expeditions still determine, to a great extent, the definition of scientific expeditions and the practices involved in the process of gathering knowledge on these journeys. While we by no means want to dismiss the relevance and exemplary function of these famous expeditions, it is nevertheless vital to consider smaller-scale or less well-known explorations in their own right. They may have modelled themselves on their great predecessors and followed instructions laid out beforehand, yet, each expedition faced different challenges and contexts and had to adapt its practices of observing and collecting accordingly. A focus on expeditions allows us to investigate a variety of knowledge acquisition processes. Moreover, with such a focus we gain more detailed insights into different forms of scientific practices, such as observation, and can investigate to what extent the act of observation is both a result of specific contexts in the field as well as of socialization processes and instructions prepared at the home base.

Before the twentieth century, many scientific expeditions were carried out by members of Western cultures. Not surprisingly, research on expeditions often focuses on the impact these ventures had on the world order and on the territories explored, for instance in terms of imperial strategies[9] or colonial allocation.[10] And without any doubt, expeditions cannot be analysed outside this context. Yet the local knowledge encountered on these expeditions also influenced the Western travellers themselves and had an impact on their practices and understanding of the foreign world.[11] It is also worth remembering that the encounter between expedition members and local populations did not only have exploitative features.[12] Thus in this volume we pay particular attention to the practices that were chosen and applied, as well as to the way knowledge was produced in dialogue with the visited destination, the discovered or observed objects, and other expedition participants and/or local populations during the expeditions. Nor can the procedures during an expedition be seen separately from its repercussions and eventual legacy. We need to understand the interplay between practices and scientific discourses during the expeditions themselves, and more closely investigate the practices and discourses these expeditions both created and were part of in order to comprehend knowledge production and acquisition.

Exploratory expeditions constitute a form of scientific work that has become widespread since the eighteenth century. They had their heyday as spectacular and heroic endeavours during the nineteenth and early twentieth centuries. They are, as Martin Thomas points out, “cultural formations, as distinctive to their epoch as the novel or the photograph”.[13] Yet these journeys were diverse in form and content, and definitions of the notion “expedition” have varied accordingly.

We have identified four dominant thematic clusters in this collection which illustrate this heterogeneity: the expedition’s motivations and aims, its structure, the division of work, and its epistemological context, for instance by embarking into the unknown. First, expeditions could pursue different aims, with some placing the focus on geographical exploration[14] or settlement,[15] others concentrating on scientific observation[16] or on economic goals with science playing a minor role. Although in this volume we look at the acquisition of scientific knowledge in particular, a clear-cut distinction from other motives is not always possible or useful. Scientific expeditions usually had a strict plan of investigation before they set out, including a set of specific goals that had to be observed during the course of the expedition and a clear idea of the scientific fields they wished to contribute to. The Latin *expeditio* originates from a military and administrative context and, although the term "expedition" became common for scientific endeavours and thus deviated from its original meaning, the character of an expedition as the *execution* or *completion* of a planned enterprise has survived as one aspect of its multi-layered meaning. The chapters of this volume show the variety of scientific interests that motivated expeditions with their focus on natural history, geology (Teresa Salomé Mota), ichthyology (Yuko Takigawa, Kurt Schmutzer), botany (Alexandra Cook, Tanja Hammel, Jan Vandersmissen), zoology (Jan Vandersmissen), helminthology (Kurt Schmutzer), speleology (Johannes Mattes), physical anthropology (Katarina Matiasek), oceanography (Peder Roberts) and geophysics (Ulrike Spring). However, due to the multi-disciplinary character of scientific fields, deviations from these research plans did occur, and were in fact an integral part of scientific research on these expeditions.

Second, expeditions were structured in various ways: they could be of long or short duration, and they could be carried out by a few people or a larger group. The expeditions investigated in this collection range from journeys of solitary travellers, who embarked on short-term expeditions to well-known areas, to the great expeditions lasting several years which aimed to explore hitherto unknown regions of the world or unknown places of nature. This variety of expeditions allows us to tentatively examine to what extent we may speak of common or even standardized scientific practices. It also gives us insights into different contexts in which knowledge was established. But let us reflect on the question of the duration of an expedition. At first sight, one might define the day of departure as an expedition's beginning and, accordingly, the day of return as its ending. However, scientific expeditions were effectively completed only when the knowledge gathered had been subject to scrutiny by other scholars back home.[17] The scientific community (in its broadest meaning, including the interested public) had to acknowledge the material as "scientific" and the scholars of the expedition as trustworthy and capable of proper scientific observation and documentation. Similarly, expeditions started long before their actual departure day, and if one were to set a date, it would be the day the idea of the expedition was conceived and articulated for the first time. Peder Roberts reminds us in his chapter that each expedition was not only a singular event but also a process: expeditions built upon previous enterprises and pointed towards future ventures, and at the same time each of them was subject to its own rules depending on circumstances. Scientific expeditions were, in other words, part of specific social, cultural and political contexts, and answered to certain expectations in the way they were structured and performed.

Third, expeditions were based on a division of responsibility. As the examples in this book show, many participants had multiple roles, with doctors working as natural scientists, military officers heading scientific investigation, and self-taught men and women setting off with the help of local guides. Sometimes such roles could be in conflict with each other, as exploration and science do not necessarily complement each other: scientists taking on the role of explorers could fail to implement the necessary practicalities of an expedition, such as preparation for and organization of daily life.[18] Individuals' personalities and their relation to the scientific community have influenced not only the outcome of expeditions but also how the acquired knowledge was viewed back home and in the scientifically interested communities.[19] Martin Thomas argues for differentiating between explorer and expedition: while the former is an individual, the latter is a collective.[20] As Vandersmissen, Schmutzer, Spring and Hammel show in their chapters, some scientist-explorers faced challenges before or after their expeditions because of their social status, their lack of scientific education or their gender. For them, the expeditions were also social experiments where they could test and challenge the boundaries and validity of these social restrictions.

Any form of travel involves processes of displacement, change and entanglement, and this is all the more true for expeditions. The knowledge gathered on these travels is inevitably influenced by context and circumstances, and thus has a dynamic relationship with its surroundings. Most expeditions drew on local knowledge and, in doing so, they combined local and global (or, more commonly in the eighteenth and nineteenth centuries, Western) knowledge. Thus, they allow for an investigation of the complex relationship between metropolis and peripheries, between different colonial aspirations and ascriptions as well as between different regimes of knowledge.[21]

Fourth, expeditions are located within different epistemological contexts of the known (and controlled) versus the unknown (and risky and open-ended). A common feature is that they are ventures based on calculated risk. It is a key objective of every expedition to minimize the risks from the very outset. And yet, at the same time, expeditions explicitly aim at the unknown—in terms of unknown geographical areas, new species or new phenomena. Whereas the course or the outcome of an expedition remains uncertain until its very end, the explorers try to minimize the risks involved by reading of previous expedition reports, by careful preparation and instruction. Expeditions are thus experiments with an uncertain outcome, but where all efforts are made to determine the variables. This is a common feature of the expeditions discussed in this collection, although the degree of risk taken and measures employed to control them varied.

We could name many more than these four clusters to illustrate the difficulty of defining "expeditions". While expeditions constitute a genre, we are aware of the genre's complexity. Hence it might be difficult to decide whether a journey should be seen as an expedition, a voyage of exploration, an extended field trip or even an excursion. While most of the chapters in this book discuss expeditions in their most common meaning as an exploration endeavour, some investigate expeditionary journeys that border on shorter field trips. We have nonetheless decided to include these borderline cases as they illuminate whether the form and duration of expeditions had an impact on the process of knowledge acquisition and on the attribution and definition of authority. Moreover, they enable us to comprehend the often hybrid nature of travel and expedition, and to take into account travels by people who usually could not embark on large expeditions for financial reasons, or gender, or lack of opportunity. In this sense, Tanja Hammel shows that women had to adjust scientific practices to their needs, for instance by combining private journeys and expeditions. Johannes Mattes points out that cave explorers used the term "expeditions" in order to legitimize their exploration and to increase its scientific significance. Alexandra Cook, on the other hand, demonstrates the expeditionary element inherent in Jean-Jacques Rousseau's excursions and prompts us to rethink any clear-cut distinction between expedition and field trip.

As a means to control this heterogeneity, we have decided to use the term "scientific expedition" as an umbrella term, broadly defining a culturally and historically specific mission carried out by a group of people with specific work tasks, and with the aim of reducing the unknown and of systematically acquiring, collecting and documenting knowledge. David Philip Miller has already pointed out in 1996 that “basic descriptors of scientific activity—for example, experiment or discovery or observation—become […] complex processes of interpretation based in particular cultural practices”.[22] As the contributions in this volume show, the production of scientific knowledge cannot be separated from its environment. It is closely (and often explicitly) entangled with various different factors—economic (Yuko Takigawa, Jan Vandersmissen), political, colonial and military (Teresa Salomé Mota, Johannes Mattes, Katarina Matiasek), aesthetic or idealistic (Alexandra Cook, Tanja Hammel, Ulrike Spring), recreational (Johannes Mattes) or national interests (Teresa Salomé Mota, Kurt Schmutzer, Peder Roberts).

Expeditions as Experiments

We propose that to understand expeditions as experiments in the sense of a heuristic tool offers several benefits: experiments provide a platform to try out new constellations of gaining knowledge, new practices, new forms of organization, identification and objectives. In addition, they are also centres of negotiation, they transgress the dichotomy of centre and periphery, and offer a space for cooperation.

We discuss the complex relationship between experiment and expedition by considering four main aspects. First, one possible connotation of expeditions as experiments, in general linguistic terms, refers to expeditions as *daring ventures.* In fact this popular meaning always resonates in narratives on the history of expeditions. It is also found in self-portrayals of the explorers. It implies that all participants undertake risks, make themselves vulnerable and gamble on the outcome. This may be seen as two sides of the same coin: the reward of the venture was its profit, and that profit was the proof of its productivity. The fact that explorers could count on, and insist on the exclusivity of their enterprise, as well as on the knowledge it gained, caused certain expectations and a particular form of self-perception. The profit gained by mastering the adventure manifested itself in the form of a new profile of such travellers.[23] This self-determination was always variable and depended on socio-cultural conditions in the home country. In the symbolic space of the expedition—given the heuristic tool of expeditions as experiments—neutralization (normalization and standardization)[24] was the most characteristic feature in terms of knowledge. Here the former social background of the participants often lost its significance and individuals could reinvent themselves as natural scientists in general, freed from their previous social status. If we understand expeditions as an experimental and multi-faceted space, not only in the sense of a physically located "place of knowledge"[25] but also overlapped by a social and symbolic meaning of space, then this meaning creates its productivity: individuals discover and create their own professional identity within this metaphorical constellation of space. In this sense, Ulrike Spring shows the emerging self-image of expedition members based on the exclusive observation of the rare aurora borealis in the Arctic. Similarly, Jan Vandersmissen and Tanja Hammel demonstrate how travelling naturalists could gain a special profile. Even those working at the fringe of the scientific community were able to make a contribution to the pool of knowledge.

Second, the term "experiment" has a history of its own beyond its timeless abstract and theoretical definition. It is well known that chemistry was based on experiments from its early beginning, but how did experiments shape the negotiation of gaining knowledge in other fields, such as botany, anthropology or geology? The understanding of the term "experiment" differs, depending on the historical context. As Alexandra Cook shows in her case study on Rousseau's "mobile botanical laboratory" from the mid-eighteenth century, contemporaries had a broad understanding of the concept of experimenting in the sense of testing an object or a substance. At this point the expedition became the laboratory in which knowledge—rather than items of nature—was examined against findings published in reports or books.

A third aspect of the relation between expedition and experiment refers to the fact that experiments, unlike field research, are socially exclusive, since only certain individuals have access to the laboratory as a physical space.[26] The same is true for expeditions. As a participant with scientific tasks, and even as a collector, it is necessary to possess certain practical skills, experience in fieldwork, familiarity with expert knowledge on the geographical destination, and also to be part of a network of naturalists.

While the laboratory is characterized to a great extent by social homogeneity, the expedition enterprise generated a more universal community of travelling natural researchers. The objects collected and the phenomena observed on expeditions also created a connection between the participating natural scientists after the event, when objects and experiences were integrated into the already existing collections of material and into further debates.

As a fourth step, we have to discuss the academic aspect of experiments. For us it was intriguing that the term "experiment" refers to a knowledge ideal of modern science. Instead of the ancient practice of contemplation and description, the experiment implies, first and foremost, an active and manipulative intervention in nature. In this sense, the rise of the experiment reversed the Aristotelian separation of nature and technology.[27] Soon modern science was exclusively defined in technological terms. From the seventeenth century at the latest, experiments were thought to be groundbreaking for scientific research by historians of natural philosophy and natural science, although many of them overestimated their importance and ignored taxonomic approaches.[28] Both contemporaries and historians saw laboratories, where experiments were carried out, as ideal locations for the generation of knowledge.[29] Historians of science such as Simon Schaffer and Stephen Shapin have examined this idealization and demonstrated that experiments were dependent on the social acceptance of the performers as *gentlemen*.[30] And yet, in many contexts, experiments retained the timeless aspect of a paradigmatic scientific practice.

Thus, in this volume we want to revalue the significance of expeditions in the cultural zone of knowledge acquisition during a period of fundamental transformation of science, knowledge and society. If expeditions are understood as experiments, they are also a variation of the laboratory, where different practices can be carried out, where the transformation from uncertain knowledge to verified knowledge can be tested, and where different discourses on knowledge are juxtaposed. Laboratories have cultural, social and epistemic dimensions. The historian of science Hans-Jörg Rheinberger defines a laboratory as an "experimental system": "a basic unit of experimental activity combining local, technical, instrumental, institutional, social, and epistemic aspects".[31]

An expedition seen as an "experimental system" brings together an ensemble of techniques, strategies, material circumstances and social actors, and enables us to understand the steps involved in the transformation process from observation to data documentation. It has often been argued that expeditions created a new space of science: new forms of scientific practices and cultural appropriations were established and constituted, an intrinsic link between observation and theory was asserted and events that used to be seen as natural phenomena were considered scientific incidents. The various case studies in this volume investigate and analyse the contributions of expeditions to this process.

We also want to pay attention to the relation between expeditions and field research. To a certain extent, expeditions were contained spaces where knowledge could be collected and analysed and theories could be verified directly in the field. In contrast to the laboratory, field research does not take place in an environment designed by human beings or in a specially equipped building.[32] While laboratories are "placeless places",[33] field research is determined by the peculiarity of a particular place and thus operates not only *in* but also *at* one place. This distinction was formulated at the end of the nineteenth century, describing an ideal situation for which the historian of science Robert E. Kohler asserted different "modes of knowledge production".[34] While in the experiment causes and effects have to be kept apart, the field researcher describes, compares, names and classifies nature in all its complexity. Kohler explains these differences by referring to cultural traditions, and argues that these boundaries, which have been negotiated and redefined since approximately 1890, are fluid.

So let us go back in time again. Several methods established in the laboratory were introduced into field research as instruments or tools. Thus, laboratory standards based on his experiences with chemistry influenced Rousseau's botanical field research, as Alexandra Cook points out in her chapter. Certain methods of collecting and preparing determined the subsequent debate on species, as Kurt Schmutzer shows in detail in his case study. Furthermore, it is important to investigate the function of expeditions according to whether we see them as experiments or highlight the field as their characteristic feature. Or we can refer to both aspects, as Ulrike Spring suggests: as special places of observation such as the Arctic, which served as laboratory as well as the field for studying the rare aurora. The contributions in this volume underpin the significance of place for scientific practices by focusing on different environments, such as Mediterranean as well as tropic and polar areas.

However, expeditions were also spaces of knowledge where technology in general[35] and instruments in particular[36] played a very special role. This is another—and the fifth—reason why we centre on expeditions as experiments rather than on their character as field research. The key features of experiments are interventions. The choice of special circumstances, conditions and instruments before and during the expedition is of pivotal significance. In this sense, the process of choosing a phenomenon for observation might be seen as influenced or even controlled by certain expectations about the outcome of an experiment or an expedition alike. Choosing and controlling a phenomenon as an object of an experiment is as pivotal as choosing a certain procedure for an expedition, which is already anticipated in the instructions or determined, for instance, by a certain preservation method which affects the results, as Kurt Schmutzer shows in his chapter.

The chapters in this volume stress various aspects of these different relationships between expeditions and experiments. We have arranged them roughly in chronological order to make explicit the changing circumstances under which expeditions were conducted but also to highlight the similarity of the challenges the explorers or travellers had to face across time, from the eighteenth to the twentieth century.

Alexandra Cook's contribution discusses Jean-Jacques Rousseau's sustained engagement with the natural sciences. Looking at his botanical expeditions, she focuses on the methodological inspiration from laboratory experimentation that resulted from his detailed knowledge of chemistry. According to Cook, Rousseau, who claimed that fields adorned with flowers should provide the botanist's "only laboratory", was undoubtedly influenced by the idea of the conventional chemistry laboratory. Cook's analysis is based on a broad eighteenth-century understanding of "*laboratoire*" as the site of the work and "*expérience/experiment*" as a "test", which differs from the strictly modern sense of the term "*expérience*". This meaning, a core aspect of the eighteenth-century understanding of the term "experiment", included activities such as testing procedures. This illuminates Rousseau's understanding of a mobile botanical laboratory, or laboratory in the field, in which elements of experimental methods were crucial as practice. But what was Rousseau testing during his endeavours? His approach included, among other things, techniques of verifying what others had seen or not seen. In so doing, he combined the results of recent accounts and reports with his findings in the field. Causes and effects that would generally have been kept apart in the experimental system constitute, in Rousseau's case, the key to the success of an expedition, in choosing the best guide and the best weather, the most suitable instruments and location.

In his chapter, Jan Vandersmissen examines Jean-André Peyssonnel's work on the classification of corals in the natural order, based on discoveries made during his expeditions in the early eighteenth century. Peyssonnel's innovative work on corals was the result of extensive travels, on the one hand, and of experiments with corals taken from the sea and carried out ashore on the other. These two aspects—expedition and experiment—do not interrelate directly with one another but each complements the other in an ideal way. Without experiments the expeditions would not have resulted in the revised classification of corals as animals. At the same time, however, Vandersmissen illuminates that the scientific community of the early eighteenth century considered knowledge gained at sea as limited, due to the widespread scepticism towards any generalization of discoveries made during travels. The relationship between centre and periphery was involved in these processes, with the long-unrecognized natural scientist Peyssonnel making a great contribution to the latter.

Vandersmissen shows in detail how Peyssonnel made fruitful use of traditional practices and tools of fishermen, and defined the maritime space as a place of knowledge for himself and his scientific questions. In so doing, Peyssonnel's maritime ventures evolved into an experimental environment to test traditional methods of fishermen. In this context, the fishermen's practical or implicit knowledge gained new value as scientific practice. As a result, Peyssonnel was able to improve his profile as scientific investigator by circulating transcripts of his results. Vandersmissen's chapter shows the reciprocal relationship between theory and practice, between expedition and experiment, as the foundation of research. An ensemble of strategies and techniques during Peyssonnel's maritime expeditions enabled him to classify corals as part of the animal kingdom.

Yuko Takigawa, in her contribution on the Russian Krusenstern expedition (1803–1806), concentrates on a temporal and spatial segment of this venture that was planned as a global circumnavigation. Her main interest is the restricted contact of the explorers with Japanese locals in Nagasaki, after the ship had anchored off the coast of the peninsula and remained there for six months. The visit had pivotal consequences for Japanese ichthyology. Although no member of the expedition was allowed ashore, apart from visiting a designated dwelling house, and fieldwork was thus impossible, certain contacts and exchanges between the natural scientists of the expedition and some Japanese locals did take place, and these were of scientific value. Only a few people had access to the Europeans' residence, which also meant that any contact was socially exclusive, just as in a location devoted to an experiment, in a *placeless place*. Takigawa underlines this comparison by pointing to the most important independent variable in the experimental place of the contact-zone between the Europeans and the Japanese: the shipwrecked Japanese sailors who had previously been brought back to Japan by the expedition and who had to reside with the Europeans. Also, the Japanese government authorized translators who, in accordance with diplomatic protocol, had access to the Europeans. But what was crucial was the food supply for the ship's crew provided by the Japanese. Fish played a central role, since it was highly esteemed in Japanese cuisine. For the European naturalists, on the other hand, the different fish specimens were welcome objects for investigation and collection. Therefore, fish turned into a dependent variable in this experimental location. Through these objects an extensive network of exchange-relationship emerged. While the Europeans valued and exploited the Japanese local knowledge of edible fish, which they turned into scientific objects, the Japanese admired the Europeans' skills of drawing and preparation, which were quite new to them. Takigawa meticulously traces how the two systems of knowledge—the local Japanese one and the Western systematic scientific one—merged to create new European knowledge of Japanese fish species.

Kurt Schmutzer's chapter, a case study of the debate about the lungfish, analyses the specific scientific outcome of the Austrian Brazil expedition (1817–1835) headed by Johann Natterer. By discussing the impact of different interests, instructions, the framework and individual practices during the journey in the making of an *epistemic thing*, Schmutzer highlights how the discovery of these specimens influenced debates after their arrival in Europe. Due to the great interest in helminths (and not in lungfish only) in Vienna, Natterer had prepared the fish without internal organs. Schmutzer uses the notion of *expeditions as experiments*—paraphrasing Rheinberger's suggestion of experimental systems—by identifying an experimental system within the arrangements for the processes of collecting and preservation in the course of the expedition. The experimental system, in the case of Natterer's expedition, was determined by the special frameworks, instructions and ideas of the Imperial Natural History Museum in Vienna for whom Natterer was collecting: a selection process on the one hand, and, on the other, the production of specific specimens, using specific preparation practices. Thus, these specimens, as epistemic things, prepared during the expedition within an experimental system, were products of an intervention in nature regulated by the controlled procedures of collecting. However, these interventions in the nature of the specimens by preparation (that is, the removal of organs) hindered the understanding of the lungfish as a species between fish and reptile.

Tanja Hammel employs our heuristic tool of looking at expeditions as experiments in a different way, compared to the contributions mentioned above. She considers the genre of travel accounts as a sort of laboratory, allowing a natural science researcher to withdraw from the scientific community and to find her own way. By referring to the biography of Mary Elizabeth Barber, an English settler in South Africa, and her journeys in the 1870s from Kimberley to Cape Town, Hammel analyses the travel account of a scientific outsider, who turned away from the science of botany and systematic approaches and initially sought to verify Darwin's theory. By addressing racism and transcendental philosophy, Barber succeeded, in this open genre of travel accounts, in reconciling issues of natural selection and of faith. Reflecting on the alleged hierarchy of African tribes according to linguistic competence, Barber developed her own attitude to African society. The autochthonous population, their animism and the relationship of humans to their environment became increasingly a topic of great significance for her. In this she was influenced both by a colonialist understanding and by the concept of animal rights and the humane treatment of working animals, and conducted a study on the attitudes of peoples of different ethnicities in this respect. Her experience with the autochthonous population was an important variable for developing new interests.

In her chapter, Ulrike Spring discusses efforts in the 1870s and 1880s to transform the moving phenomenon of the aurora borealis into a fixed scientific object to be studied. Attempts to reproduce the aurora in a laboratory had so far been futile, and as an aerial phenomenon it could not be collected and brought back to Europe. Hence the Arctic became laboratory and field at the same time. Taking as her starting point the Austro-Hungarian Arctic Expedition (1872–1874), Spring examines the various standardization strategies the participants of the expedition deployed under their leader Carl Weyprecht during and after the expedition in order to make the aurora better understood scientifically. However, as she argues, these attempts at observing and documenting were constantly undermined by the aurora's fleeting and ever-changing character as well as by its spectacular nature, testing the borders between science and art or science and religion as well as of science itself. The expedition itself reflected this ambiguity, having been carried by ice into unknown waters, leaving the outcome of the journey open until its safe return to Norway. One might thus call the expedition an experiment, with its open-endedness, the necessary adaptations to the fiendish environment, and its continuous efforts to retain control over the situation and secure the outcome of the journey. While social hierarchy and division of labour determined not only the expedition's daily life, but also the way in which the results of the observation processes of the aurora were perceived, the Arctic also functioned as a place where social differences could be suspended and potentially everybody could acquire scientific skills. In this way, the expedition was also a social experiment.

Johannes Mattes focuses on cave studies, a field for which expeditions are essential. He applies the term 'expedition' to travelling in caves and problematizes thus the boundary between speleology and cave exploration as a purely tourist phenomenon. With examples from German-speaking Austria-Hungary and later Austria, Mattes illustrates the proliferation of speleological expeditions. He describes them as a travelling laboratory which allows the testing, trying out and implementation of not only new methods and instruments such as photography and the theodolite, but also organizational and professional issues. Mattes suggests that speleology only came into existence as a discipline—albeit a synthetic one—since scientific institutions started organizing such expeditions. He elaborates his ideas by referring to two such major undertakings, the expeditions into the Gassel-Tropfsteinhöhle cave in Upper Austria in 1924 and into the ice cave Eisriesenwelt near Salzburg in 1921. Particular forms of control of procedure acted as a framework for the experimental design. Strict processes of exclusion as well as inclusion determined the planning of the procedure, which was exclusive not only socially, as in a laboratory where access is limited, but also politically, especially where Jews and women were concerned. Thus cave expeditions proved to be a political field where social developments were negotiated and also prominently presented in the public domain.

Katarina Matiasek analyses stereo photography as a means of anthropological research in Vienna. She shows how this method was first tried and tested on various expeditions and then became established as a reliable instrument. The expeditions she examines constitute an experimental field in which this method, initially a documentation technique, soon developed into a measuring instrument, and finally into an analytical tool. Unlike the free expeditions undertaken by the Viennese anthropologist Rudolf Pöch, the research in POW (prisoner of war) camps during the First World War proved to be very differently organized. They were conditioned by an enclosed location where, as if in a laboratory, the factors of the investigation could be better controlled. While the technique of stereo photography did not fundamentally change in qualitative terms, the concept of space evoked by this method was transformed from "salvage space" to "atavistic space", and then from "hereditary space" to "total space" during the Nazi era.

Peder Roberts discusses two different ways of exploring the deep sea: through oceanographic expedition ships and the bathyscaphe. While the expeditions he focuses on—the Swedish *Albatross* (1947–1948) and the Danish *Galathea* (1950–1952) expeditions—were highly relevant for constituting deep-sea oceanography as a research field, the bathyscaphe remained, to a great extent, a vehicle for testing new technology and providing spectacular feats of deep-sea exploration. In both cases, the ocean became an experimental space for testing existing knowledge and for finding out more about its largely unknown fauna and flora; technology partook in creating this space, as new instruments were devised and tried out. While research carried out from the base of the expedition ships did not allow direct observation of the ocean depths, bathyscaphes did. For the two expeditions this meant that one had to imagine the environment of recovered research objects such as fish or sediments. Hence, the space of the deep sea had to be recreated, as in an experiment. Yet, as Roberts shows, the way the deep sea was approached differed between the expeditions: not only were they embedded in different national discourses but they also used different methods to investigate the data they found. This is an illustrative example of the situatedness of any exploration and scientific research.

Teresa Salomé Mota examines a special geological expedition which set out in 1960 from Portugal—at this time under dictatorship—for the Portuguese colonies in Goa (India) and had to be terminated abruptly in 1961 due to the invasion by Indian troops. In the course of this undertaking, aerial photography was used as a new observation practice that allowed the geological mapping of a landscape that was difficult to access: even though direct intervention in the landscape was limited, aerial photography facilitated greater control. The expedition not only provided the opportunity for geological investigation, it was also of social significance since it increased the social status of the geologists. And yet, social status issues caused problems among the participants of the expedition which had to be socially negotiated. They were triggered by the privileged position of one geologist due to his position at the Lisbon Academy. At the same time, however, geology as a field science provided a welcome opportunity for the Portuguese to prove themselves as a colonial power on the Indian subcontinent, and to match the research of the Indians. Thus, the expedition had an experimental character in that it tested new research practices. In a sense, we might even understand Goa as experimental space, as the Portuguese government was able to reconsider its intentions to combine political, colonial and scientific interests during a period of colonial competition with the Indian Union.

Division of Work and Questions of Authority

Organization was a key element in preparing expeditions; potential risks and dangers had to be foreseen and various tasks to be carried out during the expedition had to be planned in advance. Appropriate clothing, instruments, food supplies, items for entertainment and education such as books had to be acquired. Not only precise preparations but also a well-functioning division of responsibilities were considered essential preconditions of expeditions, as well as the professionalization of routines in fieldwork, certain practices, instructions and the like. Everyone had their place and particular tasks, while at the same time cooperation between the participants was required. In most cases, a set of instructions was applied based on the experience of previous explorers. Thus every single expedition was part of a tradition of rules and regulations on how to acquire knowledge.[37] In addition, the scholars often depended on a network of travellers and locals who provided them with knowledge and data before or during the enterprise.[38]

While the workload may have been democratically divided, there was a clear hierarchy in place, not only among the participants themselves, but also between the expedition members and the local population. This illustrates the complexity of knowledge production and the impossibility of distinguishing between various forms of knowledge, which is implied by the notion of expeditions as centres of negotiation. Western travellers often saw themselves as carriers of global knowledge and attributed to the local population a mere local understanding. Modern Western science was seen as superior to the local scientific traditions.[39] And yet, as much research and also the chapters of this volume show, knowledge production is a continuous process which cannot be reduced to a dichotomy of local versus global or “Western”, or of local population versus Western traveller or colonialist.[40] Moreover, there is no clear-cut definition as to who in fact had local knowledge, which was highly differentiated, for example in regard to expeditions exploring colonial territories or regions in Europe. Not only locals but also representatives of colonial authorities and residents with a colonial background who had become locals (Teresa Salomé Mota, Tanja Hammel), or those who held high positions such as merchants and consuls (Jan Vandersmissen) could belong to this group.

Local knowledge was employed and actively applied in different ways: in finding and naming items (Yuko Takigawa, Kurt Schmutzer), in providing infrastructure (Teresa Salomé Mota), in generally supporting the expedition (Kurt Schmutzer), in the form of guides (Johannes Mattes), in providing necessary information before and during the expedition (Ulrike Spring, Jan Vandersmissen). It could also be found in the metropolitan centre itself, for example through the transport of material and objects between the so-called peripheries and centres. Furthermore, as Tanja Hammel shows in her case study of the white female South African traveller Mary Barber, the relation between locals and more recently settled locals was often characterized by an internal social hierarchy. While local knowledge in general was rarely considered to be equal to the knowledge of the expedition members, it could be and often was acknowledged in scientific work presented or published in the aftermath of the expedition. It was, in other words, an important aspect of the credibility of the expedition enterprise.

Tanja Hammel's contribution points to another issue which had a significant impact on the question of authority: gender. Expeditions were highly gendered enterprises in that they were mostly conducted by men and both reflected and produced specific notions of masculinity in Western societies.[41] Johannes Mattes shows in his chapter that women enjoyed less scientific credibility than men and had to use different strategies to obtain recognition for their knowledge. Different social and professional hierarchies determined not only the division, assignment and manifestation of (scientific) authority during the expeditions, but also affected the processes of observation, documentation and reception. The dividing line ran between leaders and assistants, men and women, wealthy and poor scientists, or, as Kurt Schmutzer shows in his chapter, between travelling naturalists and natural scientists or, to refer to Jan Vandersmissen's contribution, between acknowledged scientists and mere naturalists.

Scientific Practices: Observation and Documentation

There is arguably no other practice for gaining knowledge that is so obvious, fundamental, omnipresent and, at the same time, so ambiguous than observation. In the protagonists' self-representations, observing and collecting predominantly appear as inseparable characteristics of the practices used during the expeditions, as Kurt Schmutzer demonstrates. Observation dominates perception, activation of the senses, the selection of phenomena and the orientation of questioning and assessment. Distinguishing between *observatio* and *experimentum* had a long tradition,[42] and both served to explore the unknown. But whereas the former was increasingly devalued as mere passive recording and registering, the latter—due to its active role—became more and more appreciated.[43] Ulrike Spring explores what happened when the fleeting ephemeral character of the aurora made the process of observation uncertain. Referring to one of the Austro-Hungarian Arctic expeditions, she demonstrates that both *observatio* and *experimentum* were needed. The registering process during the observation procedure created and developed new terms of classification. Drawings became necessary as mimetic instruments to record observations. In this context, the aurora shifted between being perceived as a natural phenomenon and a scientific object; the expedition, thus, can be seen as an "unfinished experiment" (Spring).

Every observation, of whatever type, is a central component of active questioning. And questioning is directly linked to documentation. This includes the most varied forms of recording, the "little tools of knowledge",[44] lists and labels as well as journals, notebooks, sketches, pictures and instruments. New ways of recording observations have constantly been discovered, and it is for this reason that the productivity of observation is essential to the expedition. Here we may follow Daston and Lunbeck's apt description: "As a practice, observation is an engine of discovery and a bulwark of evidence."[45] Often, observation is not only the means, but also the end, and "a learned reflection as a distinct form for knowledge"[46] with its own standards and conditions. The variations of phenomena and practices, and the question of how to deal with them in the context of both physical and symbolic spaces are at the centre of this volume. Each chapter refers to specific constellations in which a mix of observations and particular documentary activities is discussed.

When focusing on the material aspect of the particular exploratory journey, we have to examine precautions taken and strategies used to ensure that the knowledge gained through observation during an expedition was intersubjectively verifiable. What types of medial configurations of documentation were developed and then used in a standardized way to provide a solid basis for travel as an instrument of discovery in the course of the modern age up to the twentieth century? Or (to put it differently), what kind of standardization and control mechanisms—factors we generally recognize as constitutive for an experiment—were introduced or required of expeditions? Since these different practices correspond to specific and complex cultures of knowledge, we need to take a closer look at this.

In the process of observers distancing themselves from the subjective, which became common practice during the nineteenth century, and of evaluating the object that was being explored, an important tool was developed: the instruction. This had an enormous impact as a means of standardization, regardless of the academic background it came from. It referred to the culture of exploration through observation by providing methods of documentation and influencing the various recording systems, as Katarina Matiasek suggests in her chapter on stereo photography and anthropological expeditions. Not all expeditions examined in this volume were subject to instructions. But as a common genre they had an implicit impact on the norms that influenced every form of documentation.[47] The norms themselves were also variable, depending on the cultural or political context of the particular enterprise. The contributions in this volume discuss a number of different forms of the explicit and implicit search for a form of documentation that was considered adequate for research.

Reliability of documentation was the key factor for all expeditions, regardless of their differences. Also, in this context the concept of "instruction" provides some valuable insights. In functional terms this is due to two different aspects: first, the "methodization" of knowledge acquisition and, second, the bureaucratic and administrative framework of travel.[48] Both were culturally determined and variable with regard to their contexts, and both changed over time. Moreover, both could control and influence the future of the undertaking. The most important meaning of an instruction, as well as of an expedition is, therefore, ensuring its enabling function for the future. This is because, as discussed earlier, an expedition did not begin on the day of departure, but long before, often in a certain institution such as a museum, and it usually ended in a similar establishment where the objects collected ultimately arrived, something Kurt Schmutzer demonstrates in his chapter. And even if there was no direct commissioning agent, as for example in the case of individual travellers, the objects collected were included in the collections of other investigators or institutions, as Jan Vandersmissen shows.

In addition to written records, however, the objects that were brought back—artefacts, natural specimens and visual representations of both—not only served as evidence of the completed journey and verified the experiences and observations abroad, they also stimulated further research and ensured the reception of the findings of an expedition, as Kurt Schmutzer and Yuko Takigawa demonstrate in their chapters.

In the course of the nineteenth century, efforts were made to eliminate subjectivity in observation by giving greater credibility to the instrument than to the observations of a single individual. Daston and Galison referred to this process as the production of "mechanical objectivity".[49] This was also true for expeditions in the twentieth century, when new documentation procedures became essential, as Katarina Matiasek and Teresa Salomé Mota show in their chapters on new technical procedures for recording pictures, or when observatory instruments such as the bathyscaphe were invented, as Peder Roberts demonstrates. In this way, expeditions became particular experimental spaces where, due to the exclusivity of the event, the use of mechanical instruments of knowledge seemed especially attractive.

Let us return once more to our heuristic tool of expeditions as experiments. Whereas for a long time the ancient *interrogatoria*[50] only adopted Aristotelian solutions in disputes, modern techniques of questioning used in science inspired specific follow-up research. In the seventeenth century, when the experiment was introduced at the Royal Society, it was defined as a dialogue with nature.[51] Asking questions based on experience and prior knowledge became constitutive for the design of an experiment. By analogy, we may also ask how this dialogue with nature is formulated in the very varied contexts of expeditions. This is the subject of the studies in this volume at a wide range of levels.

This volume thus re-evaluates the significance of expeditions in the process of knowledge acquisition from the eighteenth to the twentieth century, and does so in a different way than has been done so far. By understanding expeditions as experiments, we also propose to regard them as kinds of laboratories where various practices are conducted and knowledge is tested and produced. This connection is particularly apparent and fruitful when we consider it as a heuristic tool. To see expeditions in this way thus makes it possible to illuminate the various stages involved in the process of knowledge acquisition, in particular the transformation of observations into facts and documentation.

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Notes

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2. Roy MacLeod, "Discovery and Exploration", in Peter J. Bowler and John V. Pickstone, eds., *The Cambridge History of Science*, vol. 6, *The Modern Biological and Earth Sciences* (Cambridge: Cambridge University Press, 2009), 38.
3. See for instance Paul Carter, "Looking for Baudin", in Susan Hunt and Paul Carter, eds., *Terre Napoleon: Australia through French Eyes, 1800–1804* (Sydney: Historic Houses Trust, 1999), 21–34.
4. For such a historiographical overview in the context of polar exploration, see Johan Schimanski and Ulrike Spring, *Passagiere des Eises. Polarhelden und arktische Diskurse 1874* (Wien, Köln, Weimar: Böhlau, 2015), 17–19, 545–6.
5. Miller and Reill, eds., *Visions of Empire*.
6. See the phrase suggested by Bruno Latour, *Science in Action* (Milton Keynes: Open University Press, 1987).
7. See Alan Frost, *The Voyage of the "Endeavour": Captain Cook and the Discovery of the Pacific* (Sydney: Allen and Unwin, 1996).
8. Pierre Bourdieu, *Le sens pratique* (Paris: Les Éditions de Minuit, 1980).
9. See especially Felix Driver, *Geography Militant: Cultures of Exploration and Empire* (Oxford: Wiley-Blackwell, 2000); Lewis Pyenson, *Civilizing Mission: Exact Sciences and French Overseas Expansion, 1830–1940* (Baltimore: Johns Hopkins University Press, 1993); Paolo Palladino and Michael Worboys, "Science and Imperialismus", *Isis* 84 (1993): 91–102.
10. See for instance: Martin Thomas, ed., *Expedition into Empire: Exploratory Journeys and the Making of the Modern World* (New York: Routledge, 2015); Christopher Herald, *Bonaparte in Egypt* (London: Hamish Hamilton, 1992).
11. See Londa Schiebinger and Claudia Swan, eds., *Colonial Botany: Science, Commerce, and Politics* (Philadelphia: University of Pennsylvania Press, 2005).
12. Martin Thomas, "What Is an Expedition? An Introduction", in Thomas, ed., *Expedition into Empire*, 3.
13. Martin Thomas, "The Expedition as a Cultural Form: On the Structure of Exploratory Journeys as Revealed by the Australian Explorations of Ludwig Leichhardt", in Thomas, ed., *Expedition into Empire*, 65–87.

14. For instance: John Cawte Beaglehole, *The Exploration of the Pacific*, 3rd edition (London: Adam and Charles Black, 1966); Derek Howse, ed., *Background to Discovery: Pacific Exploration from Dampier to Cook* (Berkeley: University of California Press, 1990); Leslie Marchant, *France Australe: A Study of French Explorations and Attempts to Found a Penal Colony and Strategic Base in South Western Australia, 1503–1826* (Perth: Artlock Books, 1982).
15. For instance: Lorenzo Veracini, "Settler Colonial Expeditions", in Thomas, ed., *Expedition into Empire*, 51–64.
16. See for instance: Tony Rice, *Voyages of Discovery: Three Centuries of Natural History Exploration* (London: Natural History Museum, 1999).
17. Charles W. J. Withers and David N. Livingstone, "Thinking Geographically about Nineteenth-Century Science", in David N. Livingstone and Charles W. J. Withers, eds., *Geographies of Nineteenth-Century Science* (Chicago: University of Chicago Press, 2011), 1–19, here 10.
18. See for instance Larrie D. Ferreiro, *Measure of the Earth: The Enlightenment Expedition That Reshaped Our World* (New York: Basic Books, 2011), xviii.
19. Thomas, "What Is an Expedition? An Introduction", in Thomas, ed., *Expedition into Empire*, 7.
20. Thomas, "What Is an Expedition? An Introduction", in Thomas, ed., *Expedition into Empire*, 17. See also Joshua A. Bell and Erin L. Hasinoff, eds., *The Anthropology of Expeditions: Travel, Visualities, Afterlives* (Chicago: University of Chicago Press, 2015).
21. MacLeod, "Discovery and Exploration", 34–95.
22. David Philip Miller, "Introduction", in Miller and Reill, eds., *Visions of Empire*, 1–18, here 1.
23. See in general: Adriana Craciun, "What Is an Explorer?", in Thomas, ed., *Expedition into Empire*, 25–50.
24. See in general: Friedrich Steinle and Michael Heidelberger, eds., *Experimental Essays—Versuche zum Experiment* (=Intersdisziplinäre Studien 3, Bielefeld), (Baden-Baden: Nomos Verlag, 1998), especially 34–67.
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33. Kohler, *Landscapes and Labscapes*, 7.
34. Kohler, *Landscapes and Labscapes*, 18.
35. Martin Thomas, "What Is an Expedition? An Introduction", in Thomas, ed., *Expedition into Empire*, 14–15.
36. Marie-Noëlle Bourguet, Christian Licoppe and H. Otto Sibum, *Instruments*, *Travel and Science*: *Itineraries of Precision from the Seventeenth to the Twentieth Century* (London: Routledge, 2002).
37. Driver, *Geography Militant*.
38. Bourguet, Licoppe and Sibum, *Instruments*, *Travel and Science*, 9.
39. Francis Zimmermann, "Integration Problems: Introductory Report", in Patrick Petitjean, Catherine Jami and Anne Marie Moulin, eds., *Science and Empires*: *Historical Studies about Scientific Development and European Expansion* (Dordrecht: Kluwer, 1992), 33.
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41. See, for instance, Lisa Bloom, *Gender on Ice: American Ideologies of Polar Expeditions* (Minneapolis: University of Minnesota Press, 1993).
42. See Hans Poser, "Observatio, Beobachtung", in Joachim Ritter, ed., *Historisches Wörterbuch der Philosophie*, vol. 6 (Basel: Schwabe Verlag,

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43. In this context, Bacon has to be mentioned: see Paolo Rossi, *Francis Bacon*: *From Magic to Science* (Chicago: University of Chicago Press, 1968).
44. Peter Becker and William Clark, eds., *Little Tools of Knowledge*: *Historical Essays on Academic and Bureaucratic Practices* (Ann Arbor: University of Michigan Press, 2001).
45. Daston and Lunbeck, "Introduction: Observation Observed", 7.
46. Daston and Lunbeck, "Introduction: Observation Observed", 12.
47. For the significance of instructions on exploratory expeditions see Driver, *Geography Militant*.
48. Marianne Klemun, "Verwaltete Wissenschaft—Instruktionen und Forschungsreisen", in Anita Hipfinger, Josef Löffler, Jan Paul Niederkorn, Martin Scheutz, Thomas Winkelbauer and Jakob Wührer, eds., *Ordnung durch Tinte und Feder*? *Genese und Wirkungen von Instruktionen im zeitlichen Längsschnitt vom Mittelalter bis zum 20. Jahrhundert* (Wien and München: Böhlau and Oldenbourg Verlag, 2012), 391–412.
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50. See more in Justin Stagl, "Vom Dialog zum Fragebogen. Miszellen zur Geschichte der Umfrage", *Kölner Zeitschrift für Soziologie und Sozialpsychologie* 3 (1979): 611–31, here 612.
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CHAPTER 2

An Idea Ahead of Its Time: Jean-Jacques Rousseau's Mobile Botanical Laboratory

Alexandra Cook

Introduction

Despite his well-known polemics against the sciences as a source of moral degeneration, Jean-Jacques Rousseau (1712–1778) pursued a sustained engagement with the natural sciences.[1] These included chemistry and botany, in that order. The manuscript of Rousseau's *Institutions Chymiques*, discovered in 1882, attests to Rousseau's detailed knowledge of chemistry. Furthermore, references to "experiments"[2] appear in works as diverse as Rousseau's *Lettre sur la Musique Françoise*[3], and *Discours sur l'Origine et les Fondements de l'Inégalité* and *Confessions*. Experimentation, broadly construed, thus provides an important basis for the domains in which Rousseau philosophized. This experimental orientation likewise significantly shaped the practices Rousseau applied in his botanical expeditions.

This argument may seem improbable in light of the objections Rousseau mounted against chemistry when he rejected it in favour of botany.[4] He asserted that in preferring the dead to the living chemistry gives no insight into the mystery of life,[5] that it is dirty, expensive and dangerous. Chemistry, Rousseau alleged, is more attached to pride than to knowledge for its own sake: "From all this sad and tiresome toil

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much less knowledge than pride ordinarily results, and where is the most mediocre chemist who does not think he has penetrated all the great operations of nature because he has discovered, perhaps by chance, a few small tricks of the art?"[6] Worst of all, chemistry's connection with alchemy and metallurgy taints it with greed and economic inequality.[7] Yet, despite Rousseau's polemics against chemistry, his botanical expeditions derived inspiration from the laboratory and experimentation taken in its broad eighteenth-century sense (described below): (1) Rousseau regularly tested others' reports during his botanical expeditions and encouraged others to do so as well; (2) he used instruments as aids to the senses and (3) carefully organized the work to be done. These expeditions were experimental in another key respect: they were open-ended, their final results not known until the end, even if every attempt was made to control for the desired outcome.

Moreover, despite its association with a discredited science, the chemical laboratory fundamentally shaped Rousseau's general concept of doing botany: he declared that the fields adorned with flowers provide the botanist's "only laboratory".[8] At first sight this statement may seem paradoxical. Why refer to the fields as a laboratory for the botanist while criticizing the "sedentary work of the laboratory"?[9]

There is certainly more than one way to interpret this statement. On the one hand, Rousseau could be understood to subvert the notion of the laboratory by moving from a smoky, enclosed space into the sun and open air where collection, identification and preliminary preservation of plant specimens supplant the destructive transformations effected in chemistry. At the same time, the laboratory, which relies on a hierarchical division of labour and complicated, fixed and costly apparatus affordable only by the wealthy, gives way to fresh air, open spaces and beauty accessible to anyone able to acquire a few simple instruments.

In this sense, the choice of botany is therefore not only scientific or philosophical, it is also political. The botanist's realm is more transparent and democratic than that of the chemist; unlike the chemist, who relies on costly apparatus that requires servants to use, the botanist independently studies plants while "easily carrying all his tools in his [or her] pocket".[10] This independence is coupled with the moral and aesthetic appreciation of "nature, who never lies".[11] Hence unlike the chemistry laboratory, the woods and fields are tainted neither by human vanity nor by deception.[12]

On the other hand, rather than being seen to subvert the concept of the laboratory, Rousseau can be understood to invoke the broad eighteenth-century understanding of the laboratory as the site of the *work*. Such a meaning derives directly from the Latin verb "*laborare*": to work. This meaning is mooted in the *Dictionnaire de l'Académie Française* (1762) which defines the laboratory as the "[p]lace where Chemists have their furnaces & their vessels for *working*".[13] This broad, non-restrictive definition allows ample room for botanical practices to supplant chemical ones, thereby preserving the laboratory's virtues minus some of its less savoury connotations. Hence, to invoke the "laboratory" in the middle of the eighteenth century was to refer to a broad notion of work and work space, a space that botany could claim equally with chemistry.[14]

Rousseau may have had all these interpretations of the fields as the botanist's laboratory in mind. It is entirely consistent with his way of philosophizing to entertain two contrary or apparently inconsistent ideas at the same time. So he might both subvert the chemistry laboratory's social and scientific status while nonetheless deriving conceptual mileage from introducing the term "laboratory" into botany.

While the Academy's definition of laboratory does not explicitly refer to "experiment", its reference to the "[p]lace where Chemists have their furnaces & their vessels for *working*" strongly implies experimentation. The contemporary understanding of the term "experiment" originates in the Latin verb, *experiri*, to "try". The 1762 edition of the *Dictionnaire de l'Académie Française* preserved this meaning, giving a broad definition that relates to daily experience, as well as to scientific work: "Test that one makes of something, either by design or by accident. *A strange experience/experiment. New experience/experiment. To have a sad experience*, *an annoying experience. Experience/experiment is the mistress of the arts. I know this by experience/experiment. I have had experience of it*. […] Philosophers conduct experiments on nature every day."[15] Understood as "test" the term "experiment" can apply to a wide range of scientific activities, including ones that might not seem to fall under a strict modern construction of the term. These activities might include testing evidence one collects against assertions by other authorities. Moreover, the definition of the verb "*experimenter*" tracks with the meaning of the noun, "experience": "*To test* a remedy, a recipe, a secret by experiment/experience. I have tested 100 times that […]. If you doubt the effectiveness of this medicine, you can test it."[16]

The Chemical-Experimental Moment

Rousseau's importation of the laboratory into the botanical context is probably attributable to his youthful involvement with chemistry over the better part of a decade. This involvement started in the 1730s during his cohabitation with Françoise-Louise de la Tour, Mme de Warens (1699–1762), who provided the maternal care that Rousseau, whose mother died shortly after his birth, otherwise lacked. Rousseau called her "Maman"; she sponsored his conversion to Catholicism and supported his autodidactic endeavours. She also introduced him to the chemistry of plant-based pharmacology. Hence, even though Rousseau does not clearly indicate when and where he learned the practical side of chemistry, he probably acquired much of this knowledge while helping Maman manufacture herbal medicines in her home. In fact she probably used the same procedures and ingredients as did apothecary-chemists of the time since the "material culture of academic laboratories overlapped strongly with the realm of instruments, reagents, technologies and materials applied and produced in apothecary's laboratories, assaying shops, and distilleries".[17] For example, the apothecary-chemists of the Royal Academy of Sciences (Paris) focused on extracting medicinal properties from plants by distillation and solvent analysis.[18] Rousseau's acquaintance with what was known as "vegetal chemistry" is attested by an undated list of sixty-six of the Paris Academy's *Mémoires* and *Histoires* on chemistry.[19] In the *Confessions* he reports temporarily blinding himself in an experiment with invisible ink. Rousseau continued these studies after moving to Paris in 1741.

That Rousseau was influenced by Maman's pharmaco-chemical milieu, and an introduction to academic chemistry in Paris in May 1743, is suggested by his account in the *Confessions* of three "experiments" he conducted during his diplomatic service in Venice from September 1743 to August 1744. Rousseau intended these experiments to show the superiority of Italian over French music; they comprised (1) a technical comparison of two songs, one from each tradition, and equally esteemed; (2) giving French songs by Jean-Baptiste Lully (1632–1687) to Italian singers, while giving Italian songs to French singers, and (3) a performance of both Italian and French songs before an Armenian who allegedly had never heard music before (an improbable claim). In his descriptions of these experiments, Rousseau refers to the "precautions" he applied, and assesses the outcomes as more or less "decisive". These comments suggest that he was already well-versed in basic experimental procedure.[20]

Upon returning to Paris from Venice in late summer 1744, Rousseau's chemical education was stimulated by his relationship with the wealthy Dupin family. The heir to this fortune, Charles-Louis Dupin de Francueil (1716–1780), aspired to membership in the Royal Academy of Sciences (Paris)[21] and enlisted Rousseau in this pursuit: "I became attached to Chemistry. Along with M. de Francueil I took several courses with M. Rouelle, and for good or ill we began to scribble on paper about that science whose elements we barely possessed."[22] Rousseau and Francueil studied chemistry with Guillaume-François Rouelle (1703–1770), who taught at the Jardin du roi in Paris and played a key role in eighteenth-century French chemistry.[23] While he never published a work on chemistry, Rouelle was an important teacher and Academician who popularized the phlogiston theory.[24] Rouelle's teaching was experimentally and quantitatively grounded, paying close attention to temperature regulation and sealing vessels to avoid loss of reagents and products.[25]

After Rouelle's course, Rousseau set up a laboratory with Francueil at the chateau of Chenonceaux, the Dupin family home[26]: "In 1747 we went to pass the autumn in Touraine at the Chateau de Chenonceaux, the royal house on the Cher, built by Henri II for Diane de Poitiers [...]. I composed other little works there [...] and all that was done without discontinuing my work on Chemistry [...]."[27] From the 1730s into the late 1740s or even 1750s Rousseau invested considerable time and energy in chemistry.[28]

An experimental basis likewise supported Rousseau's subsequent work in other domains. For example, Rousseau portrays the *Discours sur l'Origine et les Fondements de l'Inégalité* as conjectural and hypothetical.[29] In this same work, he invokes an experimental result as evidence for his thesis concerning the fertility of an originally forested planet: "My third and most important remark is that the fruits of Trees furnish animals with a more abundant food supply than other plants can, a result that I myself obtained in comparing the products of two pieces of land equal in size and quality, one covered with chestnuts and the other planted with wheat."[30] In *Emile*, Book III, Rousseau teaches practical chemistry in the context of food adulteration. His later polemics against chemistry (referred to above) likewise reveal his familiarity with the investigative methods applied to plants in the laboratory.[31] Given that these works were written during or after Rousseau's chemical phase, it seems reasonable to infer that chemical experimentation played a key role in inspiring these experimental approaches to various philosophical issues.

Instruments and Expeditions

> *Instrument*. Masculine noun. Tool that serves the worker, [and] the artisan to make something. *Good instrument. Necessary instrument. Surgical instrument. Instrument of the Carpenter, the Mason, etc. A worker furnished with all his instruments* [...].[32]

Artisans made instruments and understood how to use them; hence Francis Bacon (1561–1626) and Dénis Diderot held up artisans and their skill-sets as crucial to the progress of science and technology. Indeed, apparatus and instruments play a key role in chemistry to this day. As an instrument-based science, chemistry is rooted in the artisanal milieu from which Rousseau—born into a family of Geneva watchmakers—came. A strong artisanal bent is revealed in his interest in book-binding, compiling herbaria and making laces.[33] His ability to use his hands to create useful and beautiful things harked back to his grandfather, David Rousseau (1641–1738), one of Geneva's great seventeenth-century watchmakers.[34]

In the *Institutions Chymiques*, Rousseau states that natural processes should be studied in "an *artificial Laboratory* on the model of nature", where "it does not suffice to look in a general way at the means she employs, one must above all perfectly know the *instruments* of which she makes use".[35] The work divides these "instruments" into "natural" and "artificial": the "natural instruments" discussed in Book 2 comprise the traditional four elements inherited from antiquity—earth, air, fire and water—while the "artificial" ones discussed in Book 3 comprise "furnaces and vessels, other chemical instruments, solvents and precipitates"[36]—the usual apparatus of the chemistry laboratory employed in distillation and solvent analysis.

Later, critiquing chemistry and mineralogy from the standpoint of a one-time adept, Rousseau displayed an accurate knowledge of the equipment used in chemical experimentation:

> To make progress in the study of minerals, it is necessary to be a chemist and a physicist. It is necessary to perform tedious and costly *experiments*, to work in *laboratories*, to spend much money and time in the midst of charcoal, *crucibles*, *furnaces*, *retorts*, smoke, and suffocating fumes, always at the risk of life and often at the expense of health.[37]

This focus on instruments was not ephemeral; from the beginning of his botanical studies Rousseau exercised great care in his selection and use of instruments, enlisting the Genevan geologist, Jean-André Deluc (1727–1817),[38] to help him acquire the indispensable portable kit of the field botanist on the move: magnifying glasses ("*loupes*"), small scissors, and tweezers for plucking small plant parts. In a letter to Deluc, Rousseau explained his reasons for acquiring these instruments: "as you have inferred, the microscope is for botany; hence I want it to have a field [*champ*] sufficient to encompass the pistil and stamens of a small flower. For the rest I rely completely on you."[39] Like Rousseau, Deluc was a master watchmaker's son and his good connections with Geneva's artisans facilitated privileged access to precision instruments:

> if our friend [Jean-André Deluc] were able to make the small tools necessary for the dissection of flowers, I am certain that his intelligence would supplement that of the workers. These tools consist of three or four magnifying glasses of different magnifications, small, delicate and slender tweezers to hold the flowers, very fine scissors, pocketknive*s* and lancets to cut them. I would be very happy to have them in duplicate, except for the magnifying glasses; because there is someone here who has the same taste as I do, and who has been ill served.[40]

This letter likewise demonstrates Rousseau's sophisticated awareness of what the botanical fieldworker's work entails; in it he engages Deluc to obtain a second set of instruments for an unnamed botanist friend, probably Pierre Alexandre du Peyrou (1729–1794).[41] With its already highly developed technical capabilities, Geneva offered Rousseau and his circle unparalleled access to the best instruments available for pursuing his botanical fieldwork.[42]

Thus, despite his objections to the values and goals of chemistry, this discipline nonetheless shaped Rousseau's approach to botanical expeditions in several ways: he (1) accepted the utility of experiments as tests across many fields of study, (2) enthusiastically adopted instruments as aids to the senses and (3) envisioned the fields as a laboratory, shifting the locus of the work from the inside to the outside where, as we shall see, "[t]he excursion is his sole *work*".[43]

The Mobile Botanical Laboratory

For Rousseau, the exemplary botanist was not a chemist or an apothecary, but rather a philosopher such as Theophrastus (ca. 370–285 BCE), who sought knowledge of plants for themselves alone[44]: "botany [...] is a study of pure curiosity that has no other utility than that which can attract a thinking being who is sensitive to the observation of nature and the marvels of the universe".[45] Botanists have traditionally preferred to collect uncultivated plants in the wild, for they consider these true species rather than mere varieties created through cultivation. For Rousseau, an added attraction of the wild plant is the beauty, variety and finality bestowed on it by the Author of nature.[46]

Collection in the field entails expeditions. Key figures in sixteenth-century botany such as Luca Ghini (c. 1490–1556), Ulisse Aldrovandi (1522–1605), Andrea Cesalpino (1519–1603) and Gherardo Cibo (1512–1600) recognized the importance of field trips to botanical teaching. Ghini established "the field trip as a standard part of student training [...]. Botanizing further strengthened the ties between mentors and disciples, as an essential rite of inclusion."[47] Having opined throughout his *œuvre* that "nature never lies", Rousseau happily joined travelling botanists to find plants in the field.

Like the chemistry experiment, the botanical expedition can test the veracity of information generated by others and yield *discoveries*: (1) finding new plants, (2) observing already known ones in new locations, and/or (3) ascertaining whether a given species/genus is found in a particular habitat or location. These activities entail using instruments, organizing animals, people and things, and recording information just as one would in a stationary, indoor laboratory. Like the chemist's laboratory, the botanist's movable laboratory has its own peculiar set of risks and problems: for Rousseau these included bad weather, spoiled specimens or none at all, soggy bedding and even lost pets!

Rousseau's depiction of Carolus Linnaeus (1707–1778) as having "studied too much in herbaria and in gardens and not enough in nature itself" highlights the importance of fieldwork.[48] In other words Rousseau believed Linnaeus regarded enclosed, controlled spaces such as herbaria and gardens as his laboratories rather than spending sufficient time in the less predictable, more open-ended field laboratory.

Yet Rousseau's characterization is misleading because Linnaeus's requirement that every botanist make a herbarium actually fostered botanical fieldwork: the first steps in constituting a herbarium are collection, preservation and identification of specimens gathered in the field.[49] Furthermore, Linnaeus promoted scientific travel by his "Apostles" to far-flung places worldwide[50] and personally led many botanical trips closer to home.[51] Similarly, the Swiss botanist, Albrecht von Haller (1708–1777), established alpine botany by making arduous excursions, as well as working from specimens collected by his many assistants and colleagues;

these efforts culminated in his landmark *Historia Stirpium Indigenarum Helvetiæ Inchoata* (1768).[52]

For his part, Rousseau adapted his already engrained habit of walking to examining the wild, uncultivated plant still "on the stalk", growing in the ground.[53] While he agreed with Linnaeus that botanists should make herbaria, Rousseau stressed that they must start their studies with the living plant, not from a dried specimen which may be missing parts, may have faded, or deteriorated: "one herborizes uselessly in an herbarium [...] if one has not started by herborizing *on the earth*. These sorts of collections should serve only to facilitate recollecting, but not for first instruction."[54]

Organization

Always keen on flawless organization, Linnaeus famously emphasized the importance of the well-organized field trip, the *herbatio*. His field trips were open to any and all paying participants of either sex and sometimes included foreign visitors and dignitaries.[55] In his *Philosophia Botanica*, Linnaeus specified procedures and details of the botanical field trip[56]: "There are RULES for those who come late, depart early, or are absent. And for the division [of labour], lunch at 2, rest at 4, and for a secretary."[57] He stipulated the length of the journey,[58] what was to be collected, recorded, and taught: the professor should give a "single DEMONSTRATION [...] lasting not longer than half an hour."[59] Linnaeus even prescribed the clothing to be worn: "Very light and very loose CLOTHING proper to botanists"[60] and the instruments [*instrumenta*] to be used—books, magnifying glass, Dillenian case, botanical needle and knife.[61]

As an admirer of *Philosophia Botanica*, Rousseau presumably knew this list even if he rather unfairly suggested that Linnaeus did not do enough field botany.[62] On all his expeditions—whether solitary walks, or group expeditions with or without a guide, whether for a day or several days—organization mattered just as much to Rousseau as it did to Linnaeus. In fact, Rousseau assumed a major role in these expeditions, issuing his own instructions to participants. In the "*collège de botanique*" with whom he explored the Val de Travers "Rousseau, as the oldest, was the captain of the small troop, charged with the discipline of the corps, and with maintaining order and subordination."[63]

It should be stressed that these expeditions entailed a significant degree of organizational complexity: the scientific side of the undertaking required not only small instruments such as magnifying glasses and cutting tools, but also the proper types of grey and white paper for drying the specimens, containers for holding the specimens and writing supplies for making notes in the field.[64] The care Rousseau took to record important observations about specimens right away in the field was revealed by his fear that the herbarium he had made for Julie Boy de la Tour[65] had been lost along the way: "I have not neglected to take some care with it. It is a loss which while small would not be easy for me to repair promptly, especially on account of a catalogue accompanied by various small *clarifications written on the spot*, and of which I have not kept a duplicate."[66]

In addition, several days' provisions had to be obtained and prepared for the journey. Pack animals had to be procured and a good guide was indispensable. Lacking many conveniences that we take for granted, such as good roads and comfortable transport, an expedition posed many logistical challenges. The group therefore needed a clear division of labour, which Rousseau spearheaded:

> Concerning the donkey for [carrying] provisions, I completely approve of it; this is a procession in which I wish to take part more than anyone: We must also agree on a treasurer or bursar who is in charge of all the supplies and the budget. As you are one of the four who knows the country the best, the only one who speaks the language, I agree that you should be asked to take charge of this duty […].[67]

Another participant reported on the result of these discussions:

> Judge[68] Leclerc supplied the provisions. M. du Peyrou had responsibility for the herbaria. Colonel de Pury was our guide; he carried the compass, for in the dark thickets of the forests it's only possible to be guided by knowing where north lies. […] I had furthermore custody of the coffee and the task of making it; armed with a lighter[69] that I preserved very carefully, it was I who lit the fire in the woods […] and gave the coffee its proper preparation.[70]

Rousseau attended to such details as reminding du Peyrou to bring the requisite books and everything needed to make coffee en route:

> I advise you not to forget our provisions of coffee, sugar, coffee pot, lighter, and the entire apparatus so that we can make coffee in the woods when we wish. Bring Linnaeus and Sauvages,[71] an amusing Book, and some games for us to entertain Ourselves somewhat if we are stuck inside during bad weather. It is necessary to foresee all eventualities in order to avoid boredom and idleness.[72]

Rousseau not only led expeditions, but also initiated them; in July 1768 he prepared "to herborize at the Grande Chartreuse with a fine and good company of botanists that I found and recruited in this region".[73]

As in the formal laboratory setting, team-work played an important role in botanical expeditions that involved more than one person. The camaraderie that Rousseau promoted in the group facilitated cooperation. Recalling "the ease and gaiety of walking journeys", Rousseau reminded one of his companions, "none of us were at all glum at Brot".[74] This sense of camaraderie is reflected in his characterizing his fellow botanists as a congenial group—a "Caravan" or "Crew".[75] Rousseau mobilized this esprit de corps to facilitate mutual research assistance, which he emphasized in a letter of introduction to one such companion: "we will help each other, and will return as little Linnaeuses. […] I salute you, Sir, and embrace you warmly; since we shall be travel companions[,] permit me to address you with familiarity in advance."[76]

Expeditions Testing the Reports of Others

Visiting the field laboratory offers the possibility of performing *tests*—a core aspect, as we have seen, of the eighteenth-century understanding of experiment. Fieldwork might test, for example, whether a species allegedly found in the past in a particular location could be found there again. Local florae provided such testable reports in spades; Rousseau made a point of verifying against such reports what could actually be found on the ground:

> [I]t seems to me that one of the great charms of botany is, in addition to seeing for oneself, that of *verifying* what others have seen; to give, on the *testimony* of my own eyes, my assent to the fine and just observations of an author seems to me a real delight: instead, when I *cannot find* what he says, I am always troubled if it is not I who sees badly. Besides, being able to see only very little on my own, I have to rely for the rest on what others have seen […].[77]

Rousseau recounts an expedition in May 1771 to Montmorency (north of Paris) with the "crew" of the Jardin du roi in which he took the initiative to test reports by three distinguished botanists—Tournefort, Bernard de Jussieu (1699–1777) and Sébastien Vaillant (1669–1722)—that *Plantago monanthos* or *P. uniflora* L. (names bestowed by Joseph Pitton de Tournefort [1656–1708][78] and Linnaeus, respectively) was growing by the lake in Montmorency.[79] The participants, led by Antoine-Laurent de Jussieu (1748–1836)[80] and André Thouin (1747–1824)[81] of the Jardin du roi, took this mission so seriously that they nearly suffered heat stroke.[82]

The botanists from the Jardin du roi (including Bernard de Jussieu's nephew, Antoine-Laurent) tested these reports and found them wanting—*P. monanthos/uniflora* was in fact nowhere to be seen:

> I would be ungracious to show off to you a herborization that I undertook at Montmorency last summer with the Crew from the Jardin du Roi; But it is certain that on my part it was an enterprise only for finding the *plantago monanthos*, which I looked for in vain. M. de Jussieu the younger[83] […] will have been able to tell you with what ardour I begged all these Gentlemen, as soon as we approached the end of the pond, to assist me in the search for this plant; which they did, and among others M. Thouin with a kindness and a solicitude which would merit a better success. We found nothing, and after two hours of useless searching, in the heat of the day and on the hottest day of the year, we stopped to breathe and rest under the trees which were not far, concluding unanimously that the *Plantago uniflora* indicated by Tournefort[84] and M. de Jussieu[85] in the neighbourhood of the pond of Montmorency *had absolutely disappeared*.[86]

In a later account of this expedition, Rousseau describes the same outcome, but instead of citing Bernard de Jussieu's *Nouvelle observation*, he refers to a report in Vaillant's *Botanicon Parisiense*:

> Last year on May twentieth at Montmorency […] I found […] that the indications of Tournefort and Vaillant are very *defective*, or that since them, many plants have *changed habitat*. I searched and engaged everyone to search with care for, among others, the *Plantago monanthos* at the end of the pond of Montmorenci, and in all the places that Tournefort and Vaillant[87] indicated, and we could *not find even one stalk* […].[88]

This expedition yielded another important result—the discovery of plant species that had *not* been reported in the floras: "On the other hand I found *several plants of note* and even quite close to Paris in places where they were *not indicated at all*."[89] Rousseau had similar experiences on other expeditions: he was surprised to find alpine mossy sandwort (*Moehringia muscosa* L.) growing in profusion in the sub-alpine Swiss Jura: "Never grows except in the Alps. I found it growing abundantly at Môtiers on the walls of the mayor's office of Verrières."[90] Similarly on Mont Pilat he found mossy strapwort (*Corrigiola litoralis* L.), a native of shorelines and sandy places rather than mountain tops: "This small plant loves the sand and riverbanks; I nevertheless found it at the top of Mont Pilat."[91]

These examples expose another experimental characteristic of the botanical expedition: its open-endedness. Before setting out there was no way to predict whether any specimens would present themselves, and if they did appear, in what state—complete, incomplete, flowering or past flowering, with or without seeds, wet or dry? An expedition might yield some, many or no collectable specimens whatsoever. The weather, the knowledge and experience of the participants, and the quality of the guide, if any, were all key factors contributing to the success or failure of the expedition.

Aware of the importance of a good guide, Rousseau and his "botanical college" explored the Swiss Jura under the tutelage of Dr Abraham Gagnebin (1707–1800), a fount of botanical knowledge.[92] While we lack detailed information about their finds, we can presume they found good specimens since Rousseau refers later to dried plants collected in Switzerland that may have derived from his outings with the "botanical college".[93]

Rousseau also showed an impressive sensitivity to the difficulties of acquiring specimens suitable for preservation; one of the greatest logistical challenges facing botanists then and now is obtaining and preserving high-quality herbarium specimens. They need to be in good condition (not too wilted or damaged) and intact, displaying leaves, reproductive organs and roots. In his letter on herbaria to Mme Delessert, which ends his eight famous letters on botany, Rousseau enumerates the precautions necessary to collect good specimens, especially in damp or unpredictable weather:

> Such is the choice that it is necessary to put into what one cuts. It is also necessary to put some also into the moment one takes for this. The plants cut in the morning at dawn, or in the evening in the dampness, or in the daytime, *during the rain* do not last.
>
> It is absolutely necessary to choose *dry weather*, and even in that weather *the driest moment* and *the hottest* of the day, which is in summer between eleven o'clock in the morning and five or six o'clock in the evening. Even then if one finds there the *least dampness* one must leave them; for inevitably they will not last.[94]

Expeditions yielding rich finds undoubtedly offered a sharp contrast with those that yielded little or nothing. An expedition in August 1769 provides a good example of the latter. Rousseau travelled from Monquin to Mont Pilat with a Dr Meynier,[95] the Abbé Baurin and Luc Antoine Donin de Champagneux (1744–1807).[96] The group lacked a guide, experienced bad weather, endured unpleasant conditions and found little to collect:

> [W]e had bad weather during practically the entire trip [...] we found a very bad hut on the mountain. Outside of [which] one mattress stuffed with fleas [...]. [We had] accidents of all kinds: one of our Gentlemen was bitten by a dog on the mountain. Sultan[97] was half-massacred by another dog; he disappeared, I believed him dead from his wounds or eaten by a Wolf [...]. The fifth point, and the worst, is that we *found almost nothing*, having arrived too late for the flowers, too early for the seeds, and having *no guide* to help us find the good spots.[98]

This largely failed expedition no doubt offered a standard against which to judge more successful ones.

Conclusion

As a site of often risky experimentation and hard-won discoveries, the conventional indoor laboratory played a critical role in the development of sciences such as chemistry and physics. The laboratory is less often associated with early-modern botany, traditionally seen as a science of collecting, observation and classification. Modern DNA analysis has of course changed the relation of botany to the laboratory.

In offering a vision of the fields as the botanist's "only laboratory", Jean-Jacques Rousseau provides a powerful way to understand key aspects of eighteenth-century botanical fieldwork. The fields can more readily be envisioned as a kind of laboratory when the laboratory—chemical or otherwise—is understood as the site of the *work*.

Botanical fieldwork shared key characteristics and goals with work in the conventional laboratory where: (1) an experience/experiment is understood as a test, (2) use of the correct instruments/tools is indispensable, and (3) difficulties and risks abound, including bad weather, deficient guide books, loss of specimens and threats from wild animals. Botanical expeditions were probably more logistically complicated and difficult than they are today, lacking conveniences that we take for granted. Yet, then as now, outcomes remain uncertain and difficult to control: an expedition might yield many excellent specimens worth preserving or none at all; expected species might elude discovery while totally unexpected ones might appear instead. Poor conditions might call the undertaking to a halt. Like the experiment in the traditional laboratory, the botanical expedition entailed risk and difficulty, but also the possibility of a good result. The botanist's excursion was therefore truly "work" and the fields his or her "only laboratory".

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Notes

1. Jean-Jacques Rousseau, "Discours sur les Sciences et les Arts", in Bernard Gagnebin and Marcel Raymond, eds., *Œuvres Complètes*, iii (Paris: Gallimard, 1964—hereafter *OC*), 1–30. For a discussion of Rousseau's paradoxical position on the sciences, see Alexandra Cook, *Jean-Jacques Rousseau and Botany, the Salutary Science* (Oxford: Voltaire Foundation, 2012), Chapter 1.
2. Jean-Jacques Rousseau, *Institutions Chimiques*, ed., Christophe van Staen (Paris: Honoré Champion, 2010). In contrast to Rousseau, van Staen uses the spelling "*chimiques*".
3. The French term, "*expérience*", denotes both "experiment" and "experience"; so the French term generally has a broader meaning than "experiment" in modern English, not to mention in philosophy of science, which applies strict canons in defining what constitutes an experiment. See for instance Karl Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959) on experiment's role in falsification; see Hans-Jörg Rheinberger's notion of "experimental systems" in *Toward a History of Epistemic Things: Synthesizing Proteins in a Test-tube* (Stanford: Stanford University Press, 1997).
4. A full discussion of these reasons lies beyond the scope of this essay. See Cook, *Jean-Jacques Rousseau and Botany*, Chapter 2.
5. All translations are mine unless otherwise indicated. Jean-Jacques Rousseau, "Fragments de botanique", in *OC*, iv (1969), 1249–56, here 1249–50.
6. Jean-Jacques Rousseau, "Reveries of the Solitary Walker", Charles E. Butterworth (trans.), in *Collected Writings of Rousseau* (Hanover, NH: University Press of New England, 2000; hereafter designated as *CW*), vol. 8, 1–90, here 63; Jean-Jacques Rousseau, "Les Rêveries du Promeneur Solitaire", in *OC*, i (1959), 993–1099, here 1067.

7. Jean-Jacques Rousseau, "Discours sur l'Origine et les Fondements de l'Inégalité", in *OC*, iii (1964), 131–223, here 194.
8. Rousseau, "Fragments de botanique", 1250.
9. Jean-Jacques Rousseau, "Introduction" to "Fragmens pour un Dictionnaire des Termes d'Usage en Botanique", in *OC*, iv (1969), 1201–9, here 1201. Note that this so-called "Introduction" was probably an unrelated document added by Rousseau's editors to other documents of uncertain provenance to form the posthumous *Fragmens pour un Dictionnaire des Termes d'Usage en Botanique*. See Cook, *Jean-Jacques Rousseau and Botany*, 306–7.
10. Rousseau "Fragments de botanique", 1250. Rousseau refers to "tools" while Linnaeus, as discussed below, uses the term "instruments" to designate the very same tools. That the terms might be synonyms is suggested by the *Dictionnaire de l'Académie* (Paris: Chez la Veuve B. Brunet, 1762), which defines "instrument" by reference to "tools", discussed below.
11. Rousseau, "Fragments de botanique", 1250; see also Rousseau, "Discours sur l'Origine de l'Inégalité", 133; Jean-Jacques Rousseau, "Rousseau Juge de Jean Jaques, Dialogues", *OC*, i (1959), 667–976, here 833.
12. Rousseau, "Rêveries", 1073.
13. My emphasis; *Dictionnaire de l'Académie Française*, 2.
14. *Pace* Kuklick and Kohler, we should not assume that Rousseau's reference to the laboratory invests the fields and thereby botany with a seriousness and significance that they were thought to lack; botany had long been a respectable part of medicine while chemistry was a relative newcomer trying to shed dubious associations with alchemy. See Henrika Kuklick and Robert E. Kohler, "Introduction", *Science in the Field*, *Osiris* 11, 2nd series (1996), 1–14.
15. Original emphasis; *Dictionnaire de l'Académie Française*, 698.
16. My emphasis; *Dictionnaire de l'Académie Française*, 699.
17. Ursula Klein and Wolfgang Lefèvre, *Materials in Eighteenth-Century Science: A Historical Ontology* (Cambridge, MA: MIT Press, 2007), 34; see also Bernadette Bensaude-Vincent and Bruno Bernardi, "Pour Situer les Institutions Chymiques", *Corpus* 36 (1999): 5–38, here 11.
18. See Alice Stroup, *A Company of Scientists: Botany, Patronage, and Community at the Seventeenth-Century Parisian Royal Academy of Sciences* (Berkeley: University of California Press, 1990), 98 and Cook, *Jean-Jacques Rousseau and Botany*, 36 ff.
19. "Catalogue de tous les mémoires de chymie contenus dans tous les volumes de l'histoire de l'Académie R[oyale] des Sciences", MsR 81, fol. 2–26, Bibliothèque Universitaire et Publique de Neuchâtel; see also Cook, *Jean-Jacques Rousseau and Botany*, 43.

20. Jean-Jacques Rousseau, "Lettre sur la Musique Françoise", in *OC*, v (1995), 291–328, here 299–302.
21. The Dupin family owed their great wealth to tax farming, which Rousseau condemned in "Les Confessions de J. J. Rousseau", in *OC*, i (1959), 5–656, here 363–4.
22. Jean-Jacques Rousseau, "The Dupin family owed their great wealth to tax farming, which Rousseau condemned in "Les Confessions", 292–3.
23. Rhoda Rappaport, "G.-F. Rouelle: An Eighteenth-Century Chemist and Teacher", *Chymia* 6 (1960): 68–101.
24. Now a discredited reject in the dustbin of science, phlogiston was widely accepted in the eighteenth century and played a crucial conceptual role in the evolution of chemistry. John Christie, "Experimental and Speculative Work in Eighteenth-Century Britain: Franklin, Priestley, Cavendish and Hutton", paper presented at iCHSTM, Manchester, UK, 27 July 2013.
25. Rappaport, "G.-F. Rouelle", 98. Notes from Rousseau and Dupin de Francueil's course with Rouelle can be found in J. J. Rousseau, "Procédés du Cours de M. Rouelle", fol. 117–38 and MSR 86, "Table des Procédés du Cours de Chymie M. Rouelle", fol. 139–50, Bibliothèque Publique et Universitaire de Neuchâtel, MSR 85.
26. Bensaude-Vincent and Bernardi, "Pour situer les *Institutions Chymiques*", 17.
27. Rousseau, "Confessions", 287; Rousseau, "Les Confessions", 342.
28. Bensaude-Vincent and Bernardi, "Pour situer les *Institutions Chymiques*".
29. Rousseau, "Discours sur L'Origine et les Fondements de l'Inégalité", 132–3.
30. Rousseau, "Discours sur l'Origine et les Fondements de l'Inégalité", 198. Nothing further is known about this experiment. *OC*, iii, 1362, n. 5.
31. This knowledge is revealed by phrases such as "the combination of their compounds" and "more inaccurate chemical analysis". Rousseau, "Fragments de botanique", 1249. For the translation of "*mixtes*", see Jon Eklund, *The Incompleat Chymist*: *Being an Essay on the Eighteenth-Century Chemist in his Laboratory*, *With a Dictionary of Obsolete Chemical Terms of the Period*, Smithsonian Studies in History and Technology 33 (Washington, DC: Smithsonian Institute Press, 1975), 32. Rousseau's reference to "inaccurate chemical analysis" might point to distillation, which yields similar results across a wide range of plants. See Cook, *Jean-Jacques Rousseau and Botany*, 40–2.
32. Original emphasis; *Dictionnaire de l'Académie Française*, 939. Other examples given are musical and mathematical instruments. Note that "instrument" is defined as "tool" and "tool" is defined as "instrument", so the two terms are virtually synonymous, although "tool" might be slightly more strongly associated with manual labour: "TOOL. Masculine noun. Any instrument of which Artisans, Labourers, Gardeners, etc. make use for their work. *The tools of a joiner, of a Carpenter. Ploughing tools. The hammer*

is a tool of great use. Bring your tools." Original emphasis; *Dictionnaire de l'Académie Française*, 274.

33. These "laces" are used to tie shoes or corsets.
34. David Rousseau's impressive watches were displayed for Jean-Jacques Rousseau's 2012 tercentenary at the Patek Philippe Museum in Geneva: "Des Montres Signées Rousseau/Timepieces Signed Rousseau".
35. My emphasis; Rousseau, *Institutions Chimiques*, 103.
36. Rousseau, *Institutions Chimiques*, 407.
37. Rousseau, *Reveries of the Solitary Walker*, 63. Rousseau had earlier alluded to "the considerable number of unhealthy occupations that shorten life expectancy or destroy the constitution such as mining, the preparation of metals, [and] minerals, especially Lead, Copper, Mercury, Cobalt, Arsenic and Realgar". Rousseau, *Discours sur l'Origine et les Fondements de l'Inégalité*, 205, n. ix.
38. See Marita Hübner, *Jean Andrè Deluc* (*1727–1817*)*: Protestantische Kultur und moderne Naturforschung* (Göttingen: Vandenhoeck & Ruprecht, 2010).
39. Rousseau to Jean-André Deluc, 20 December 1764, *Correspondance Complète*, ed. Ralph A. Leigh, 52 vols. (Genève: Institut et Musée Voltaire, 1965–98—hereafter *CC*), xxii, 256.
40. Rousseau to François-Henri d'Ivernois, 20 July 1765, *CC*, xxvi, 106–7; see also Rousseau to Jean André Deluc, 15 December 1764, *CC*, xxii, 240–1.
41. Du Peyrou, a wealthy plantation owner and businessman of Dutch and Huguenot extraction, lived in the Prussian principality of Neuchâtel, where he protected Rousseau as best he could up until 1765. The two men shared an interest in botany and du Peyrou fostered Rousseau's early botanical pursuits. He largely directed the posthumous publication of Rousseau's works. See Cook, *Jean-Jacques Rousseau and Botany*, 122–3.
42. On Rousseau's interest in scientific instruments, see Bernadette Bensaude-Vincent, "La Nature Laboratoire", in Bernadette Bensaude-Vincent and Bruno Bernardi, eds., *Rousseau et les Sciences* (Paris: L'Harmattan, 2003), 155–74, here 161 ff.
43. My emphasis; recall the origin of the word "laboratory" in the Latin *laborare*, "to work". Rousseau, "Fragments de botanique", 1250.
44. Rousseau, "Rêveries", 1063.
45. Rousseau to Mme Delessert, March/April 1774, *CC*, xxxix, 234. As a part of natural philosophy botany "was an autonomous study separate from theology and natural theology, but whose practitioners had at the forefront of their minds […] the same God whose attributes the theologians studied". Andrew Cunningham and Perry Williams, "De-Centring the 'Big Picture': *The Origins of Modern Science* and the Modern Origins of Science", *British Journal for the History of Science* 26 (1993): 407–32, here 421.

46. According to Rousseau, studying botany soothes the tumults of the soul and lifts our sights to the Author of nature; rather than providing ingredients for medicines, plants promote psychological well-being by serving as a subject of contemplation. See Cook, *Jean-Jacques Rousseau and Botany*, 2–3, 11–16.
47. Paula Findlen, *Possessing Nature: Museums, Collecting, and Scientific Culture in Early Modern Italy* (Berkeley: University of California Press, 1994), 166.
48. Rousseau, "Confessions", 538; Rousseau, "Les Confessions", 643.
49. Carolus Linnaeus, *Linnaeus' Philosophia Botanica*, trans. S. Freer (Oxford: Oxford University Press, 2003), 18.
50. Mariette Mantelkow and Kenneth Nyberg, "Linnaeus's Apostles and the Development of the Species Plantarum", *Symbolae Botanicae Upsalienses; Arbeten från Botaniska Institutionen i Uppsala* 33 (2005): 73–80.
51. These were recorded in Linnaeus's travel reports on Lapland, Öland and Gotland. See his *Öländska och Gothländska Resa* (Stockholm and Uppsala: Gottfried Kiesewetter, 1745).
52. Albrecht von Haller, *Historia Stirpium Indigenarium Helvetiæ Inchoata* (Bern: Societatis Typographicæ, 1768); see also Alix Cooper, *Inventing the Indigenous: Local Knowledge and Natural History in Early Modern Europe* (Cambridge: Cambridge University Press, 2007).
53. Rousseau, "Les Confessions", 643.
54. My emphasis; Rousseau, in "Letters to Chrétien-Guillaume de Lamoignon de Malesherbes", 230; Rousseau to Malesherbes, 19 December 1771, *CC*, xxxviii, 301.
55. See Hanna Hodacs, "Linnaeans Outdoors: The Transformative Role of Studying Nature 'on the Move' and Outside", *British Journal for the History of Science* 44 (June 2011): 183–209.
56. Such field trips or "herbationes" were "public events, open not just to students but to any man or (occasionally) woman who cared to learn natural history the Linnaean way (assuming, that is, that they could afford the fee)". See Hanna Hodacs, "In the Field: Exploring Nature with Carolus Linnaeus", *Endeavour* 34(2) (published online January 2010): 45.
57. Linnaeus, *Philosophia Botanica*, 331.
58. "Milliaria ad summum duo cum dimidio": 2.5 miles at most.
59. Linnaeus, *Philosophia Botanica*, 331. Linnaeus would have certainly dominated any expedition in which he took part, however informally.
60. Linnaeus, *Philosophia Botanica*, 330.
61. Linnaeus, *Philosophia Botanica*, 331. Note that Linnaeus refers to the same items that Rousseau calls "tools" in his letter to d'Ivernois cited above (n. 39).
62. Rousseau expressed his admiration of Linnaeus's *Philosophia Botanica* as greater than all the books of "morality". Rousseau to Linnaeus, 21 September 1771, *CC*, xxxviii, 267–8.

63. François Louis d'Escherny, *Mélanges de Littérature*, *d'Histoire*, *de Morale et de Philosophie*, vol. 3 ("Rousseau et des philosophes", Paris: Bossange et Masson, 1811), 66.
64. Rousseau to Mme Delessert, 11 April 1773, *CC*, xxxix, 139.
65. Julie Emélie Willading née Boy de la Tour (1751–1826) was a younger sister of Madeleine Catherine Delessert née Boy de la Tour (1747–1816), the recipient of Rousseau's eight letters on botany. Rousseau made a magnificent herbarium for Julie Willading-Boy de la Tour that her descendants donated to the Zentralbibliothek Zürich. See Ruth Schneebeli-Graf, *Das Zürcher Herbar von Jean-Jacques Rousseau* (Zürich: Wohnmuseum Bärengasse, 1980) and Alexa Renggli, "Das 'Petit Herbier pour Mademoiselle Julie Boy de la Tour' von Jean-Jacques Rousseau", *Jahrbuch der Schweizerischen Gesellschaft für die Erforschung des 18. Jahrhunderts/Annales de la Société Suisse pour l'Étude du XVIIIe siècle/Annali de la Società Svizzera per lo Studio del Secolo* xviii 4 (2013): 113–27.
66. My emphasis; Rousseau, "Letters to Mme Madeleine-Catherine Delessert, the so-called *Elementary Letters on Botany*", in *CW*, 8, 130–72, here 144; Rousseau to Mme Delessert, 16 July 1772, *CC*, xxxix, 81.
67. My emphasis; Rousseau to Abraham de Pury, 10 June 1765, *CC*, xxv, 32.
68. D'Escherny's term, "*justicier*", translated here as "judge", means "member of the tribunal". See Fritz Berthoud, *J.J. Rousseau au Val de Travers, 1762–1765* ([Paris, 1881] Geneva: Slatkine Reprints, 1970), 178, n. 2. Compare the old English term, "justiciar".
69. The term d'Escherny uses is "*briquet*", which the *Dictionnaire de l'Académie* defines as a "small piece of iron that is used to draw fire from a stone". *Dictionnaire de l'Académie*, 217. He might have used iron pyrite rock and a flint.
70. D'Escherny, *Mélanges*, 66.
71. D'Escherny, *Mélanges*, 41. Rousseau is referring to fundamental Linnaean works such as the *Systema Naturae* (first published in 1735 and expanded in several later editions); see Carolus Linnaeus, *Systema Naturae 1735*: *Facsimile of the First Edition*, introduction and trans. M. S. J. Engel-Ledeboer and H. Engel (Nieuwkoop: B. De Graaf, 1964). He also mentions François Boissier de Sauvages de la Croix (1706–67), a famous professor of medicine at Montpellier, who generally favoured Linnaeus, but argued that characters such as leaves should also be used in classification. See *Methodus Foliorum Seu Plantæ Floræ Monspeliensis/Methode Pour connoître les Plantes par les Feüilles* (The Hague: n.p., 1751). Rousseau requested this work from Nicolas Bonaventure Duchesne in a letter of 19 May 1765, *CC*, xxv, 301–2 and received it in late summer or early fall of the same year. He sold it—heavily annotated with Linnaean binomial names and other information—to Daniel Malthus (1730–1800) circa

1775; Malthus's son, the economist Thomas Robert "population" Malthus (1766–1834), willed it with his book collection to Jesus College, Cambridge. See Alexandra Cook, "Jean-Jacques Rousseau's Copy of Albrecht von Haller's *Historia Stirpium Indigenarum Helvetiae Inchoata* (1768)", *Archives of Natural History* 30(1) (2003): 149–56; Henry Cheyron, "Ray et Sauvages Annotés par Jean-Jacques Rousseau", *Littératures* 15 (1986): 83–99.

72. Jean Jacques Rousseau, "Letters to Pierre-Alexandre du Peyrou", in *CW*, 8 (2000), 196–9, here 196; Rousseau to Pierre Alexandre du Peyrou, 11 June 1765, *CC*, xxvi, 32.
73. This "company" included Marc-Antoine-Louis Claret de Latourrette and the Abbés de Grange-Blanche and Jean Baptiste François Rozier. Rousseau to du Peyrou, 6 July 1768, *CC*, xxxvi, 8; see also Rousseau to Luc Antoine Donin de Champagneux, 12 August 1769, *CC*, xxxvii, 126–7.
74. Brot (now Brot-Dessous) is in the Val de Travers in the Swiss canton of Neuchâtel. Rousseau, "Letters to Pierre-Alexandre du Peyrou", in *CW*, 8 (2002), 201–3, here 202; Rousseau to du Peyrou, 16 September 1769, *CC*, xxxvii, 142. Similarly, "my voyages on foot having always been up until now all very cheerful, conducted with companions who were in as good a mood as I was". Rousseau to the Count de Laurencin, 10 October 1769, *CC*, xxxvii, 158.
75. Rousseau to du Peyrou, 29 April 1765, *CC*, xxv, 204.
76. Rousseau to the Abbé Baurin, 8 August 1769, *CC*, xxxvii, 119. Little is known about Baurin (fl. 1769–87), a Catholic priest and archdeacon of Salmorenc in the present-day Department of Rhône-Alpes.
77. My emphasis; Rousseau, "Letters to Malesherbes", in *CW*, 8, 233; Rousseau to Malesherbes, 17 April 1772, *CC*, xxxix, 37.
78. This is probably an abbreviation of *Plantago Palustris*, *Gramineo Folio*, *Monanthos Parisiensis*. See Joseph Pitton de Tournefort, *Institutiones Rei Herbariae*, vol. 1 (Paris: de l'Imprimerie Royale, 1700), 128. This plant is known in English as shore plantain.
79. *P. uniflora* L. replaced earlier names conferred by Tournefort, Leonard Plukenet (1642–1706), Robert Morison (1620–83) and others; see Carolus Linnaeus, *Species Plantarum*: *A Facsimile of the First Edition 1753*, with introductory essays by William T. Stearn, vol. 1 (London: The Ray Society, 1957), 115. The scientific name of this plant is now *Littorella lacustris* E.H.L. Krause.
80. Antoine-Laurent de Jussieu (1748–1836) was the nephew of Bernard de Jussieu (1699–1777), demonstrator at the Jardin du roi, Paris, and a father of the natural method in botany. Known as "the nephew" or "the younger", Antoine-Laurent de Jussieu likewise contributed to the development of the natural method. See his *Genera Plantarum* (Paris: Herissant and Barrois, 1789).

81. André Thouin was head gardener at the Jardin du roi (Paris) during the *ancien régime* and professor of cultivation at the Jardin du roi's successor, the Jardin des plantes; he coordinated an immense botanical network documented in Emma C. Spary, *Utopia's Garden*: *French Natural History from Old Regime to Revolution* (Chicago: University of Chicago Press, 2000).
82. Antoine-Laurent de Jussieu confirms Rousseau's regular participation in these expeditions: "[d]uring the last five years of his life, he regularly attended the special herborization that the nephew M. de Jussieu conducted every week, in summer, with M. Thouin and a small number of friends or selected students." My emphasis; Antoine-Laurent de Jussieu, "Sixième Notice sur le Muséum", *Annales du Muséum d'Histoire Naturelle* 11 (1808): 14, note (1).
83. That is, Antoine-Laurent de Jussieu.
84. According to Tournefort, this plant grew around the lake now known as Lake of Enghien: "[t]his plant is found around the pond of Montmorency, when one has passed la Chaussée on the ascent toward Saint-Gratien". Joseph Pitton de Tournefort, *Histoire des Plantes qui Naissent autour de Paris* (Paris: de l'Imprimerie Royale, 1698), 517–18. Rousseau probably knew this area well, having resided there from April 1756 to June 1762.
85. Bernard de Jussieu refers to this plant growing "on the shore of Saint Gratien pond", hence concurring with Tournefort, *Histoire des Plantes*, 128. See Bernard de Jussieu, "*Observation nouvelle sur les fleurs d'une espèce de Plantain nommée par M. De Tournefort dans ses Élemens de botanique*, Plantago palustris gramineo folio monanthos Parisiensis, pag. 104", *Histoire de l'Académie Royale des Sciences. Avec les Mémoires de Mathématique et de Physique*: *Tirés des Registres de cette Académie 1742* (Paris: de l'Imprimerie Royale, 1745), 137.
86. My emphasis; Jean-Jacques Rousseau, "Letters to Marc-Antoine-Louis Claret de Latourette", in *CW*, 8, 214–27, here 225; Rousseau to Latourrette 25 January 1772, *CC*, xxxix, 22.
87. "It is found in abundance beside the pond of Montmorency on the walled side by of the park of Saint-Gratien." Sébastien Vaillant, *Botanicon Parisiense* (Leiden: Jean and Herman Verbeek and Amsterdam: Balthazar Lakeman, 1727), 160.
88. My emphasis; Rousseau, "Letters to Malesherbes", in *CW*, 8, 234; Rousseau to Malesherbes, 17 April 1772, *CC*, xxxix, 38.
89. My emphasis; Rousseau to Malesherbes, 17 April 1772, *CC*, xxxix, 38.
90. Note accompanying a specimen of mossy sandwort (*Moehringia muscosa* L.) in Rousseau's herbarium for Julie Boy de la Tour. The towns of Môtiers (737 metres) and Verrières (930 metres) in the Val de Travers, are located in the Swiss Jura, whose highest peak is 1720 metres.

91. Note accompanying a specimen of mossy strapwort (*Corrigiola litoralis* L.) in the herbarium Rousseau made for Julie Emélie Willading-Boy de la Tour. The Pilat massif rises about 1000 metres (3300 ft) above its surroundings and lies in the extreme east of the French Massif Central range looking over the Rhône valley. The orientation of the massif from southwest to north-east creates a variety of climates in a relatively small area. Fruit trees and vineyards are cultivated on the south-facing slopes, while conifers, juniper and ferns grow on the cooler north side.
92. Gagnebin was Albrecht von Haller's most important botanical correspondent. Urs Boschung et al., eds., *Repertorium zu Albrecht von Hallers Korrespondenz 1724–1777*, vol. 1 (Basel: Schwabe, 2002), 157. On Gagnebin's vast knowledge, see d'Escherny, *Mélanges*, 42–3.
93. Rousseau to the Duchess of Portland, 20 October 1766, *CC*, xxxi, 41.
94. My emphasis; Rousseau, "Letters to Mme Delessert", 162; Rousseau to Madeleine-Catherine Delessert, 11 April 1773, *CC*, xxxix, 141.
95. Little is known about Meynier, who "gave the impression of liking Botany, and desiring to cajole me, why I do not know, thought there was nothing better to achieve this than to assume an affected air". Rousseau to du Peyrou, 16 September 1769, *CC*, xxxvii, 142.
96. "There has been much speculation concerning the composition of the small group that accompanied JJ on the famous expedition to Mont Pilat. JJ himself declares that he was accompanied by only 'three gentlemen.' There was, therefore, besides Baurin and Meynier, a third person, perhaps Donin de Champagneux (see nº 6599)." Rousseau to the Abbé Baurin, 8 August 1769, *CC*, xxxvii, 119, editor's note.
97. Sultan was Rousseau's beloved dog. Not only did Sultan survive, he reappeared at his master's home "calm and completely healed". Rousseau to du Peyrou, 16 September 1769, *CC*, xxxvii, 142. Recounting the same incident, Rousseau wrote: "A botanical excursion […] that I made to Mont Pila[t] […] was disastrous, with rain the whole time; I found few plants, and I lost my dog who was injured by another one, and ran away; I feared him dead from his wound in the woods, when upon my return I found him here in good shape, I could not imagine how he could have travelled twelve leagues and crossed the Rhône in that state." Rousseau to Laliaud, 27 August 1769, *CC*, xxxvii, 129–30.
98. Rousseau, "Lettres to Pierre-Alexandre du Peyrou", 202. Rousseau to du Peyrou, 16 September 1769, *CC*, xxxvii, 142.

CHAPTER 3

Experiments and Evolving Frameworks of Scientific Exploration: Jean-André Peyssonnel's Work on Coral

Jan Vandersmissen

Introduction

The key player of this study—the French physician and naturalist Jean-André Peyssonnel—had a lifelong connection both to expeditions and corals. He organized research missions at sea along the coasts of Provence and in the course of a voyage to North Africa where he studied coral with the help of fishermen. Later in his career he complemented his findings with new insights derived from experiments with marine specimens he found on the beaches of the island of Guadeloupe where he was sent in the capacity of *médecin du Roi*. One key element in Peyssonnel's work, in which he differed from some of his fellow naturalists, was his vision of truth: simply putting forward a theory, a thesis or a presentation of a new "system" in nature was not enough. Knowledge about coral could be based solely on observation of specimens in their natural environment, and by experiments *in situ*, which would reveal the characteristics of coral, for example chemical tests. The first assumed a displacement in the form of an expedition, the second set up a research infrastructure and a methodology

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adapted to the exploration context. Both activities were crucial for the identification of corals in a broader sense.

The central question of this chapter is how the relationship between travel and experiment functioned in the case of a momentous investigation that ultimately would prove that coral belongs to the animal kingdom. During the Age of Enlightenment it was still far from obvious that explorers experimented in the course of their journeys. However, this study argues that the marine environment in which Peyssonnel's research activities took place was on the contrary conducive for combining travel with experiment. This inter-relationship shows two sides, which I aim to unravel in detail. First, in a broad and holistic sense, travel "as" experiment: that is to say, exploratory journeys seen as deliberate and purposeful actions necessary for verifying hypotheses and claims—in this case either put forward in writing by classic and modern "scholars" who often never saw the sea up close, or transmitted orally by fishermen who, over the centuries, had accumulated practical knowledge of life in the sea. Second, in a restricted sense, experiment as a specific form of investigation carried out "during" a voyage, distinct from but complementary to observation. In the eighteenth century, experiments on marine organisms were still a novelty and caused controversy, certainly when taking place outside traditional centres of learning. The fact that Peyssonnel set up such experiments when travelling in faraway countries, where working conditions were such that academic peers in Europe were unable to verify, let alone repeat them, made his findings vulnerable to criticism. Thus, the geographical concept of location and its entanglement with experiment-based knowledge production will be given special attention here. Such an approach seems justified, certainly with regard to *ancien régime* France, where the centralizing royal power held a tight grip on scholars, formalized through the Académie Royale des Sciences, and where personalities close to the powers that be claimed the position of supreme authority in the field of natural history. Hence, the aim of this chapter is also to illustrate how this apparatus functioned when confronted with experience through experiment "from outside", that is to say, produced in the course of travel.

In my view the "coral case" underlines how important scientific exploration has been for the acquisition of new insight in the mysteries of nature. At the same time it shows that scientific exploration never was a static phenomenon with fixed characteristics. Even during the lifetime of a single naturalist, it was possible that exploratory work was carried out in what one could call "evolving frameworks of exploration". The example of Peyssonnel's quests—spread over three continents—points to at least three of these frameworks.

The first is that of the "trip". Here scientific exploration is carried out in proximity to a fixed residence or workplace. This proximity does not necessarily imply that the explorer is already acquainted with the terrain he is working on. In the case of Peyssonnel, the field of investigation is the sea. It is situated in front of his home, but at the same time the sea's natural history remains a big unknown. Freedom of choice is an important aspect here. The explorer freely decides to feed his curiosity by moving to places nearby where he hopes to find answers to his questions. This activity offers a complement to an education based on the reading of classic and modern authors. By moving towards unknown territory the explorer needs the help of intermediaries who possess specialized knowledge and help him with the interpretation of the phenomena he observes. In Peyssonnel's case, fishermen fulfilled this important role. Their practices gave him a new way of getting knowledge, an activity which served for him as "experimental space", so to speak.

The second framework is that of the expedition. The expedition is no longer an individual form of endeavour. On the contrary, it involves a chain of people supervised by one central authority. This can be a private patron, but also the state can fulfil this role. The patron commissions the expedition, assigns one or several explorers to follow specific instructions, and pays them for their work as long as they respect their part of the contract. But for the explorer this can lead to ambiguous situations. From time to time the circumstances encountered in the field justify an adjustment of the exploratory programme. Here, the patron's authority sometimes blocks free investigation. Not respecting this authority might create new scientific knowledge, but at the same time can lead to conflict. Peyssonnel's career offers a classic example: at one moment during his expedition he decided to follow his own course, and that decision had a profound impact on his relationship with the French Crown.

The third framework is that of a colonial mandate that gives the possibility of developing a personal programme of exploration. The colonial mandate offers many advantages to scientific discovery. An often isolated but fixed residence allows the colonial officer to organize a fully equipped working space. At the same time, he operates in a new environment—a colonial setting with an abundant exotic nature that poses new challenges to his curiosity. The distance from the central government under whose authority the colonial officer operates reduces external control of his work and facilitates free endeavour. In leisure time, the colonial officer can fully devote himself to the exploration of the territories within the perimeter of his assignment. In Peyssonnel's case this included the Caribbean seas surrounding the island of Guadeloupe.

The research activities of Jean-André Peyssonnel form the backbone of this chapter. Born in Marseilles on 19 June 1694 and dying on the island of Guadeloupe on 24 December 1759, this physician and explorer-naturalist always remained on the side of the history of exploration. Jean-André Peyssonnel belonged to an old and influential family that left traces in the intellectual and social history of Provence—most male members of this family were professionally active either as physicians in Marseilles, or as lawyers and councillors at the Parliament in Aix. Consequently it is almost self-evident that Peyssonnel's career has been of interest to researchers specialized in regional history.[1] Jean-André Peyssonnel is universally recognized for having demonstrated the animal nature of coral. Strangely enough, his writings on this matter have only exceptionally received attention from historians of science. Most source-based studies are old and have become obsolete.[2] In recent work Peyssonnel's achievements are instrumental to arguments and claims regarding research issues of a much wider scope.[3] The relatively poor state of historical research is surprising because many dissertations and letters written by Peyssonnel are preserved in archives all over the world. An analysis of these documents may allow historians of science to give a new dimension to the debate on the nature of coral, linking with one move the Mediterranean to the Atlantic world.

Sources of a Lifelong but Forgotten Scientific Journey on Three Continents

The lack of interest in Peyssonnel's work can be explained by the fact he did not publish a lot. This had to do with his lack of scientific authority at a time when his research reached a productive peak. In libraries one can find the theses Peyssonnel defended at the University of Aix in 1718 and also pamphlets on medicine dating from the beginning of his career. Also of some importance are his writings on the plague that ravaged the population of Marseilles in the years 1720–1721.[4] Jean-André Peyssonnel's growing interest in marine issues is evident from an essay on currents, published in 1726.[5] Peyssonnel had to wait until the end of his life before some of his more elaborated papers appeared in the *Philosophical Transactions* of the Royal Society of London. Among these contributions one can find a summary of his work on coral. This particular text appeared in the 1751–1752 issue under the title *An Account of a Manuscript Treatise, Presented to the Royal Society, Entitled Traité du Corail* [...].[6] Later volumes of the *Philosophical Transactions* contain another ten papers sent in by Peyssonnel either from France (where he stayed briefly in 1755–1756) or from Guadeloupe (after he returned home in the course of the year 1756) and read at the meetings of the Royal Society, treating various subjects: volcanology, medicine, oceanography, natural history.[7]

Peyssonnel lived at a time when the battle between manuscripts and printed material had not yet been won by the printing press. It is certain that throughout his life he maintained the habit of sending copies of his letters and manuscript dissertations around the Atlantic world. Thus one can identify several manuscript treatises in which he described his research on coral. It can be assumed that they all find their origin in a kind of mother text to which Peyssonnel continuously made changes. This text must have been constantly improved and expanded over time. The principal sources are Ms 1035–1036, which is part of the collection of the Muséum national d'Histoire naturelle (MNHN) in Paris, and Add. Ms 4219, which is preserved at the British Library (BL) in London. These are vast manuscript texts containing several hundreds of pages. Both share almost exactly the same content and are known under the general title *Traité du Corail*. Moreover, it is almost certain that both the Paris and London variants are copied from a yet unidentified text that is much older and grew over a period of almost thirty years, at least since the 1720s. Indeed, some fragments of the Paris and London manuscripts correspond almost word for word with the content of manuscript dissertations on coral that date from the years 1723 and 1726.[8]

There exists also a series of more than a dozen letters Peyssonnel addressed to Jean-Paul Bignon, Guillaume Delisle and Pierre Chirac. These letters give an account of the expedition Peyssonnel undertook in 1724–1725 to the regencies of Tunis and Algiers. Again one can see that the same letters circulated in various copies. Almost identical manuscript versions can be found in at least four places in France.[9] This illustrates Peyssonnel's method very well: being aware of the importance of sharing his findings with both his protectors and high-placed scholars who were able to value his work and promote it among peers in Paris, he copied his own texts while working in the field and forwarded them to several correspondents.

Practical Knowledge Versus Erudition

Jean-André Peyssonnel was born in an environment where science was an integral part of everyday life. His father, the physician Charles Peyssonnel, was himself a prolific writer on natural philosophy. Through the teachings of his father Jean-André became interested in natural history. During his time at the College of the Oratory in Marseilles his interest in science was further stimulated and he continued his studies at the University of Aix-en-Provence where he obtained the degree of Doctor of Medicine. In the years 1719–1721, the period of the great plague in Marseilles, Jean-André Peyssonnel began to work as a physician but he was already interested in the natural history of the sea. In fact as a young man he had seen a part of the world. He had travelled in the French West Indies, Saint Domingue, Puerto Rico, the Mississippi Delta and Egypt (1710–1714). These voyages accustomed him to life at sea, but more importantly they stimulated him to develop the right mentality for observation. They made him understand the importance of keeping what he called "*une tranquilité d'esprit*"—which was not always easy in a fisherman's boat.[10] Peyssonnel himself thought these early voyages were essential to his education as a naturalist of the sea. Observing marine life was dangerous business. The sea belonged to fishermen, not to scientists. But thanks to the long sea voyages he made during his youth, Peyssonnel had the feeling he was able to overcome all difficulties. His body was resistant to the vagaries of the sea. Seasickness had no impact on him. Sharing daily life with sailors, discussing with them the routine of fishing as well as the nature of the marine products they took from the sea, stimulated his "curiosity". It was through these contacts, which functioned for him as a period of testing new practices, that Peyssonnel was able to draw up a list of questions for which he found no answers when reading scholarly books. And this list would later form the basis for a personal research programme in which exploratory observations in combination with experiments took a central place.

Upon his return to Marseilles, Jean-André Peyssonnel was ready to make his own observations. With the sea in walking distance he started to do research on "marine products" such as coral, sponges and algae. The local community of fishermen was important for assembling practical insights into marine life. From the very beginning Peyssonnel was aware of the usefulness of the knowledge they had accumulated. He lived among the fishermen in the parish of Les Accoules, where he heard their stories and saw how they organized their business. Jean-André and his brother

Charles rendered services to the Prud'hommes pêcheurs—a juridical body that settled disputes within the community but also functioned as its official representative.[11]

An educated man such as Peyssonnel also consulted sources of a more intellectual nature. During his education at the university he acquired a solid knowledge of classical thought, which was still held in high esteem—even when some claims and arguments of ancient authors conflicted with new observation-based knowledge obtained through fieldwork or experiment. The same goes for the theories developed by armchair scholars in the early modern period. They had entered the university curriculum and were seen by many educated people as forms of eternal truth. Peyssonnel's case illustrates a mentality shift typical of the early Enlightenment. To him the writings of scientific authorities were just elements of a personal research agenda which also included travel. Indeed, travel was an essential tool that must allow the verification of older claims and the creation of new knowledge through direct observation. In a sense it was a vehicle for free investigation. It was by a critical reading of what Peyssonnel called the "Ancients" and the "Moderns" that an investigative trip could be prepared on a sound intellectual basis. And that trip would lead to the acquisition of new forms of insight.

Peyssonnel first sought information on "marine products" in the works of Renaissance authors such as Pietro Andrea Mattioli, Jacques Daléchamps, Carolus Clusius, Ferrante Imperato, the brothers Jean and Gaspard Bauhin. He drew the conclusion that the authors had "a slight knowledge of marine plants". He hoped to find more in the work of the botanist Joseph Pitton de Tournefort but, although this scholar had travelled in the Levant, Peyssonnel was disappointed by him. Then he turned to Pliny the Elder, Albertus Magnus and Ulisse Aldrovandi.[12] When reading the "Ancients" and the "Moderns" he discovered that of the wide variety of marine products they had studied, they had focused mainly on coral. In fact, coral raised a series of specific questions about its "nature". For the "Ancients" coral was a stony structure. Theophrastus mentions it in *De Lapidibus* (38), Dioscorides in *De Materia Medica* (V, 121). It is often described as a sea plant that hardens into stone when pulled from the sea, a coagulation created as a result of contact with air. Ovid, in his *Metamorphoses* (IV, 741–52), tells the story of the transformation of algae into coral when Perseus laid down on a beach the decapitated head of Medusa. Pliny the Elder in his *Naturalis Historia* (XXXII, 11, 21–4) describes coral as a stony structure that protects against danger.

After his first readings, Peyssonnel made the plan of examining all that had been "told" about this "plant", both by the "Ancients" and by more recent authors, in order to gather this knowledge into one dissertation. He was convinced that this approach could help him to better understand the nature of other "products of the sea". After Pliny the Elder "others have only repeated what he has said", he noted. He confessed, however, that more accurate observations were to be found in works written by authors who really turned to the sea.[13] Indeed, by reading these authors Peyssonnel understood the importance of studying coral at sea and hence the necessity of organizing exploratory journeys. The travels made by Nicolas Claude Fabri de Peiresc in 1625 offered a good example, especially since they demonstrated the importance of experiments. Due to his excursions in the Mediterranean, this scholar had been able to analyse the liquid inside living coral, thus helping him to develop theories on the reproduction of coral, as he considered the liquid to contain seed. Peyssonnel understood that the writings of Robert Boyle on the subject were based on material sent in by his correspondents from East India, Madagascar and other distant countries.[14] Very important examples to Peyssonnel were the expeditions carried out by the Italian Paolo Boccone, who had made observations at sea near Malta and Sicily while working closely together with local fishermen.[15]

Observations at Sea and Experiments in the Laboratory

With the discovery of "coral flowers" in 1706 by the naturalist Luigi Ferdinando Marsigli science took a step forward. Naturalists thought they finally had evidence that coral was of botanical origin. The novelty introduced by this "virtuoso" was the systematic study of coral in a "living" state.[16] Marsigli travelled to Provence and accompanied coral fishers offshore on numerous occasions. When the coral branches came out of the water he deposited them in a bowl filled with seawater so they would survive. Returning to his workplace in Cassis, he observed them under the microscope and then subjected them to all sorts of experiments. For example, he deposited coral branches in distilled seawater as well as in liquids with varying degrees of salinity in order to see whether this would have an impact on the growth of coral. Furthermore, he distilled chemical components from the bark of coral, in search of substances that were typical for plants. Finally, he presented his observations and the results of his analysis to colleagues at the Société Royale des Sciences in Montpellier and at the Académie Royale des Sciences in Paris. These scholars published them in the series of dissertations of their respective academies or in the *Journal des Sçavans.*

Jean-André Peyssonnel followed exactly the same track. From 1719 onward he participated in coral fishing near Marseilles.[17] He spent days at sea, mostly in the *calanques* near Mazargue, where he became fascinated by the complicated work of loosening coral from the rocks. The fishermen used small boats for this. It was hard work. They had to deal with the unpredictable dangers of the sea. At the same time they had to handle heavy tools made from wood and equipped with weights and nets. These tools were lifted down to the bottom of the sea. The nets got entangled with the coral, but only with the help of heavy manpower could the branches be dislodged from the rocks and pulled to the surface. Peyssonnel's confrontation with the physical aspect of coral fishing was the start of his research in this field.

His place of residence facilitated the exchange of views with fishermen. A new world opened before his eyes when they pulled living coral out of the sea. Surprised by the fact that no one had ever talked about coral's variety of forms and structures, he resolved to make a complete study of what he called "*plantes marines*".[18] Inspired by Marsigli, Peyssonnel kept freshly fished coral alive in a bowl. He obtained it by participating in fishing trips organized by the coral fishers who lived near his house. This allowed him to investigate "coral flowers". At first he shared the opinion that coral is of botanical origin. The flowers were one proof, but also other external characteristics convinced him coral really was a plant: the presence of roots, a trunk, ligneous fibres, and so on.[19] He communicated his results in the same way as Marsigli had done. In 1723, he presented to the Société Royale des Sciences of Montpellier "the botanical observations I have on coral, on its milk, and on its flowers".[20] The same year Peyssonnel turned to the Académie Royale des Sciences in Paris, where he was in contact with Antoine de Jussieu[21] who, on 20 March 1723, presented to his colleagues Peyssonnel's *Dissertation sur l'Orange de Mer*.[22]

This was the time when observation in the field of natural history was systematized, with a more repetitive routine that came to complement experimentation. Peyssonnel and his contemporaries were convinced that repetitive observation might lead to the discovery of new "systems" in nature, but as one can see from Réaumur's critique on Peyssonnel's claims (see p. 276), this repetitive routine in itself was not enough to produce convincing arguments.[23] This was also the period when chemistry was in full transformation. Marsigli had already applied chemical analysis to "products of the sea" in order to understand the nature of their substances. Especially the "juice" or "milk" of coral fascinated this researcher. Peyssonnel was influenced by Marsigli's practice and also turned to chemical experiments. Reading the book of Johann Ludwig Gans, *Corallorum Historia* [...], published in 1630, Peyssonnel was stimulated to continue research in this direction. In its quest to find the components of matter, the Académie in Paris had established expertise in this field over many decades. In the late seventeenth century, Paracelsian principles still dominated chemical thought. Gradually, chemists tried to find the fundamental laws that regulated the transformation of matter.[24] More recently, influential members such as Lemery and Geoffroy had carried out chemical analyses in a specialized laboratory, increasingly focusing on plant chemistry. Peyssonnel read their work and copied their methods almost to the letter. By means of distillation he thought he had found "the [...] principles ordinary to plants".[25] He sent the results back to the Académie in Paris. As soon as he could leave Marseilles he travelled to Paris where the members of the Académie received him on 8 July 1723.[26] One might assume that Peyssonnel worked in the laboratories of some of the members of the Académie specialized in chemistry. One month after his arrival in the French capital, the chemist Etienne-François Geoffroy appointed Peyssonnel as his correspondent.[27]

A Pioneering Expedition to North Africa

Peyssonnel went to Paris in the first place to present his work to scholars. But he also wanted to verify if his intellectual capacities could convince people to give him a real job. He requested the king's court to create a chair of naval medicine at Marseilles, and he also asked to be appointed in this position. His efforts were to no avail.[28] He had to look for another solution to make a living. Stimulated by Bignon, who was one of Peyssonnel's protectors, he proposed he be put in charge of a "scientific" expedition to North Africa. Peyssonnel himself saw his voyage as a "royal commission" facilitated by his protector. The goal was to visit the coast of Barbary and to collect flowers, seeds, even complete specimens of plants. It must be emphasized that it was not a voyage by explicit "order of the King"—it was not the king who "commanded" the undertaking—but a voyage facilitated by the French authorities. Indeed it was Jean-Frédéric Phélipeaux, Count of Maurepas—the minister in charge of the French Navy—who intervened at court to provide him with passports and letters of introduction.[29] The minister explicitly said Peyssonnel had the obligation to make observations in the field of natural history. Both the Académie and eminent specialists such as the geographer Delisle and the botanists Chirac and de Jussieu gave him specific instructions. With the official documents in hand, Peyssonnel returned to Marseilles and secured himself the support of the Chamber of Commerce, an institution that maintained excellent contacts with agents throughout the Mediterranean.[30]

In May 1724, Peyssonnel arrived in Tunis where he made contact with high-ranking officials, visited several places and circles, and began to write letters dealing with various subjects: geography, history, the customs of the country, antiquities, and of course nature. He visited the coast as well as the interior, where he enlisted the help of various intermediaries: fishermen, guides, consuls, merchants and so on. One could say that his interests were of an eclectic nature. The letters in which he described his findings, and which were addressed to Bignon, immediately started to circulate: Bignon forwarded them to de Jussieu, Delisle and so on.[31] One needs to add that de Jussieu and Chirac also received botanical specimens, some of which ended up in the Jardin du Roi. These included plants that might be useful to France's industry, for example for dying fabrics. Unfortunately for Peyssonnel, the Count of Maurepas did not want to give him financial help to continue his research. He claimed Peyssonnel had left without a direct order from the king.

It was a matter of subtleties. As he conceived it, Peyssonnel travelled on his own account.[32] The king had indeed not issued a formal "command". The authorities had just facilitated the voyage under the pressure of Bignon and the Académie. Moreover, his instructions only included work in the field of natural history. But Peyssonnel had taken the liberty of travelling far inland, where he dedicated his time to archaeology and local history. Nobody had explicitly asked him to do that. Furthermore, he thus spent a fortune. He also seems to have been more interested in studying things that came out of the sea than in collecting plants and seeds for the king's garden. Thus, the minister's underlying message was that if Peyssonnel had financial problems, this was mainly because he had been so reckless as to neglect the basic content of his instructions. In short, it was his own fault, and he had to live with the consequences. Nevertheless, the explorer-naturalist continued his work for a while. In fact he spent more time in the regency of Algiers than in the regency of Tunis.

At the Académie Royale des Sciences, Peyssonnel's scholarly merits became the subject of a debate. On 21 July 1725, the pensioners and associates chose Bernard de Jussieu and Jean-André Peyssonnel to be proposed to the king to fill the vacant position of assistant botanist.[33] But the authorities were embarrassed by the financial position of Peyssonnel, and thus the king gave this position to Bernard de Jussieu.[34] Meanwhile Peyssonnel had moved from the regency of Tunis to that of Algiers, where he started new research activities at sea near La Calle. In February 1725, he participated in coral fishing near Bastion de France, which allowed him to observe coral in its living state. Coral fishing was a real industry here. It was organized by French licence holders on a scale comparable to that in the seas of Provence. When confronted with the opportunity to continue his research on marine productions, Peyssonnel did not hesitate for one minute. With the consent of local officials he was allowed on board the boats of the coral company. Again, this work was not described in his official instructions. Peyssonnel's activities resembled those he had accomplished in Marseilles. Again he worked in collaboration with the coral fishers. But now he had to draw the conclusion that coral was "produced" by "insects". He again found "coral flowers" but, as he made time for a careful observation, he was now sure to observe the "nettle" inside, which he held responsible for the formation of the stony skeleton.

He disposed of a "laboratory", installed at his place of residence, equipped with a microscope and some chemical utensils. Experiments gave him proof of the animal nature of the organism: "I made coral bloom in vases filled with sea water and observed that what we believe to be the flower of this so-called plant is in reality an insect similar to a small nettle […] this insect thrives in water and closes in air or when I poured acid liquors on it or [when] I touched [it] with the hand, which is common to fish and to testaceous insects of a slobbery and vermicular nature […]."[35] The chemical experiments confirmed that coral shared the characteristics of other animal organisms. With his expedition to North Africa, Jean-André Peyssonnel was able to solve the riddle of the true nature of coral, but due to a lack of resources he had to return to France. Financial problems continued to plague him. The voyage had ruined him and, from the correspondence with Bignon, one can deduce that nobody was really eager to intervene on his behalf.[36] In addition Peyssonnel would have to face opposition from the Académie Royale des Sciences.

Power and Authority

For a naturalist's career under the ancien régime the system of patronage and protection was essential. This is why Peyssonnel was placed under the protection of Bignon, who brought him into contact both with the ministerial power of the Count of Maurepas and the scientific authority of the Académie Royale des Sciences. But commitments were not always stable. As noted above, the minister had first facilitated the expedition to North Africa but then withdrew financial support. The Académie appreciated Peyssonnel's research but, faced with his views on the nature of coral, soon changed its position. After his return, Peyssonnel had incorporated the results of his research into new writings. As usual, he communicated them to Bignon, explaining his belief that coral is a form of animal life. He presented a complex classification of marine productions, including coral, pores, "madrepores", "litophytons" and so on. The common element of all these creatures was the "tubes" or "cells" he had seen under the microscope in North Africa. In his opinion, this organized "inner structure" housed the "nettles", that is, tiny living creatures, sometimes also designated by the name of "fish".[37] Moreover, chemical experiments pointed to animal characteristics. Bignon forwarded the dissertations to Antoine de Jussieu, requesting him to assess them thoroughly. In a response sent directly to Peyssonnel on 11 March 1726, de Jussieu showed his doubts.[38]

Around June 1726, Peyssonnel's dissertations were in the hands of René Antoine Ferchault de Réaumur, a member of the Académie who was considered an authority in the field of small invertebrate animals. As a matter of convenience, these were all called "insects".[39] Réaumur showed his disdain for Peyssonnel's writings and refused to acknowledge the accuracy of their content. Réaumur's anger was directed against Peyssonnel's claim that his observations proved the correctness of his "system". The statements in his dissertations, Réaumur thought, did not allow such a generalization. Indeed, Peyssonnel did make use of the upcoming term of "system" to give more weight to his findings. He argued that what he had found out about the internal structure of coral through his exploratory work—the tubular structure and the presence of tiny animals—was very similar to what he had discovered about other marine organisms. Thus, in his opinion, his research could form the basis for a complete revision of the natural history of the sea. In Réaumur's estimation, such audacity was inappropriate. So Réaumur answered:

> I agree with you that so far nobody has dared to look at coral […] as the work of insects; one can not dispute the novelty and singularity of this idea, but naturally I confess I find it hardly possible to establish this in the general manner as you have done […] [T]o me corals never seem to be built by nettles […] in the way you accept that they work […], I do not believe, with regard to coral, that there is another system to propose than the one I once talked about with you; that only their bark is a proper plant, and that this plant makes a stony material that forms the stem necessary to support it; thus I see all the difficulties about the lack of organization in coral disappear.[40]

Réaumur slammed the door in his face. Peyssonnel found his career paralysed. Réaumur decided to settle the case in order to prevent further discussion. He wrote his own dissertation about the subject and presented it to his colleagues in August 1727.[41] In the following two decades his point of view was seen as the only one deserving the status of authority. It was soon published in the series of the Académie.[42] Réaumur destroyed Peyssonnel's arguments. The problem was that Peyssonnel had neglected to give an explanation for the stony structure of coral. In Réaumur's view, Peyssonnel focused on the "insects" he claimed to have seen, but these "insects" could well be just "visitors" or occasional residents of a plant with a stony structure. How the structure itself had come into being, Réaumur argued, Peyssonnel did not explain, not even with his experiments. To Réaumur it was simple: coral is a plant with an internal structure of "canals" and "cells" in which—due to filtration through the bark—a crystalline "juice" circulates towards the extreme parts of the branches. The "juice" petrifies progressively, except at the extremities, implying that coral can grow upwards. In Réaumur's opinion this "plant" is a vehicle for a mineral "fluid". Next to the view of such an eminent mind, the observations and experiments carried out by a mere "correspondent" of the Académie were considered next to nothing. Hence, coral remained in the plant kingdom.

Rehabilitation

His reputation weakened, kept at a distance by royal power and without a job, Peyssonnel had to seek his chances elsewhere. On 19 December 1726, Peyssonnel was granted the position of *médecin du Roi* on the island of Guadeloupe.[43] He must have seen it as a second-rate job, but the salary allowed him to pay his debts. He decided to start a new life in the French

West Indies. He settled, married and they had children. His life as a physician was a tranquil one. Guadeloupe was a dull place at the edge of the French colonial network, only of interest to people who invested in sugar, coffee or the African slave trade. Contact with Europe was limited and, due to the isolation, Peyssonnel sank into oblivion in France. However, Peyssonnel remained active. Even if he had no opportunities to share the results of his research with academic peers, Jean-André investigated the whole island. His research activities resulted in an impressive series of studies on various subjects. The French authorities ordered him to investigate the health problems of a large number of slaves on the island. This led to the first systematic investigation of leprosy in the French West Indies, carried out in 1728 and a second time in 1748. Peyssonnel also was interested in volcanology and climbed La Souffrière, the highest mountain on the island. He wrote about climatology, botany and marine organisms living in the Caribbean Sea. His writings on coral confirmed what he had seen earlier in North Africa, and he complemented his findings with new experiments on sponges, algae and other organisms found on beaches or fished out of the sea.[44]

In Europe, the practice of science evolved, first by focusing more on the microscopic investigation of freshwater organisms, second, by sending researchers to coastal areas where they carried out observations and experiments on living marine productions. Thus, after many years, attitudes towards Peyssonnel's work began to change. Immediately after Réaumur had published his own dissertation on coral, his views found their way into the pages of the ultimate European bestseller in the field of natural history of the 1730s: Abbé Pluche's *Le Spectacle de la Nature*.[45] But in the third edition of this work, published in 1742, the author sings a different tune, describing corals as animal organisms, in short, as "insects".[46] How can one explain this U-turn? In the early months of 1741, Réaumur received a letter and some specimens from Abraham Trembley, a naturalist from Geneva who had moved to Holland. There he discovered aquatic organisms with the characteristics of plants in a freshwater pond. After a more attentive examination, Trembley understood these creatures had "arms" and possessed the property of regeneration. When he split one of them, the two parts of the creature seemed to survive on their own. It was a spectacular discovery with philosophical consequences. The materialists now asked in a loud voice whether or not the Soul too was "divisible".[47] Trembley himself fed the debate. All over Europe he requested colleagues to repeat his experiments.[48] This discovery also would deal a serious blow to the pre-formationist theories, which emphasized that regeneration of body parts in small animals can be explained by the presence of germs containing a pre-formed body part.[49]

Thus, in 1741, the issue was discussed at meetings of the Académie in Paris. Réaumur executed the scissions on the organisms he had started to call "polyps" and which are now classified with the hydra. Trembley's *Mémoires pour Servir à l'Histoire d'un Genre de Polypes d'Eau douce, à bras en Forme de Cornes* would not be published earlier than 1744, but some researchers already understood the implications of this "problematic" discovery and had taken things in hand themselves.[50] The botanist Bernard de Jussieu—Antoine's brother—understood that certain marine organisms had to be re-examined as a result of the questions provoked by Trembley's experiences. In September 1741, he took a simple initiative, rarely imitated by his colleagues at the Académie: he left Paris for a trip to the beaches of the Atlantic in order to work with specimens freshly taken from the sea.[51] Bernard de Jussieu explored the coasts of Normandy. A microscope and a magnifying glass in hand, and equipped with a series of glass bowls, he started to collect various organisms. He discovered in a number of these organisms "small tubes, each of them containing a tiny insect".[52] He returned on several occasions, always finding confirmation of his earlier discovery. He now started to use the name "polyp" in a marine context. When he had moved at least four organisms from the plant kingdom to the animal kingdom, Bernard de Jussieu made up his mind and came to a stunning conclusion: Peyssonnel had it right.[53] What is important here is that the discussion on Trembley's "polyp" offered a case where an organism that had always been classified as a plant moved to the animal kingdom. The case gave the impetus to a reconsideration of the hypothesis on the nature of coral as the latter apparently also included "small insects" comparable to polyps. It was the first step towards a rehabilitation of Peyssonnel. In the meantime, Réaumur had sent Jean-Étienne Guettard to the coast of the Poitou region. He too confirmed the presence of polyps in the Atlantic Ocean. Seaside exploration again offered the key to a final breakthrough in research.

At the end of 1741 Réaumur understood something important had occurred, and consequently a revision of the old theory was needed.[54] He integrated the knowledge produced by his colleagues into the monumental multi-volume publication on insects he had already been preparing for some years. Again the academic elite was obliged to accept the new standard Réaumur established in the sixth volume of *Mémoires pour Servir à l'Histoire des Insectes*, published in 1742: coral is a shell-like structure that hides in its interior colonies of polyps. Réaumur seized the opportunity to correct the errors of the past and to rehabilitate Peyssonnel to some degree. Finally, scientific authorities accepted Peyssonnel's findings based on exploratory knowledge, collected out of sight of the circles of learning in Europe. The new ideas found their way to the 1742 edition of Pluche's encyclopedia as well as into Buffon's monumental synthesis *Histoire naturelle, générale et particulière, avec la description du Cabinet du Roy.* In the first volume of this work, published in 1749, the author described the "old marine plants" as little animals resembling "shell fish". He confirmed that Peyssonnel had been the first to discover the animal nature of coral. The new concepts were quickly distributed to all corners of Europe. One can even observe a progressive emergence of a specialized domain of research: marine biology. It would attribute a privileged position to a new subject of research: the zoophytes. The Italian Vitaliano Donati studied the small organisms living in the Adriatic Sea and confirmed in broad lines the conclusions of his French colleagues. Donati communicated his discoveries to the Royal Society of London, where the Fellows published them in the *Philosophical Transactions.*[55] Donati's studies also formed the basis of the article on coral written by Louis Jean-Marie Daubenton for Diderot and d'Alembert's *Encyclopédie, ou Dictionnaire raisonné des Sciences, des Arts et des Métiers.*[56] In Uppsala, Carolus Linnaeus integrated the new knowledge of coral in his classification, in particular in the fourth edition of his *Systema Naturae* (1744).[57] In 1748, William Watson had the occasion to explore the coasts of Sussex together with Trembley. In this way, the interest in polyps also increased in England.[58]

Unfortunately Peyssonnel's own texts were no longer circulating, but changes were under way. Being aware of the interest shown in his work, but clearly not feeling at ease with the world of learning in France, in 1751 Peyssonnel sent a manuscript to the Royal Society in which he discussed his views on coral. He had a feeling that in France "some lovers of natural history usurp my work and my discoveries [...]".[59] On 7 May 1752, his manuscript was presented to the Fellows. They decided to publish it in the *Philosophical Transactions.* In the coming years almost a dozen papers written by Peyssonnel were published in the same series. Most were just short English abstracts of much longer French dissertations. In 1755, Peyssonnel decided to undertake a long voyage from the West Indies to France. He wanted to settle some family affairs in Marseilles but he took the opportunity to restore his scientific fame. He renewed contacts with scholars in Paris and London and made an appearance at several academies in the provinces. Accepted as a Fellow of the Royal Society he also became a corresponding member of the academies of Rouen, Lyon, Angers, La Rochelle and Bordeaux.

An important day in Peyssonnel's life was 28 January 1756, as he was invited to give a lecture at the Académie in Paris. There are no sources that explain the reason for the invitation. Probably it was a combination of things. Some of his friends—Buffon and Daubenton, who had published favourable articles on his research—were academicians. After an absence of thirty years, they were undoubtedly happy to receive Peyssonnel in their company. Now that his name was cleared, even Réaumur had no reason to oppose his presence. On the contrary, it was an opportunity to show that the process of reparation was completed and that the knowledge deduced from observations and experiments during Peyssonnel's voyages had finally entered the academic salon. After all, Peyssonnel was officially a corresponding member of the Académie. And it is certain the academicians were interested by what he had achieved during his long residence in the West Indies. He spoke about a subject dear to him: the currents of the sea. Not those he had investigated in the Mediterranean, but the ones he had analysed in the Caribbean.[60]

Conclusion

One may conclude that the knowledge produced by Peyssonnel during his trips, expeditions and journeys, and through interaction with fishermen, can be brought back to a story about the circulation of knowledge in academic circles. In each local context this knowledge received new appraisals, sometimes ending up in disputes and confrontations between scholars, as the examples of the Académie Royale des Sciences and the Royal Society have shown. Peyssonnel's case reaffirms that knowledge production through exploration is primarily a localized process. The most important space of knowledge that functioned as a localizing and dynamic element was probably the fishermen's boat, where he directly discussed living matter with holders of specialized knowledge of sea life. But the home laboratory was also a place where experimentation led to "illumination". Here he could put different knowledge forms together, as in an experimental setting. As knowledge was transformed, translated, revised and retranslated, it was either enriched with new meanings or eroded by the separation from the original environment. The character of the journey—a trip made on the basis of his own free initiative or a formal expedition ordered by the state—left its mark both on the knowledge that was produced and on the way this knowledge was appraised in the scholarly world. On the other hand, the Peyssonnel case also shows that the production of knowledge on the natural history of the sea was a process in which experimentation received growing attention in the course of the eighteenth century, especially when chemistry intervened and started to act upon botany and zoology.

Peyssonnel returned to Guadeloupe in 1756. He died three years later, probably quite satisfied with the fact that his ideas had finally received the blessing of scholars in Europe. Nevertheless, one has to admit that, although he was rehabilitated, his work was already in the process of becoming outdated as a result of work produced by a new generation of naturalists. Scholars such as John Ellis or Daniel Solander applied a more systematic approach and incorporated the knowledge of coral in a wider context, taking as a basis the binomial classification system designed by Linnaeus, laying the foundations of a "global" approach that would lead to the studies on the structure and distribution of coral reefs conducted by Charles Darwin.

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CHAPTER 4

Japanese Ichthyological Objects and Knowledge Gained in Contact Zones by the Krusenstern Expedition

Yuko Takigawa

Introduction

Since the beginning of the age of discovery in the fifteenth century, numerous worldwide expeditions from European countries brought massive amounts of materials to Europe as commercial goods. These included both natural and artificial items that attracted Europeans' curiosity and wonder.

The eighteenth century is widely known as the age of natural history. Once Linnaeus had introduced the system of scientific classification based on binominal nomenclature, it became both a widespread and ambitious goal for enthusiastic scientists and voyager naturalists to classify species from the entire world. An increasing amount of natural history items and records of observations were collected and brought back to Europe. Based on these materials, new species were described, classified and published according to the Linnaean system of taxonomic classification, which resulted in new scientific knowledge of the world. Even before the Linnaean system was introduced, many scholars had tried to classify and systematize a

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wide variety of species. Yet it was Linnaeus' binominal system that allowed for easier recognition and identification of distinctive species by introducing a two-part name: the first part indicates the *genus* to which the species belongs, the second part classifies the *specific* species within the genus.

Scientific curiosity alone, however, was not the predominant purpose of expeditions to the unknown world; there were also political and economic motives. All the same, from the late eighteenth century, worldwide expeditions undoubtedly became great opportunities for activities such as observing, collecting, examining and classifying first-hand materials according to the Linnaean system to document biodiversity. The Krusenstern expedition (1803–1806), the first Russian circumnavigation, was one of these Western scientific missions to the East in the early nineteenth century. Although it is a well-investigated enterprise, its contributions to science and especially to ichthyology, have remained almost unknown until now. Among other scientific achievements, this chapter sheds light on the investigation of Japanese fish *during* and *after* the expedition, by investigating the role of three groups of people involved: the Western expedition members who managed to obtain local objects and knowledge to pursue their scientific missions *during* the expedition; Japanese locals, who actually collected Japanese fish and offered local knowledge to the Western visitors in Nagasaki; and Western biologists who examined the objects brought back from Japan and produced new scientific knowledge of Japanese fish *after* the expedition. I will show that the expedition consisted of multiple layers of *experiments* involving Japanese, Russians, Germans and French across different times and spaces. Production of scientific knowledge on Japanese fish in the early nineteenth century, therefore, took place in an experimental social space: the isolated and controlled situation of the expedition while in Japan.

Historical Relations Between Japan and Russia

Since the late eighteenth century, the Russian Empire tried to make contact with isolated Japan whenever she returned Japanese castaways with missionaries in order to establish commercial relations with Japan. At that time, Japan had closed herself off to European countries, apart from Holland and the Dutch East India Company, which monopolized trading with Japan. The Russians' first contact with Japan was made in 1792. Adam Laxman (1766–1806?), a Finnish-Swedish military officer and son of the famous natural historian Erik Laxman (1737–1796), was commissioned by Catherine the Great as the first official Russian ambassador to Japan.[1] Laxman sent back three Japanese castaways, including Daikokuya Kodayu (1751–1828), who had stayed in Russia since he and his crew were shipwrecked in 1782. The Russian envoy arrived in Nemuro, the easternmost point of Hokkaido, Japan. Laxman had tough negotiations with *Bakufu*, the Japanese feudal government, for about eight months, aiming to establish commercial relations with Japan. Although Laxman did not achieve the desired result, he brought back a Japanese permit, "Shin-pai", an official letter of permission allowing one Russian ship to visit Nagasaki for trade negotiations with Japan.

In 1799, Catherine the Great's son Tsar Paul I (1754–1801) chartered the Russian-American Company to promote Russia's commercial and economic interests in East Asia, the North Pacific and in Alaska to expand Russian colonial territories in America.[2] For this reason, Russia needed ports and bases in East Asia to secure food and fuel supplies for the transfer to America. Thus, commercial relations with Japan became all the more important.

Following Laxman's visit, Russia's second contact with Japan came about on the occasion of Russia's first circumnavigation expedition from 1803 to 1806 under the command of Captain Krusenstern (Ivan Fedorovich Krusenstern, 1770–1846). The Nagasaki permit given to Laxman allowed one Russian ship admission to Nagasaki for trading negotiations.[3] Thus, Tsar Alexander I (1777–1825), Paul's successor on the Russian throne, decided to send four Japanese castaways back to Nagasaki with the Krusenstern expedition to facilitate trading negotiations. Nikolai Rezanov (1764–1807), chairman of the Russian-American Company, was commissioned as Russian ambassador to Japan[4] and given authority to negotiate with Japan. Thus, Russia's first round-the-world expedition was conducted to pursue commercial and economic interests. In 1803, two ships, the *Nadezhda* and the *Neva*, set sail from Kronstadt, a Russian seaport located near the head of the Gulf of Finland, also known as the fortress of St Petersburg. By way of Copenhagen, Falmouth, Tenerife, Santa Catharina, Brazil, Cape Horn, the Marquesas Islands, Hawaii and Petropavlovsk in Kamchatka, the *Nadezhda* sailed to Nagasaki, whereas the *Neva* did not head for Japan.

Scientific Mission of the Krusenstern Expedition

Although one of the primary objectives of the Krusenstern expedition was to establish trade relations with Japan, it also involved scientific investigations in and around Japan as well as throughout the whole journey. With such a mission, Russia was clearly aware of the academic importance of natural history in Japan.

Before the Russian ships departed from Russia, A. F. Sebastianov (1782–1821), a member of the Russian Academy of Sciences, gave detailed instructions for scientific observations during the voyage, which were supposed to be followed by the natural historians on board.[5] Sebastianov emphasized four areas as potential places of natural scientific interest: (1) Cape of Good Hope; (2) Japan; (3) Kamchatka and a numbers of islands which are located between Kamchatka and America; and (4) North America. In his instructions, Sebastianov referred to famous natural historians who had previously travelled around the world, such as Carl Peter Thunberg (1743–1828) and the father and son Johann Reinhold Forster (1729–1798) and George Forster (1754–1794). Sebastianov pointed out that because these natural historians joined commercial voyages, their scientific missions were limited to geophysical observations. He assumed that they had not had enough time for scientific observations and natural historical investigations at the places where they stopped on their voyages.[6] Although all three of them were naturalists and natural historians, they were not commissioned in this capacity. Thunberg, for instance, was employed as a medical doctor and had to provide medical care during the mission. Therefore, Sevastianov emphasized the importance of conducting scientific observations and investigations at the respective destinations.

Thunberg, a Swedish physician, disciple of Linnaeus and famous natural historian himself, stayed in Japan from 1775 to 1776. His collections contain the first zoological and botanical specimens from Japan that were brought back to Europe and actually used for biological classification based on the Linnaean system, although the Dutch physician Martinus Houttuyn (1720–1792) beat Thunberg in describing and classifying the new species by examining Thunberg's Japanese specimens. Thunberg also examined the botanical collections from Japan in London, which had been brought back by Engelbert Kämpfer (1651–1716), compared them with his own collections and classified them according to the Linnaean system that was still unknown to Kämpfer.[7] As one of the first natural historians who visited Japan, Thunberg and his works attracted much interest among European intellectuals. Sebastianov, for instance, was fascinated by the Japanese fauna and flora on which Thunberg reported. He confirmed that there was still potential for scientific investigations in Japan, which was inaccessible for most Europeans.

Sebastianov's instructions encouraged the participating natural historians to engage in scientific observations and in collecting both natural and artificial materials during the entire voyage. The instructions covered almost all scientific and cultural fields such as geography, zoology, botany, oceanography, meteorology, anthropology, ethnology and so on.[8] Sebastianov listed fourteen practices to be engaged, including: to collect scientific objects such as specimens from land and sea, to make notes of the date, time, provenances and usage of the collected items for scientific field records, and to classify them according to the Linnaean system.[9] Drawings of the collected objects, as well as of landscapes and ritual practices which the expedition encountered during the voyage, were to be produced.[10] Sebastianov also requested that the acquired natural history materials should be transferred to the Russian Imperial Cabinet. He warned the members of the expedition not to keep any of the collected items for themselves or take them for others.[11] However, not everyone followed his instructions. Georg Heinrich von Langsdorff (1774–1852), a German-born natural historian and participant of the Krusenstern expedition who was later in the service of the Russian Empire, for example, kept his finds for his own collection and donated some of the fish specimens to the Berlin Museum für Naturkunde. Langsdorff's fish collection will be discussed later.

Judging from Sebastianov's guidelines, the Krusenstern expedition predominantly aimed to investigate natural historical phenomena during the voyage. Three natural historians joined the expedition: apart from the already mentioned Baron Langsdorff there was Wilhelm Gottlieb Tilesius (1769–1857), a physician, natural historian and explorer from Thuringia, and the Swiss astronomer, physicist and mathematician Johann Caspar Horner (1774–1834).[12] During the rule of Catherine the Great (1729–1796), it was common to invite well-known intellectuals, artists and scientists from all over Europe to Russia. This might have been the reason for the fact that the expedition left Russia without the participation of a Russian naturalist.

Contact Zones Between Japanese and Europeans

For the participating natural historians of the Krusenstern expedition as well as for the biologists who examined the Japanese finds in Europe and produced knowledge later, the successes of the enterprise solely depended on obtaining natural history objects in Japan. In view of the extremely difficult conditions of the voyage, this was a heavy task. It is crucial to discuss exactly what happened in Nagasaki, where contacts between Europeans and Japanese took place, what efforts were made to solve these difficulties and how the Japanese natural history objects were obtained. The following descriptions are based on both historical literature sources and real objects brought back from Japan to Europe. As an analytical tool, Mary Louise Pratt's concept of "contact zones" can be applied for this specific case in Nagasaki.[13] As will be described below, Nagasaki can be interpreted as a "contact zone"; these are, according to Pratt, "social spaces where disparate cultures meet, clash, and grapple with each other, often in highly asymmetrical relations of domination and subordination".[14] I will discuss this in detail later.

Under the command of Captain Krusenstern, the *Nadezhda* arrived in Nagasaki in September 1804. Waiting for a reply from *Bakufu* (Japan's feudal government), the Russian delegates had to stay in Nagasaki for more than half a year, from 1804 to 1805. The Japanese harbour town was also the base of the Dutch East India Company and its trading port on the artificial island of Dejima. However, *Bakufu* forebade any contact between the Dutch merchants and the Russian travellers. Some of the Russian delegates were ordered to stay in the area of Umegasaki (*Megasaki* in Russian), without establishing contact with the Dutch. Only when Rezanov fell ill, he insisted on being accommodated on land. The rest of the Russians had to stay on board their ship *Nadezhda*. Either way, they were not allowed to act on their own account, which meant that any form of scientific activity was severely restricted. Eventually Langsdorff and Tilesius managed to find a solution: they were successful in negotiating with their Japanese food suppliers. Langsdorff wrote in his travel account as follows:

> All means of exerting ourselves for the promotion of science and knowledge were precluded, so that the mind grew contracted for want of freedom and a wider range in which it might expand itself. The fish alone brought to us as provisions afforded an object for scientific investigation, and by secret promises we at length prevailed upon our caterer to bring us every time different kinds of fish: with these, Counsellor Tilesius and myself sometimes entertained ourselves very agreeably.[15]

The research I have conducted from 2010 to 2013 on the Langsdorff fish specimens has revealed that among the various kinds of fish in this collection not all species were edible or commonly used as food, including those with only little meat or regarded as being inedible or even poisonous, such as the puffer fish. Therefore, contrary to the official excuse of "fish for food", the Europeans did indeed receive (and collect) any kind of fish that they were interested in.

Hermann Ludwig von Löwenstern (1771–1836), who joined the Krusenstern expedition as a lieutenant, recorded lists of items purchased by the Russians. A partial list of provisions from 6 December 1804 to 5 April 1805 contains a few items for natural history collections, such as eight pieces of "large snails" and twenty-four pieces of "various birds".[16] The item "fish for stuffing" was clearly distinguished from fish for food, which was recorded as "fresh fish" and "salt fish". The items labelled as "fish for stuffing" weighed 128 *Kati* (Japanese unit of weight at that time) or 4 *Pud* and 32 *Pfund* in Russian units of weight,[17] which is the equivalent of about 77–78 kg. On the basis that a sea bream of 30 cm in length weighs about 800 g, 77 kg would be as many as 97.5 individual fish. Even though the size and weight of fish can vary depending on different species and individual fish, this total amount of weight shows that a significant number of fish were provided to the Russian delegates for scientific purposes.

Löwenstern wrote in his diary that Japanese interpreters also gave fish and other animals to the natural historians. According to his entry of 20 February 1805, "Tilesius is being richly supplied by the untertolks with fish and all kinds of animals that he draws very skillfully."[18] Usually interpreters (= *tolks*) were keen on Western knowledge and information and therefore in favour of the foreign visitors, although they were actually not allowed to be in contact with them. It is quite understandable that they secretly tried to fulfil the requests of the Western scientists as much as possible, even going so far as to give them some small things, for instance, India ink, a couple of pictures, fans, tobacco pipes and so on.[19] It was, however, under risky conditions: "If anything were discovered, they probably would have paid with their lives."[20]

Supported by Japanese locals such as caterers and interpreters, Langsdorff and Tilesius obtained a vast variety of fish in Nagasaki. Many of them were preserved as stuffed specimens. Most of the scientific activities of the Western natural historians were recorded in a diary. For example, on 3 March 1805 Rezanov wrote that Langsdorff was given a thin, long and very mysterious-looking fish which was stuffed immediately. He assumed that it belonged to a previously unknown species which could now be described and classified.[21]

Interestingly enough, the Westerners' scientific activities greatly impressed the local people, including the four Japanese castaways on board the Russian ship. Even after their return to Japan, *Bakufu* initially refused to receive them so that they had to stay with the Russians in Nagasaki for six more months. Otsuki Gentaku (1757–1827), a well-known Japanese scholar of Dutch studies (*Rangakusha*) and contemporary of Langsdorff and Tilesius, published a book titled *Records of a Voyage Around the World* (*Kankai ibun*), based on interviews with the castaways who were sent back with the *Nadezhda* after *Bakufu* had reaccepted them after all. To them, the Western scientific practices such as stuffing fish and birds looked surprising and impressive, since these scientific procedures were not common in Japan at that time. They described them as follows:

> Fish, birds and botanical specimens, which had been requested, were brought to the building where the Russians stayed, and they were sketched in detail. Some made drawings, and some made bird specimens as if they were alive, after having them skinned, gutted, stuffed and put artificial eyes in. Among them, a copper pheasant looked as if it was going to jump into the air and fly away. Once the objects were brought into the building, every one of them was drawn. They even drew vegetables and asked their names, the recording person repeated it and added [it] to the drawings. Even if there was no drawing of a plant or an animal, they recorded all [the] names they heard. Not a single species, a single kind would be missed. Among them, a medical doctor called Langsdorff was especially good at both drawing and stuffing.[22]

On board the *Nadezhda* not only the naturalists such as Langsdorff but all other members of the expedition were keen on joining the scientific activities. The Japanese castaways told in the interviews that no one spent a day in vain. They helped with tasks such as surveying, recording, drawing and so on.[23]

At that time, Ota Nampo (1749–1823) was a well-known writer of humorous poems (*Kyoka*) and an intellectual who was in contact with many artists and well-educated Japanese. Born into a lower Samurai family, his interests included cultural and geographical knowledge of foreign countries, even though Japan was closed off at that time. Coincidentally, he lived in Nagasaki, where he worked as a civil servant, at the same time as the Russian expedition arrived there. Based on his first-hand observations, Ota Nampo wrote essays in which he described the Russian travellers in various aspects, including their scientific activities. His descriptions illustrate the communication between European expedition members in the temporary house, the painters and himself:

> The painters on the ship did nothing else than drawing, even when they were staying in the temporary house. One day, one of them showed me his drawings, which depicted almost everything he had encountered during the journey—the ocean, mountains, humans, animals and plants. He said that his drawings would be published after the journey. Among them, I am not

> quite sure from which country, were human portraits—one was of a man, who looked normal, but another one showed a woman with a long and tipped lower lip. I have also heard stories about many objects from various countries and even about small insects which were put in flasks filled with oil [...]. In Nagasaki, there was nothing that was not drawn. One day I visited them, and as soon as one of the painters saw me wearing an ankle long cotton raincoat, the painter rushed at me and sketched.[24]

Ota Nampo's essays show how curious and keen he was to learn more about the expedition. He also copied some of the sketches that were already mentioned, that is, the portrait of the South American woman with a long and tipped lower lip, and commented about it: "the drawings looked really strange".[25] By copying the pictures, Nampo used scientific practices himself and gained anthropological knowledge of South American people. In this respect, Nampo was probably aware of the fact that the Western expedition members used drawings as scientific tools to gain new knowledge about Japan in as many aspects as possible, although he did not mention this explicitly.

In addition to the records written by the European travellers, accounts from local people in Nagasaki illustrate that not only the natural historians but everyone available on board contributed to the scientific mission. They were keen on recording almost everything they saw. The painters' work, in particular by one artist, possibly Tilesius, was also described by both Japanese castaways and Ota Nampo. In other words, they meticulously followed Sebastianov's instructions on general scientific observations.

The European travellers had to stay in Nagasaki for more than half a year. It is fair to assume that they tried to collect as many scientific objects and as much information as they could throughout their stay. Nevertheless, even after a long wait and tough negotiations, the Europeans were not able to establish commercial and trading relations with Japan. *Bakufu* still wanted to maintain the strict national isolation policy, apart from relations with the Dutch. According to Nagasaki diplomats, it was a period of unusually high political tensions. After the expedition left Nagasaki without having established commercial relations, both Japanese and Dutch officials were relieved. In his essay on Nagasaki, Ota Nampo described an episode illustrating the political situation as follows:

> I was told that two days after the Russian ship had left, the Dutch captain invited the Japanese interpreters and held a huge banquet, offering Dutch food for the Dutch people and Japanese food for the Japanese interpreters. It

> went on until late at night; there was eating, drinking and singing, and at some point they even got naked and went wild. It seems to me that this was down to the fact that the Russians did not get the trading permission they wanted.[26]

However, as already mentioned, there is evidence that personal contacts between the members of the Russian expedition and Japanese locals did happen, despite the official ban, for instance when one of the European painters of the expedition, probably Tilesius, showed Ota Nampo his drawings, or when Japanese interpreters gave Langsdorff and Tilesius fish. After being brought back to Europe, these fish played an important role as natural history objects for producing scientific knowledge, which will be discussed later.

As described above, Umegasaki in Nagasaki was what Mary Louise Pratt has defined as "contact zone" between East and West, under "asymmetrical" political and diplomatic tensions between *Bakufu* and the Russian embassy: the former tried to keep the country closed up, the latter aimed to establish trading relations with Japan as a base for Russian trading aspirations in the Far East. Being restricted to a small area in Nagasaki, the Russian delegation had to follow Japanese orders. Yet although the Japanese government was fully aware of the expedition's political agenda as being commissioned by imperial Russia, it could not be refused due to representations from Russia. In the "contact zones" of Nagasaki, due to high asymmetrical tensions for both sides, the situation got rigid and unchanged. Meanwhile, the Western natural historians, however, could manage to obtain objects and to gain local knowledge through either official or unofficial exchanges with the local people. They transformed the local objects into scientific ones. In this context, it is worth emphasizing the roles and meanings of *expeditions as experiments* again; *expeditions* were the only available means to reach the "contact zones", where Westerners, despite some challenges, could gain natural history objects and local knowledge through exchange with the locals. The "contact zone" in Umegasaki, however, was also socially exclusive and hence functioned as a "placeless place" reminiscent of a laboratory.

Fish Drawings and Different Perceptions of Fish

Questions can be raised as to why the Japanese people in Nagasaki gave the foreigners different kinds of raw fish without worrying that they might gain too much knowledge and information about fish. Here, it is worth considering the differences between the views regarding natural history in Japan and in the West at that time. Traditionally, an interest in natural history was quite common in Japan. Not only were there many works published by scholars such as encyclopaedias and reference books, but also huge amounts of natural history drawings. Some of them were very elaborate and drawn in great and realistic detail, which shows that natural objects were carefully observed and studied.[27] Among them, the best-known work of fish images is *Shurinzu* from the middle of the eighteenth century. In addition to its artistic value, which has long been fully appreciated, its scientific value has only recently come to light.[28] *Shurinzu* contains pictures of more than 300 species of fish.[29] These works demonstrate that Japanese scholars of the eighteenth century were able to differentiate between different kinds of species and had a good understanding of biodiversity.

Although Japanese naturalists distinguished between different species and classified them into groups according to their morphological appearances, they did not use the same system as their Western fellow natural historians. Due to Japan's closed-door policy, the Linnaean system of classification was not fully introduced in Japan. Yet this was not the only reason why Japanese natural history differed from its European counterparts. Rather, it was because of a distinct theoretical approach that resulted in different classification systems. Nishimura has shown that natural history in Japan did not aim to establish a systematic way of identifying animals and plants by finding similarities among different species.[30] He has also pointed out that Japanese naturalists did not intend to develop a universal classification of species, for example by examining the structure of the inner organs.[31]

It is worth considering that fish had a different meaning and played a different role for the Japanese locals in Nagasaki than for the expedition members. The former regarded fish as an important food resource, because it was Japan's traditional and main diet since ancient times. People in Nagasaki must have had acquired a lot of practical knowledge on fish; they probably knew a wide variety of species, their names and habitats, the best seasons for fishing, different ways of how to cook fish, etc., as a result of their everyday life and work. Thus, it is fair to assume that locals, including officials and civil servants in Nagasaki, regarded fish also as the sole food for the expedition members. Whoever provided them with food must have been quite puzzled, maybe even embarrassed, when he learned that the Europeans only wanted raw instead of cooked fish. But in the end, for this person fish was merely food, cooked or uncooked. The expedition members, on the other hand, distinguished carefully between “fresh fish”, “salt fish” and “fish for stuffing”, as we have mentioned earlier. These differentiations, namely between fish as food and fish as scientific objects illustrate that the European naturalists were fully aware of their scientific mission of collecting and bringing back natural history items. Whereas the Japanese regarded fish merely as food, for the members of the expedition fish were objects to produce knowledge on Japanese natural history. Indeed, fish specimens brought back to Europe were actually used for scientific practices, and therefore played a significant role as media to gain ichthyological knowledge of Japanese species.

Natural Objects as Key for Producing Knowledge: The Langsdorff Collection

Although the delegates from Russia failed to establish trade relations between Japan and the tsardom, the fish specimens collected by the expedition members were brought to Europe. As Langsdorff wrote in his travel account, he could obtain Japanese fish in Nagasaki from food suppliers and interpreters. All scientific objects collected by the natural historians during the Krusenstern expedition officially belonged to the Russian Empire. According to Sebastianov’s instructions, the participating natural historians were prohibited from keeping any of the collected materials for themselves. Yet not all scientific objects obtained in Nagasaki ended up in the Russian Imperial Cabinet; some went to various academic institutions throughout the whole of Europe.

As a result of my research from 2010 to 2013, it has become clear that the Japanese ichthyological materials brought back by the Krusenstern expedition were widely scattered across Europe. Fish specimens, labelled as “Langsdorff collection”, have been incorporated in the German Museum für Naturkunde in Berlin, the French Muséum national d’Histoire naturelle in Paris and the Russian Zoological Institute of Russian Academy of Sciences in St Petersburg. In addition, the original drawings by Tilesius, which were used for the Krusenstern atlas, have been stored at Leipzig University, Germany. In other words, Japanese fish materials from the expedition to Nagasaki became part of the natural history collections in four institutions of three countries.

The Langsdorff fish collection, which was obtained during the Krusenstern expedition, played a significant role for European scholars studying Japanese ichthyology during the early nineteenth century. Even today, these objects are still of high research value as type specimens for modern ichthyologists, as will be discussed later in detail.

Langsdorff was clearly aware of the importance and rarity of his Japanese fish collection. According to a letter he wrote in 1821, it was a collection of mostly unknown Japanese fish and, to his knowledge, unprecedented in Europe apart from the collection in the Russian Imperial Cabinet in St Petersburg.[32] It is fair to ask how Langsdorff could avoid leaving his collection in Russia. Fortunately, some materials in the St Petersburg branch of the Archive of the Russian Academy of Sciences may present some answers to these questions. First, Langsdorff was not formally employed by the Russian state on his voyage with the *Nadezhda*. He had asked Rezanov for permission to participate in the expedition even without a salary or a contract. Archival documents show that Langsdorff was fully satisfied with these conditions: "If His majesty wishes to pay for my work, he can do it after the expedition."[33] This document suggests that, from the very beginning, Langsdorff was eager to maintain an independent position on board in order to conduct his natural historical research on his own. Other sources show that after leaving Japan, Rezanov asked Langsdorff to join his expedition to Russian America as a physician. On 12 June 1805, Langsdorff signed a contract with Rezanov in Petropavlovsk Harbour, which did not mention Langsdorff's collection.[34] For this reason, Langsdorff was under no obligation to hand over his collection to Russia. Langsdorff also mentioned that he had donated parts of his collection to Russia, yet not according to contract but of his own free will.[35] In the end, some specimens of the Langsdorff fish collection ended up in the Russian Imperial Cabinet in St Petersburg, the rest were sent to the Berlin museum, probably because he was German himself.

Among the objects of the Langsdorff collection in the three museums, fish specimens in Berlin and Paris are especially important since they include many *type specimens*. When a new species is described based on a specimen, this specimen becomes a *type specimen*, a representative of the species that acts as a name bearer. Therefore, specimens have been stored in academic institutions as reference objects for taxonomic classification and for examinations in order to conduct taxonomic revisions in the future. It was the French biologists George Cuvier (1769–1832) and Achille Valenciennes (1794–1865), who used the Langsdorff collection for their monumental work *Histoire Naturelle des Poissons* (1828–1850),[36] which describes many Japanese species, including previously unknown ones.

Therefore, many specimens of the Langsdorff collection were defined as *type specimens*. Apparently Cuvier and Valenciennes had no access to the Langsdorff collection in St Petersburg, since there is no reference to this location in their works.

According to the lists of the Historische Bild- und Schriftgutsammlungen of the Museum für Naturkunde Berlin, Langsdorff originally donated eighty-nine fish specimens to the Zoological Museum of Berlin in 1821.[37] My research findings confirm that from these eighty-nine specimens, thirty-six are mentioned in the registration book of the museum today, of which thirty-two specimens from Japan still exist (Fig. 4.1).

Another part of the Langsdorff collection is held in the Muséum national d'Histoire naturelle, Paris. Here, I could confirm specimens of five species stored in six lots of glass bottles or on tables. (one lot = a bottle, a box, a table, … contains only one species, but the numbers of specimens in one lot varied—it could be one, or it could be a few/some.) The third batch from the Langsdorff collection, which was not available to Cuvier and Valenciennes, is located in the Zoological Institute of Russian

Fig. 4.1 The Langsdorff collection in Berlin. ©ZMB/Yuko Takigawa

Academy of Sciences, St Petersburg, Russia. My research could identified twenty-nine lots in the registration book. Among them, fourteen lots were accessible. According to the registration book, a total of fifteen lots were missing: two lots were lost during the Leningrad blockade during the Second World War; twelve lots during a flood of the River Neva in 1924; and one for unknown reasons.

The Langsdorff collection has great scientific value based on, first, taxonomic classifications in biology, and, second, the history of science in the context of East–West exchanges in "contact zones". The Langsdorff collections in Berlin and in Paris include many sets of *type specimens* that are holotype, syntype and lectotype; their type status will be discussed in future publications on ichthyology. The Langsdorff collection is the second oldest Japanese fish collection, dating back to the beginning of the nineteenth century, after the Thunberg collection in the late eighteenth century. The fish specimens from Japan were extremely rare in Europe at that time, which was fully appreciated by Cuvier and Valenciennes. That explains why Cuvier and Valenciennes often used terms such as *japonicus* or *niphonius* when they named new species, clearly in order to indicate their origin Japan.

Making Use of Japanese Local Knowledge by Western Natural Historians

The Langsdorff collection not only consists of scientific objects, but also provides local scientific information, such as Japanese names. This evidence is useful for the discussion as to how local knowledge and information was offered and integrated into scientific works by the Western naturalists. Since Langsdorff and Tilesius were not allowed to collect fish by themselves, they had to obtain raw fish from food suppliers. Japanese interpreters also helped by giving them fish and other materials for their scientific investigations. In this context, local Japanese people acted as suppliers of scientific objects. Essays and records by Ota Nampo suggest that some intellectuals also supported the European naturalists in their scientific enquiries during their unofficial contacts. The fish list, which was copied from the Langsdorff list, illustrates that Japanese people also provided Japanese knowledge, for instance Japanese names of the fish they gave to the Westerners. In 1841 Johannes Müller (1801–1858) and Jakob Henle (1809–1885) described the ray fish *Urolophus aurantiacus* based on the Langsdorff collection in Berlin.[38] The description contained the provenance of the ray fish, namely the Gotō Islands, far from Nagasaki, which is an indication that Japanese people also offered information on the provenance or the fishing grounds when they passed on the ray fish. The Japanese names for the fish were also provided by the locals in Nagasaki. These, as well as provisional scientific names, are recorded on the list by Langsdorff. Martin Lichtenstein (1780–1857), the director of the Zoological Department, copied this list when Langsdorff donated the 89 fish specimens.[39] Sometimes the listed names slightly differ in spelling from modern Japanese, but it is still possible to deduce the local names of the fish. Cuvier and Valenciennes copied the list, and they often referred to the Japanese names. Compared with the list copied by Lichtenstein, the text of the *Histoire Naturelle des Poissons* sometimes uses the wrong spelling for the names. The fish *Priacanthus japonicus*, for instance, was described by Cuvier in 1829, based on the fish specimen of the Langsdorff collection, which still exists as a holotype in the Museum für Naturkunde, Berlin. Its local Japanese name is given on the Lichtenstein list as "Horranda Mebaru", meaning *Holland* (*Dutch*) *Mebaru*. In addition, the list also mentions the provisional scientific name given by Langsdorff, "*Polyprion japonicus*", which the text refers to.[40] "Mebaru" in *Horranda Mebaru*, however, was spelled as "Mobaru" in the text, probably due to the very small handwriting. As an aside it should be mentioned that the name *Horranda Mebaru* is no longer common in Japan, but other documents and literature sources indicate that it was used in the Edo period (1600–1867). Although the genus name has been changed, the specific name given by Cuvier is still valid in the current scientific name of this species *Cookeolus japonicus* (Cuvier, 1829). As a second example, a scorpionfish with its local Japanese name *Aracabu* was incorrectly spelled as "hrocabu",[41] again due to the handwriting of the list. By comparing the text of the *Histoire Naturelle des Poissons* with the Lichtenstein list in the archive, I could verify some Japanese fish names in the former.

In some cases the local Japanese names recorded by Langsdorff were used for the scientific name. Valenciennes gave the new scientific name and also kept the local Japanese name, for instance *Serranus ura*.[42] The specific name, *ura*, which would be spelled *ara* today, was given based on the local name of the species in Nagasaki. However, today, *Serranus ura* is regarded as a synonym of *Epinephelus trimaculatus*. Even though the local name is no longer valid in the current scientific name, it is still possible to trace the name's history and its origins.

The European naturalists in Nagasaki also had opportunities to see the Japanese wood-block printed book *Umi no sachi*, an illustrated fish book with an anthology of *Haiku* poems, a traditional form of Japanese poetry.

For each image the editors had selected corresponding poems about a fish or another aquatic animal.[43] *Haiku* poems brought the fish to life, so people could admire seasonal beauty, irony, humour, personifications, etc. In the late eighteenth century, the interest in natural history had increased in Japan, so the fish drawings were more realistic in the 1762 edition than any other earlier work.[44] Most Japanese enjoyed *Umi no sachi* mainly as a literary product of *Haiku* poems with fish illustrations. They were probably quite impressed by the realism and the attention to detail in the drawings, but they did not regard them as scientific. Only a few intellectuals and natural historians were aware of their scientific properties. For example, Ota Nampo described an episode where Hiraga Gen'nai (1728–1779), a famous natural historian of his time, sent *Umi no sachi* to a Dutch person as a gift, designating it as a "Japanese Fish Book". Nampo humorously added that the foreigners might regard *Haiku* poems as morphological descriptions of the illustrated fish.[45]

Tilesius produced many illustrations of natural history objects in Nagasaki, including very precise drawings of fish figures. Many of the original drawings are now held by the Kustodie and Art Collection of the Universität Leipzig, Germany.[46] Some of them were used for the Krusenstern's atlas published in 1813.[47] The annotations written by Tilesius at the margins of the drawings show that he referred to the fish illustrations of the *Haiku* anthology book, *Umi no sachi*. For example, in his notes on the drawing of the fish *Cheiloductylus zonatus*, Tilesius mentioned Table 7 of the Japanese fish book and the name "*Taka-no-fa*". Table 7 in the second volume of *Umi no sachi* shows the same species with its Japanese name (today *Takanoha*). The same applies to *Hamo*, the Japanese conger fish. Tilesius even copied some illustrations of squid and octopuses from the Japanese book as if he had drawn these images by looking at the living objects.[48] According to his letter to Thunberg,[49] Tilesius could not obtain the book *Umi no sachi* for himself, but he must have seen it during his stay in Nagasaki. It is fair to assume that one of the interpreters or visiting intellectuals showed or lent him the book, and that they read it for Tilesius and taught him the Japanese names of the fish mentioned in the book, since he did not read Japanese. According to Sondermann, Tilesius wrote in his diary on 22 February 1805:

> Today, some small fish were brought to me. Their Japanese names were attached to them. Among them, some have not been referred to by either Thunberg or Kämpfer […].[50]

Taking all the evidence into consideration, both Langsdorff and Tilesius were successful in cooperating with the local Japanese people. Thus they were able to receive material and non-material support, in other words, objects and knowledge. Without the support of the locals, they would not have achieved their scientific objectives in Japan.

Cuvier and Valenciennes also referred to *Umi no sachi*, of which a copy was held in Paris,[51] as "the Japanese printed book", and used it as a source for information on Japanese fish. Cuvier even described the new species *Cybium niphonium* solely based on the drawing in the Japanese book of *Umi no sachi*.[52] The scientific name is still valid today as *Scomberomorus niphonius* (Cuvier, 1832), which supports the idea that local Japanese knowledge on fish was rendered in the drawing. Thus, Cuvier used the fish illustrations in the book as scientific source material to produce new knowledge. From this point of view, Japanese local knowledge on fish, especially its morphological characteristics, colours and patterns, were referred and passed to the Western naturalists through the Japanese publication.

Producing Scientific Knowledge Based on the Collected Objects

The political and commercial objective of the Krusenstern expedition to establish trade relations between Japan and Russia certainly failed. Yet its ichthyological achievements were definitely a success. Supported by the power and authority of the Russian Empire, the expedition members were able to obtain Japanese objects for their investigation and used the *expedition* as an *experiment* by trying to establish commercial contacts with the closed Japanese state, but also by utilizing their involuntary confinement as a way of conducting scientific investigations on previously little known specimens. Despite adverse circumstances, fish specimens and their images were collected and drawn in the Nagasaki "contact zone" by the German natural historians Langsdorff and Tilesius, who brought them back to Europe.

The Langsdorff collections in Berlin and Paris were used for scientific examinations, whereas the collection in St Petersburg was not. This may suggest three points of discussion. First, the Russian round-the-world expedition by Captain Krusenstern was in fact a contribution to science, especially by obtaining scientific objects from Japan, which enabled European naturalists to examine these objects and to gain knowledge. The scientific material was collected mainly by two German natural historians. For this reason, the Krusenstern expedition can be regarded as both a Russian and a cosmopolitan scientific enterprise. Second, because foreign scientists participated in the expedition, the Russians could not insist that the collected material was exclusively brought to St Petersburg. Third, the Langsdorff collection, which was brought to Berlin and later to Paris, was studied by the French biologists Cuvier and Valenciennes who used it to describe new species.

On the other hand, the Langsdorff collection in St Petersburg was not used by scientists at that time, thus it did not directly contribute to science. To be sure, the Langsdorff collection in Berlin, except for ray fish, also remained in the storage rooms of the museum without being studied by German natural historians, until the French zoologists used it for their research. However, in the end it was the scientific efforts and the fieldwork of the two German naturalists and the Russian state commissioning the circumnavigation expedition that enabled Cuvier and Valenciennes to conduct their ichthyological research on Japanese fish. Therefore, one may conclude that the ichthyological contribution to science made by Cuvier and Valenciennes was the result of the collaboration of Europeans who could obtain first-hand materials and local knowledge from Japan, *during and after* the expedition to produce new knowledge in the early nineteenth century.

Conclusion

The voyage of the Krusenstern expedition, commissioned by the Russian Empire, significantly contributed to scientific practices, especially in ichthyology. It was only within the framework of an *expedition* that access to Nagasaki became possible. The Japanese harbour town served as a “contact zone”, since it was a place where the members of the expedition were able to collect natural history objects in Japan, despite its closed-door policy. The German natural historians Langsdorff and Tilesius greatly contributed to the scientific results of the expedition by gaining local knowledge and Japanese fish specimens, which they brought back to Europe in the early nineteenth century. The French zoologists Cuvier and Valenciennes used the information and the collections from Nagasaki, especially those which were held in the Museum für Naturkunde in Berlin, in order to produce new knowledge on Japan—an inaccessible country at that time. The scientific material collected in Nagasaki was brought to four institutions in three countries probably due to the fact that the members of the expedition were non-Russian nationals from different European countries. Local knowledge on Japanese fish was passed to European natural historians both directly and indirectly. Not only were they given raw fish, but also local names and information about the fishes' habitat during their stay in the "contact zones" of Nagasaki. Both the visiting European naturalists in Japan and those back in Europe could obtain scientific information on Japanese fish through fish specimens, information collected with the aid of the locals, and Japanese books.

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CHAPTER 5

Naturalists at Work: Expeditions, Collections and the Creation of "Epistemic Things"

Kurt Schmutzer

In September 1836, the Austrian zoologist Leopold Fitzinger (1802–84) examined an unusual rarity among the thousands of specimens which his colleague Johann Natterer (1787–1843) had collected during his eighteen years of travel in Brazil: a South American lungfish (*Lepidosiren paradoxa*). Natterer had returned from Brazil just a few days earlier and his extraordinary find was now one of the first to be described.

What was so astonishing for the Viennese naturalists was the fact that *Lepidosiren paradoxa* not only have gills but also small, fully operative lungs. They look like fish but can breathe air on the surface of the water like reptiles. Of all the living species of Dipnoi (referring to the existence of both lungs and gills) known today, *Lepidosiren paradoxa* was the first to be discovered by a European naturalist. It was a small scientific sensation, and not only for Fitzinger and Natterer.

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Making Discoveries

Leopold Fitzinger was so excited that only two days after he had received this peculiar specimen he wrote a letter on the results of his examination to the renowned Bohemian naturalist Kaspar von Sternberg (1761–1838). Fitzinger considered the new species to be one of the most interesting discoveries of recent times and the "most perfect transition from the reptiles to the fishes".[1] He urged Sternberg to report this discovery to the "Assembly of German Naturalists and Doctors" who were holding their annual meeting in Jena at that very time. Apparently he wanted to make sure that this sensational find would add to his own reputation and that he would get full credit for the discovery. Fitzinger's letter to Sternberg was presented to the assembly in Jena on 26 September and published in the scientific journal *Isis* the following year. It is still recognized as the first description of that species, which according to zoological nomenclature still carries Fitzinger's name: *Lepidosiren paradoxa*, Fitzinger 1837.[2]

In his letter Leopold Fitzinger also mentioned that there had been discussions between him and Natterer on the nature of the South American lungfish, whether it was a reptile (as Fitzinger thought) or a fish (which was Natterer's opinion). In this context Fitzinger complained that in the specimens he had received there were no internal organs left for anatomical investigation. They had fallen victim to Natterer's "far too passionately executed chase for helminths",[3] as Fitzinger put it. Natterer had removed the organs while searching for parasitic worms (helminths). Now Fitzinger would have trouble classifying the lungfish. Nevertheless the remaining parts of the respiratory organs seem to have been sufficient to decide the question: in 1837, Natterer published a description based on Fitzinger's investigations, stating that the lungfish is a reptile (Fig. 5.1).[4]

Fitzinger's letter and Natterer's paper were the stimulus for a lively scientific debate as to whether lungfish should be recognized as fish or as reptiles and about their placement relative to other bony fish and land vertebrates or quadrupeds. We will return to this debate later. Unfortunately, at this point in my research I cannot produce further evidence that this was not the only case where the output of Natterer's expedition to Brazil proved to be unsatisfactory for the demands of natural scientists who wanted to study the specimens sent from remote areas of the world to the laboratories and museums in Europe, and who found themselves—at least sometimes—quite unhappy with the objects and data submitted by travelling naturalists.

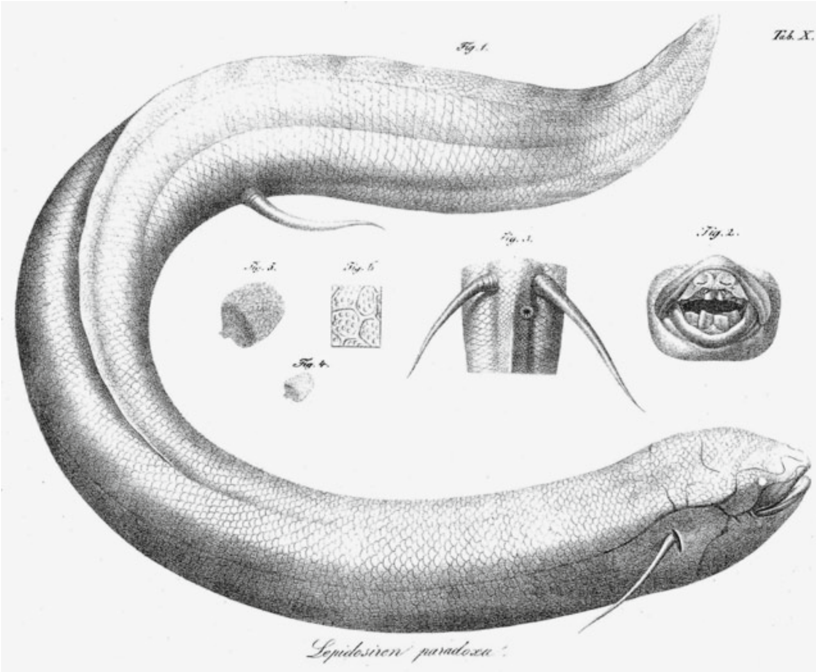

Fig. 5.1 *Lepidosiren paradoxa*, engraving published with Johann Natterer's paper (with details of head, anus with back extremities and scales in three different enlargements). *Annalen des Wiener Museums der Naturgeschichte* 2 (1840), Tab. X

With the presentation of the new species, a debate was opened and re-opened, and a dynamic process developed which also included the procedures of collecting performed during Natterer's travels. A scientific expedition was not simply a voyage that brought naturalists to the objects they were to collect, thus producing reliable, exact, objective and secure data and specimens, which could then serve as reliable sources for all kinds of scientific questions and investigations.

The case of the *Lepidosiren paradoxa* debate shows that the qualities of the specimens produced in the process of travelling and collecting depended on certain conditions and prerequisites, while the qualities of the specimens could have considerable impact on further investigations and problems of scientific research. Producing knowledge and establishing new insights as scientific facts on the basis of specimens is a persistent process, a permanent exchange between facts one already knows and new phenomena which are to be recognized and incorporated into specific fields of knowledge. To understand the phenomenon of a scientific expedition and its outcome in the form of specimens it is necessary to examine the expedition's purposes and intentions, existing special interests and practices in the field. These activities can be understood as a form of a developing experiment, based on well-established plans, instructions and methods, but open to the yet unknown, rather than a mere gathering and picking of things and facts.

Specimens and Epistemic Things, Expeditions and Experiments

A scientific voyage, such as the Austrian expedition to Brazil in 1817, was intended to assemble collections of botanical, mineralogical and zoological specimens: samples of birds, fish, mammals, insects, plants, trees, minerals and precious metals. To these specimens two functions were ascribed: first, they would serve as representatives of their species in the museum's display and, second, as part of a museum's collection as objects of scientific research in terms of their structure, nature and placement within the hierarchies of natural history (description and classification). In both functions the specimens contribute to a process of investigation, perception and understanding, either for visitors to the museum or for scientists in their laboratory. They are what Hans-Jörg Rheinberger calls "epistemic things".

According to Rheinberger "epistemic things" are material entities that constitute the object of inquiry. They are not necessarily objects, but can also be physical structures or biological functions. While a term like "scientific object" could encompass all kinds of objects, things or instruments connected with scientific investigation, indicating a one-way-relation between a (passive) object and an inquiring, perceiving subject (the scientist), Rheinberger's idea of "epistemic things" refers to a process where these things play an active role in generating knowledge. He focuses on those objects, structures and functions, which scientists make use of for elaborating scientific problems, for debating and proving their opinions, for achieving understanding. As objects of inquiry, selected and created by scientists, "epistemic things" are not simply hidden objects to be brought to light that bear the answer to questions already formulated but question-generating machines in a dynamic process of investigation. "Epistemic things" embody what one does not yet know.[5] They appear to be carriers of potential knowledge and consequently Rheinberger's concept calls the qualities of those "things" as a source of information into question.

In the present case, the "epistemic things" in question are really objects in a narrow sense: zoological specimens—dead, preserved animals like the *Lepidosiren paradoxa*. These specimens are not just found and collected but carefully selected and created by naturalists. They are no longer natural objects, only presenting the image of a species, but appear to be loaded with questions and background knowledge, changeable according to the meaning one assigns to them and open to various interpretations. Rheinberger's concept of "epistemic things" points to the fact, that these things are made, cultural objects, and that they are produced with a set purpose, that is, to be used in a process of generating knowledge. With the *Lepidosiren paradoxa* as an example I will try to examine how these "epistemic things" emerge. A variety of procedures, materials, means and meanings, knowledge and prescience is involved in the production of "epistemic things". It may be productive to think about the impact of these elements on the outcome of a collecting procedure as well as on the kind of knowledge the naturalist of the time expected to obtain with these specimens.

The production of "epistemic things" takes place under conditions which do not arise from the *nature* of things, but from considerations about the *function* of these "epistemic things" in the course of producing knowledge and about the proper ways and methods to obtain them. What should a specimen look like? How will it best be preserved for examination? What information about its nature should it represent? What is the kind of knowledge or scientific problem for which a specimen should be useful? These and other questions form a frame for creating these "epistemic things", which in turn are the basis for observations about their nature or qualities. "Epistemic things" are not only objects for scientific research as a part of examinations and experiments. They are also result and part of an "experimental system".

Rheinberger defines an "experimental system" as an arrangement of "epistemic things" and procedures which produces something new or currently unknown to the scientist. It produces unprecedented and unanticipated events. It is not merely a testing procedure for the verification or falsification of a particular hypothesis, but an arrangement that is open to the unknown and that will produce—according to its task, its possibilities and its limitations—productive information and knowledge that is not yet at the scientist's disposal.[6]

The case studies used by Rheinberger focus on classic twentieth-century laboratory research environments. Still Rheinberger's considerations encourage reflecting upon early modern scientific travels and collecting in a more dynamic, flexible view. This approach stresses the process, the "collection in the making" and the variability of scientific procedures instead of definitive results, success or failure in a history of constant scientific improvement. In this view, the expedition and its proceedings are part of an experimental setting, including travelling and collecting practices as well as investigations performed after the expedition. The expedition does not end with the journey itself, and scientific research does not start in the laboratory or at the desk at home.

Travelling and collecting, as performed in the eighteenth and early nineteenth century. was a search for the unknown par excellence and its transformation into verified knowledge which is generally acknowledged and accepted within the scientific community. Like any other experiment the arrangements of an expedition followed well-established rules, and its proceedings were defined and regulated by instructions.

When referring to an expedition more is meant than just the travels of naturalists and explorers. Expeditions were characterized by more or less clearly expressed aims and scientific tasks, usually determined by written instructions. For these carefully planned enterprises their respective organizers developed structures for scientific investigations that included the assignment of specific jobs and areas of responsibility to the individual members (usually experienced specialists in their field), orders regarding scientific procedures (observation, collecting, documentation), duration and area of investigation and general codes of practice.

Instructions were the framework for performing an expedition and the guidelines for travelling naturalists while pursuing the tasks entrusted to them. They were a key instrument for a methodical approach to new understanding and knowledge and provide important information about what naturalists intended to do and what the interests of their respective principals were. At the same time they were a bureaucratic tool for the administration and organization of an expedition.[7]

Usually instructions claimed to be binding, but in practice they were, rather, recommendations to which principals and naturalists could refer if any divergent opinion about the assigned commission might arise. Subjected to unusual climate, illnesses and a multitude of other obstacles, it was quite impossible for explorers to follow their instructions to the letter. Circumstances frequently forced the naturalist abroad to act flexibly and to alter the plans—drafted on safe and secure desks in faraway Europe with the help of fragmentary knowledge of a foreign country—on their own responsibility. But these guidelines given by instructions certainly affected the naturalists' practices.

In comparison to the experiments Rheinberger refers to, expeditions are less severely and clearly organized, and the actual proceedings of a travelling naturalist (hunting, collecting, preserving and documentation) are not repeatable, only the techniques applied. But control and revision by repetition are essential for the definition of modern experiments or experimental systems. Nevertheless it seems worth analysing the practices of a naturalist like Natterer when he produces "epistemic things" in the course of his expedition in order to understand why and how they are created and what scientific investigation can or cannot achieve with them. During an expedition current knowledge and techniques will be tested and new specimens and information will be acquired. Rheinberger's considerations suggest re-examining the structures, procedures and practices performed when a naturalist leaves his study or his museum and joins an expedition to explore "undiscovered" nature.

Certainly to think or write about the natural environment of, for example, Brazil in terms of "undiscovered nature" or "unknown species"—as early nineteenth-century naturalists did—is based on their notion of European superiority and their concepts of natural history. In this view items—whether zoological, botanical or mineralogical—appear as if "hidden" in the woods and plains of Brazil, just waiting to be discovered, classified according to actual standards of European science and put on a shelf in the museum. But in fact they undergo various procedures and treatments until they achieve the status of an "epistemic thing".

As a case study, I will focus on Johann Natterer's travels, which led him to Brazil in 1817 together with a whole team of Austrian naturalists and painters. Whereas nearly all the other members of the expedition returned to Austria within a few years, Natterer stayed in Brazil for 18 years and returned to Vienna only in 1836. In his capacity as the zoologist of the expedition team he hunted, preserved and collected thousands of fish, mammals, amphibians, reptiles and insects.[8] Today his specimens form a substantial part of the Museum of Natural History in Vienna and they are still valuable sources for scientific research. The occurrences of Natterer's travels and explorations provide extensive information to reflect the possibilities of an expedition as a particular “experimental system” and the “epistemic things” produced in the course of this expedition. In this system the collecting procedures performed by naturalists are vitally important for the transformation of living animal beings into specimens, into “epistemic things”.

In “classical” natural history the acquisition of specimens for a collection of natural history seems easy in theory. If we take a look at how Johann Natterer assembled his large collections, what do we see at first hand? During his almost endless journeys throughout Brazil he shoots and picks up individual animals which will be preserved, packed and shipped to Europe, and finally put on display in the museum as representatives of a group or a species. But to “transplant the treasures of nature from distant parts of the world to homeland soil”[9] is not a simple, straightforward task, as this quotation might imply. It involves a variety of practices and procedures. It is indeed an experiment carried out under particular conditions and limitations.

Several aspects influence the structures and procedures that are performed during a scientific expedition. As a general rule, naturalists like Natterer follow instructions issued by their superiors and principals, who was, in the present case, the director of the Imperial Cabinet for Naturalia (*Hofnaturalienkabinett*) in Vienna, Karl von Schreibers (1775–1853). The demands of the Cabinet as a place of representation and prestige, as well as special interests in certain fields of natural history, direct the attention of the naturalist while collecting. Conditions of travelling, especially in tropical areas, have an impact on the results of the collecting process, as well as the methods and resources used in order to preserve the specimens.

Working for the Museum

The Imperial Cabinet for Naturalia had a key role in organizing and conducting the Austrian expedition to Brazil. Although open to the public, the Imperial Cabinet was in fact a private collection of Emperor Francis I (1768–1835, emperor of Austria since 1804). All its activities were financed and coordinated by the imperial court and its staff. Karl von Schreibers, the director of the Imperial Cabinet, was appointed head of the expedition and he also drew up the scientific programme and the official instruction for the members of the expedition. Consequently, to obtain specimens for the Imperial Cabinet and to enlarge the collections of the Emperor’s museum was the main purpose of the expedition. As the Cabinet was not only a place of scientific research but to a great extent an important place of imperial representation, great value was ascribed to the visual qualities of the specimens with regard to the display in the museum's exhibition.[10] The Austrian expedition to Brazil was intended first and foremost to serve the interest of the Imperial Cabinet for Natural History as a museum.

Of all members of the expedition Johann Natterer had the strongest ties to the Imperial Cabinet. His father, Joseph Natterer senior (died 1823), and his brother Joseph Natterer (1786–1852), were both employed as curators, and he himself began his career as an unpaid volunteer in 1801. As a well-trained naturalist, Natterer performed his collecting process systematically, but at the same time he selected his specimens with their probable exhibition in the museum in view. Natterer himself often expressed his close connection and identification with the Imperial Cabinet. Although he showed great eagerness in collecting more different species than any other collector and in helping "his" museum to reach a "higher level of perfection" in respect of the number and variety of species presented there, he also delighted in frequently mentioning the "fine specimens" and "gorgeous show-pieces" which he would be able to offer to the museum.[11] This reflected very well the two main functions of the specimens in the museum: to serve as the basis for scientific description, classification and systematology and to represent a species in the museum's exhibition in a suitable or even impressive way.

Travelling

The journey itself may be considered an essential part of the structures of this experiment called "the expedition". One of the most important elements is time: according to the official instruction, the Austrian naturalists had to follow a detailed schedule which determined the routes and duration of every single journey carried out in Brazil. Together with a demand for travelling quickly, the instruction insisted that the main purpose was to obtain an overview of botanical, mineralogical and zoological objects instead of focusing on fewer or more exactly defined special research tasks.[12]

Of course, in practice no single member of the expedition could manage to keep up with the instruction's schedules. Owing to transport problems, weather conditions, health problems and the underestimation of the amount of work required to organize their travels, and to carry out the tasks of collecting, preservation and packing, the journeys in general took longer than expected. But the basic approach—to cover as much territory as possible (which could also be understood as a reference to the quantity of species or samples which the naturalists expected to find there)—was observed by all members of the expedition.

Natterer's travels into the interior of Brazil obviously followed this pattern. In a quick succession of day's marches his *tropa* (a caravan of mules as pack animals) covered large distances without longer stays in one place if this was not strictly necessary. Several weeks or months were thus spent constantly on the road. It was only when Natterer reached a larger settlement, or a place which he found interesting for hunting and collecting, or when he was forced to remain in one location during the rainy season (when travelling was nearly impossible) that he stayed in the same area for a longer period.[13] This mode of travelling also structured space and collecting. When Natterer was on his way with the *tropa*, an intensified process of hunting and collecting was out of the question. Again, it was only when he was in a location for a longer time that it was possible to perform his duties, as Natterer himself admitted.[14] Although Natterer crossed Brazil from the south-eastern province of São Paulo to the country's north-western borders to Venezuela, and travelled thousands of miles for a worthwhile haul, the selection of specimens for his collections was in fact limited to several areas where he had the opportunity and sufficient time to carry out his investigations. The vast spaces of Brazil were perceived differently and explored at different levels of intensity. Some areas were only to be passed through as a path (or on obstacle) on the way to other spaces, some spaces and their fauna became representative of "Brazil" through their presence in the collection. The order to travel quickly, to cover as many areas as possible and to achieve an overview of Brazilian flora and fauna of course promoted the enlargement of the collection and the enhancement of the Imperial Cabinet, but it served first and foremost the function of the museum as a place of prestigious display and its desire to display as many different species as possible.

The official instruction for the Austrian expedition also included a paper listing the names of animals which were of interest. But that list is not to be understood as a kind of scientific agenda for Natterer and his colleagues but as a wish list for the Emperor's collections. The list was not obligatory, it was incomplete and it allowed room enough for everything that could be interesting. In fact the instruction summarized the responsibilities of the expedition in quite a cursory manner: to make observations with regard to geography, physics, anthropology, ethnography, economy, technics and especially in natural history and "to collect and to get hold of as many natural products as possible of all kinds, of all realms, classes and orders".[15]

Research on intestinal worms (helminths) was the only specific scientific task Natterer was entrusted with, but it had a major influence on his collecting procedures. It initiated one of the largest collections of parasitic worms in the world (today in the Museum of Natural History in Vienna), and it also caused Natterer to dissect all the animals he shot in search for the parasites. Helminthology was not Natterer's own special field, but that of Karl von Schreibers and Johann Gottfried Bremser (1767–1827). Knowledge about intestinal worms was scarce at the time and only small collections to study these animals existed. To know more about the different kinds of worms, their morphology and distribution was considered to be very important, because worms were a great health problem and they were at the time one of the few known sources of illnesses.[16] Schreibers and Bremser published one of the first accounts on helminths in 1811, and Bremser, as curator of the helminthic collection at the Imperial Cabinet, was eager to get new, unknown specimens from Natterer. In order to analyse and systematize the family of worms, Bremser examined more than 25,000 specimens. Although a general practitioner originally, he spent twelve years on the study, description and arrangement of the Cabinet's large collection of worms.[17]

In fulfilment of Schreibers' and Bremser's research plans, Natterer dissected all the animals he found in search of helminths. Of course he could have tried to preserve the internal organs by storing them in glass jars filled with alcohol. But that was not what he was interested in. First of all, he wanted to preserve the body of the animal and its appearance as exactly as possible, which could best be achieved by removing the internal organs. Second, the hundreds of glass jars which Natterer took with him on his journeys were reserved mainly for the preservation of helminths. Obviously he saw no point in using his glasses and transport capacity for the preservation of dissected organs. This is what caused Fitzinger to lament Natterer's "far too passionately executed chase for helminths".

Collecting

To be familiar with given or established subjects, procedures, materials and equipment, and at the same time to be open and alert to the new and unknown that may appear during the process of investigation, is essential for the development of new insights.[18] As a prerequisite, naturalists should know how to organize the collection and transportation of specimens. Most important, they should know how to preserve the specimens for their long journey home and their display at the museum. Naturalists were certainly not mere amateurs or daring adventurers, but in general learned and experienced people. Although not an academic, Johann Natterer, for instance, had received extensive training in natural history, chemistry, botany, anatomy and drawing. His father taught him how to preserve dead animals in the course of joint travels throughout Austria.[19]

Observation ("*Beobachtung*") appears to be one of the key methods for Natterer and his fellow naturalists. "To make observations" and "to make collections" occur as a pair of terms on several occasions in connection with the expedition to Brazil. They are applied as the standard descriptions of the naturalists' goals and tasks in newspaper texts referring to the expedition[20] and also used in the official instruction for the expedition to describe and summarize the main activities of the naturalists in Brazil.[21] For Johann Natterer himself, "observation" and "collecting" form an inseparable combination. Whatever may happen, he reassures Karl von Schreibers in a letter, "it will not lack in collecting and observing".[22] When writing to a politician (who was a mineralogist himself and familiar with natural history) in order to describe his intentions, he mentions "discoveries, observations and collections"[23] in a quite general way. But in all his letters and reports Natterer never reflects on his personal approach to "observation". It seems that for him "observation" is a kind of self-evident method that echoes Buffon's repeated opinion on the best way of learning and performing natural history: viewing, reviewing and viewing again,[24] a visual task performed every day without further consideration.

Nevertheless in the process of selecting among the great variety of animals those individuals which he considers to be representative for a certain family or species, observation and watching are Natterer's most important methods to ascribe "distinguishing features" (Foucault) to the "epistemic things" he is creating. Evidently Natterer must have spent a lot of time with visual perception, either when searching for animals or when comparing his finds with other specimens or when taking measurements of the specimens or when making sketches and watercolours. Frequently he refers to a specific single observation which he regards as important for the special qualities of an animal. Through observation and comparison he works out the "difference" (Rheinberger) that is necessary for the constitution of a new "epistemic thing" clearly distinguished from any other object within the hierarchies of natural history. In order to maintain these distinguishing features, preservation procedures to fix and stabilize a living being (or at least some of its characteristics) are inevitable.

Preserving

A specimen carefully preserved can claim authenticity because it is of the same substance as the living thing, whereas a model (or a sketch) would only look like it. Invested with authenticity the specimen can be declared either to be representative of its species or to be a unique specimen.[25] For Natterer and many of his contemporaries classification was based to a large extent on the description of external features such as form, colour and other characteristics of an animal's appearance. In order to maintain the legibility of these characteristics for future investigation and to supply the museum with those impressive "show-pieces" to which Natterer so proudly referred, it was necessary to stop the biological decline after killing the animal and to try to preserve the appearance as close to nature or as true-to-life as possible. To achieve this goal required a variety of techniques.

The most difficult challenge for a travelling naturalist (apart from surviving the exertion) is the preservation of his specimens. Especially zoologists have to deal with many problems. Animal bodies decay rapidly, particularly in a tropical climate, and voracious insects are an unceasing threat. Moreover the specimens must be brought into a condition which enables them to endure a long period of transportation, either during the expedition itself or if sent home by ships and coaches on journeys that would last several months or even years, always subject to changing weather conditions, damp cargo spaces and careless handling.

In early nineteenth-century natural history it was considered essential to preserve an authentic impression of the individual animal as close to its living form as possible. Following this epistemic ideal of "Truth-to-Nature"[26] should guarantee reliable evidence for classification and it would enable the museum to present a specimen which would not signify just a single, individual animal, but an ideal type, containing all the qualities and distinctive features of the species it represents. The means of preservation on hand had a strong influence on the qualities of the specimens.

Until the end of the eighteenth century it was only possible to store bones, shells or other hard body-parts for a longer period of time. Small fish, amphibians or molluscs could be immersed in alcohol in glass jars (wet specimens), or one could try to preserve smaller vertebrates like birds by drying them. Additional treatment with alum, herbs, pepper, tobacco, cinnamon or camphor would prevent insects from devouring the specimens. How to create dry specimens of the skins of large mammals, amphibians or fish remained an unsolved problem for a long time.

The most effective means of preserving animal bodies proved to be the use of poisonous arsenic, first developed in France around 1770. Although much debated for its dangerous effects for the user, it guaranteed a permanent durability of pelts and hides. After the removal of all internal organs and soft parts, the skins had to be treated with a mixture of white arsenic powder, salt of tartaric acid, camphor, soap and quicklime powder. It was only with this improvement in taxidermy—the art of preserving animals—that travelling collectors like Natterer acquired the necessary means to preserve their finds in the course of extensive expeditions, and museums of natural history gained the possibility of building up large, stable and lasting collections.[27]

Both methods—wet or dry specimens—had their advantages and disadvantages. The use of alcohol for wet specimens in glass jars hardened the tissue, it bleached the colours and the results were generally not very attractive to show, but the animal could be preserved as a whole. Alcohol could also be used for specimens of soft parts or internal organs. For animals with furs or feathers preservation in alcohol was not feasible. Birds preserved in a dry state could be presented with all their beautiful plumages and the hides of dangerous panthers could be moulded into impressive poses. But transforming an animal into a dry specimen inevitably created a torso and reduced the possibilities of further anatomical examination.

Although Natterer took the lead and was the main participant in creating his collection, he was not the only one involved. His Austrian assistant Dominik Sochor (died 1826) not only hunted but also learned to preserve animals. Detailed information is lacking but obviously Natterer employed indigenous people for hunting and fishing too, and some of his Afro-Brazilian servants and slaves (which he bought like any other Brazilian or European "master" at that time) helped with preservation.[28] But not all of the specimens were collected or preserved by Natterer and his team. Several remarks in his letters and diaries suggest that Natterer acquired some of the "epistemic things" in his collection from local fishermen, at markets or from tradesmen, who themselves produced specimens and sold them. Whatever epistemic value Natterer ascribed to these objects that were not obtained through his own, experienced hands, he clearly saw no impediment to including them in a collection of otherwise carefully selected specimens which were generally produced by himself or at least under his supervision.

"To make observations", as Natterer put it, referred not only to the visualization of an object but also to the act of writing down information. Letters, notes, diaries and description accompanied the process of collecting and they were an essential part of it. The specimens alone (dissected and fragmented as they were) could not carry all the information necessary for classification, since some parts of the animal bodies could not be preserved by any means. Therefore it was important to document all parts that were subject to change with the help of sketches, drawings or watercolour paintings, such as the colour of the eyes, feet, beaks and generally (for birds) all parts without feathers. A major problem occurred with those animals whose colours faded immediately after death, as was the case with fish, amphibians and all wet specimens in general. In consequence, Natterer not only took all necessary measurements of length and width of all the birds, mammals, amphibians and fish he had gathered, together with descriptions of those parts of the body that were subject to change after death, but he also recorded the appearance of many of the animals he collected in watercolour paintings and sketches.[29]

Even though several members of his team obviously participated in collecting and preserving, documentation (writing, sketching and painting) was Natterer's sole responsibility. Whatever the team's input regarding local names, traditional knowledge or observations on the animals collected might have been is not recognizable any more. Natterer frequently recorded local names and other information that he must have gotten from local people or those he employed. But in those texts which accompany his finds (reports, letters, notes, diaries), and which would be the source for future scientific research, Natterer appears as the only author of new knowledge.

These various forms of records produced by Natterer were (and still are) "epistemic things" as well as the specimens themselves, and together with the specimens they formed in turn such samples of "epistemic things" as could be questioned and analysed by scientists in the course of further investigation. As we have seen, conditions of travelling, the necessity to transport the collected specimens for months and years, and the methods of preservation permitted the delivery of only a limited, even fragmentary amount of information embodied in specimens, textual records and illustrations. Nevertheless, as long as collecting, preservation and documentation were performed according to recognized standards, the "epistemic things" produced by these procedures were accepted as the basis for classification and research.

Debating Nature

The two specimens of *Lepidosiren paradoxa* brought to Vienna by Natterer confronted scientists with a serious problem regarding classification. The remains of these lungfish, which were available for dissection, showed striking resemblances to fish but also had anatomical features usually associated with reptiles. Natterer initially thought the specimen to be a fish, Fitzinger favoured the idea that it must be a reptile. But to finally decide to which class they should belong was not easy. Due to the preservation process, Fitzinger and Natterer only had limited possibilities for their examination. When dissecting the *Lepidosiren* they had to rely on what they saw, and that was primarily the external features and some small remnants of its internal organs. Although he used a tube to investigate the nose of his specimen, Fitzinger was not able to find conclusive evidence as to whether the nose was connected with the mouth or not (which was then considered to be essential for the distinction between fish and reptiles). Related to the internal organization of the animal they could "unfortunately make only very little communication".[30] However strongly Fitzinger and Natterer were convinced by their investigation, these remarks at least show that they were aware of a problem.

In a meeting of the Linnaean Society in London in April 1839, the famous British zoologist Richard Owen (1804–92) discussed Fitzinger's letter of 1836 and Natterer's paper of 1837, stating that according to his own observations on the West African lungfish (*Protopterus annectens*), this species is clearly to be classified as a fish. Owen used several characteristics to come to this decision, mostly evidence concerning internal organs: the straight intestines with a spiral valve, the absence of pancreas and spleen, the single auricle of the heart, the number of bronchial arches, the position of the gills and—most of all—the fact that the nasal sacs opened only externally and had no communication with the mouth, which he considered (incorrectly) to be typical for fish.[31] Some of these observations could not be made by Fitzinger and Natterer, while Owen obviously had a specimen at hand which presented other characteristics and other information that he could use for his own classification.

But Owen's critique of the Viennese naturalists in 1839 was not the final word in the debate. His full description of the West African lungfish (*Lepidosiren annectens*) was published in 1841, reaffirming his opinion that the species belonged to the class of fish. At the same time, Theodor Ludwig Bischoff (1807–82), professor of anatomy in Heidelberg, obtained permission from the Imperial Cabinet to perform a more complete dissection of Natterer's specimens of *Lepidosiren*. He concluded that *Lepidosiren* was a reptile and sided with Fitzinger, Natterer, the French zoologist Henri Milne Edwards (1800–85), who dissected *Lepidosiren* of the Muséum national d'histoire naturelle in Paris, and others. Years of scientific investigation and debate were necessary to find a satisfactory solution. One of the obstacles was that the scientists were forced to work with preserved specimens that provided limited information owing to the preservation process already described. Bischoff had only one of the two specimens Natterer had collected for examination, Owen's two specimens had been collected in the Gambia River by an English traveller, and Milne Edwards used the specimen already present in the museum's collection. All of these specimens were obviously incomplete, and so their dissection allowed diverging interpretations with regard to classification.[32]

In 1844 the Austrian anatomist Joseph Hyrtl (1810–94), then professor at the University of Prague, obtained a new specimen of *Lepidosiren paradoxa* from the Imperial Cabinet in Vienna. Karl von Schreiber invited Hyrtl to anatomize this specimen, which was not only well preserved but still had all its internal organs. With the dissection of this *Lepidosiren* Hyrtl was able to publish a thoroughly investigated description.[33] Hyrtl argued that it was not sufficient to decide by judging single characteristics whether *Lepidosiren* were fish or reptiles, and he rejected Owen's declaration that the lack of communication between nose and mouth was the one distinguishing feature between them. He came to the conclusion that *Lepidosiren* combine characteristics of both fish and amphibians. It had the skeleton of a fish, but a cardiovascular system and respiratory organs like amphibians. Nevertheless he concluded that (except for the heart and lungs) the inner organization of the organs, such as the vascular system, the mucous membrane of the skin and the digestive organs, were like those of fish.[34] Hyrtl was the first to be able to make such an extensive account because he had a specimen which provided all parts of the animal. His description was based on the analysis of the entire inner organization of the animal and he was not forced to speculate on the significance of single characteristics.

The debate about the classification of Lungfish continued throughout the 1840s and 1850s and was finally settled with the discovery of a third genus of Lungfish, the Queensland lungfish (*Neoceratodus forsteri*) in Australia in 1870. Albert Günther (1830–94), ichthyologist at the Natural History Museum in London, gave an authoritative description of the animal in 1871 and declared it (and its American and African relatives) to be a fish.

Adjusting Knowledge

Apparently the circumstances of the expedition, the methods of collecting and preservation, the actual conventions of natural history and the demands of the Imperial Cabinet as a museum, had their share in the debates, the confusion and the difficulties in deciding the case. The preserved specimen acquired in a certain process of collecting could not display all the characteristics of the living animal and therefore could only supply a limited range of information. The qualities of the specimens available had great influence on the results of scientific research and what scientists could detect.

Another problem arose with the question of which characteristics should be used for classification and how much importance should to be assigned to which of various characteristics in order to put an animal in its "proper" place in nature. Is the lack of communication between nose and mouth a sufficient indicator that the animal in question is a fish, as Owen argued? What are the consequences of the existence of both lungs and gills? Or can the position and size of the lungs tell us if the body we see is to be called a fish or a reptile? Or would the structure of the scales contribute to the solution of this mystery? Fitzinger and Natterer, for example, must have attached some importance to this point, since Fitzinger sent some examples to the meeting in Jena for the scientists to have a look at them,[35] and the scales were presented three times in different sizes in the illustration which accompanied Natterer's paper in 1837 (see Fig. 5.1). In any case, the scientists involved were puzzled by the unusual structure of Lungfish, for which they found no explanation within the existing system of natural history. The established system of classification (as a consequence of the valuation of certain characteristics) at first left no appropriate place for the specimens in question. This place was established only in the course of the debate. Milne Edwards admitted that nature offered more varieties and possibilities then scientists until then had supposed:

> When Zoology is only studied in systematic works, it is often supposed that each class, each family and each genus, present to us boundaries precisely defined, and there can be no uncertainty as to the place to be assigned in a natural classification to every animal, the organization of which is sufficiently known; but when we study this science from nature itself, we are soon convinced on the contrary, and we sometimes see the transition from one plan of structure to an entirely different scheme of organization take place by degrees so completely shaded one into the other, that it becomes very difficult to trace the line of demarcation between the groups thus connected.[36]

What can be done if the specimen in question will not fit into the system, or does not yield to the established hierarchy and classification of natural history? It is easy: adjust the system. In 1844 the German physiologist Johannes Peter Müller (1801–58) suggested the introduction of a new subclass "Dipnoi" to describe the "double breathing" of both lungs and gills in the Lungfish. Although a large variety of terms were offered and tried in the following years, most of this nomenclature was rejected, but Müller's proposal survived.[37] Today all Lungfish are still placed as fish in class Sarcopterygii, subclass Dipnoi.

Adjusting the system is not an arbitrary act. It is part of a process of shaping and re-shaping the *unknown* in its transformation to something *known* that is the essence of scientific research. Without ideas in the mind and prescience the formulation of scientific questions would be impossible; without dynamics and the willingness for adaptation there would be no change, no improvement of knowledge. That is "science in the making".[38]

In natural history the specimens collected around the world and assembled in a museum's collections had and still have an important role in that dynamic process of generating new knowledge. As "epistemic things" they contribute new information to this process. I have tried to examine how the circumstances and conditions under which these "epistemic things" were produced, defined and influenced their qualities, which in turn had an impact on scientific investigations and debates. Being culturally made objects these "epistemic things" cannot be separated from the reasons for the way they were created, and to what purpose they were created. The agenda of an expedition, its implementation and the practical work of travelling naturalists are certainly of consequence. With the case of *Lepidosiren paradoxa* I tried to explore how far this impact might reach. When scientists in a laboratory take a specimen into their hands, they deal not with a single, isolated object, but with a complex ensemble of information. While using a specimen as an "epistemic thing", procedures and contexts which were involved in the making of this specimen come into question: instructions, methods of hunting, collecting and documentation, means of preservation, special interests, personal requests and demands of a museum's display—all these aspects had a share in why and how an "epistemic thing" like *Lepidosiren paradoxa* came into being.

Notes

1. Leopold Fitzinger, "Vorläufiger Bericht ueber eine hoechst interessante Entdeckung Dr. Natterer's in Brasilien", *Isis* V, VI and VII (1837): 379: "vollkommenster Uebergang von den Reptilien zu den Fischen".
2. Fitzinger's letter was also published in the official report of the Jena meeting; see Dietrich Georg Kieser and Jonathan Carl Zenker, eds., *Amtlicher Bericht über die Versammlung deutscher Naturforscher und Ärzte zu Jena im September 1836* (Weimar: Voigt, 1837), 99.
3. Fitzinger, "Vorläufiger Bericht ueber eine hoechst interessante Entdeckung Dr. Natterer's in Brasilien", 379.
4. Johann Natterer, "Lepidosiren paradoxa, eine neue Gattung aus der Familie der fischähnlichen Reptilien", *Annalen des Wiener Museums der Naturgeschichte* 2 (1840): 165–70. Although the editions of the *Annalen* consulted at Staatsbibliothek München and Nationalbibliothek Wien bear "1840" as date of printing, the volume or parts of it must have been available in 1837/8. The debate that followed would not have been possible if the *Annalen* were published only in 1840. In any case the publication of the *Annalen* was announced in *Isis* V (1838): 345–7, together with a detailed summary of Natterer's paper.
5. Hans-Jörg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (Stanford: Stanford University Press, 1997), 1–5, 28–33.
6. Rheinberger, *Toward a History of Epistemic Things*, 24–8.
7. Philippe Despoix, *Die Welt vermessen. Dispositive der Entdeckungsreise im Zeitalter der Aufklärung* (Göttingen: Wallstein, 2009, French original: *Le Monde mesuré. Dispositifs de l'exploration à l'âge des Lumières* (Geneva: Librairie Droz, 2005), 81–97; Marianne Klemun, "Verwaltete Wissenschaft—Instruktionen und Forschungsreisen", in Anita Hipfinger, Josef Löffler, Jan Paul Niederkorn, Martin Scheutz, Thomas Winkelbauer and Jakob Wührer, eds., *Ordnung durch Tinte und Feder? Genese und Wirkung von Instruktionen im zeitlichen Längsschnitt vom Mittelalter bis zum 20. Jahrhundert* (Wien: Böhlau, München: Oldenburg, 2012), 391–412. See also Felix Driver, *Geography Militant: Cultures of Exploration and Empire* (Oxford: Blackwell, 2001).

8. See Kurt Schmutzer, *Der Liebe zur Naturgeschichte halber. Johann Natterers Reisen in Brasilien 1817–1836* (= Veröffentlichungen der Kommission für Geschichte der Naturwissenschaften, Mathematik und Medizin 64) (Wien: Akademie der Wissenschaften, 2011).
9. Karl von Schreibers, *Nachrichten von den kaiserlich-österreichischen Naturforschern in Brasilien und den Resultaten ihrer Betriebsamkeit*, 1 (Brünn: Traßler, 1820), III: "Naturschätze ferner Welttheile auf vaterländischen Boden zu verpflanzen".
10. Schmutzer, *Der Liebe zur Naturgeschichte halber*, 20–32.
11. Schmutzer, *Der Liebe zur Naturgeschichte halber*, 209–20.
12. "Dienstinstruktion", Wien, 14 January 1817, Haus-, Hof- und Staatsarchiv Wien, Staatskanzlei, Brasilien, Karton 1, Konvolut 2, Wiss. Expedition.
13. Schmutzer, *Der Liebe zur Naturgeschichte halber*, 133–8.
14. Johann Natterer to Joseph Natterer (letter), Borba, 21/28 December 1829, Wienbibliothek, Handschriftensammlung, Natterer, 7883: "Während der Reise kann man wenig arbeiten. Das meiste muss in den Standquartieren geschehen." The same applies to Natterer's travels by boat in the Amazon Basin.
15. "Dienstinstruktion", Wien, 14 January 1817, Haus-, Hof- und Staatsarchiv Wien, Staatskanzlei, Brasilien, Karton 1, Konvolut 2, Wiss. Expedition: "so viel als möglich Naturprodukte aller Art, aus allen Reichen, Klassen und Ordnungen einzusammeln und beyzuschaffen".
16. Verena Stagl and Helmut Sattmann, *Der Herr der Würmer. Leben und Werk des Wiener Arztes und Parasitologen Johann Gottfried Bremser* (*1767–1827*) (Wien, Köln, Weimar: Böhlau, 2013), 85–96, 115–38.
17. Johann Gottfried Bremser, *Lebende Würmer im lebenden Menschen. Ein Buch für ausübende Aerzte* (Wien: Carl Schaumburg, 1819), VI–VII.
18. Hans-Jörg Rheinberger, "Wissenschaft und Experiment", in Anne von der Heiden and Nina Zschocke, eds., *Autorität des Wissens. Kunst- und Wissenschaftsgeschichte im Dialog* (Zürich: diaphanes, 2012), 123–5.
19. Schmutzer, *Der Liebe zur Naturgeschichte halber*, 16–18.
20. *Wiener Zeitung*, no. 114, 19 May 1817, 453; *Österreichischer Beobachter*, no. 140, 20 May 1817, 726; *Vaterländische Blätter für den österreichischen Kaiserstaat*, no. 44, 31 May 1817, 176; *Wiener Zeitschrift für Kunst*, *Literatur*, *Theater und Mode*, no. 145, 4 December 1821, 1218.
21. "Dienstinstruktion", Wien, 14 January 1817, Haus-, Hof- und Staatsarchiv Wien, Staatskanzlei, Brasilien, Karton 1, Konvolut 2, Wiss. Expedition.
22. Karl von Schreibers, *Nachrichten von den kaiserlich-österreichischen Naturforschern in Brasilien und den Resultaten ihrer Betriebsamkeit*, 2 (Brünn: Traßler, 1822), 11. For the frequent use of the combined terms "collections and observations" see also Johann Natterer to Karl von Schreibers (letter), Rio de Janeiro, 25 June 1821, Weltmuseum Wien,

Archiv, Natterer; Johann Natterer to Archduke Johann (letter/draft), [1812/1813], [Wien], Weltmuseum Wien, Archiv, Natterer.

23. Johann Natterer to José Bonifacio d'Andrada e Silva, Ipanema, 20 April 1822, Weltmuseum Wien, Archiv, Natterer.
24. George-Louis Leclerc de Buffon, *Histoire naturelle générale et particulière. Tome premier, Premier discours: De la manière d'ètudier & de traiter l'Histoire naturelle* (Paris: Impr. Royale, 1749), 9–12: "les objets […] en les voyant souvent […] par voir beaucoup & revoir souvent […] j'ai dit qu'il fallout commencer par voir beaucoup". See also Lorraine Daston and Elizsabeth Lunbeck, eds., *Histories of Scientific Observation* (Chicago: University of Chicago Press, 2011).
25. Hans-Jörg Rheinberger, *Epistemologie des Konkreten. Studien zur Geschichte der modernen Biologie* (Frankfurt/Main: Suhrkamp, 2006), 336–42; Hans-Jörg Rheinberger, "Präparate—'Bilder' ihrer selbst. Eine bildtheoretische Glosse", *Bildwelten des Wissens. Kunsthistorisches Jahrbuch für Bildkritik* 1(2) (2003): 9–19.
26. Lorraine Daston and Peter Galison, *Objektivität* (Frankfurt/Main: Suhrkamp, 2007; Engl. orig.: *Objectivity*, New York: Zone Books, 2007), 59–119.
27. Paul Lawrence Farber, "The Development of Taxidermy and the History of Ornithology", *Isis* 68 (1977): 550–66.
28. Schmutzer, *Der Liebe zur Naturgeschichte halber*, 196–7.
29. Kurt Schmutzer, "Metamorphosis between Field and Museum: Collections in the Making", special issue: *Moved Natural Objects: Spaces in Between*, Marianne Klemun, ed., *HoST—Journal of History of Science and Technology* 5 (2012): 68–83, (http://www.johost.eu/vol5_spring_2012/kurt_schmutzer.htm).
30. Natterer, "*Lepidosiren paradoxa*, eine neue Gattung aus der Familie der fischähnlichen Reptilien", 167.
31. Richard Owen, "A New Species of the Genus Lepidosiren of Fitzinger and Natterer", *Proceedings of the Linnean Society of London* 1 (1839): 27–32. His rejection of Fitzinger's and Natterer's description quickly spread and a German translation of the London proceedings was published the same year. See Arend Friedrich August Wiegmann, "Lepidosiren ist kein Reptil", *Archiv für Naturgeschichte* 5(1) (1839): 398–403.
32. See Elizabeth Babbot Conant, "An Historical Overview of the Literature of Dipnoi: Introduction to the Bibliography of Lungfishes", in William E. Bemis, Warren W. Burggren and Norman E. Kemp, eds., *The Biology and Evolution of Lungfishes: Based on Proceedings of a Symposium Held During the American Society of Zoologists Meeting in Denver, Colorado, December 27, 1984* (New York: Alan Liss, 1986), 6–9. See also John A. Long, *The Rise of Fishes: 500 Million Years of Evolution* (Baltimore:

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35. Fitzinger, "Vorläufiger Bericht ueber eine hoechst interessante Entdeckung Dr. Natterer's in Brasilien", 380.
36. Henri Milne Edwards, "On the Natural Affinities of the Lepidosiren; and on the Differing Opinions of Mr. Owen and M. Bischoff with Regard to Them", *Annals and Magazine of Natural History* 6 (1841): 466. This article is a translation of Henri Milne Edwards, "Remarques sur les affinités naturelles du Lepidosiren", *Annales des Sciences Naturelles* 14 (1840): 159–62.
37. Conant, "An Historical Overview of the Literature of Dipnoi", 9.
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CHAPTER 6

Mary Barber's Expedition Journal: An Experimental Space to Voice Social Concerns

Tanja Hammel

Reception of Women on Expeditions

Women went on expeditions; few of them are generally known. Three women have recently attracted most scholarly attention. French naturalist Jeanne Baret (pseudonym Jean Baré, 1740–1807) who, dressed as a man, joined Louis Antoine de Bougainville on *La Boudeuse* and *L'étoile* on journeys to the South Pacific between 1766 and 1769. She is thought to have been the first woman to sail around the world.[1] The other two are English botanical oil painter Marianne North (1830–90), who travelled the world to paint flowers and built her own gallery at the Royal Botanic Gardens Kew, London, and German naturalist and collector Amalie Dietrich (1821–91), who collected plant, animal, human specimens and ethnographic objects in Australia and sold them to German museums.[2]

Women's travel and nature writing have been analysed with an emphasis on essential gender difference. This gender difference was seen in both style and content. Women allegedly produced "more private, fragmented episodic autobiographies […] which impose an overarching design on

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their lives or travels", while men wrote "formal, distilled autobiographies in which their primary concern is an objective valuation of the significance of the whole life (or journey) [...]. Women tend to record, to surrender to experience; men to judge, to schematize experience."[3] The difference in content was explained by writing for a predominantly female audience interested in domestic life or the unavailability of "the masculine heroic discourse of discovery" to women.[4] Some women scholars saw women travellers as ethically superior to their male contemporaries, which is why they critically remarked upon colonial rule and identified "Others" as individuals and not as homogeneous racial groups.[5] In cases when women's writing did not differ from men's, it was argued that they "escaped gender", adopted a "temporary male status" and found a "neutral voice".[6] Gender essentialist studies—some informed by feminist theory—have emphasized women's difference. Ecofeminists in the 1990s maintained that women formulated "distinctly female traditions in science and nature writing".[7]

These interpretations of women's expedition journals reinvigorate the nineteenth-century debate about gender difference. Let me illustrate this problematic exemplarily with Jeanne Baret's reception. Baret has almost fallen into oblivion. While seventy plants, insects and molluscs were named after the expedition botanist Philibert Commerson, Baret only received such an honour in 2012, when biologist Eric Tepe, on the recommendation of British historian Glynis Ridley, named a nightshade plant *Solanum baretiae*.[8] The assumption that Commerson and Baret knew each other before the expedition, that Commerson knew her sex and they were a couple[9] reduces her scientific merits to a romantic deed enabled by Commerson's goodwill. This belittles Baret's achievements and does not give her the standing she deserves.

Women crafted their expedition journals in the same ways as men. Both mixed autobiographical and historical narrative with scientific prose, dramaturgy and political propaganda. The difference was that women had no direct say in legislative and executive matters, which is why their writing opened them possibilities that they hardly encountered elsewhere.

Wanderings in Science and Society

This chapter focuses on the expedition journal of British-born naturalist Mary Elizabeth Barber's (née Bowker) (1818–99). The Bowkers migrated to the Cape Colony in 1820. She grew up on Oliveburn and Tharfield, two farms near Port Alfred, Albany, at the eastern frontier of the Cape Colony. For five years she was in courtship, an on-and-off affair, before she and sheep farmer Frederick William Barber (1813–91) married.[10] They spent their lives on various farms near Graaff Reinet and on the Zwart Kei River near Queenstown. Late in life she aptly wrote that she had "scarcely ever been a week or fortnight in one place" and had "been a vagabond upon the face of the earth".[11] Her husband had been given the farm Lammermoor near Queenstown for his war services, but due to financial difficulties they let it and moved to her brother's farm, Highlands. The news that diamonds had been found in Griqualand West was more than welcome to her husband, who left to try his luck in what later became Kimberley. There the Barbers spent the 1870s, with their daughter and two sons.

Barber compiled her account during a journey from Kimberley via Cape Town to Durban in 1879. Unlike young men explorers who inscribed themselves into their scientific community through their expedition journals, Barber was 61 years old when she embarked on her journey and already a well-received naturalist, a member of the South African Philosophical Society and connected to a network of eminent scientists. Contrary to British explorers, she had spent virtually all her life at the Cape. The unschooled autodidact had, since the 1840s, contributed to the classification of butterfly and plant species.[12] She had written about their metamorphosis, mimicry and fertilization.[13] This had brought Barber into contact with eminent British naturalists such as Joseph Dalton Hooker and Charles Darwin, and had allowed her to publish her papers in British journals such as the *Journal of the Linnaean Society* and the *Transactions of the Entomological Society*.[14] This was remarkable, as women could not become members of most scientific societies. But like Baret, Barber was neglected and later reduced to her role as an informant and collector for famous British men scientists.[15] Her travel account has not attracted any scholarly attention.

Her travel account was explicitly written for publication and out of pecuniary motives.[16] Her twenty short chapters and her brother lepidopterist James Henry Bowker's (1822–1900) three include eleven pencil-sketched figures, twenty-three historiated initials and noteworthy information on local flora and fauna. Her illustrations are indicative of the travel account's content and priorities. Figure 6.1 does not demonstrate much artistic ability. It depicts the travel group consisting of two European men, three women and "two natives". The naively drawn human figures remain shadowy, but she (the second figure on the left) appears to be in control of the situation. In her writing she presented herself as a solitary, independent traveller and did not mention who the other four Europeans in her travel group were.[17] From her husband's letters, however, we know that George Hull and his wife—family friends—and he accompanied her to Cape Town. From there, her husband left for England to see his brother. The ox wagon in which they travelled is at the centre of the pencil-sketch. The rear wheel is sunken in; the wagon is outspanned near a source of water in an otherwise bare landscape. The pencil drawing is sketchy, drawn with short lines and the landscape is indicated. While the wagon is overly detailed, its wheels are neither of equal size, fixed nor on the ground. The wagon is tipped backwards which is representationally wrong and makes the wagon appear to float. The illustration demonstrates that her expertise is in botany, as the plant leaves are depicted in great detail (Fig. 6.1).

The illustration demonstrates that she mostly contributed to botany in her travel account. But "with but few flowers or plants of interest"[18] she turned to flint implements and archaeology. At the same time she

Fig. 6.1 "Our travelling wagon—outspanned, near Salt River Mountain", © copy of the Manuscript at Cory Library, in Alan Cohen's Private Archive

circulated Darwin's evolutionary theory by natural and sexual selection through local examples. She discussed and stressed theories on the origin of gold and coal, thereby contributing to a general understanding of geological principles. She thus contributed to various disciplines, and circulated knowledge about the land and its inhabitants. Her travelogue is an odyssey: the incidental details of her journey are the focal point of her account. It is not as fragmented as Marianne North's posthumously published autobiography *Recollections of a Happy Life*, compiled on the basis of her journals, where transitions between paragraphs are missing and the reader struggles to determine geographical and personal context when sentences rapidly shift from one topic to another.[19] But Barber's travelogue is a collection of tenuously connected episodes, which basically follows her journey. Her vocabulary and narrative is simple and full of anecdotes to address a broad readership. Occasionally, she slips in loanwords from French or Latin that she carefully taught herself as lists in her notebooks show.[20] In contrast to Barber's previous publications in scientific, popular science and horticultural journals, this genre allowed her to experiment.

Barber most likely produced it in the 1880s when she was hoping to join her husband in England. By 1879, their marriage seemed strained. He left for England to see his brother, a photographer near Bristol. In fact it seems to have been an unhappy match, a pragmatic union of two strong-willed individualists.[21] In her travelogue, she writes "true love is not always smooth; it is not always the fatal insurance system by which they are affected; they have to battle against rude billows and howling storms: it is moreover the verdict of Nature that all things should perish."[22] She suffered under the separation. She regretted that men were not as faithful and selfless as dogs and played with the wishful thought of her husband being a faithful dog and she being his master.[23] The separation must have been a disgrace for her.

For the passage to England to join him and convince him to return to the Cape with her, she definitely needed money that she hoped to gain from selling her texts. She had previously compiled "natural history tales" from her journals and notes on natural history in the style of Revd Gilbert White's *The Natural History and Antiquities of Selborne* (1789) in the late 1860s. This posthumous publication of White's letters to naturalists, compiled by his brother Benjamin, was rather a quaint model, but Barber aimed at imitating its commercial success. In her tales, she aimed to write for a general readership, particularly children, and included scientific and vernacular names, but “no scientific detail or systematic arrangement”. A few of these natural history tales were published in the early 1870s.[24] For the travel account, she could have aimed for serial publication in *The Cape Monthly Magazine*, a journal established in 1857 modelled after highbrow British quarterlies that aimed to interlock colonial academic institutions, and in which she had previously published similar texts.[25]

“It is better to be out of the world than out of the fashion”, Barber wrote.[26] Travel books were the most read genre after the novel. She knew the fashion well, referenced and quoted from previous expedition journals, such as those by Charles Darwin, David Livingstone, William Burchell and Joseph Dalton Hooker.[27] As was common practice in the genre, she also anticipated her readers’ future ventures by recommending places to lodge and techniques for collecting and preserving specimens.[28] Evolutionary theory made scientists obsessed with individualism, heritage and nurture so that numerous autobiographies by British evolutionary thinkers appeared. Literary critic Alexis Harley has recently termed this genre *autobiology*.[29] Barber’s travel account similarly explores humans’ role in nature, and the different races’ as well as women’s place in South African society.

Despite her efforts, her travel account was not published during her lifetime. The manuscripts do not appear to have been final drafts, as they include crossed-out passages. One manuscript was in the possession of her grandson Raymond B. Mitford-Barberton, who transcribed it in the late 1950s and early 1960s, when he presumably gave it the title *Wanderings of South Africa by Sea and Land*. He donated his manuscript and transcription to the Albany Museum and serially published an abridged version in the *Quarterly Bulletin of the South African Library* in 1962–3 at the height of apartheid.[30] Descendants of 1820 settlers treasured the text as a memento of their pioneering ancestors, whom they feared the National Party would silence in the official historiography, and a manifesto of Social Darwinist ideology. Another manuscript ended up in her great-grandson Gareth Mitford-Barberton’s private archive in England. Her narrative is that of a scientist, who, in a situation where everything is unknown, constructs her belonging to the Cape and to science, her “sovereign remedy to drive away care”.[31] It is a journal of essence of self, written at a late point in life—a text so her words can live on after her death and to voice her concerns with settlers’ and particularly settler women’s status in South African society. In what follows I explore how Barber’s scientific discourse allows insights into how she manipulated facts to order the social world around her.

Advocating for Women's Rights

Barber eschewed stereotypical gender characteristics. As part of the nature/nurture debate she argued for natural equality, and criticized culturally constructed difference. Barber obfuscated, disrupted and negated the boundaries between masculinity and femininity. She obscured the obsolete distinction between feminine and masculine space and dissociated herself from essentialist associations of the female body as "inextricably linked with weakness, emotion, irrationality, ignorance, reproduction".[32] So as to be perceived as an authority on natural history and an innovator, she prided herself on her knowledge, heroized herself by stressing the physical hardships she had to endure—lack of sleep, no bed, no food and disastrous roads—and her virtues such as bravery and courage.[33] She also criticized men who carried women when exiting a ship and who reduced women to their appearance.

At the same time, she encouraged women. She required them not to be selfless and lay down their lives for others. She promoted their scientific activities, such as those undertaken by Mr Acutt's daughters, who helped her collect plants, and her botanist colleague Katherine Saunders, whose paintings of wild flowers Barber particularly admired.[34] She also encouraged women to be independent and do what were stereotypically male actions, such as "paddling their own canoes quite independent of the lordlier sex", which can also be read as a demand for women to lead their own lives independent of their male relatives.[35] She encouraged women to break free and not pay too much attention to superficial and stereotypically female virtues such as beauty and care.[36]

Constructing Social Order Through Plant Descriptions

Through her botanical research Barber shifted from being a devout Anglican to a doubting animist and "stout Darwinian".[37] For years after reading Darwin's *On the Origin of Species* (1859), she would find herself in inner turmoil, torn between her professional thoughts as a naturalist and her private religious belief. She discussed her doubts with her main correspondents, botanist William Henry Harvey (1811–66) at Trinity College Dublin and Joseph Dalton Hooker (1817–1911), director of the Royal Botanic Gardens Kew, London. Harvey was an outspoken Darwin critic.[38] He was not alone; plant taxonomists saw their work endangered by the theory that claimed that species were not fixed and changed over time.[39] Besides seeing his profession at danger, difficult personal experiences—such as loss of family members and loneliness—made him seek consolation first in his Quakerism then in Catholicism.[40] In the 1860s, Barber remained convinced that God held nature "in perfect order […] in harmony and love" and in her travel account she praised the infinity of "wonders" in "the Book of Nature" that the human mind was "unable to grasp", and attributed species that were "wonderfully adapted" to the "varied conditions" of their habitats to "the hand of Providence".[41] Both Harvey and Barber gradually reached a compromise position: there was evolution by natural selection, but God had programmed the laws of variation in creation. After Harvey died, Barber's research became more complicated. She addressed Hooker in desperate need for a replacement, but, unlike Harvey, he had no interest in South African plants.[42] This made her pursue her research even more vigorously and in 1867 Barber's position as "a believer" in "the laws of natural selection" appeared in print for the first time.[43]

In her travel journal she stated that the world was more than 6000 years old and collected evidence for evolutionary theory by natural and sexual selection. She regretted the divided churches, the empty church buildings and the loss of Christian values in the colony that made her stray from Anglican prayers and services.[44] People had become unrighteous, greedy for wealth and power. She found comfort in praising nature and was convinced that if it was possible for humans:

> to offer up their souls in true and earnest prayer, it would be here in the forest, in this solitary church, "far from the madding crowd", surrounded by the beauties of nature, the work of God's hands in the temple, of the woods.
>
> "If thou art worn and hard beset
> With sorrows that thou woulds't forget,
> Go to the woods and hills—no tears
> Dim the sweet face that Nature wears."[45]

Botany had always been her "sovereign remedy to drive away care", so it came with no surprise that she turned to botany when her husband left in 1879. At the same time, she protested "against anything being found out and explained until at length the world we live in becomes threadbare and devoid of all that is wonderful and mysterious". It is an interesting statement for a naturalist, who had spent most of her life explaining species and processes previously unknown to science. She even argued that it was "a mistake to know too much", as people would lose their "veneration for the hidden things of nature" and become "too matter of fact", abandoning "all sentiment and poetical feelings".[46]

She developed a natural theology reminiscent of animism and saw humans, animals and plants as connected. Flowers and animals were "almost [her] companions"[47] and she studied nature as a whole, looking for interrelationships and interactions between species and phenomena. This is reminiscent of the younger South African author Olive Schreiner (*1855) who, inspired by Ralph Waldo Emerson and the American transcendentalists, developed a pagan animism.[48] Schreiner strongly believed in human, animal, plant and spiritual connectedness.[49] She undermined hierarchical thinking by unifying the natural, spiritual and human realms and saw "long unbroken lines of connection" in nature, "a great, pulsating, always interacting whole".[50] According to this worldview, there can be no inequality or discursive violence on the basis of difference—human, plant, or animal.

Barber's spatial descriptions provide insight into her imagined society. A year before the first Anglo-War, she repeatedly emphasized the discrepancy between "respectable" 1820 Settler farms and the Boers' "deserted farm houses".[51] She admired the inhabitants of Beaufort West and felt she had quit civilization when she was beyond the town's limits, where she encountered "wide plains of solitude and barbarism".[52] In the "essentially Dutch town" of Worcester she faced Boer idleness,[53] while she saw the churches, schools, public offices and hotels in Durban and the "park-like and picturesque" scenery in Natal as examples of 1820 Settlers' industriousness.[54]

An anecdote encapsulates Barber's social hierarchy that she justified with

Social Darwinist vocabulary: when Barber's travel group was turned out of a first class carriage, and put into a third class one "which had lately been appropriated by natives", she experienced this as "a clear, cruel case of oppression, the strong against the weak, who were evidently 'going to the wall'. This was the Theory of Evolution being put into practice." As there was no hotel, they ended up taking the third class carriage.[55] This is a powerful metaphor for Barber's vision of the 1820 Settlers as first, the Boers as second and the Africans as third class people.

To manifest this social hierarchy Barber drew on plants. The grapple plant (*Uncaria procumbens*) served as a metaphor for British settlers who were prepared to go to war to maintain their position in the struggle for life.[56] South African species were superior to invasive Australian plants such as the Blue Gum Tree, *Dorthesia*, the Australian blight and the Australian beef tree[57] that she experienced as "interlopers" and which harrowed up her "African feelings".[58] The invasive plant debate served as a metaphor for the competition between Australia and South Africa to be the exemplary model settler colony in the southern hemisphere. Barber criticized the British, who had planted pine trees in the Cape district to the exclusion of indigenous trees that were better adapted to the country, more varied and interesting. She seems to have used this metaphorically to stress 1820 Settlers' superiority over British travellers or Britons who came to the Cape much later and knew little about local flora.[59] At the same time, the mangroves (*Rhizophora mangle*) are admired for their survival under difficult situations, their providing security "as the lighthouse to the ship, so will the mangrove be to the traveller", an analogy for the importance of locals' knowledge that provides security for settlers.[60] The wild fig's propagating in trunks or hollows of trees until they form a trunk of their own served as an analogy for settlers' clinging to foreign land and making it their own.[61] On Robben Island Barber visited a lunatic asylum and encountered a "Kafir" whom she described as the "living death". She added "if some exasperated, thrice ruined frontier farmer had seen this Kafir, they would have exclaimed 'Oh that they were all as good and quiet as he is'." She compared him to an invasive plant that should be eradicated, a "huge succulent plant, some wonder of the vegetable kingdom, like many of the Mexican species of the Cactus tribe".[62]

Barber harshly criticized the older generation of Boer farmers who, despite being "included in the list of civilized men", had not acted accordingly and had destroyed nature. They were deemed responsible for desiccation, for "driv[ing] out the original inhabitants", for almost extinguishing the antelope, elephant, buffalo, giraffe, rhinoceros, hippopotamus, lion and "the wild Bushman". Their activities transformed the vegetation from charming fields of grass to scrubby, bitter Karroo bush.[63] For all the environmental problems, "the low uneducated class of Boers", who had "undoubtedly been a drag on this colony" and were "steeped in hopeless ignorance", were said to have been responsible.[64] Barber used them as a scapegoat for arguing for British settlers' innocence, overlooking how British colonialism profited from Dutch colonialism.

Until 1870 she had only been in contact with "the Kafir and Fingoe tribes", "with the other numerous races [she had] had no intercourse and kn[e]w nothing of their manners and customs".[65] She "probably knew" some Xhosa men and women, but "only from a reserved distance", as anthropologist Robert Shanafelt argued.[66] In Kimberley she was for the first time in close contact with Africans. She then contributed to the debate that language determined social hierarchy. In the late 1860s, linguists at the Cape had argued that the languages spoken in the Colony exemplified early forms of the three language families, and that "Bushman (proto-Bantu)" was the language spoken by people at an early stage of evolution.[67] Philologists were convinced that in the Cape Colony they could come into contact with their own earliest history.[68] German zoologist and philosopher Ernst Haeckel had been convinced that the Bushmen and "Kaffirs" were closest to the ape.[69] Barber found the "Koranna language" and that of the wandering Bushmen indistinguishable from those of baboons and monkeys. But she saw the "Kaffirs" on a higher evolutionary stage than the "Koranna" who were at the lowest, despite the fact that they originated from European colonists and Khoekhoe living in the Cape in the seventeenth and eighteenth centuries.[70]

Her travelogue references her African collectors and informants by name, while she does not mention the Europeans who travelled with her. Perhaps she was disappointed in her husband who left for England and his friends who supported him, which also made her more open to and dependent on her African companions. She referred to "Matabele boy" Kamel and Klaas from Cape Town most prominently.[71] They acted as wagon drivers and collected butterflies, cooked for her and allowed her to devote all her time to her research. Klaas is strictly condemned. He is said to be so dumb that he could not even boil water.[72] Once he was sent to bring water, but brought the hungry cattle from the feeding ground to the wagon instead, which made Barber mad and she strictly condemned his "low scale of intellect".[73] He lost a coffee kettle and the lid of a pot, which made Barber argue that he was more stupid, ruder and less sophisticated than Stone Age people.[74] Upon another occasion, Klaas is said to have broken their whipstick.[75] One might wonder why she hired him, or at least why she kept him on. But her travelogue repeatedly portrays him as "rather good-looking",[76] which might be part of why. Good looks do not make a relationship last, however, and eventually their difficulties bubbled over.[77] One day when Klaas was in a bad mood and hit one of the hind oxen, a member of her party "kicked him soundly", so that he left.[78] Barber never interpreted his actions as acts of resistance, which is striking, since she observed that he was as unhappy with the travel group as she with him. Similarly, she mentioned that an anonymous "Kafir wagon driver" collected butterflies, which were "spoiled".[79] Later she wrote about the insect Mantis that damaged butterflies. At no time did she mention that the Mantis could have damaged the specimens.[80] She never interpreted a damaged butterfly as an act of her African collaborators' resistance either. Barber used this instance to stress the discrepancy between that collector's skills and her brother's and her own, which contributed to the "finest collection of South African species of butterflies in the world" at the South African Museum in Cape Town, while their collaborators, whom she degraded as helpmates, collected damaged butterflies that she could not use as specimens.[81] In Victoria, Barber's travel party employed Cobus, with whom she was very satisfied as he treated the cattle well, worked hard, was balanced and "a true South African wagon driver, full of fun, and anecdotes of 'the road': he was every body's friend, and always willing to make himself generally useful".[82] She described him as an obedient friend and useful servant who was well adapted to settler society, "a reformed, recognizable Other" with "a difference that is almost the same, but not quite", to use Homi Bhabha's words. She lingered on the "ambivalence" between herself and him. This "mimicry" could unintentionally become subversive and be "one of the most elusive and effective strategies of colonial power and knowledge",[83] as the case of Klaas and the unnamed entomological collector had shown.

Barber envisioned a society in which Africans and settlers could harmoniously live together under 1820 Settlers' rule. She uses animal metaphors to state that she wishes that Africans were civilized like wild animals that were domesticated. If that were the case, domesticated animals such as the starling and vulture could establish a strong friendship with cats and dogs.[84] Her choice of metaphor also illustrates her conviction that the 1820 Settlers were the real owners of the land, much like the dog and cat are the real pets and thus superior in her hierarchy.

This wish and her encounter with John William Colenso (1814–83) influenced her perception of the amaZulu. As Bishop of Natal, Colenso had translated the Bible into Zulu, in collaboration with an assistant. Thereby his assistant, an intelligent Zulu philosopher called William Ngidi (c. 1830–?), questioned passages, and this had initiated Colenso's reflections on whether the Pentateuch and the Book of Joshua should be understood as literally or historically accurate. This, as well as his counter-cultural views on Zulus, led to a scandal in the High Church party in South Africa and in

England.[85] Her encounter with Colenso strengthened her religious doubts and she found that they had much in common.[86] She now saw the amaZulu as idle and inferior to workers from the Indian subcontinent ("Coolies") on sugar plantations.[87] She admitted that she knew and cared little "about the habits and customs of these people".[88] They ranked similarly to the amaXhosa and amaFengu above the amaZulu and the Griqua. These examples illustrate that her thoughts were ambivalent and that she always moved between Social Darwinist and "noble savage" representations.

In the 1860s, she had advocated against vernacular species names that she called "barbarous".[89] Now she used Zulu names for snakes and plants such as *Umzambete* (*Millettia caffra*), *Ibululu* (Puff adder), *Imamba*, *Inhlonhlo*, *Inhlangwana* for deadly vipers of Natal.[90] She also advocated for vernacular place names, the "much prettier, and less confusing […] native names of any country" and against "repeated and threadbare names" that "denote too plainly the utter blank which must prevail in minds unable to select or invent new names for new countries" such as "'East London West', whatever that means?"[91]

Barber's brother was sympathetic with fugitive amaZulu[92] and learned much from them about nature. He "very often [found] that there is something behind the scenes in the queer tales which the natives relate regarding animals".[93] He referred to certain birds such as the Crested Eagle (*Spizaetus occipitalis*) or bush hawk that took the amaZulu in Natal to their missing cattle and were therefore valued and protected. In the end, Barber and Bowker felt they had to make sure that they were not too sympathetic with the amaZulu, so Barber used Bowker's quote that he was not "a negrophilist", but that he "early love[d] fair play in all [their] dealings, so that in the years to come [they] have no regrets with regard to [their] former proceedings".[94] This statement summarizes Barber's general practice of mediating between Africans and Europeans, appreciating Africans' knowledge, but lowering them. The amaZulu and amaXhosa were most esteemed and most fought against by the British settlers. Barber's journey took place when Britain invaded Zululand and defeated the amaZulu in 1879. That Barber and Bowker adapted information gained by the amaZulu at the point of the nation's defeat is an admission of the wealth of amaZulu knowledge, as well as their appropriation of Africans' knowledges and practices.

In sum, Barber presented British settlers as ethically superior to Africans and Boers. The racial hierarchy Barber established in her travel account gave ground to the moral justification for nineteenth-century imperialism, fanned the settlers' egocentric nationalism and acted as political propaganda against the Boers.

Conclusion

While Darwin aimed to write himself into science, Barber used her expedition journal to leave her scientific community behind, but also to provide her implied readers with insights into an interwoven world of nature and an emerging and consolidating settler society that called the place its own.[95] Rather than the scientific innovation found in her scholarly journal articles, in this work, mainly through her plant descriptions, she produced a socio-cultural topography and contributed to scientific debates on a segregated society. In Barber's society there was gender equality and harmony among ruling European settlers—who were naturalized to the country like the American prickly pears that adapted to Cape Colonial climate—and civilized, "domesticated" Africans.

Many contemporary feminists of European origins were racist. Attempting to cast off gender, Marianne North, for instance, saw "darker foreign peoples not only as subhuman but as even lower evolutionarily than monkeys".[96] She described their suffering and death with remarkable coldness and saw, described and visualized locals as "homogenous masses, picturesque spectacles, and unseemly primitives loaded with superstitions and barbaric customs".[97] Barber's relationships with Africans were more complex, as we have seen.

In June 2015 British biochemist and Nobel laureate Tim Hunt caused a scandal at the World Conference of Science Journalists in Seoul, when advocating for sex-segregated laboratories.[98] Three years earlier, the European Research Council's campaign "Science: It's a girl thing"[99] reproduced gender stereotypes rather than demonstrating that science was actually a sphere in which numerous women had always been successful. Women naturalists are largely absent from museum displays, popular culture and in history of science publications they are a small minority. The initial examples of how Jeanne Baret and other women travellers and scientists have been received has shown that if we want to change women's status in science, we need to open our eyes to women scientists' achievements in the past and present. To do so I challenged the patriarchal image of expeditions as male endeavours and provided insights into the micropolitics of knowledge creation.

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Notes

1. See for instance Glynis Ridley, *The Discovery of Jeanne Baret: A Story of Science, the High Seas, and the First Woman to Circumnavigate the Globe* (New York: Crown Publishers, 2010). Baret is one of a few women who, disguised as men scientists, achieved the utmost success and are part of scholarship and popular culture, such as military surgeon in the British Army. James Miranda Stuart Barry (c. 1789–99–25 July 1865, born Margaret Ann Bulkley). See for instance Lauren Beukes, "The Curious Case of the Cross-Dressing Doctor—Dr James Barry", in *Maverick: Extraordinary Women from South Africa's Past* (Cape Town: Oshun Books, 2004), 34–46.
2. See for instance Eadaoin Agnew, "'An Old Vagabond': Science and Sexuality in Marianne North's Representations of India", *Nineteenth-Century Gender Studies* 7(2) (2011): 1–17; Rebekka Habermas and Alexandra Przyrembel, "Einleitung", in Rebekka Habermas and Alexandra Przyrembel, eds., *Von Käfern, Märkten und Menschen: Kolonialismus und Wissen in der Moderne* (Göttingen: Vandenhoeck & Ruprecht, 2013), 9–26; Rebekka Habermas, "Intermediaries, Kaufleute, Missionare, Forscher und Diakonissen. Akteure und Akteurinnen im Wissenstransfer", in Habermas and Przyrembel, eds., *Von Käfern, Märkten und Menschen*, 43.
3. Catherine Barnes Stevenson, *Victorian Women Travel Writers in Africa* (Boston: Twayne Publishers, 1982), 9–10.
4. Mary Louise Pratt, *Imperial Eyes, Travel Writing and Transculturation* (London: Routledge, 1992), 159, 160, 213.
5. Sara Mills, *Discourses of Difference: An Analysis of Women's Travel Writing and Colonialism* (London: Routledge 1991), 3, 21, 22, 44.
6. Patricia Murphy, "'Escaping' Gender: The Neutral Voice in Marianne North's *Recollections of a Happy Life*", in *In Science's Shadow: Literary Constructions of Late Victorian Women* (Columbia/London: University of Missouri Press, 2006), 142; Dea Birkett, *Spinsters Abroad: Victorian Lady Travellers* (Oxford: Basil Blackwell, 1989), 136, 137, 192, 115, 123.
7. Barbara T. Gates, *Kindred Nature: Victorian and Edwardian Women Embrace the Living World* (Chicago: University of Chicago Press, 1998), 7.
8. See: "Jeanne Baret, Botanist and First Female Circumnavigator, Finally Commemorated in Name of New Species", http://www.history.com/news/2012/01/03/first-woman-to-circle-the-globe-honored-at-last/eurekalert.org, 3 January 2012, accessed 22 November 2015.

9. See Milbry Polk and Mary Tiegreen, *Frauen erkunden die Welt: Entdecken—Forschen—Berichten* (München: Frederking & Thaler Verlag, 2006), 41.
10. David Hilton-Barber, *The Saint, the Surgeon and the Unsung Botanist* (Makhado: Footprints Press, 2014), 52.
11. Barber to Mary Anne Bowker (her brother John Mitford's wife), in Alan Cohen, "A Wedding and Two Wars", in *In a Quiet Way: The Life of Mary Elizabeth Barber, South Africa's first Lady Natural Historian 1818–1899* [unpublished biography in Alan Cohen's private archive], Manuscript, 34.
12. See Mary Gunn and Leslie E. Codd, *Botanical Exploration of Southern Africa* (Cape Town: A. A. Balkema, 1981), 88.
13. Mary Elizabeth Barber, "On the Structure and Fertilisation of *Liparis bowkeri*", *Journal of Linnean Society* (*Botany*) 10 (1869): 455–8; Mary Elizabeth Barber, "On the Fertilization and Dissemination of *Duvernoia adhatodoides*", *Journal of Linnean Society* (*Botany*) 11 (1871): 469–72; Mary Elizabeth Barber, "Notes on the Peculiar Habits and Changes Which Take Place in the Larva and Pupa of *Papilio nireus*", *Transactions of the Entomological Society* 4 (1874): 519–21.
14. Mary Elizabeth Barber, "On Fascination", *Scientific Opinion* 2 (1869): 165–6; Mary Elizabeth Barber, "The Aloe, its Habits and Culture", *Journal of the Royal Horticultural Society* 2 (1870): 80–3; Mary Elizabeth Barber, "On Carnivorous and Insectivorous Plants [abstract]", *Gardener's Chronicle* (1871): 46.
15. See for instance the museum display at the History Museum, Albany Museum Complex, Grahamstown, Case Number 15 (Bowker Case); Alan Cohen, "Mary Elizabeth Barber: South Africa's First Lady Natural Historian", *Archives of Natural History* 27(2) (2000): 187–208.
16. The preface is indicative of this, as well as passages such as "I do not consider our little sketch complete without publishing his letters which will form the conclusion of our wanderings in Natal and Zululand." Barber, *Wanderings*, Vol. III, MS 10560 (c), 122.
17. See for instance Barber, *Wanderings in South Africa*, 1879, Vol. 1, MS 10560 (a), 1.
18. Barber, *Wanderings*, Vol. III, MS 10560 (c), 126.
19. Patricia Murphy, "'Escaping' Gender: The Neutral Voice", 148.
20. The late Gareth Mitford-Barberton's private archive, at the moment with relatives in Banbury, England.
21. For more on her marriage see Tanja Hammel, "Thinking with Birds: Mary Elizabeth Barber's Advocacy for Gender Equality in Ornithology", *The Micro-Politics of Knowledge Production in Southern Africa, Kronos: Southern African Histories* 41 (2015): 85–111.
22. Barber, *Wanderings*, Vol. II, MS 10560 (b), 80.

23. Barber, *Wanderings*, Vol. I, MS 10560 (a), 13–14.
24. Kew Library, Art & Archives, Directors' Correspondence, Vol. 189, Highlands, 9 March [1869 or 1870]; Kimberley Diamond Fields Griqualand West, 30 June 1874. She asked Hooker, who had organized their publication, to send her the earnings. The natural history tales have not been traced.
25. Barber, *Wanderings*, Vol. I, MS 10560 (a), 1–2.
26. Barber, *Wanderings*, Vol. I, MS 10560 (a), Preface, 2.
27. Charles Darwin's *The Voyage of the Beagle* (1838–39), botanist Joseph Hooker's *Himalayan Journals* (1855), William Burchell's *Travels in the Interior of Southern Africa* (1822) and newspaper articles in the *Graham's Town Journal* on David Livingstone (1813–73) inspired her. As they were widely popular and bestselling, they became her role models.
28. Barber, *Wanderings*, Vol. I, MS 10560 (a), 39, 42.
29. Alexis Harley, *Autobiologies: Charles Darwin and the Natural History of the Self* (Lewisburg: Bucknell University Press, 2015).
30. "Wanderings in South Africa by Sea and Land", 1879, by Mary Elizabeth Barber (Part One), *Quarterly Bulletin of the South African Library* Part One 17(2) (1962): 39–53; Part Two, 17(3) (1963): 61–74; Part Three, 17(4) (1963): 103–16; Part Four, 18(1) (1963): 3–17; Part Five, 18(2) (1963): 55–68.
31. Letter to Harvey in *Flora Capensis*, quoted in Ivan Mitford-Barberton, *The Barbers of the Peak: A History of the Barber, Atherstone, and Bowker Families* (Oxford: Oxford University Press, 1934), 85.
32. Murphy, "'Escaping' Gender: The Neutral Voice", 147.
33. Catherine Barnes Stevenson, *Victorian Women Travel Writers in Africa* 4; see Barber, *Wanderings*, Vol. I, MS 10560 (a), 2–4, 5.
34. Barber, *Wanderings*, Vol. III, MS 10560 (c), 111, 121.
35. Barber, *Wanderings*, Vol. II, MS 10560 (b), 77.
36. Barber, *Wanderings*, Vol. II, MS 10560 (b), 78
37. Barber to her older brother Thomas Holden Bowker, Highlands, 14 June 1865, History Museum, Albany Museum Complex, Grahamstown Journal: S.M.D. no. 932.
38. For more on Harvey's views on evolutionary theory see Peter J. Bowler, "Charles Darwin and his Dublin Critics: Samuel Haughton and William Henry Harvey", *Proceedings of the Royal Irish Academy* 109C (2009): 409–20.
39. Darwin to Hooker, Down, 1 August 1857, Darwin Correspondence Project, Letter 2130: http://www.darwinproject.ac.uk/letter/entry-2130 [14/7/13]. Others were American botanist Asa Gray (1810–88) and a German botanist in Australia, Ferdinand von Müller; see for instance Sara Joan Miles, "Charles Darwin and Asa Gray Discuss Teleology and Design",

published by the American Scientific Association *PSCF* 53 (2001): 196–201; Edward Kynaston, *A Man on Edge: A Life of Baron Sir Ferdinand von Mueller* (Ringwood: Penguin Books Australia, 1981), 170–82.

40. See William H. Harvey, *Charles and Josiah, Or Friendly Conversations Between a Churchman and a Quaker* (Dublin: Hodges, Smith & Co., 1862).
41. Barber to Hooker, Kew Library, Art & Archives, Directors' Correspondence, Vol. 189, Letter 114, Highlands, 9 May 1867; Barber, *Wanderings*, Vol. II, MS 10560 (b), 66, 69.
42. Barber, *Wanderings*, Vol. II, MS 10560 (b), 47.
43. Edgar Leopold Layard, "519. *Numida mitrata*", in *The Birds of South Africa: A Descriptive Catalogue of All the Known Species Occurring South of the 28th Parallel of South Latitude* (Cape Town: Juta, 1867), 266–7.
44. Barber, *Wanderings*, Vol. I, MS 10560 (a), 38; Vol. III, MS 10560 (c), 120–1.
45. Barber, *Wanderings*, Vol. III, MS 10560 (c), 110.
46. Barber, *Wanderings*, Vol. I, MS 10560 (a), 45; Vol. 2, MS 10560 (b), 46.
47. See for instance Henry Wadsworth Longfellow: "A Mine", Historical Society website, www.hwlongfellow.org/poems_poem-php?pid=201, ©2000–2014 Maine Historical Society [20 April 2014].
48. Barber to Hooker, Kew Library, Art & Archives, Directors' Correspondence, Vol. 189, Letter 114, Highlands, 9 May 1867.
49. see Letter from Schreiner to Arthur Symons in: Samuel C. Cronwright-Schreiner, *The Life of Olive Schreiner* (Boston: Little, Brown, and Company, 1924), 187–8; Letter to Havelock Ellis, Buxton, 5 August 1884, Harry Ransom Center, University of Texas, Austin: HRC/CAT/OS/2a-iv; Ruth Knechtel, "Olive Schreiner's Pagan Animism: An Underlying Unity", *English Literature in Transition, 1880–1920*, 53(3) (2010): 259–82, here 261; Olive Schreiner, *From Man to Man* (*Or Perhaps Only …*), (London: Unwin, 1926), 181.
50. Ruth Knechtel, "Olive Schreiner's Pagan Animism", 261.
51. Barber, *Wanderings*, Vol. I, MS 10560 (a), 38–9.
52. Barber, *Wanderings*, Vol. I, MS 10560 (a), 43.
53. Barber, *Wanderings*, Vol. II, MS 10560 (b), 51.
54. Barber, *Wanderings*, Vol. II, MS 10560 (b), 67, 71, 85.
55. Barber, *Wanderings*, Vol. III, MS 10560 (c), 111.
56. Barber, *Wanderings*, Vol. I, MS 10560 (a), 22; Vol. III, MS 10560 (c), 91.
57. Barber, *Wanderings*, Vol. I, MS 10560 (a), 10, 31; Vol. III, MS 10560 (c), 104.
58. Barber, *Wanderings*, Vol. II, MS 10560 (b), 53.
59. Barber, *Wanderings*, Vol. II, MS 10560 (b), 53.
60. Barber, *Wanderings*, Vol. II, MS 10560 (b), 83.
61. Barber, *Wanderings*, Vol. III, MS 10560 (c), 101.

62. Barber, *Wanderings*, Vol. II, MS 10560 (b), 63.
63. Barber, *Wanderings*, Vol. I, MS 10560 (a), 11–12.
64. Barber, *Wanderings*, Vol. I, MS 10560 (a), 11–12, 32.
65. Barber to Darwin, after February 1867, Darwin Correspondence Project, Letter 5745, http://www.darwinproject.ac.uk/entry-5745, accessed 14 May 2015.
66. Robert Shanafelt, "How Charles Darwin Got Emotional Expression out of South Africa (and the People Who Helped Him)", *Comparative Studies in Society and History* 45(4) (2003): 827.
67. See German linguist Wilhelm Heinrich Immanuel Bleek's (1827–75) *On the Origin of Language as a First Chapter in a History of the Development of Humanity* (1867), edited with a preface by Ernst Haeckel (translated from the German original); W. H. I. Bleek, "Scientific Reasons for the Study of the Bushman Language", *Cape Monthly Magazine* 7 (1873): 150; David N. Livingstone, "Debating Darwin at the Cape", in Patrick Harries, Martin Lengwiler, eds., *Knowing the World: Science and Expert Knowledge between Europe and Africa 19th and 20th century*, forthcoming 2016, manuscript, 10.
68. Patrick Harries, *Butterflies and Barbarians: Swiss Missionaries and Systems of Knowledge in South-East Africa* (Oxford: James Curry, 2007), 47.
69. German zoologist and philosopher Ernst Haeckel (1834–1919), in his introduction to *On the Origin of Language* (1869), the English translation of the previously published German text, Darwinized the entire study. Ernst Haeckel, "Editor's Preface", in Wilhelm H. I. Bleek, *On the Origin of Language* (New York: L. W. Schmidt, 1869), iv.
70. Barber, *Wanderings*, Vol. I, MS 10560 (a), 5–7.
71. See for instance Barber, *Wanderings*, Vol. I, MS 10560 (a), 1, 3, 9, 15, 25, 31, 32, 34, 44.
72. Barber, *Wanderings*, Vol. I, MS 10560 (a), 3.
73. Barber, *Wanderings*, Vol. I, MS 10560 (a), 25.
74. Barber, *Wanderings*, Vol. I, MS 10560 (a), 9.
75. Barber, *Wanderings*, Vol. I, MS 10560 (a), 15.
76. See e.g. Barber, *Wanderings*, Vol. I, MS 10560 (a), 1.
77. Barber, *Wanderings*, Vol. I, MS 10560 (a), 18.
78. Barber, *Wanderings*, Vol. I, MS 10560 (a), 34, underlined in the original.
79. Barber, *Wanderings*, Vol. III, MS 10560 (c), 88.
80. Barber, *Wanderings*, Vol. III, MS 10560 (c), 91–2.
81. Barber, *Wanderings*, Vol. III, MS 10560 (c), 88–94; 57.
82. Barber, *Wanderings*, Vol. I, MS 10560 (a), 34–5.
83. Homi Bhabha, *The Location of Culture* (Oxon/New York: Routledge, 1994), 122.
84. Barber, *Wanderings*, Vol. I, MS 10560 (a), 36–7.

85. Barber, *Wanderings*, Vol. III, MS 10560 (c), 101.
86. It started with the publication of *The Pentateuch and the Book of Joshua Critically Examined* (1862). See for instance Jeff Guy, "Class, Imperialism and Literary Criticism: William Ngidi, John Colenso and Matthew Arnold", *Journal of Southern African Studies* 23(2) (1997): 219–41, here 221.
87. "Wanderings in South Africa by Sea and Land, 1879 by Mary Elizabeth Barber (Part Four)", *Quarterly Bulletin of the South African Library* 18(1) (1963): 13.
88. Barber, *Wanderings*, Vol. III, MS 10560 (c), 106.
89. Barber to entomologist Roland Trimen, Royal Entomological Society Archives and Library, St Albans, England, Trimen Correspondence, Box 17, Letter 39, Highlands, 6 September 1864.
90. Barber, *Wanderings*, Vol. III, MS 10560 (c), 105, 113.
91. Barber, *Wanderings*, Vol. III, MS 10560 (c), 98.
92. Barber, *Wanderings*, Vol. III, MS 10560 (c), 118.
93. Barber, *Wanderings*, Vol. III, MS 10560 (c), 116–17.
94. Barber, *Wanderings*, Vol. III, MS 10560 (c), 137.
95. Barber, "Locusts and Locust Birds", 193–218; Barber, *The Erythrina Tree and Other Verses* (London: Rowland Ward Limited, 1898); Barber, "Stapelias", *Kew Bulletin* (1903): 17–19 [posthumously published presumably by William Turner Thiselton-Dyer].
96. Murphy, "'Escaping' Gender: The Neutral Voice", 164.
97. Murphy, "'Escaping' Gender: The Neutral Voice", 165.
98. Akshat Rathi, "Nobel Laureate Says Shockingly Sexist Things About 'Girl' Scientists, Kind of Apologizes", *Quartz*, http://qz.com/424329/nobel-laureate-says-shockingly-sexist-things-about-girl-scientists-kind-of-apologizes/ 10 June 2015, last accessed 19 September 2015.
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CHAPTER 7

Materializing the Aurora Borealis: Carl Weyprecht and Scientific Documentation of the Arctic

Ulrike Spring

In 1875, the German-born and Trieste-based polar explorer Carl Weyprecht, having recently returned from a two-year expedition to the Arctic, published an article on the aurora borealis in the liberal Vienna newspaper *Neue Freie Presse*. He finished his text with the following reflections:

> And down there we stand, we piteous, tiny men, and speak of science and progress, and fancy that with our reason we are capable of overhearing the secrets of Nature; there we stand and look up at the riddle, which Nature has written with flaming letters on the dark night sky, and can only marvel and confess that we basically know nothing.[1]

Here, Weyprecht touches upon several topics prominent in Western culture of the nineteenth century, such as the relations between the rational and the mystic, and between culture and nature. He draws on a tension common in descriptions of the aurora: the limits of human knowledge

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and science when faced with this astounding spectacle of nature. The late nineteenth century was also a time when general and scientific interest increasingly shifted its focus to the sky, the air above. Efforts to conquer the air through means of transport had been popular since the eighteenth century, with the air balloon being the most prominent example. Jules Verne's highly inventive scientific romances, published from 1863 onwards, encouraged this general interest in the air. Towards the end of the nineteenth and particularly from the early twentieth century onwards, knowledge about space rapidly increased and was to define expectations and images of the future.

Nineteenth-century research of the aurora was an integral part of this desire to know more about cosmic questions. The previous centuries had witnessed repeated efforts to explain the aurora and its association with cosmic and terrestrial phenomena. In the mid-1800s, researchers discovered the link between the frequency of solar spots and the aurora.[2] Increasingly, the aurora was, in Weyprecht's words, considered "the only phenomenon that could provide knowledge about […] the outmost borders of the atmosphere—it was "a new connecting bridge to other worlds".[3]

In the 1870s, however, his wish to understand the aurora was challenged by the difficulty of applying scientific standards onto it, and of making it into a scientific object that could be studied, analysed and displayed. Unlike other nature phenomena that were studied increasingly using photographs, the aurora only slowly became part of this reproduction process.[4] Laboratory experiments at that time yielded little success in explaining the aurora.[5] Scientific analysis of the aurora therefore had to rely on observation and on traditional documentation in the form of writing and drawing. This, on the other hand, helped, as I have argued elsewhere,[6] to make the aurora a fitting object for scientific popularization: in an age of mechanical reproduction and scientification of society, the aurora kept its aura of originality and authenticity, as something magical or mystic, thus constantly counterbalancing attempts at scientific objectification or disenchantment. This oscillation between science and magic has been typical of the discourse of the aurora up to the present day. In their book on Kristian Birkeland and the aurora observatory near Alta in northern Norway founded by him, Nielssen and Petterson imagine the early twentieth-century researchers at the observatory when observing the aurora: "It was a matter of taking out the photography equipment and note pad in order to make observations. But sometimes, we must believe, they just stood there and enjoyed the view, enchanted by the mighty light show against the dark sky."[7]

In the following I will discuss these efforts to transform nature into science, to translate experience into data. I will draw on the observation and documentation practices by members of the Austro-Hungarian Arctic Expedition (1872–4) and analyse their efforts in subjugating the fleeting phenomenon of the aurora to both human and scientific rule. I am particularly interested in the various attempts to materialize the aurora, and in the different practices that were employed and invested with varying degrees of scientific authority. As long as the aurora could not be captured on photographs or films, observing by eye or with the help of instruments, writing and making lists and tables were the dominant and most acknowledged modes of incorporating it into scientific knowledge. Drawing—another prominent scientific practice of documentation and of establishing evidence—could only be used to a limited extent to turn the constantly changing aurora into an object of inquiry. While I do not want to imply that scientific drawings in fields such as botany or geology were mimetic representations of the original, the botanist or geologist could at least aspire to faithfully represent them; they were, moreover, descriptions and models and functioned as proof that the plant or flower existed.[8] Visual representations of the aurora in the 1870s, on the other hand, had to remain artworks, hence challenging the boundaries of scientific discourse. Moreover, whereas naturalists were usually able to observe and collect physical objects in the field, in the case of the aurora this process was reduced to observing and collecting impressions. Unlike physical objects such as plants, stones and animals, the aurora could not be captured, put into bottles or boxes and brought home. In other words, only the memory remained once the actual phenomenon had disappeared or the field had been left. In order to be able to grasp it as a scientific object, it thus had to be materialized.[9] To "materialize" the aurora is not to imply that it turns into a tangible object, rather that it continuously materializes by being translated, stabilized or performed.[10] While this applies to much scientific research such as in meteorology and astronomy, an additional challenge was introduced by the specific locality of the aurora: it can be seen regularly only in the polar regions.[11] Many scientists and most people hence only knew the aurora from descriptions and not from personal experience, or, if they did, they were rarely familiar with its spectacular character as seen in the north. An important task for the Central European expedition members was to rekindle and communicate memories of the aurora: they had to find a language with which to convey the spectacularity of the aurora not only to their readers, regardless of whether they had a scientific or just a general interest in the north, but also to themselves.

I suggest that, as a result of all these uncertainties, the aurora necessarily oscillated between scientific, literary and artistic discourses, constantly challenging efforts of categorization and scientification. Hence the acts of observing and documenting the aurora functioned as experiments that helped to test the boundaries of science. Most of all I am interested in the processes that lead to constituting a natural phenomenon as scientific. As I will show, the specific contexts of the Arctic and of a polar expedition are essential for understanding these processes.

Observing the Aurora

The Austro-Hungarian Arctic Expedition left the north Norwegian town Tromsø for the Arctic in June 1872, under the double leadership of sea officer Carl Weyprecht (command at sea) and army officer Julius Payer (command on land), and with a crew of 22 men on board, who, with the exception of one Norwegian, were all gathered from various parts of the dual monarchy. The expedition was an experiment in the traditional meaning: it was a risky venture into unknown areas with an uncertain outcome. The expedition was also a scientific experiment as little was known about conducting science in the Arctic; moreover, it was a social and cultural experiment as it recruited seamen from the Adriatic rather than from the north, something unheard of in polar explorations. Weyprecht would later state that the discovery of the southern seamen's easy adaptability to the challenges of the polar region was more important than the discovery of new land.[12] The original plan was to explore the sea north of Siberia and possibly find the as yet unknown north-east passage. However, just before reaching the islands of Novaya Zemlya, their ship the *Tegetthoff* was frozen in the ice and drifted towards the north-west instead of the east. After a year of drifting, in August 1873, the expedition members set eyes on a land that they decided to name "Kaiser-Franz-Joseph-Land" in honour of the emperor of Austria and king of Hungary. In May 1874, and after three sledge expeditions on the newly discovered territory, the men decided to leave their ship, which was still frozen into the ice, and make their way back to Europe on foot and by boat. After months of enormous toil they finally reached Novaya Zemlya where they were taken on board by a Russian skipper. Early in September 1874, the men reached Vardø on the north-eastern corner of Norway, where they were met by the first of many celebratory receptions. From the time of its departure until its return more than two years later, the expedition thus was characterized by the unknown—first the mysteriousness of the places aimed for, then being forced by the ice to embark on a different and uncertain path, and finally having to adapt their daily routine and their scientific practices to the new surroundings and to the little-known challenges of the Arctic environment.

Instructions and instruments should help to reduce the number of these uncertainties. Before embarking on the expedition, the leaders had acquired the necessary instruments for the planned scientific observation. Some of them were donated to the expedition by the war ministry and the Academy of Sciences in Vienna.[13] Some instruments were provided by Johann Lamont, director of the observatory in Munich,[14] others were purchased.[15] Despite an increasing number of scientific expeditions to the Arctic in the second half of the nineteenth century, little was known about the environment and the conditions for scientific research there; the scientific practices employed by the expedition and the experiences they met there were to provide important knowledge for future polar expeditions. In 1881, Weyprecht published instructions particularly aimed at the researchers participating in what was later called the First International Polar Year (IPY) in 1882–3. Based on his years in the Arctic, he recommended that instruments be as uncomplicated as possible in order to increase exactness.[16] He also suggested the use of Lamont's travelling theodolite[17]; according to Weyprecht, his expedition had been the first to use this light theodolite for magnetic and aurora observations.[18]

The expedition members encountered various challenges in their efforts to conduct proper scientific observation of the aurora. In particular the polar environment posed a serious trial: because of the midnight sun, the aurora could only be observed in the dark months; due to the darkness of the polar night the use of light sources for the act of observation was required; the cold made the handling of instruments and the documenting of the observations a permanent struggle; and in the winter of 1872–3 almost constant ice pressures and movements, in addition to the accompanying danger, made focused and exact observation almost impossible, although the men managed to conduct regular observations.[19] One advantage the men had was the relative lack of movement of the ship after the first winter: Whereas expeditions are usually characterized by movement, thereby constantly altering the conditions of observation, the

Austrian-Hungarians spent much of their time off the newly discovered land and could thus conduct most of their observations from approximately the same spot. Accordingly, Weyprecht distinguished between two periods of observation: the first from August 1872 to November 1873, when the ship was moved by drift ice; the second from November 1873 to May 1874 at Franz Joseph Land.[20] Unfortunately, we know little of the observation and documentation processes of the aurora during that time. We know, however, that much of the scientific work was done by eye, helped by some mechanical devices such as the above-mentioned theodolite, an inclination compass, magnetic variation instruments and a spectral instrument that was used to measure the spectra of the aurora.[21] Weyprecht would however consider the results based on the spectral instrument's measuring as unreliable, since the instrument turned out to be too weak.[22] In the second winter, magnetic instruments were used for observing meteorological variations, including observations of the aurora. Weyprecht appointed a second observer who was solely responsible for noting down the position of the aurora.[23] In particular, Weyprecht wished to find out more about the connection between the aurora and magnetic disturbances, and, as he later explained, he would ask one man to observe the movement of the needle while another would observe the movement of the aurora. In order to minimize human interference, the men were asked to conduct their observations independently from each other so that "mutual interference" could be avoided.[24] Here Weyprecht followed the rules of laboratory work by trying to eliminate possible contaminants.

Daston and Lunbeck point out that "[l]ike experiment, observation is a highly contrived and disciplined form of experience that requires training of the body and mind, material props, techniques of description and visualization, networks of communication and transmission, canons of evidence, and specialized forms of reasoning." However, as they also emphasize, from the early nineteenth century onwards, scientific observation had increasingly been considered to consist of a "mere registration of data" and hence could be relegated to assistants.[25] This ambivalence in the practice of observation was reflected in the observers chosen and the observation conducted on the expedition. In an internal instruction before leaving Vienna, the tasks of scientific work had been equally distributed: Weyprecht, lieutenant Gustav Brosch and petty officer Eduard Orel were responsible for observations concerning astronomy, physics and meteorology, Payer for cartography, geology and glaciology, and the physician Julius (Gyula) Kepes for zoology and botany.[26] Weyprecht, however, assigned to four expedition members the responsibility of documenting the northern lights in a so-called "Nordlichtjournal" from February 1873 until the ship was abandoned in May 1874. The four men were Brosch, Orel, boatswain Captain Pietro Lusina and ice master Captain Elling Carlsen.[27] During the first winter the machinist Otto Krisch, who died early in 1874, also participated in the meteorological and aurora observations.[28] None of them, including the two leaders Weyprecht and Payer, were professional scientists, something that was not uncommon at a time when the differentiation between amateur scientist and scientist proper was not yet clearly marked. At the same time, scientific knowledge and social position on board played important roles in the way scientific work was distributed and evaluated.

Weyprecht, who would later have the main responsibility for most scientific analysis, interpretation and dissemination of the expedition's observations, although helped by others,[29] was scientifically schooled, though he lacked a formal education in science and hence could be categorized as an amateur scientist when he embarked on the expedition. He would in the years to come until his early death in 1881 acquire a more solid scientific standing and reputation, but still considered himself a scientific "bungler" because he did not have university education.[30] Brosch and Orel were assigned scientific authority due to their positions as officers, while captains Lusina and Carlsen were probably selected because of their social position on board and their intellectual capacity rather than because of their scientific knowledge. The polar sea skipper Carlsen (and the only Norwegian on board) did, however, have long experience in providing objects and scientific observations for scientifically interested men in Norway, but his role had consisted mainly of being a collector of knowledge and an amateur observer of scientific phenomena rather than their analyst. These differences in background, positions and interests were, if we believe Weyprecht, also reflected in the results: while the officers were very thorough in their observations, noting down most traces of the aurora in the meteorological journal, he suspected that the two captains tended to document only the most spectacular displays in this particular journal.[31] The choice of not employing other crew members illustrates the social exclusivity of scientific observation.

Weyprecht would later make it clear that he considered professional scientists an asset for research in the north. In his famous lecture in Graz in 1875, in which he launched the idea of what would later become the first IPY, he wished that scientists and not laymen would conduct the observation processes in the polar regions. While laymen were useful as collectors, the participation of professional scientists was a necessity if one was to achieve "solid scientific success".[32] In his aforementioned instructions for the researchers of the aurora and of magnetism at the IPY, published shortly before his death in 1881 and only a year before the IPY was inaugurated, he adjusted his former demand, yet without renouncing his belief in the superiority of professional scientists. He dedicated the booklet chiefly to his "inexperienced successor".[33] As he stated, the great amount of phenomena to observe and the lack of room available on polar expeditions often made it difficult to take along many specialized scientists on expeditions to the north; and even a scientist would face unexpected challenges on his first journey due to unknown conditions. All the more important, then, was the adaption of the instruments and methods of observation to the environment. Weyprecht thus considered first-hand experience as superior to "any theoretical preparations".[34] And, as he repeatedly indicated, most of the writers of the available textbooks on conducting research on the aurora and magnetism in the north lacked this kind of experience.[35] In the late nineteenth century, expeditions to the north were still experimenting with how to survive and to adapt to the fiendish environment.

Weyprecht pointed here to different forms of scientific authority that, in turn, accounted for different ways of creating scientific evidence: one that acknowledged the distinction between professional and amateur scientist (although, as mentioned above, these positions were in the 1870s more broadly defined than today), and one that prioritized direct experience, being in the field, over "armchair-science". Observation functioned as a guarantor of authenticity and scientific precision, as opposed to the interpretations of those who had to rely on images and descriptions of the aurora made by others. Weyprecht and the other expedition members, in particular his co-leader Payer, with his many literary writings and drawings of the north, could thus become authorities on the aurora: the former primarily in a scientific sense, the second in an artistic one. This ascription was confirmed and strengthened by the unique environment—very few of their readers had been to the Arctic, no one had been to Franz Joseph Land and not many had actually seen the aurora with their own eyes. A third possibility was not mentioned by Weyprecht, probably because it was considered not scientific, although it was present on the expedition through Elling Carlsen's participation: the local knowledge of the people living in the north. By drawing a clear distinction between those who had been in the field and those who had not, Weyprecht ascribed himself authority in the difficult process of documenting the aurora.

The limited access to the aurora was reflected by the spatial organization of the observation activity, by being physically set aside from everyday life and potential interference from other expedition members. In the first year, the observations were conducted in "a simple linen tent"[36] built on the ice outside the ship, later to be replaced by huts made of wood and snow.[37] These places served as observatories and had laboratory-like features, such as restricted access and the scientific practices carried out there. In these "labs", to borrow from Gieryn, "[w]ild nature" was "repositioned in a technical and cultural environment that [gave] all power to the investigators".[38] In general, the distinction between ship and observatory was strictly kept; this was also due to the potential influence of the ship's iron on the collection of data.[39] At the same time, a ship was a key element in scientific expeditions to the north: for Weyprecht, magnetic observations in the north were highly dependent on a ship as a base, and he recommended building observation spots close to the ship because of the frequent snowstorms.[40]

To a certain extent, the Arctic itself served both as a field and as a laboratory in the context of the expedition. A field-site, in that only in the subpolar and polar regions could the aurora be observed and studied on a regular basis, where it was more "real" than it ever could have been in a laboratory. Moreover, in the field the bodies of the explorers themselves, their understanding and sensing of the Arctic as a result of being there for over two years, could take over the role of mechanical instruments.[41] But the Arctic was also a laboratory in that it was a contained space where, if we consider the expedition genre, only a few daring men had access; the Arctic was set apart from the outside, that is Europe in this case, and in the 1870s and 1880s, only in this space could the object of investigation, the aurora, be studied reliably and repeatedly, with the help of mechanical devices, human bodies and strict instructions and rules.[42]

Documenting the Aurora

The experience of the aurora is immediate, sudden and overwhelming—one way of comprehending it is to take a step back, into the realm of scientific observation, by opening a gap between the observed and the observer. Efforts to narrow the gap or even to close it, are one of the aims (and claims) of science and can continuously be seen in the discourse on the aurora.

As mentioned above, Weyprecht assigned to four expedition members the responsibility of documenting the northern lights in a so-called northern lights journal.[43] Yet, when leaving the ship behind in the ice in May 1874 (no remains of the ship have ever been found), the return to Europe on foot and by boats did not allow any excess weight or room, and Weyprecht decided to leave the journal behind, because, as he later regretfully stated, he then thought that "the mere description" could not generate "positive results".[44] But he took along the journal with the meteorological observations where some aurora observations were noted down—he even asked some expedition members to copy the notes from the heavy folio books into a smaller notebook in order to save weight.[45] The interpretation of the aurora in the years to come thus had to rely on these peripheral observations, in addition to Weyprecht's own notes.[46] Another shortcoming derived from the different ways in which observation had been conducted. As mentioned above, while the officers were very thorough in their observation noting down most traces of the aurora, the two captains tended to record only the most spectacular aurora displays in this journal. While the final number of the aurora displays was thus not fully reliable, Weyprecht nevertheless regarded the "relative figures" as "sufficiently exact", since the observers took regular turns at observation.[47] Yet he was less certain about the scientific merit of the observations on the position of the aurora, as it turned out to be impossible to conduct regular measurements of its location with exact instruments; he had to rely on estimations and compass bearings instead.[48] While Weyprecht knew very well the deficits of his aurora material and hence analysis, he nevertheless was aware of the new knowledge he brought to the field. His aim was to add new information to the growing literature on aurora observations, and he saw the publication of his findings justified by the fact that the aurora displays were different from the ones already observed and especially from the ones observed further south and which often had been the basis for research on the aurora.[49] Accordingly, his visionary plans for simultaneous scientific observation at various places on the globe (which were realized in 1882–3 with the IPY) included the observation of the aurora in the north and possibly the south. The uncertainty of scientific knowledge about the aurora borealis, and the desire to change this into certainty were an important motivation for Weyprecht before setting out to the Arctic in 1872; and the aurora's ambivalent character, as a scientific object oscillating between the known and the unknown, the scientific and the speculative, was reflected in his texts.

Weyprecht and his colleagues applied various documentation strategies in their efforts to incorporate the aurora into scientific and general knowledge. Traditionally in science, the documentation process takes place in several phases, starting with preparing for the expedition, taking notes while observing and finally publishing the outcome with its focus on analysis and interpretation, or public presentation in form of exhibitions or lectures. Weyprecht pointed out, in a letter to the German geographer August Petermann two months after the return of the expedition, that while the scientific significance of the data collected was beyond doubt, proper analysis was required and it would take at least a year before results would be ready to be published. He was critical of attempts by Petermann and journalists to interpret the results of the expedition themselves, and he stated explicitly that he did not wish to read or hear of any conclusions based on the results of the expedition by anyone without knowledge of the details.[50] Here, the distinction between scientific authority based on direct experience and that based on second-hand information becomes obvious.

The documentation process also depended on the target audience. As Payer noted in the introduction to his expedition report from 1876, while the scientist would wish for scientific observations, tables, maps and drawings, and the future polar explorer for information on errors, for advice and experiences from previous travels, the greater audience would demand a focus on "interesting" subject matter.[51] This distinction was reflected in Payer and Weyprecht's writings, with the former addressing a more popular audience and the latter writing mostly for scientifically interested readers. While their topics and their forms of presentation were adjusted to genre and expected readership, the language they used crossed these boundaries. Indeed, Weyprecht's scientific descriptions of the aurora were characterized by the near impossibility of describing it due to its shifting nature and the different speeds at which it changed. He observed that he had repeatedly to amend his opinions about the qualities of the aurora, even when he had already determined them as verified, and that a correct description of the aurora was almost impossible due to its constantly changing character and to different auroras appearing in several shapes.[52] In a lecture for a scientifically interested audience he stated: "The phenomenon defies any description and any conventional categorization, it displays continuously new shapes and often changes from one moment to the next." For instance, despite his efforts, he was never able to describe the beginning of the aurora: "[T]he apparition is there, but how and where it has come from cannot be said."[53]

The efforts to transform experience into observation and to translate the observed into data, into something that could become the basis for scientific understanding, meant to fix the observed object, to materialize the aurora. To be more precise, it was the memory of what had just happened on the sky the expedition members tried to sketch down—and inevitably had to fail in, because of the divide between the fleeting spectacular phenomenon and the stable language and imagery used to describe it. Here, the power over nature, which the laboratory bestows on the investigators, failed.

Materializing the Aurora

How, then, could these ephemeral traces be materialized, how could the fleeting, the evasive, the absent be transformed into scientific data, fact and reliable memory? After all, this is a crucial aspect of what scientific documentation is about: trying to translate nature's "writing" into human writing, making it part of memory, and hence available for inquiry. Weyprecht's metaphor of the aurora as "flaming letters" against the dark sky was indeed appropriate.

The practice of writing in scientific knowledge gathering, as a form of scientific practice, is essential: it potentially fixes natural phenomena for eternity, it relates to previous human knowledge and anticipates future knowledge, and it is one of *the* symbols of human civilization. Furthermore, writing controls memory; nature and its objects become part of the archive of human knowledge. The expedition's aim was to make the aurora part of that archive and, in order to do so, the fleeting character of the aurora had to become part of scientific and popular memory: it literally had to be *inscribed* into human knowledge. On a more practical level, the aurora, with its secretive and spectacular character held an appeal which could make the expedition more "interesting", to refer to Payer's words.[54]

In their efforts to capture the aurora in scientific language and measurements, in written text and tables, the explorers faced an additional challenge: the lack of an internationally acknowledged nomenclature, that is, an agreement on the terminology used to describe the various shapes of the aurora. This uncertainty affected the act of observation during the expedition as well. As Weyprecht pointed out, the terms used by the expedition members were more or less arbitrary and chosen by the observers themselves,[55] something which posed a challenge to the reliability, repeatability and controllability required of scientific investigation. He lamented the difficulty in comparing previous observations with his own because of the confusion of terms; indeed, one could never be sure whether the other observers had seen the same phenomena as oneself.[56] Here the difficulty of capturing the aurora through empirical categorizable observation becomes explicit. Robert G. David observes this imprecision also in the British discourse of the Arctic in the nineteenth century: one common challenge "was the lack of language codes and artistic conventions suitable for describing and depicting the very different environment that the explorers encountered".[57]

In his publications on the northern lights in the wake of the expedition, Weyprecht wished to forestall a similar situation by creating a common language in which he tried to account for the shifting appearances of the aurora. It was a common language in the literal sense, as Weyprecht tried to find terms in French, English and German which all signified the same phenomena.[58] In his instruction brochure from 1881, he attempted to create a classification system for the description of the aurora, which was more elaborate than the ones he had used earlier: whereas he had distinguished five characteristic forms in 1878—arches, streamers, threads, corona and haze[59]—he now added "dark segment" and "polar shine".[60] He differentiated also between various movements of the aurora and categorized it according to form, position, height, intensity, direction, movement of light, concentration of the rays, colour. He even added a category for notes and sketches regarding deviations or interesting observations,[61] thereby acknowledging the limits of his categorization scheme in analysing a shifting phenomenon such as the aurora (Fig. 7.1).

Maybe an even greater challenge than finding a common nomenclature was to match one's own experience of the actual phenomenon with the appropriate terms: inevitably, there had to be a discrepancy, as static terms could not describe the fleeting impressions and as the readers had to rely on their own imaginations and associations. For instance, during the expedition Weyprecht and his colleagues had differentiated between "*Strahlenwurf*" (fall of rays) on one hand and suddenly appearing rays on the other; however, in the notes taken back to Europe, this distinction

Datum	Stunde	Form	Position	Höhe	Intensität	Richtung des Zuges	Lichtbewegung	Concentrationspunkt d. Strahlen	Farbe	Anmerkungen
Nach bürgerlichem Tage		Nach der oben gegebenen Eintheilung I, II, III, IV, V, VI, VII. Bei I, II, IV ist hinzuzusetzen, ob das Polarlicht in Strahlen geordnet ist oder ob es nur aus verschwommener Lichtmasse besteht.	Angabe der Quadranten (magnetisch), über welche sich das Polarlicht erstreckt.	Höhe der beiden Ränder über dem Horizonte, im magnetischen Meridiane gemessen.	4. sehr stark 3. stark 2. schwach 1. sehr schwach.	Ungefähre Richtung, nach welcher sich die ganze Erscheinung bewegt, Nord-Süd, oder Süd-Nord.	*a* oder *b*, in ersterem Falle West-Ost oder Ost-West.	Die Angaben der Messung, d. h. Azimuth jedes gemessenen Strahles und Winkel mit der Horizontalen.	Jede vom gewöhnlichen Weiss abweichende Färbung.	Alle auffallenden Eigenthümlichkeiten, wo möglich Skizzen.

Fig. 7.1 Carl Weyprecht, Table for aurora observation, in Weyprecht, *Praktische Anleitung zur Beobachtung der Polarlichter*, 48 Universitätsbibliothek Wien

had not been used, with the various forms being covered only by the term "*Strahlen*" (rays).[62] The readers thus had neither material nor imaginary access to the great variety of the aurora displays.

As the examples show, efforts to transform the constant changing of the aurora into something stable through scientific language and tables were challenged by the aurora's very character. As Weyprecht stated in an article aimed at a wider readership, "no words can describe it in all its magnificence".[63] Indeed, one might argue that the aurora took over agency, infecting the observer's and documentarist's language and attempts to describe it. Hence the tables listing the readings of the aurora try to pin down its continuous movements, but they are always delayed and only able to catch some essential changes or shapes instead of the phenomenon as a whole. While Weyprecht employed the rhetoric of spectacularity and wonder mainly in his texts aiming at a wider readership, he also used literary imagery in his descriptions intended for scientists and polar explorers. In his scientific presentation of the aurora observations he would describe the aurora as a "mere rag" that "bends in gracious folds" and resembled a "flapping pennant played with by a light breeze" or as a "broad band of fire".[64] The boundary between literary and scientific language is porous and more or less continually transgressed; I would argue, however, that in addition to its still being a scientific riddle, the specific nature of the aurora made it particularly prone to this process. In other words: while scientific descriptions of the aurora attempted to capture the phenomenon through schemata and lines by objectifying it, at the same time they stood in an ambivalent relationship to its literal indescribability. In particular Payer, with his flowery language, would place the aurora firmly on the border between literature and science.[65] Metaphors of the aurora in his book included "whirling fumes" and "immense flames".[66]

The aurora thus always remained partly in the field of art, which becomes most apparent in another strategy of documentation and materializing: drawing and painting. Whereas drawing is a common scientific practice, it is regarded as most reliable when being mimetic. Drawings of the aurora, however, can only be the result of the artist's memory, they are necessarily imaginations and impressions and not scientific reproductions in terms of precision and direct reproduction. The aurora would only become visually reproducible by the advanced technology of cinematography.

While Weyprecht himself did not draw, Payer and Orel were talented painters and made several drawings of the aurora. Payer's image of the aurora repeated in visual terms his textual comparisons of the aurora as flames; the dramatic focus on the ship, with the aurora seemingly setting it on fire, anticipated his later career as a professional artist specializing in historical paintings of polar expeditions (Fig. 7.2).

Fig. 7.2 Julius Payer, "Ice Pressures During a Display of Northern Lights in January 1873", in Payer, *Die österreichisch-ungarische Nordpol-Expedition in den Jahren 1872–1874*, 193

Whereas Weyprecht tried to find a mimetic and reliable representation of the aurora through scientific language, Payer employed literary and visual languages. May we here speak of two different approaches to science, to how to turn nature into an object for scientific inquiry, represented by the two expedition leaders? If we follow this line, Weyprecht's efforts to find a scientific register for the aurora were inspired by the taxonomic tradition of Carl Linnaeus, whereas Payer was drawing on Alexander von Humboldt's nature painting (*Naturgemälde*), an aesthetic approach to objects of natural history.[67] Weyprecht in general turned away from Payer's Romantic claim to be able to use intuitive methods—painting and literary writing—to describe the aurora (and the Arctic in general), as he did not consider this scientific. However, in Romanticism and idealism, it is precisely the subjective experience that gives authenticity to representations, and, as I tried to show, this was part of the efforts of deciphering 'nature's writing' by the expedition members. Hence Payer's and Orel's images would be part of the authentication of the aurora, while Romanticism also affected Weyprecht's efforts to adhere to scientific empiricism. These different yet converging approaches were typical of the ambivalence the aurora (and scientific discourse in general) fell into, and while in 1874 Weyprecht's method was doubtless considered the more scientific one, the specific character of the aurora would challenge the boundaries of this definition of science.

The Porous Borders of Science

The aurora was more than just an object of science or of admiration and wonder. It was a symbol of idealism, of the pristine Arctic nature, as yet untouched by the taints of modern culture and materialism. For Weyprecht, the more than two years in the north were a cultural and social experiment not only in respect to the aforementioned Adriatic seamen but also on an individual level: the Arctic served as catharsis and offered the possibility to grow as a human being. To a certain degree, the north and its phenomena became the very symbol of the ideals of science: to aspire to higher aims for the well-being of human kind. Weyprecht wrote in 1876: "And above all one does not know what enthralling interest the magnificent and unfamiliar natural phenomena located in the vicinity of the pole, evoke in any person whose higher ambitions have not yet been overcome by the care for material pleasures."[68] Indeed, we may say that the north was the ideal place for scientific investigation, as here—due to the lack of distraction—the focus had to be on the whole: "Here, attention, not being dispersed and influenced by the particular, concentrates on the powers of nature themselves." In the north, everybody might turn into a scientist or thinker, even without actually wishing it: "Instinctively, the thinking person changes from a mere admirer into a researcher, and even the seaman, who usually passes everything without attention, turns into an unconscious thinker when faced with the phenomena, which he marvels at and of which he had never guessed."[69] Social differences were temporarily suspended, nature turned man into a potential scientist or at least thinker, and the epitome of this experience was the aurora.[70]

In Weyprecht's texts, the aurora continually oscillated between being a natural phenomenon that eluded categorization and scientific-objective documentation—an aesthetic rather than a scientific object—and one that could be pinned down in schemes and tables and laid open for scientific inquiry. The various efforts to materialize it, to make it part of the archive of human knowledge, were challenged by the difficulty of deciphering nature's writing. Weyprecht preferred strategies based on scientific empiricism: he could guarantee the authenticity of writing because he had witnessed the aurora and had been to the north himself. The problem was, however, that the object written about was itself *already* writing (Derrida's "always already", there is no presence behind representations, only the trace of one). Even if he had been able to present the aurora itself to corroborate its written description, the fact would still be that it was as uncontrollable as writing as the object to be represented is never stable. The aurora was only a trace of whatever purportedly authentic presence which wrote it, that is of whatever scientific principal lay behind it. That is one of the dilemmas of science and scientific writings on the aurora and other natural phenomenon; we might thus say that the aurora became an allegory of the uncontrollability of knowledge. Weyprecht's texts suggest that he was very much aware of this dilemma; he wished to understand the aurora, to decipher the flaming letters on the sky, yet at the same time knew that nature was the ultimate authority and allowed only traces to be caught. Chemla reminds us that scientific texts just like other texts, are designed, and that the act of designing "represents a constitutive part" of a scientist's activities.[71] One should add that the objects participate in this designing process. The observation and documentation of the aurora would, just like the expedition, ultimately remain an unfinished experiment.

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Notes

1. Karl Weyprecht, "Bilder aus dem hohen Norden. 1. Das Nordlicht", *Neue Freie Presse* morning edition, 14 March 1875: 2. All translations by the author. Much of the research for this chapter builds upon: Johan Schimanski and Ulrike Spring, *Passagiere des Eises. Polarhelden und arktische Diskurse 1874* (Wien, Köln, Weimar: Böhlau Verlag, 2015).
2. Asgeir Brekke, "Nordlyset og arven etter Birkeland", in Gaute T. Einevoll and Eirik Newth, eds., *Naturens kode. Har fysikken avslørt naturens hemmeligheter?* (Oslo: Gyldendal Akademisk 2005), 164.
3. Carl Weyprecht, *Die Nordlichtbeobachtungen der österreichisch-ungarischen arctischen Expedition 1872–1874* (Wien: Kaiserlich-königliche Hof- und Staatsdruckerei, 1878), 50.
4. Photography became an important means for scientists to study the aurora, as here it could be fixed and more easily analysed. The Danish northern lights researcher Sophus Tromholt was the first to photograph the aurora in 1885, though this was only partly successful and the photograph is lost today. The first photographs that still exist were taken by the German physicist Martin Brendel in 1892. See Asgeir Brekke and Alv Egeland, *Nordlyset: Kulturarv og vitenskap* (Oslo: Grøndahl Dreyer 1994), 84, 94; Alf Ragnar Nielssen and Arvid Petterson, *Nordlyspionerene: Menneskene og observatoriet på Halddetoppen i Alta* (Oslo: Grøndahl Dreyer 1993), 13, 15, 94.
5. Parallel to the efforts by the expedition to document the aurora, scientists in Europe tried to submit the aurora to the rules of a human-made laboratory. In particular, the Swedish physicist Erik Edlund actively tried to explain and understand the aurora through experiments in the laboratory and would launch his theories of the aurora on the basis of experiments with a magnetic ball in 1878. Only a few decades later, around 1900, Kristian Birkeland would analyse the connection of aurora display and geomagnetic disturbances in a laboratory. Alv Egeland and William J. Burke, *Kristian Birkeland: The First Space Scientist* (Dordrecht: Springer, 2005), 3.
6. Ulrike Spring, "Between Spectacle and Science: The Aurora in Central Europe, 1870s–1880s", *Acta Borealia*, special issue: Robert Marc Friedman, Per Pippin Aspaas and Sven Widmalm, eds., *The History of Research into the Aurora Borealis* 29(2) (2012): 197–215.
7. Nielssen and Petterson, *Nordlyspionerene*, 22.
8. Kärin Nickelsen has in her studies demonstrated the constructed nature of botanical illustrations. See, for example, "Draughtsmen, Botanists and

Nature: Constructing Eighteenth-Century Botanical Illustrations", *Studies in History and Philosophy of Science*, Part C: *Studies in History and Philosophy of Biological and Biomedical Sciences* 37 (2006): 1–25; "'Abbildungen belehren, entscheiden Zweifel und gewähren Gewissheit'—Funktionen botanischer Abbildungen im 18. Jahrhundert", *Wiener Zeitschrift zur Geschichte der Neuzeit*, special issue: Veronika Hofer and Marianne Klemun, eds., *Bildfunktionen in der Wissenschaft* 7(1) (2007): 52–68.

9. See Tine Damsholt, Dorthe Gert Simonsen and Camilla Mordhorst, eds., *Materialiseringer. Nye perspektiver på materialitet og kulturanalyse* (Århus: Aarhus Universitetsforlag, 2009).
10. Tine Damsholt and Dorthe Gert Simonsen, "Materialiseringer. Processer, relationer og performativitet", in Damsholt, Mordhorst and Simonsen, eds., *Materialiseringer*, here 14–16.
11. There also existed theories at the end of the nineteenth century, however, claiming that the aurora could be studied best in mid-latitudes. See Robert Marc Friedman, "Introduction: The Aurora in History", *Acta Borealia* 29(2) (2012): 116.
12. "Rückzugstagebuch vom 15.5.1874 bis zum 3.9.1874", in Frank Berger, Bruno P. Besser, Reinhard A. Krause, Petra Kämpf and Enrico Mazzoli, eds., *Carl Weyprecht* (*1838–1881*). *Seeheld*, *Polarforscher*, *Geophysiker. Wissenschaftlicher und privater Briefwechsel des österreichischen Marineoffiziers zur Begründung der internationalen Polarforschung* (Wien: Verlag der Österreichischen Akademie der Wissenschaften, 2008), 409.
13. See letter from Weyprecht to Littrow, 18.2.1872, in Berger, Besser, Krause, Kämpf and Mazzoli, eds., *Carl Weyprecht* (*1838–1881*), 347. See also Carl Weyprecht, *Die magnetischen Beobachtungen der österreichisch-ungarischen arctischen Expedition 1872–1874* (Wien: Kaiserlich-königliche Hof- und Staatsdruckerei, 1878), 10, fn. 1.
14. Weyprecht, *Die magnetischen Beobachtungen*, VIII.
15. "Rechnungsabschluss des Comité für die österreichisch-ungarische Nordpol-Expedition", *Wiener Zeitung*, 18 December 1874: 1106.
16. Carl Weyprecht, *Praktische Anleitung zur Beobachtung der Polarlichter und der magnetischen Erscheinungen in hohen Breiten* (Wien: Moritz Perles, 1881), 11.
17. Weyprecht, *Praktische Anleitung*, 13.
18. Carl Weyprecht, "Die 2. Österr.-Ungar. Nordpolar-Expedition unter Weyprecht und Payer, 1872/4. Schiffslieut. Weyprecht's Vortrag über die von ihm geleiteten wissenschaftlichen Beobachtungen, gehalten in Wien 18. Januar 1875", *Petermann's Geogr. Mittheilungen* II(1875): 69.
19. Weyprecht, *Die Nordlichtbeobachtungen*, 1; Weyprecht, *Die magnetischen Beobachtungen*, 5–6.
20. Weyprecht, *Die magnetischen Beobachtungen*, 10.

21. Julius Payer, *Die österreichisch-ungarische Nordpol-Expedition in den Jahren 1872–1874, nebst einer Skizze der zweiten deutschen Nordpol-Expedition 1869–1870 und der Polar-Expedition von 1871* (Wien: Alfred Hölder, 1876), 196; Weyprecht, *Die Nordlichtbeobachtungen*, 36. For the magnetic observations that could also include aurora observations see the list of instruments in Weyprecht, *Die magnetischen Beobachtungen*, 10.
22. Weyprecht, *Die Nordlichtbeobachtungen*, 36.
23. Weyprecht, *Die Nordlichtbeobachtungen*, 36, fn.
24. Weyprecht, "Die 2. Österr.-Ungar. Nordpolar-Expedition", 69.
25. Lorraine Daston and Elizabeth Lunbeck, "Introduction: Observation Observed", in Lorraine Daston and Elizabeth Lunbeck, eds., *Histories of Scientific Observation* (Chicago, IL: University of Chicago Press, 2011), 3.
26. "Die Instructionen für die Oesterreichische Polar-Expedition", *Das Archiv für Seewesen* 8(6) (1872), 272–4.
27. Weyprecht, *Die Nordlichtbeobachtungen*, 1.
28. Weyprecht, *Die magnetischen Beobachtungen*, VI.
29. For instance Brosch and Orel. See Weyprecht, *Die magnetischen Beobachtungen*, VII.
30. See letter to his mother from 21.11.1876, in Berger, Besser, Krause, Kämpf and Mazzoli, eds., *Carl Weyprecht* (*1838–1881*), 477–8.
31. Weyprecht, *Die Nordlichtbeobachtungen*, 2.
32. Carl Weyprecht, "Anhang. Vortrag gehalten vor der 48. Versammlung deutscher Naturforscher und Aerzte in Graz von Schiffslieutenant Carl Weyprecht", in Carl Weyprecht, *Die Nordpol-Expeditionen der Zukunft und deren sicheres Ergebniß, verglichen mit den bisherigen Forschungen auf dem arktischen Gebiete* (Wien, Pest, Leipzig: A. Hartleben's Verlag, 1876), 38.
33. Weyprecht, *Praktische Anleitung*, foreword.
34. Weyprecht, *Praktische Anleitung*, foreword.
35. See for instance: Weyprecht, *Praktische Anleitung*, foreword.
36. Weyprecht, *Praktische Anleitung*, 37, fn.
37. See Weyprecht's explanation of such huts in Weyprecht, *Die magnetischen Beobachtungen*, 6–7. In November 1873 two snow huts were built as observatories. See Anton Krisch, ed., *Tagebuch des Nordpolfahrers Otto Krisch, Maschinisten und Offiziers der zweiten österr.-ungar. Nordpol-Expedition* (Wien: Verlag der Wallishausser'schen Buchhandlung, 1875), 95–6.
38. Thomas F. Gieryn, "City as Truth-Spot: Laboratories and Field-Sites in Urban Studies", *Social Studies of Science* 36(1) (2006): 5.
39. Weyprecht, *Die magnetischen Beobachtungen*, 7.
40. Weyprecht, *Die magnetischen Beobachtungen*, 7; Weyprecht, *Praktische Anleitung*, 10.
41. See Gieryn, "City as Truth-Spot", *Social Studies of Science*: 6.
42. For the hybrid space of lab and field see Gieryn, "City as Truth-Spot", *Social Studies of Science*: 6.

43. Weyprecht, *Die Nordlichtbeobachtungen*, 1.
44. Weyprecht, *Die Nordlichtbeobachtungen*, 1.
45. Weyprecht made a list of the objects which were carried back in the boats after leaving the *Tegetthoff*, among them: one box of journals, the zoological collection, one compass, one sextant, logarithms and ephemerids, one telescope, one chronometer. “Rückzugstagebuch”, in Berger, Besser, Krause, Kämpf and Mazzoli, eds., *Carl Weyprecht* (*1838–1881*), 371–5.
46. Weyprecht, *Die Nordlichtbeobachtungen*, 2.
47. Weyprecht, *Die Nordlichtbeobachtungen*, 2.
48. Weyprecht, *Die Nordlichtbeobachtungen*, 2.
49. Weyprecht, *Die Nordlichtbeobachtungen*, 2.
50. Letter from Weyprecht to Petermann, 1.11.1874, in Berger, Besser, Krause, Kämpf and Mazzoli, eds., *Carl Weyprecht* (*1838–1881*), 423.
51. Payer, *Die österreichisch-ungarische Nordpol-Expedition*, VIII.
52. Weyprecht, *Die Nordlichtbeobachtungen*, 23.
53. Weyprecht, “Die 2. Österr.-Ungar. Nordpolar-Expedition”, *Petermann's Geogr. Mittheilungen*, 68.
54. Payer, *Die österreichisch-ungarische Nordpol-Expedition*, VIII.
55. Weyprecht, *Die Nordlichtbeobachtungen*, 42.
56. Weyprecht, *Die Nordlichtbeobachtungen*, 10.
57. Robert G. David, *The Arctic in the British Imagination*: *1818–1914* (Manchester: Manchester University Press, 2000), 12.
58. Weyprecht, *Die Nordlichtbeobachtungen*, 10–11.
59. Weyprecht, *Die Nordlichtbeobachtungen*, 2.
60. Weyprecht, *Praktische Anleitung*, 38–40. The English terms are Weyprecht's own translation.
61. Weyprecht, *Praktische Anleitung*, 40, 48.
62. Weyprecht, *Die Nordlichtbeobachtungen*, 8.
63. Payer, *Die österreichisch-ungarische Nordpol-Expedition*, 202; Carl Weyprecht, “Bilder aus dem hohen Norden. 1. Das Nordlicht”, *Mittheilungen aus Justus Perthes' geographischer Anstalt über wichtige neue Erforschungen auf dem Gesammtgebiete der Geographie* 21(1875–76): 349. Weyprecht would use this comparison in several contexts. See for example Weyprecht, “Die 2. Österr.-Ungar. Nordpolar-Expedition”, *Petermann's Geogr. Mittheilungen*, 67.
64. Weyprecht, *Die Nordlichtbeobachtungen*, 4.
65. See Schimanski and Spring, *Passagiere des Eises*, 534–41.
66. Payer, *Die österreichisch-ungarische Nordpol-Expedition*, 192.
67. Lölke understands the “*Naturgemälde*” as an effort by Humboldt to develop a literary modus which correlates with stringent scientific methods. Ulrich Lölke, “Von der Expansion zur Lokalisierung der Wissenschaften in multikulturellen Gesellschaften: Australische und europäische Erfahrungen”,

in Anja Schwarz and Russell West-Pavlov, eds., *Polyculturalism and Discourse* (Amsterdam, New York: Rodopi, 2007), 215.
68. Weyprecht, *Die Nordpol-Expeditionen der Zukunft*, 4.
69. Weyprecht, *Die Nordpol-Expeditionen der Zukunft*, 5.
70. Weyprecht, *Die Nordpol-Expeditionen der Zukunft*, 6.
71. Karine Chemla, “What Is the Content of This Book? A Plea for Developing History of Science and History of Text Conjointly”, in Karine Chemla, ed., *History of Science*, *History of Text* (Dordrecht: Springer, 2005), 226.

CHAPTER 8

Going Deeper Underground: Social Cooperation in Early Twentieth-Century Cave Expeditions

Johannes Mattes

Speleology—A Travelling Field of Science

At the turn of the twentieth century, the practice of subterranean expeditions went through a profound change. The number and depth of caves investigated in Europe's karst regions (especially in the south of France, the Alps, Moravia and the Dinarides) increased, which necessitated new technical resources and social forms of cooperation among the participants in subterranean tours. In the context of the exoticization of underground expeditions, which is evidenced by the economic boom in show caves and the opening of grotto railways in urban leisure parks, cave exploration had become increasingly popular and was practised by a growing number of urban citizens.[1]

Similar to other scientific expeditions, subterranean research expeditions represent complex social ventures, based on a high degree of hierarchy, organization and pre-planning. In the course of these projects, a

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group of experts and supporting personnel are normally dispatched from an institution with the instruction to carry out research, collect measurements, make observations and collect objects.

Underground expeditions also served as "travelling laboratories", where scientific data and documentation with plans or photographs were generated and different methods of knowledge acquisition were practised.[2] First, the subterranean geography was recorded on maps and charts, and then particular objects or species, such as minerals, fossils or beetles were collected and recorded. Using such collected data, specific experiments were conducted, for example, reconstruction of hydrography of a mountain by colouring subterranean rivers and monitoring karst springs. Scientists such as Georg Kyrle conducted part of their laboratory activities in the caves; for example, chlorinating subterranean streams to reconstruct the hydrology of karst landscapes or installing apparatuses to measure underground temperatures and air pressure.[3] These practices of observation and documentation not only legitimize the scientific self-image of speleology but also illustrate the social hierarchy and competition between the participants of an expedition. Similar to conquerors, the expedition participants expected to have the right to discovery of a cave, a claim that required a clear chain of command and strict division of labour, because it was logistically possible for only a small number of the explorers to penetrate deep inside a cave. While private organizations or clubs were often responsible for the practical implementation of a speleological on-site expedition, state research institutions and museums played a key role by arranging, displaying and studying the materials found. Therefore, federal research institutes and museums often claimed the measurement results, finds, and artifacts that were examined and collected during subterranean expeditions.

The subterranean expeditions operated under extreme conditions of darkness, cold or dangerous vertical topography, with the participants experiencing physical problems such as disorientation, abrasions or goose bumps. Therefore, speleological expeditions generally involved spontaneous improvisations and practical experimentation. Although pre-expeditions were usually undertaken and a clear assignment of tasks was determined prior to a venture, to reduce the necessity of deviating from the plan, cave expeditions also involved social subversion and deregulation. Since the underground is hidden from direct sight, it was impossible to control whether the members of an expedition were actually working together as per plan.

In addition, the cooperation of experts from different branches of science was necessary for subterranean expeditions, thus linking different branches of cultural and natural sciences such as geography, geology, mineralogy, hydrology, meteorology, palaeontology, zoology, botany, anthropology, archaeology, prehistory, and art history.[4] This broad interdisciplinary claim of speleology, which emerged around 1920, after years of dependence on other dominant fields of research, was more a theoretical demand than a common practice. In this context, cave expeditions served an experimental function. During such speleological ventures, multiple forms of cooperation and interaction between different branches of science and their methods were tested in practice. In particular, research trips of the early 1920s can be regarded as role models for further speleological ventures and as representatives of the first interdisciplinary expeditions.[5]

This chapter examines the experimental forms of social and interdisciplinary cooperation in speleological expeditions from the beginning of the twentieth century. The question is how cooperation as a form of social experimentation was practised in detail and how the organization of subterranean expeditions took place. The geographical focus lies on Europe, especially Austria, where both the state and private speleological organizations laid the foundations for large-scale subterranean expeditions.

For this chapter, two specific underground research trips, undertaken by the Austrian Academy of Science and the Speleological Club of Vienna in the caves of the Northern Limestone Alps in the early 1920s, were chosen because of the publicity they attracted, the scale of these operations and availability of good source material. The two expeditions exemplify the aforementioned relationship between the social, cultural, and scientific dimensions of underground expeditions, and emphasize the experimental social and interdisciplinary setting in which observation took place and scientific knowledge was produced or negotiated.

Subterranean Expeditions: Semantics and Politics

Since the nineteenth century, travellers have regularly visited caves in Europe. Gradually, some of these tourists came to feel they were no longer travellers or passive visitors and began to consider themselves as "explorers". The contemplative nature of curious travellers changed into an imperialistic perception of nature, and their observations turned into exploration. Simultaneously, the former tourists began claiming their right to the discovery of the caves, and called their trips "expeditions" and "underground campaigns", similar to the military expression for expeditions of armed forces. Cave trips which required a more intense cooperation and division of labour among the participants, gradually began to differ from the underground excursions of travellers into show caves.

This process also required a semantic modification of the terms formerly used such as "(subterranean) excursion", "reconnaissance", "hike" or "travel", all of which signified types of underground activities carried out by individuals or small groups for research and educational purposes. In particular, the introduction of the expression "expedition" to cave research emphasized the necessity of pre-planning, especially for the exploration of vertical shafts. Adolf Schmidl, author of the first monograph on speleology, used the scientific and more prestigious expression "expedition" (though inconsistently) to characterize cave visits with scientific or instructive purposes, as the meaning of the term underwent a profound modification toward the end of the nineteenth century.[6] Édouard-Alfred Martel, who was one of the first to constantly refer to his cave tours as "expeditions" and research "campaigns",[7] preferred to keep the large numbers of employees involved in his endeavours in the background. For him the term "expedition" implied a well-defined chain of command and emphasized the more prestigious scientific claim of cave exploration and practical speleology.[8]

In particular, the expression "campaign" is one of various military terms in Martel's publications and signified that more extensive research projects that were planned like armed operations over months and consisted of several single expeditions. From 1888 to 1913, Martel organized annual "campaigns" during his summer holidays in over fourteen different countries. Both well-known and unknown caves, underground rivers and deep shafts were explored in several "subterranean expeditions", whose specific topographical difficulties can be compared with above-ground expeditions to high mountains: "The reports of our multiple expeditions will show how many difficulties have to be vanquished, how many obstacles have to be conquered and how many risks have to be run. […] One of the most important activities of cave explorers is to draw plans and topographical views."[9]

Subsequent to the First World War, when the exploration of shafts several hundred metres deep became popular and risky ventures, speleology was perceived in the context of "extreme alpinism" and "the battle for the mountains", in which mountaineering became a metaphor for national ascent and predominance.[10] The speleological societies quickly recognized the propaganda value of the fascination with underground exploration. In addition, in the expedition diaries and protocols of caving clubs, speleologists depicted their research and discoveries as underground campaigns, or hard-fought “subterranean battles”[11] against nature. Simultaneously, it was no longer enough “simply” to take risks or survive dangerous adventures. An idealistic impetus became necessary, which conformed to national interests and discourses. As with other alpinist or naval expeditions, heroism, masculinity, elite-awareness, militarism, colonial revisionism, and nationalism significantly dominated the perception of speleological expeditions in the 1920s and 1930s.

Having penetrated to the most interior and hidden parts of the earth, the cavers conjured up images of heroism, self-sacrifice, patriotism, and courage. The popular culture of the explorer was enforced by the well-directed propaganda of speleological organizations and the nationalistic press.[12] As Franziska Torma satisfactorily illustrated through the example of extreme alpinism, the “myth of the explorer” was based on the entanglement of “a strict work ethic, the toughening of the (male) body […] and the motif of a (colonial) adventure”.[13] This national concept of heroism not only celebrated the survivors of hazardous alpine or subterranean expeditions as heroes but also integrated dead explorers as martyrs in their collective commemoration and glorification as role models for subsequent explorers.

However, the gender inhomogeneity of speleological expeditions can be regarded as significantly different from contemporary alpine ventures. The reasons why female cave explorers were allowed to participate in difficult shaft explorations as climbers in the 1920s and 1930s were pragmatic. On the one hand, several speleological clubs suffered a loss of members and trained personnel during the First World War. On the other hand, unmarried female speleologists often had sexual relationships with male participants and therefore were given the opportunity to take part in the expedition. Nevertheless, the names of the female speleologists were mostly not mentioned in the expedition protocols and the leadership positions in the expeditions were exclusively reserved for male explorers.

Climbing a peak in alpinism as well as reaching the bottom of a shaft during cave expeditions were symbols of self-esteem and of the cultural and scientific predominance of one’s nation. In the environment of the new cultural boom of exoticism and body consciousness in the 1920s, speleologists thought of themselves as a social elite, penetrating deeper into the earth than anyone else. This awareness of being part of an elite, which involved particularly the shaft climbers and expedition leaders, was linked to an increase in instructions for the participants, a heightened emphasis on social hierarchy and the formation of competitive subgroups during a venture. As many caving activities were undertaken in alpine clubs, the male-dominated caving societies often brought speleology and alpinism together, at the same time as pursuing right-wing politics.

Subterranean Expeditions as Social Ventures

While earlier excursions to horizontal caves normally consisted of five to ten visitors, the more difficult topography of vertical caves and remote pits, explored from the end of the nineteenth century, required additional pre-planning, cooperation, resources, and staff. A large number of employees and great administrative effort were necessary, particularly for the handling of the shaft equipment and its transportation to the cave entrance in remote mountain areas. Teamwork and self-discipline when handling the surveying instruments became increasingly important for cave exploration. In the first part of the twentieth century, difficult cave expeditions comprised 50 to 150 participants, who were often required to stay below ground for several days, and were frequently staged to raise awareness of speleology, emphasizing its scientific claims and the personal role of individual explorers.

Furthermore, the social structure of the underground expeditions experienced a profound change. While the subterranean excursions of the eighteenth and nineteenth centuries consisted of an employer, guides, and carriers, the later expeditions included new research groups with a different form of social cohesion and cooperation. Expeditions of the twentieth century still had a clear hierarchy, but the social differences between the participants were implicit rather than explicit. This change in the social structure of research groups resulted in increased social disciplinary action and instruction for members, noticeable since the 1890s, when the exploration of deep shafts became an important issue. In his comprehensive overview *Höhlenkunde*, Franz Kraus[14] wrote about discipline and leadership in difficult cave expeditions: "A leader of an expedition is absolutely necessary! When everybody wants to be the commander and there is no discipline, accidents may occur and can cost lives. In speleology an uncoordinated operation is even more reprehensible than in alpinism."[15]

The speleologists' experience of fellowship and collective suffering during the First World War led to a renewal of the idea of camaraderie.[16] Although expedition leaders tried to enforce the myth of underground solidarity with songs, lyrics and group photographs, especially after the First World War, cave expeditions became places of constant competition. The illusion of companionship can be also seen as a method of clouding the issue of social distinction in strictly hierarchical groups. In most cases, the glorification of camaraderie, which is apparent in expedition reports of the 1920s, served as a cover for the competitive behaviour among the cave explorers. The male concept of fellowship excluded members of other groups, such as women and foreigners, who were often not mentioned in the expedition protocols.

New techniques and equipment, such as cable reel phones reaching down shafts of more than 400 m, led to the assumption of militarist obedience in the field of speleology. Franz Mühlhofer, an army officer and president of the German Speleological Association, made the following announcement concerning a major expedition in 1923: "All participants of the expedition have to commit themselves to follow all the directions of the leader and the relevant section commander."[17] The military directness of this written instruction was quite unusual, even for a speleological society after the First World War. In the end, 130 explorers and carriers took part in the expedition.

Working together inside the cave on the principle of the division of labour, cave expedition members had clearly defined tasks and authorizations concerning their proximity to the undiscovered areas. The social practices aiming to limit and legitimize the privilege of the first look were extensive.[18] For this reason, the strict division of labour between the cave explorers—including scientific personnel, surveyors, cartographers, climbers, rope assistants, and photographers—corresponded to the members' social status. In contrast, women, local guides, and lamp holders or carriers, who also participated in an expedition, were not assigned "explorer", which was reserved for male members of speleological clubs. Established scholars, high club functionaries or military officers were designated as expedition leaders or cave cartographers, with the privilege of penetrating the undiscovered areas first. Club officials often occupied leadership positions. Therefore, the foundation of caving clubs and the different functions performed by their members can also be interpreted as an attempt to restore the clear social hierarchy of earlier expeditions. Locals were normally responsible for holding the lamps or widening and upgrading the underground paths with ropes. This enabled the explorers to penetrate the caves without any difficulties. But only a small number of the expedition members participated in the interpretation of the subterranean places and enjoyed the reputation that came with this privilege.

While cave expeditions were previously organized by independent individuals or club members, speleological societies and government bodies began to institutionalize expeditions in the form of particular commissions, integrating special paragraphs into their statutes and attempting to regulate the increasing number of expeditions. The aim was to control the publicity of an expedition and the focus of the research outcome. In Austria the "Principles for the organization of the governmental speleology", which were published in 1920, stipulated the state's claim regarding the exploitation of underground resources: "Planned cave expeditions. § 5. The Cave Commission has to present the Ministry [of Agriculture] quarter-annually a plan of the proposed cave expeditions and has to inform the expected leader of an expedition about the decision of the Ministry."[19] In spite of the strong government tendency to monopolize cave expeditions during the 1920s, many speleological clubs and scientific institutes still organized independent underground expeditions. However, in most cases, the appointment of the leader and the necessary administration remained the final responsibility of the national heads of the organization.

Besides expeditions in the Alps, speleological societies also undertook several foreign expeditions to prehistoric cave sites in south-east Europe between 1922 and 1937.[20] To finance these expensive archaeological excavations and research travels to Greece, Italy (Capri, Sardinia), and Yugoslavia, the speleological clubs initiated special fundraising balls and dance events for the nobility and the wealthy bourgeoisie. The income was "destined for the expedition fund"[21] of the club and became the matter of numerous debates between the officials regarding the geographical focus and funding of especially ambitious research trips abroad. Due to the unresolvable disagreements concerning the distribution of the financial resources, renegades from the German and Austrian Speleological Association founded their own clubs; examples include the Verein Alpiner Höhlenforscher (Alpine Speleologists Club) or the Expeditionsverband Deutscher Höhlenforscher (Expedition Association of German Speleologists). Having established their own speleological societies, the responsible cavers hoped to fulfil the official requirements to obtain public subsidies. For that purpose, research projects often involved mountaineering goals, which attracted more public attention than exclusively speleological aims.

In many cases, ambitions were larger than the monetary possibilities, as illustrated by a “financial participation certificate” of the Expedition Association of German Speleologists:

> We thank and certify that the owner of this diploma has sponsored the expedition *From Tyrol to the Himalaya* of *Expeditionsverband Deutscher Höhlenforscher*. He has supported the inspection of almost unknown high mountains in the Middle East, the exploration of blind animals in caves and the soil, ethnographic investigations and the collection of cave legends of the Indo-Germanic, Semite and Turkic people in the karst areas of Greece, the Levant, Persia, Afghanistan, and Kashmir. In particular the cultic caves of Zeus and Cybele in Asia Minor, the Mithra caves of Zarathustra and of Treverzent in Persia and Afghanistan, the homeland of Parzifal and Lohengrin, will be explored.[22]

Ultimately, this plan was not realized in this extensive form and was shortened to a research journey to Megara in Greece and Crete. These ventures were organized over several years and consisted largely of prehistoric excavations and speleological explorations.[23]

Expedition of the Speleological Club of Vienna into the Gassel-Tropfsteinhöhle Cave

One of the major caving expeditions carried out in the Austrian-Hungarian Empire during the early 1920s was undertaken by the Speleological Club of Vienna into the Gassel-Tropfsteinhöhle cave near Ebensee in Upper Austria. Since the club had experience in other technically demanding expeditions, where a new global depth record of 437 m below ground was set, it received assistance from the Austrian Armed Forces. In addition, the expedition was supported by other state organizations such as Austrian Federal Forests, Austrian Railways and the Ministry of Agriculture and Forestry, which provided the venture with transportation devices, such as a special train, ship and horse carriages, and with assistance personnel from the Federal Salt Industry in Ebensee.

The venture was initiated by the Viennese speleologist and alpinist Hans Hofmann-Montanus, who had visited the cave during a pre-expedition in spring 1924. Accompanied by two local cave explorers, the owner of the local steamship company and an alpinist from Styria, Hofmann-Montanus entered the already-known cave, until they reached a deep shaft, where a previous expedition of the Speleological Club of Upper Austria had given up two years before because of the difficult topography. For reasons of competitiveness, the ambition of the speleological club of Vienna was aroused and, on the basis of the pre-expedition, Hofmann-Montanus developed a plan based on a large number of experienced staff, the use of shaft equipment, and the inclusion of local cavers which he believed would lead to success.

The aim of the expedition was to explore and survey a 100 m deep shaft at the end of the already known part of the cave. The strong research interest in the deep parts of the cave was not only caused by exploratory motifs but also resulted from controversy over scientific theories on speleogenesis. Alfred Grund's theory of cave formation, which argued that caves develop primarily as a result of solution in the groundwater zone, was strongly opposed by speleologists like Walther von Knebel, Friedrich Katzer, Édouard-Alfred Martel and Hermann Bock, who were mainly influenced by their knowledge of underground rivers in the southern part of the former Austro-Hungarian Empire.[24] Thrown back to the borders of the First Austrian Republic,[25] the speleologists specifically looked for evidence of subterranean streams in the Northern Limestone Mountains.

Finally, the expedition was planned for three days, 14–16 November 1924, and extensive preparation took place over several months.[26] Due to the demanding technical conditions involved in surveying the cave, the participants had to bivouac one night underground. Heinrich Lechner, a major in the Austrian army, was nominated as the leader of the expedition and the 27-year-old prehistorian Adalbert Markovits was appointed as his deputy. More than 50 carriers were engaged; a special train from Vienna, a ship and several horse carriages were hired to reach the entrance of the mountain cave. Twenty-eight speleologists from Vienna and approximately ten local cavers took part as scientific personnel, surveyors, cave cartographers, shaft climbers, rope assistants, signallers (to run a radio and telephone system), photographers, and lamp holders. Further, scientific measurements of meteorological conditions were analysed; the length and depth of the cave was surveyed and the flora and fauna were examined. Fossils of different prehistoric animals, such as cave bears (*Ursus spaeleus*), and living species of cave crayfish and cave isopods were collected. In accordance with the contemporary definition of speleology as an integrative scientific field, the expedition consisted of different experts, who were working together as part of particular task forces, such as "photography, cartography, cave morphology, reconnaissance, palaeontology, hydrography, meteorology, zoology, botany, telephone network, and technical assistance".[27]

The expedition was initiated by local cavers from Ebensee, who began their explorations in 1918, with the aim of opening a show cave for the economically depressed alpine region after the First World War. As part of their explorations, they tried to verify regional legends of caves where gold was hidden. In the context of the popularization of science at the turn of the twentieth century, workers in the local chemical industry had heard of speleology and were fascinated by the stories and photos of the underground presented in exhibitions, magazines and on postcards.[28] Instead of noble metals, the locals encountered a huge stalactite cave and decided to open it to the public in order to make money for the locality. For them, natural history and speleology had the function of increasing local wealth and economic power.

The specific division of labour between the participants of the expedition corresponded to the social hierarchy of the explorers. While the reconnaissance, cartography, and scientific tasks were mostly undertaken by members of the speleological club, the local cavers, who had invited their colleagues, were excluded from the "first look". However, one participant from Ebensee, who had a leadership function in the chemical industry, was allowed to go down the shaft because of his personal relationship with the members of the speleological club.

The photos that were taken during the expeditions in the newly discovered areas show only Viennese speleologists, mostly in dominant poses. In addition, the urban cave explorers symbolically took possession of the underground areas by writing the name of their club and the date of their expedition on the wall of a discovered hall. In the expedition reports published in several newspapers and alpine magazines, the speleologists from Ebensee were referred to as "lamp holders" or "simple-minded local researchers".[29]

How far a participant in the expedition was permitted to enter the undiscovered areas was fixed in advance and was subject to strict control. In the expedition protocol of the Viennese speleological club, an asterisk and number added to the name of each participant indicated if and to what level a speleologist was allowed to descend:

> Overall leadership: Major Lechner (descent into the shaft and reconnaissance), Deputy: Mr. Markovits (entrance level and scientific work).

> Participants: Major Lechner (*3), Markovits (3), Hofmann-Montanus (*3) Lindenbach, [Alex] Rippel (*3), Dr. Schönfellinger (*1), R[olf] Thym (*3), Heger, Derflinger (*3), Pritz, Cernitz, Potschek, Wehrmann, Hahn (*3), Gnambs (*), Schwarz (*), the women: Carola Daum (*3), [Camilla] Lilly Cech, Sicha, [Mrs. Pepi] Schreiner, Cavalry Captain Friesen, Lise Eberau (*1), Willhelm Daum (*1), Reitzner (*), Ripka, Kolpatschek, Holzinger, Frl. Schüller (*1), Frl. Wratny. Participants from Ebensee: Pergar, Hofinger (*1), Jordan and a woman. Further 50 gymnasts and mountaineers from Ebensee were responsible for transporting the technical equipment and food to and from the cave.[30]

An explanation as to why a female participant from Ebensee was made anonymous is that all the subsequently published reports, articles, and protocols concerning the expedition were written by the Viennese speleologist without the involvement of the colleagues from Ebensee. In the publications, their family names, and even the name of the cave explored, contained spelling mistakes.[31]

The expedition, undertaken by speleologists who were predominantly from the bourgeois elite of Vienna, excluded not only the working-class local cavers but also female explorers from the privilege of the first look. For example, Lise Eberau and Carola Daum, two well-trained female cave explorers, were not allowed to climb to the bottom of the shaft. They had to wait on a small rocky ledge serving hot tea to the male explorers.[32] Another social practice for the exclusion of women was that female participants were mentioned separately from the male speleologists in the expedition reports or public media. For example, subsequent to the expedition a newspaper reported that thirty-five "explorers" and six "ladies" (among them experienced shaft climbers) took part.[33] Further, the group photo of the expedition, one of the most important methods of maintaining the illusion of companionship, showed the leader and the engaged carriers but excluded the female speleologists.

Although several newsreels and relevant mountaineering magazines published personal experience and official expedition reports, the published scientific output was comparatively low.[34] This can be explained by the fact that expedition members were not highly qualified scientists with the possibility of publishing their results in esteemed periodicals. The fact that the cave plan finalized several weeks after the expedition was never published in a speleological periodical might be a sign of its low quality. Therefore, one of the aims of the subsequent expedition, undertaken in 1925, was to draw a detailed topographical map of the cave.

On the other hand, the low scientific output also resulted from the intensive competition between the participants and the Speleological Club of Upper Austria, whose president and geologist Theodor Kerschner laid claim to the collected fossils, animals and plants. These objects, which represented the success and scientific claims of the expeditions, were too significant to leave in the hands of the local cavers or his Viennese colleagues. As the curator of the former holdings of the Speleological Museum in Linz, Kerschner tried to expand the collection for the state museum in Upper Austria. One year before the expedition, the Speleological Club of Upper Austria clarified that "all the finds, which are made in caves on the soil of Upper Austria, must be handed over to the curator of the Speleological Museum".[35]

Finally, ten months after the research in the Gassel-Tropfsteinhöhle took place, an expedition of the German and Austrian Speleological Association adopted and expanded upon the results of the previous expedition.

Austrian Academy of Science Expedition into the Eisriesenwelt Cave

The second major expedition presented here was undertaken in 1921 by the Austrian Academy of Science into the ice cave Eisriesenwelt near Salzburg. At that time, Eisriesenwelt, which had been surveyed and was 25 km in length, was known as the longest cave system in Europe.[36] Although the expedition had probably more than 40 participants, only university graduates were mentioned in the expedition reports.[37] The venture was split into a pre-expedition visit (16–20 March 1921) and a main expedition visit (30 March to 9 April 1921). The venture took 16 days, during which the participants spent approximately 140 hours underground. A small mountain hut next to the cave entrance served as accommodation for the scientists.

Local hunters had been aware of Eisriesenwelt cave for a long time; the cave was visited by the natural scientist Anton von Posselt-Czorich in 1879. In 1913, Alexander von Mörk, a speleologist from Salzburg, reported his discovery of the "longest ice cave of the world"; at that time, the assertion was more speculation than plausible assumption based on scientific research.[38] The cave became known as Eisriesenwelt (World of the Ice Giants) on account of its large dimensions.[39] After the First World War, Eisriesenwelt became the destination for numerous underground expeditions, a popular cave to study among Austrian cavers and a symbol of Austrian speleologists' international leadership in their field (Fig. 8.1).[40]

Fig. 8.1 Participants of an expedition into Eisriesenwelt. Archive of the Austrian Speleological Association (photographer unknown, 1922)

During the pre-expedition, which was undertaken by Ernst Hauser, who was the initiator of the venture, and members of the Speleological Society of Salzburg, the day schedule and group assignments were fixed and the overnight accommodation was decided upon. In the subterranean areas located near the cave entrance, the whole expedition operated together as a joint venture. In the more remote areas, the organization of the expedition was changed to small groups, operating autonomously, and consisting of a scientist, an assistant, guides and carriers. The fact that no official expedition leader was nominated and the scientists (most of them had experience in caving) sent out by the Austrian Academy of Science interacted without disciplinary hierarchies, underlined the experimental claim of this specific venture. The scientific articles published subsequent to the expedition particularly underline the high degree of exchange and communication between the scientists, who wrote several articles together or referred to each other in their publications: "The Eisriesenwelt became a natural scientific laboratory itself, where geologists, mineralogists, radiologists, and physicists produced scientific knowledge."[41]

While previous speleological expeditions (from 1913 to 1920) in the Eisriesenwelt were undertaken to investigate the length of the cave and to draw plans of its subterranean extensions, the expedition of the Austrian Academy of Science became a successful experiment and a model for how different fields of science could be integrated into one specific form of organization. In addition, the venture was initiated with the assertion that it represented the first joint venture with experts from several different disciplines exploring one single cave extensively.[42] The local press and speleological journals reported on "this particular expedition, unique in the history of science" as "The first time that such an integrative examination of one cave system has been carried out by reputable experts of all relevant scientific disciplines in such an extensive manner. This must be accentuated, because the Eisriesenwelt near Salzburg offers the greatest scope of work."[43]

Primarily, the organization of the expedition was divided into five scientific groups: zoological, geomorphological, meteorological, geological, and cartographic. In addition, one group had to take photos to complete the scientific documentation of the cave exploration, which involved the finalization of a plan, the collection of cave animals, and measurements to reconstruct the formation of the cave and the specific ice conditions inside.[44] Scientists from the Museum of Natural History and the University in Vienna, Otto Wettstein (zoologist), Otto Lehmann (geographer), Ernst Hauser (chemist) and Julius Pia (palaeontologist and geologist) were accompanied by speleologists from Salzburg, who continued with the surveying and mapping of the cave. The expedition was dominated by close cooperation between the scientists from Vienna and the speleologists from Salzburg, who, in contrast to the local cave explorers of the Gassel-Tropfsteinhöhle, held academic degrees and came from the same social class. Without the appointment of a distinct expedition leader, the social composition of the research teams varied from day to day and new forms of interdisciplinary cooperation were experimented with. For example, while the chemist Ernst Hauser and the engineer Robert Oedl worked together on the meteorology of the ice cave, Otto Lehmann and Julius Pia shared their ideas on the cave's geography and geology. In the evening, the mountain hut next to the Eisriesenwelt, serving as accommodation, became a place of interdisciplinary exchange and discussions concerning recent observations and finds in the cave.

Besides the work of the researchers, photography played an important role in the documentary claims of the expedition and its members. Captured by the well-known German alpine photographer Alfred Asal, the images from the more than 100 exposed glass plates illustrate the process of scientific observation, including the objectivity of the research. The automatic technique of photography guaranteed realistic replication of the topography inside the cave and eliminated the influence of interpretation or an illustrator.[45] For Asal "speleology not only needs impressive pictures or paintings, but primarily accurate reproductions regarding the morphology of a cave".[46]

With the common usage of photography in cave exploration in the 1920s, the credibility of speleologists was experiencing a profound change. A good example is Otto Lehmann, the expedition's geographer, who took his own photos to use as part of the documentation for his research; he also drew the plans of Eisriesenwelt cave.[47] Photographs were also seen as a legitimate basis for the description of a cave. Many cave cartographers were also amateur photographers. From this time onward, instructions for cave surveys began to emphasize the accuracy and exactness of a plan.[48] A survey should copy a cave with the precision of a photograph and should be accomplished without the subjectivity of the observer.[49] In a published expedition report of the Austrian Academy of Science, the speleologist Robert Oedl wrote the following with regard to precision: "An exploration of a cave without a very accurate description is worthless and this can only be done by a cave plan, which pictures everything. […] Only on the basis of the most accurate cave plan, scientific questions can be solved."[50]

Despite adverse environmental conditions, speleologists began to test different types of survey equipment and theodolites underground. The Eisriesenwelt cave was one of the first cave systems to be surveyed with theodolites. According to the published expedition reports, the survey and cartography of the cave played a central role. The relevant articles were more than a simple report of the results of the expedition; rather, they represent a textbook on surveying caves, which assists and offers instruction to other cave cartographers. By combining the results of exact meteorological and cartographic measurements, the researchers were able to reconstruct the geoisotherms of this large ice cave.[51] The use of accurate instruments required the speleologists to undertake special training. This led to increased interdisciplinary action within the survey group and an identification of the researchers with the instruments. The precision of the instrument depended on the surveyor and this established a pattern of cooperation and communication within the survey group. The surveyor and his assistants had to interact objectively like instruments, ensuring the smooth collection of data. For Robert

Oedl, the leader of cartographical section of the expedition, the learning of cave surveying methods "is a long way paved with thorns and requires a great deal of patience".[52]

The expedition was simultaneously linked to cosmopolitan and local knowledge; in several publications and oral presentations, the results were not only discussed in the academic field but also presented in a more illustrative way for the public, such as in popular books and articles.[53] In addition to some ten scientific articles, a monograph on the results of the expedition consisting of over fifty photographs and a plan supplement were finally printed five years after the expedition.[54] The fact that the monograph was published by members of the Speleological Society of Salzburg and that the scientists from Vienna, such as Lehmann, Pia and Wettstein, were not involved as editors, underlined the influence of the local caving club on the organization of the expedition.

The broad distribution of publications supported the economic interest of the Speleological Club of Salzburg as it was able to open the Eisriesenwelt as a show cave for the public; today, Eisriesenwelt is an enterprise that receives more than 100,000 visitors per year. The photos that were shot during the expedition were also used for popular oral presentations in Germany and Austria, and some of them were integrated into the exhibition of the Speleological Museum in Salzburg, founded in 1922. Therefore, the expedition helped popularize scientific exploration and promote speleology and the opening of show caves to the public. On the one hand, the exploration itself and the technical equipment used were presented as a triumph for natural science and humanity. On the other hand, the scientific expedition was a symbol of national identity. During the economic depression of the 1920s, expeditions were rarely funded, though they remained important for cultivating Austria's image as a nation capable of scientific leadership. However, this concept of national identity excluded foreigners and Jews from participating in cave expeditions.

It is also striking that Leopoldine Fuhrich, a female biologist and speleologist from Salzburg, who assisted Wettstein and other scientists during the expedition, did not have the opportunity to publish in the contemporary speleological periodicals and journals. This can be seen in the context of the exclusion of women and Jewish researchers from the scientific and alpinist communities during the interwar period. A similar exclusion to that of Fuhrich was experienced by the Jewish speleologist and chemist Ernst Hauser, who initiated the expedition. Even though he was not nominated officially by either the academy or the speleological society, Hauser had the function of the expedition leader. While the contribution of the Austrian Academy of Science to the expedition was limited to the commissioning and payment of the scientists, Ernst Hauser (who also joined the venture as a meteorologist) paid for all other costs, including the equipment, carriers and food.

An explanation for the omission of Hauser is that the Speleological Club of Salzburg, which organized the expedition for the Academy of Science, had double moral standards. While the cavers introduced a regulation excluding non-Aryans into club life, they unofficially allowed Jews to become members for financial reasons. In spite of his Jewish descent, Ernst Hauser, son of the president of the Austrian Industrial Association (Industriellenverband) and, in later years, a reputable professor of chemistry at the Massachusetts Institute of Technology, joined the Speleological Club of Salzburg in 1911, when he was 15 years old. At the time of the expedition, he had just obtained a doctoral degree in chemistry from the University of Vienna.[55] He offered the speleological club a substantial part of his property for the expedition and the construction of the show cave on the condition that the public path to the cave be named after his wife, who committed suicide three months before the expedition.[56] Although the club officials took Hauser's money, they did not agree that the path would be named after a Jewish woman; two months after the expedition was completed, they held a vote on the exclusion of the unofficial leader of the expedition from the Speleological Society of Salzburg.[57]

Conclusion

To summarize, expeditions played a central role in the acquisition of knowledge in the field of speleology; they were carried out as joint ventures, where different social forms of cooperation were experienced in practice. As in above-ground laboratories, access to the object of the research was regulated and based on methods of inclusion and exclusion, which dominated the organizational setting of subterranean expeditions. Scientists and academic non-professionals, townsmen and locals, and male and female speleologists, acted together in an experimental system and struggled for the exclusivity of the "first look". Female cave explorers and Jewish speleologists, however, were often not mentioned in the expedition reports or pictured in the group photos of the expeditions. While some speleological ventures had a clear social hierarchy, others, such as the expedition of the Austrian Academy of Science, experimented with alternative forms of scientific teamwork and did not nominate an official expedition leader.

In particular, cave expeditions became a decisive factor in the establishment of speleology as a scientific field of research, implementing new cave surveying techniques and the medium of photography as methods of fieldwork and establishing its tools as scientific instruments. As a combination of geographical exploration and scientific observation, expedition reports emphasized the accuracy and precision of speleological fieldwork, integrating measurement charts and photographs into scientific debate. According to the contemporary concept of speleology as a group or synthetic science, subterranean expeditions such as the research venture into the Eisriesenwelt cave recognized themselves as experimental, where the multidisciplinary research approach to caves was tested in practice and the possibilities for further cooperation between the scientists were explored. Crossing boundaries and serving as a link between traditional academic disciplines, subterranean expeditions represented interdisciplinary platforms, where different scientific knowledge circulated and was shared without the formality of a specific academic setting. In addition, the multidisciplinary design of subsequent expeditions solidified the scientific setting of speleology as a synthetic field of research and finally became an obstacle to its establishment as a genuine academic discipline at universities due to the lack of its own specific scientific methods.[58]

In the context of national politics, the dissemination of spectacular photos and adventurous reports of cave expeditions to the general public worked to popularize the field of science. Many other European countries, such as France, considered major subterranean expeditions to be a symbol of the progress of science and the nation. In the middle of the nineteenth century, the “destruction of the sublime” led to a “change of emphasis in exploration”[59]; hence, the speleologists positioned themselves within the framework of geographic inquiry and accentuated man’s role as conqueror of the world.

Although many similarities exist in contemporary alpine expeditions concerning the social and political implications, the gender non-homogeneity of the participants of speleological ventures can be seen as a significant difference. In contrast to mountaineering, which had early on become a mass movement, caving and speleological expeditions always remained a marginal phenomenon and included space for improvisation and experimentation. By going deep underground, speleology was also perceived as having a culture of obscurity, secretiveness and unconventionality, where social forms of cooperation could be tested and experienced, which should not come to light or be mentioned in official expedition protocols and publications.

Notes

1. See Georg Hartwig, *Die Unterwelt mit ihren Schätzen und Wundern, eine Darstellung für Gebildete aller Stände* (Wiesbaden: Kreidel, 1863).
2. See Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, MA: Harvard University Press, 1987), 220.
3. See, among others, Georg Kyrle, *Kombinierte Chlorierung von Höhlengewässern* (Wien: Speläologisches Institut, 1928).
4. Speleological journals and yearbooks were published to spread the results of cave exploration to a larger audience, to distribute the publications among other learned societies and to establish the study of caves as a new scientific field.
5. See Johannes Mattes, *Reisen ins Unterirdische. Eine Kulturgeschichte der Höhlenforschung in Österreich bis in die Zwischenkriegszeit* (Wien, Köln, Weimar: Böhlau, 2015).
6. See Franz Kraus, *Höhlenkunde—Wege und Zweck der Erforschung unterirdischer Räume* (Wien: Carl Gerolds Sohn, 1894), 245–88.
7. For a good overview on the "campaigns" undertaken by É.-A. Martel between 1888 and 1913, see Trevor Shaw, *The History of Cave Science: The Exploration and Study of Limestone Caves to 1900* (Sydney: Sydney Speleological Society, 1992), 260.
8. See Édouard-Alfred Martel, *Les Abîmes, les eaux souterraines, les cavernes, les sources, la spéléologi*e (Paris: Librairie Charles Delagrave, 1894), 11–12.
9. Martel, *Les Abîmes, les eaux souterraines, les cavernes, les sources, la spéléologie*, 9–10, 24.
10. Lee Wallace Holt, *Mountains, Mountaineering and Modernity: A Cultural History of German and Austrian Mountaineering, 1900–1945* (Ann Arbor: ProQuest, 2008).
11. The term "battle" was used by speleologists themselves from the 1920s to the 1940s, especially in unofficial letters and personal diaries, and it describes a time-consuming and exhausting research project in one specific cave, which takes place over several years or decades. In contrast, the term "expedition" means a clearly defined venture with a distinct task, programme and personnel.
12. See Beau Riffenburgh, *The Myth of the Explorer* (Oxford, New York: Oxford University Press, 1994).
13. Franziska Torma, "Auslandsbergfahrten", in Martin Achrainer, Frederike Kaiser and Florian Trojer, eds., *Berg Heil! Alpenverein und Bergsteigen 1918–1945* (Wien, Köln, Weimar: Böhlau, 2011), 431–58, here 453.
14. Franz Kraus (1834–97): businessman and pioneer in speleology. Correspondent for the Geological Survey and Fellow of the Museum of Natural History in Vienna. With Franz von Hauer and Ferdinand von Hochstetter, he founded the world's first Speleological Club, located in Vienna.

15. Kraus, *Höhlenkunde—Wege und Zweck der Erforschung unterirdischer Räume*, 270.
16. See Helmuth Zebhauser, *Alpinismus im Hitlerstaat*: *Gedanken*, *Erinnerungen*, *Dokumente* (München: Rother, 1998); Dagmar Günther, *Alpine Quergänge*: *Kulturgeschichte des bürgerlichen Alpinismus* (*1870–1930*) (Frankfurt am Main: Campus, 1998).
17. Franz Mühlhofer, "Ausschreibungen für 1923", *Mitteilungen des Hauptverbands Deutscher Höhlenforscher* 1(1) (1923): 19.
18. See also Augustine Brannigan, *The Social Basis of Scientific Discoveries* (Cambridge: Cambridge University Press, 1981).
19. Staatliche Höhlenkommission, "Organisationsgrundsätze für die staatliche Höhlenforschung", *Berichte der staatlichen Höhlenkommission* 1 (1920): 6–13, here 8.
20. In the 1920s, speleologists also published reports of expeditions which were undertaken in foreign countries before 1914. A good example is an expedition report of a research trip to Libya (Cyrenaica), written by Franz Mühlhofer, the president of the German and Austrian Speleological Association. Franz Mühlhofer, *Cyrenaika* (Wien: Verlag der Österreichischen Bundeshöhlenkommission, 1923).
21. Landesverein für Höhlenkunde in Niederösterreich, Entrance Ticket for the "Houseball" on 15 March 1923 (Archive of the Speleological Club of Vienna and Lower Austria, 1923).
22. Expeditionsverband Deutscher Höhlenforscher, Diploma for Supporters of the Research Journey "From Tyrol to Himalaya" (Archive of the Speleological Club of Tyrol, approx. 1930).
23. See, for instance, Adalbert Markovits, "Höhlenbildung der Kaki-Skala (Landschaft Megaris, Griechenland)", *Mitteilungen über Höhlen- und Karstforschung* 11(2) (1933): 1–18.
24. For the controversy on cave formation, see Alfred Grund, *Die Karsthydrographie, Studien aus Westbosnien* (Leipzig: Teubner, 1903); Friedrich Katzer, *Karst und Karsthydrographie* (Sarajevo: Kajon, 1909); Walther von Knebel, *Höhlenkunde mit Berücksichtigung der Karstphänomene* (Braunschweig: Friedrich Vieweg & Sohn, 1906); Édouard-Alfred Martel, "Critique de l'ouvrage du Dr. Alfred Grund Die Karsthydrographie", *Notices spéléologiques–Spelunca* 7 (1909): 343–7; Hermann Bock, "Der Karst und seine Gewässer", *Mitteilungen für Höhlenkunde*, *Karstmelioration und Urgeschichte* 7(3) (1913): 1–23.
25. First Austrian Republic (1919–34): after the First World War, the Treaty of Saint-Germain-en-Laye designated the frontiers of the Austrian Republic, which ceded German-populated regions to Italy, Czechoslovakia and Yugoslavia.
26. See Landesverein für Höhlenkunde in Niederösterreich, "Zur Erschließung der Gaßl-Tropfsteinhöhle bei Ebensee", *Der Bergsteiger* 2(48) (1924): 395.

27. Landesverein für Höhlenkunde in Niederösterreich, *Freitag d. 14. bis Sonntag d. 16. November 1924*, *Gasslhöhle* (Archive of the Speleological Club of Vienna and Lower Austria, 1924).
28. See also Johannes Mattes, "Von Industriearbeitern, Soldaten und Höhlentouristen—Forschungsgeschichte und Beschreibung der Gassel-Tropfsteinhöhle bei Ebensee (Oberösterreich)", *Mitteilungen des Verbandes der deutschen Höhlen- und Karstforscher e.V.* 58(2) (2012): 40–8.
29. Landesverein für Höhlenkunde in Niederösterreich, "Neue Erfolge der Höhlenforschung im Salzkammergute", *Der Bergsteiger* 2(49): 402.
30. Landesverein für Höhlenkunde in Niederösterreich, *Freitag d. 14. bis Sonntag d. 16. November 1924*, *Gasslhöhle*.
31. See Landesverein für Höhlenkunde in Niederösterreich, "Neue Erfolge der Höhlenforschung im Salzkammergute", *Neues Wiener Tagblatt* 27 November 1924: 8.
32. See Hans Hofmann-Montanus, *Berge einer Jugend* (Wien: Verlag der Österreichischen Bergsteiger-Zeitung, 1948), here 132–6.
33. See Landesverein für Höhlenkunde in Niederösterreich, "Neue Erfolge der Höhlenforschung im Salzkammergute", 402.
34. See Béla (Adalbert) Markovits, *Ein Ausflug in die Gaßl-Tropfsteinhöhlen bei Ebensee* (Gmunden: Salzkammergut-Druckerei, 1926).
35. Landesverein für Höhlenkunde in Oberösterreich, "Berichte aus den Verbandsvereinen, Oberösterreich", *Mitteilungen des Hauptverbands Deutscher Höhlenforscher* 1(2–3) (1923): 39–42, here 42.
36. For a detailed description of the cave, see Walter Czoernig-Czernhausen, *Die Höhlen des Landes Salzburg und seiner Grenzgebirge* (Salzburg: Verein für Höhlenkunde in Salzburg, 1926), 75–7.
37. See Otto Lehmann, Julius Pia, Ernst Hauser, Robert Oedl, and Otto Wettstein, "Bericht über die im Auftrage und mit Unterstützung der Akademie der Wissenschaften durchgeführte Expedition in die neuentdeckte große Eishöhle im Tennengebirge", *Anzeiger der Akademie der Wissenschaften*, *Mathematisch-naturwissenschaftliche Klasse* 58 (1921): 79–86.
38. See Walter Klappacher, "Die Entdeckung und Erkundung der Eisriesenwelt—eine schwierige Geburt", in Johannes Mattes, ed., *Alexander Mörk von Mörkenstein. Maler*, *Literat*, *Höhlenforscher* (Wien, Salzburg: Tandem Verlag, 2011), 94–6, here 96; Anton von Posselt-Czorich, "Höhlenwanderungen im Salzburger Kalkgebirge, Teil 2", Zeitschrift des Deutschen und Österreichischen Alpenvereins 11 (1880): 259–75.
39. See, among others, Walter Klappacher, "Salzburger Höhlengeschichte—Teil 1: Die Höhlenforschung bis zum Beginn des Ersten Weltkriegs", in Anke Oertel, Uwe Brendel and Roland Hecht, eds., *Festschrift 100 Jahre Landesverein für Höhlenkunde in Salzburg* (Salzburg: Landesverein für Höhlenkunde in Salzburg, 2011), 8–38.

40. See Landesverein für Höhlenkunde in Salzburg, "Entdeckungs- und Erschließungsgeschichte, ausführlicher Bericht über die Ergebnisse der Höhlenexpedition der Akademie der Wissenschaften in Wien, 1921", *Speläologisches Jahrbuch* 3 (1922): 1–4.
41. Erwin Angermayer, "Zur Geschichte der Höhlenforschung in Salzburg," *Speläologisches Jahrbuch* 13/14 (1932/33): 1–12, here 7.
42. See Robert Oedl, "Die moderne Höhlenforschung", in Max Rohrer, ed., *Die Höhle in Sport, Wissenschaft und Kunst* (München: Verlag der Alpenfreund, 1922), 1–15, here 6.
43. Anonymous, "Die Eisriesenwelt im Tennengebirge–Expedition der Akademie der Wissenschaften in Wien zur Durchforschung der Eisriesenwelt", *Salzburger Volksblatt* 9 April 1921: 5.
44. Alfred Asal, "Lichtbildaufnahmen, ausführlicher Bericht über die Ergebnisse der Höhlenexpedition der Akademie der Wissenschaften in Wien, 1921", *Speläologisches Jahrbuch* 3 (1922): 31–3.
45. See Lorraine Daston and Peter Galison, *Objektivität* (Frankfurt a.M.: Suhrkamp, 2007), 133.
46. Alfred Asal, "Photographische Aufnahmen in Höhlen", in Max Rohrer, ed., *Die Höhle in Sport, Wissenschaft und Kunst* (München: Der Alpenfreund, 1922), 36–8, here 36.
47. See Otto Lehmann, "Morphologische Beobachtungen, ausführlicher Bericht über die Ergebnisse der Höhlenexpedition der Akademie der Wissenschaften in Wien, 1921", *Speläologisches Jahrbuch* 3 (1922): 51–121.
48. See Ludwig Teißl, *Die Herstellung von Kartenskizzen natürlicher Höhlen* (Wien: Österreichischer Bundesverlag für Unterricht, Wissenschaft und Kunst, 1925), here 9–12.
49. See Robert Oedl, "Auswertungsmöglichkeiten von Höhlenvermessungen", *Speläologisches Jahrbuch* 3 (1923): 138–44.
50. Robert Oedl, "Vermessung, ausführlicher Bericht über die Ergebnisse der Höhlenexpedition der Akademie der Wissenschaften in Wien, 1921", *Speläologisches Jahrbuch* 3 (1923): 5–30, here 5.
51. See Ernst Hauser and Robert Oedl, "Eisbildungen und meteorologische Beobachtungen, ausführlicher Bericht über die Ergebnisse der Höhlenexpedition der Akademie der Wissenschaften in Wien, 1921", *Speläologisches Jahrbuch* 4 (1923): 17–47. Robert Oedl, "Über Höhlenmeteorologie, mit besonderer Rücksicht auf die große Eishöhle im Tennengebirge", *Meteorologische Zeitschrift* 40(2) (1923): 33–7.
52. Robert Oedl, "Vermessung, ausführlicher Bericht über die Ergebnisse der Höhlenexpedition der Akademie der Wissenschaften in Wien, 1921", 6.
53. See, for instance, Ernst Hauser and Robert Oedl, "Eishöhlen—Die Erklärung dieser eigenartigen Naturerscheinung nach den neuesten Forschungsergebnissen", *Kosmos–Handweiser für Naturfreunde* 19 (1922): 146–51. Ernst Hauser and Robert Oedl, "Eishöhlen—Ein Beitrag zu ihrer

physikalisch-meteorologischen Erklärung", *Die Naturwissenschaften* 9(36) (1922): 721–4.

54. See Julius Pia, "Geologische Beobachtungen, ausführlicher Bericht über die Ergebnisse der Höhlenexpedition der Akademie der Wissenschaften in Wien, 1921", *Speläologisches Jahrbuch* 4 (1923): 48–65; Erwin Angermayer, Alfred Asal, Walter Czoernig-Czernhausen et al., *Die Eisriesenwelt im Tennengebirge* (Wien: Speläologisches Institut, 1926); Otto Wettstein, "Zoologische Beobachtungen, ausführlicher Bericht über die Ergebnisse der Höhlenexpedition der Akademie der Wissenschaften in Wien, 1921", *Speläologisches Jahrbuch* 4 (1923): 66–8.
55. Ernst Hauser became the prime suspect in a well-known and unfounded criminal case. Victim of the anti-Semitic boulevard press, he was accused of murdering his wife, Susanne Hauser-Devrient, daughter of a famous Viennese actor, in December 1920. It is not surprising that Hauser decided to accept a postdoctoral position at the University of Göttingen (in Germany) in 1921.
56. For the suspicion against Ernst Hauser, see also Walther Rode, *Antrag auf Einleitung der Voruntersuchung wider Dr. Ernst Hauser und Genossen wegen des Verbrechens des Mordes* (Wien: Koch & Werner, 1927).
57. See Walter Klappacher, "Arierparagraf und Antisemitismus im Salzburger Höhlenverein—In Erinnerung an Dr. Ernst Hauser", *Die Höhle—Zeitschrift für Karst- und Höhlenkunde* 56 (2005): 100–4.
58. See Johannes Mattes, "Disciplinary Identities and Crossing Boundaries: The Academization of Speleology", *Earth Sciences History* 34(2) (2015): 275–95.
59. Riffenburgh, *The Myth of the Explorer*, 34.

CHAPTER 9

A Mutual Space? Stereo Photography on Viennese Anthropological Expeditions (1905–45)

Katarina Matiasek

> *Sooner or later, in a different way in each case, the effort of mapping is interrupted by the encounter with the unmappable.*—Joseph Hillis Miller[1]

Introduction

In its simulation of spatial vision, stereo photography was among the new technologies of the nineteenth century that fundamentally expanded familiar ways of experiencing the world. While it circulated in popular displays of its day, many anthropological explorers relied on its supposed ability to purvey a "faithful" scientific record of human appearance in faraway corners of the colonial world. Contrary to previous accounts of a disappearance or even a "collapse" of the stereoscope around 1900,[2] Vienna's major expeditions of mostly physical anthropology have left a substantial, and as yet largely ignored,[3] stereo photographic legacy after

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this date. The following chapter looks into the creation of anthropological knowledge on these expeditions by way of stereo photography.

On the basis of guidelines for scientific observation on anthropological expeditions from the second half of the nineteenth century, I will first trace how photographic media, and stereo photography in particular, have considerably challenged existing concepts of documentation within the rising discipline of anthropology. I will then show how, on each expedition, particular qualities of the stereoscopic arrangement were exploited to accommodate the successive anthropological concepts of salvage, atavism, hereditary segregation and total capture. In a circular mechanism inherent to visual evidence,[4] stereo photographic representations were thus configured as epistemic "spaces of knowledge". As such capable of unfolding indefinitely,[5] they were increasingly advanced from being a means of visual documentation in the field to a promising instrument of quantitative scientific analysis after the field—and can thus be seen to have overlaid the unfolding spaces of the actual expedition in the aftermath.

These missions do not all represent expeditions to faraway shores in the classical sense. The continuity in terms of their protagonists, as well as their increasingly laboratory-based methods of investigation that tenaciously built on each other, however, suggest their reading as one overarching and expanding experimental enterprise. Whether they be to distant lands or conversely to "foreign" enclaves within their home or allied territories, these expeditions all ventured forth towards a tangible rasterization of mankind. Finally, I will discuss the immersive capacity of stereo photography, which allows it to transcend its original contexts of construction and become a possible mutual space between scientific observer and stereoscopic subject today—vacillating between cosmopolitan representation and local presence of the depicted Other.[6]

Travel Instructions: Beyond the "Distorting Lens"

The stereoscopic method was developed in the laboratory of the British physicist Charles Wheatstone (1802–75) to investigate human binocular vision.[7] His experimental mirror stereoscope from 1838 used geometrically constructed line drawings of left-eye and right-eye views, and revealed their disparity as the cue for spatial perception. Soon after the announcement of the photographic process in the following year, smaller lens stereoscopes were already equipped with daguerreotypes.[8] The introduction of the photographic negative that allowed for multiple copies on paper also increased the accessibility of stereo photography after 1850. The guidelines for scientific observation edited in preparation of the Austrian frigate *Novara*'s (1857–9) circumnavigation of the world were the first of their kind in the German-speaking region to mention photography as a means of scientific documentation, and expressed an "interest in collecting stereoscopically rendered race figures". During the actual expedition, however, the sophisticated photographic equipment recommended was neglected aboard in favour of newly introduced abstract head drawings. Based on the average data of each population under anthropometric scrutiny, these were held to represent distinctive "mathematical race masks".[9]

Photography was only cautiously adopted by the emergent discipline of physical anthropology. According to Gustav Fritsch (1838–1927), founding member of the Society for Anthropology in Berlin, its main shortcomings were the recording of "too many inessential details", as well as perspectival foreshortening.[10] For the stereoscope, Fritsch identified another source of distortion: the capturing of image pairs with a distance exceeding the natural interocular separation. While this enables the three-dimensional representation of faraway objects, it also gives an exaggerated impression of depth.[11] Anthropological photography thus had to compete, initially, with two established drawing methods: the idealistic portrait that allows for the highlighting of essential features, and the realistic line graphics of the *Lucaesian* apparatus that projects objects in their correct metric proportions.

These inconsistencies were soon to be settled by a series of conventions that aimed at institutionalizing and internationalizing the methodological inventory of physical anthropology during the second half of the nineteenth century. In creating a technical protocol that even amateur anthropologists could follow, this step also defined new boundaries for the discipline.[12] At the 1883 meeting of the German Anthropological Society in Frankfurt, a system of cranial landmarks and orientations was agreed upon that could, in the words of the eminent pathologist Rudolf Virchow (1821–1902), be used to measure "every head, be it living or dead".[13] Fritsch was the first to lay down comprehensive standards for the "scientific demands" of anthropological photography for the *Guideline for Scientific Observations during Travels*, edited by the German polar explorer Georg von Neumayer (1826–1909) in three consecutive editions from 1875. Stressing the importance of physiognomic over ethnographic representations, Fritsch defined front and side profile views for portraits, and preferably naked full-body photographs. Perspectival foreshortening was to be avoided by choosing a long focal distance, while a measuring rod placed inside the picture was to serve as later "verification".[14] The first International Congress of Photography, held in Paris in 1889, finally established the stereographic portrait standard with a distance between the two camera lenses that equals the natural eye base.[15]

These agreements on methodology reflect a fundamental change in the status of representations within the new biological sciences: for all their optical irregularities, photography-based media were increasingly held to be realistic. Fritsch's 1875 guideline recommended the photographic image as a corrective means for the "subjective notions" and the "treacherous memory" of the traveller.[16] Lorraine Daston and Peter Galison have subsumed this growing tendency to replace fallible human observation under the term of "mechanical objectivity". In their characterization of a rising non-interventionist paradigm, objectivity was understood as the restraint of scientists from imposing themselves "on the image of nature".[17]

As will be shown, photography assumed an integral role in Viennese anthropology during the first half of the twentieth century. Its protagonists, however, made no reference to Fritsch's prominent publication. In a lecture on "Photography on Anthropological Expeditions" delivered to Vienna's Photographic Society in 1910, the discipline's Austrian founding figure Rudolf Pöch (1870–1921) instead attributed his standards to *Bertillonage*.[18] Devised for penal identification by the French criminal anthropologist and early anthropometric classifier Alphonse Bertillon (1853–1914), this laboratory system was published a decade after Fritsch's similar and equally consistent guidelines for "anthropometrically useful photography".[19] Pöch's preference for Bertillonage must be attributed to his professional photographic training under the pioneering photographic scientist Josef Maria Eder (1855–1944). Eder had committed himself to the propagation of Bertillon's system after a study visit to the Paris Police Prefecture in 1894, and within the year had arranged for a German translation of the 1890 manual *La Photographie judicaire*, with his own introduction.[20] Consistently resorting to both Bertillon and Pöch in their expedition photography, Vienna's anthropological community would carry Bertillon's experimental laboratory system to the field over subsequent decades.

Salvage Space: Oceania (1904–6) and South Africa (1907–9)

The earliest stereo photographs in Viennese anthropology date back to trained medical doctor and travelling explorer Rudolf Pöch. They cover anthropometric portraiture as well as ethnographic impressions taken on his multidisciplinary research expeditions to Oceania (1904–6) and to South Africa (1907–9). These journeys established Pöch's long-standing reputation as a pioneer in anthropological fieldwork and multimedia documentation—an affirmative view that has increasingly become subject to critical revision since the turn of the millennium.[21] Around 100 stereoscopic glass plates from each endeavour are preserved at the Department of Anthropology at the University of Vienna.

Having declined an offer by his former teacher Felix von Luschan (1854–1924) from the Ethnology Museum in Berlin to commission and endow him,[22] Pöch launched his first expedition to Papua New Guinea on his own terms and also financed it privately. He succeeded in recovering his expenses by selling "collected materials" to various Viennese institutions, among them the Museum of Natural History or the Schönbrunn Zoo.[23] According to the itinerary submitted to the Imperial Academy of Sciences for recommendation to the respective colonial governments, Pöch's main scientific goal was to research the "position" of the indigenous groups of Papua New Guinea in relation to "the other black races" and to verify the existence of "real dwarfism" among them.[24] This denotes a mission on behalf of evolutionism. Within this first general framework of anthropology, mankind was conceived as classified into ascending stages and subjected to consistent developmental sequences.[25] Small stature was considered a "primitive" feature indicative of evolutionary proximity to a common ancestral human "race"—a popular hypothesis that Pöch intended to substantiate in the field. Upon his return from Papua New Guinea in 1906, the Imperial Academy of Sciences assigned him to a new two-year expedition to South Africa.[26] His proposal to study the Khoisan peoples of the Kalahari Desert, again under the premise of short-statured "remnants" representing a primordial human stage,[27] was immediately granted funding.

Pöch's regular expedition reports to his Viennese patrons display a wide range of methods, including somatic, linguistic and cultural observations, and meticulously list growing "collections" spanning skeletal remains, anthropometric and medical data, photography, early cinematographic and phonographic recordings, art works, or zoological and botanical specimens.[28] The fact that his scientific repertoire covered both physical anthropology and ethnography—a scope that he considerably narrowed later in favour of a purely biological anthropology[29]—points to Pöch's professional ambitions in both disciplines on the eve of their academic institutionalization in a joint Chair at the University of Vienna. Despite his efforts to distinguish himself as an authoritative pioneer of a rising twin science, "Pöch was no lone, heroic researcher who had decided to enter an unpredictable world of the primitive."[30] On both expeditions, he travelled with the previously arranged support of the colonial authorities, took fixed quarters in mission or government stations, and sought to connect with officials who had mastered the local languages and helped "to deal with the natives".[31] While he did mount extensive inland excursions to remote regions of both Papua New Guinea and South Africa, Pöch's systematic anthropological documentations were largely carried out within the administrative, medical and juridical confines of the colonial apparatus[32]—in mission schools, police stations, indigenous hospitals and asylums, or in prisons.

While these restrictive structures are usually masked by the actual scientific representation, they can be seen to have extended into the protocol of anthropometric photography. In addition to the straight *en face* and profile view after Bertillon, Pöch started to introduce semi-profile portraits to record "peculiarities of the face"[33] and to give a "good impression of character"—albeit in a more casual setting, wherein the sitter could assume a preferred posture.[34] It was only in the context of his later prisoner-of-war studies that this semi-profile view was declared a mandatory documentation format. Stereo photography was not part of Pöch's original travelling agenda. He started to use this medium on an unscheduled interruption of his two-year Papua New Guinea expedition that forced him to "take the detour to Australia" in 1905. Pöch expressed his intention to make use of this occasion though, and announced anthropological investigations "also on the Australian Aboriginal".[35] During a tour of the Clarence District, recommended by the Australian Museum for the "racial purity" of its indigenous populations,[36] Pöch first used a Lancaster stereo camera, most likely just acquired in Sydney, to document Aboriginal camps and a state-run asylum.[37]

Over the further course of both of his journeys, stereo photographs were taken upon each major change of location and strictly in groups of six exposures, representing a half-pack of glass negatives. While this work was rooted in the tradition of the travel stereographs that had created a new form of popular ethnography in the 1880s,[38] Pöch emphasized the stereoscope's efficiency for physical anthropology as its "faithful impression of the facial relief"[39] would allow for a correct evaluation of "primitive features at first glance". Like Gustav Fritsch, he recommended new stereoscopic cameras that would not exaggerate perspective.[40] Pöch's additional use of the stereo camera reflects a certain documentary fever that produced a multitude of parallel records during the founding years of anthropology, indicating indecision as to whether the documented "stages" of human development should be grouped into ideal types or into anthropometric classes. But, importantly, Pöch stated that "every investigation of an ethnic group whose existence will have ceased in the foreseeable future" was of personal value to him.[41] This discloses the evolutionist assumption that the defined human "upward" development would also inevitably lead to the extinction of non-Western cultures. Pöch had carefully chosen his itineraries in order to salvage "living documents"[42] in the face of their imminent disappearance.

In the context of anthropological photography's concern with salvage, Christopher Pinney has pointed out a quality of the medium articulated by Roland Barthes as its "spatial immediacy and temporal anteriority, the photograph being an illogical conjunction between the here-now and the there-then".[43] The stereoscopic method, in particular, offered a tangible space to rescue authenticity out of destructive historical change. Authenticity here was seen to have existed "just prior to the present, but not so distant or eroded as to make collection or salvage impossible".[44] Simulating a survival in effigy, Pöch's stereo photographic archive of "ancient races [...] that protrude into the present like a fossil"[45] promised the anthropologist a last access to their spatial physical presence.[46] This seems to fulfil a promise inherent to this medium since the days of its inception. "Form", the stereoscopic pioneer Oliver Wendell Holmes (1809–94) had predicted in 1859, "is henceforth divorced from matter. [...] Give us a few negatives of a thing worth seeing, taken from different points of view, and that is all we want of it. Pull it down or burn it up, if you please."[47]

Atavistic Space: The Prisoner-of-War Camps of the First World War (1915–18)

It was not until Rudolf Pöch's next and rather different explorations that anthropological stereo photography was first used on a large scale. Meanwhile, promoted to the rank of associate professor,[48] he conducted extensive anthropological research in Austro-Hungarian prison camps throughout the First World War. Primarily on the initiative of Vienna's Anthropological Society,[49] permission for these surveys was requested from the Ministry of War in 1915. The war situation, with prisoners from all European and Asian parts of Russia, was rated "a non-recurring research opportunity" that should not be passed up.[50] Their concentration on a few sites in the monarchy's territory would promise a "rewarding task" in comparison to the "difficult, time-consuming and costly investigations under usual circumstances".[51] The project was granted multiple funding through the Imperial Academy of Sciences as well as the Anthropological Society.[52] Pöch led an Anthropological Study Commission, consisting of his own students, to ten different camps with mainly Russian prisoners from the eastern front between 1915 and 1917. Upon the invitation of his former teacher Felix von Luschan, Pöch and his main assistant Josef Weninger (1886–1959) extended the project during the next two years to include the German camp of Wünsdorf near Berlin and the Romanian camp of Turnu Magurele then under German rule.[53] Pöch claimed to be prepared for investigations on a "large and important exotic mixture" of African and Indian colonial soldiers from the western front imprisoned there by his previous overseas expeditions.[54]

In its promise of anthropological knowledge from a panoply of geographic sites, the endeavour can be considered a streamlined *Ersatz* expedition that substituted the unpredictable anthropological field for the controlled conditions of the camp. An identically laid-out working space was installed at each prisoner-of-war camp, usually in the local health care barracks.[55] This moving anthropometric laboratory devised different working stations for a rationalized capturing of personal data, head and body measurements, morphological schemes, plaster casts, or standardized portrait and full-body photographs.[56] Each station was run by one member of the commission with a weekly rotating division of tasks. Apart from "efficiency and exactitude",[57] this research design promised a consistent recording of approximately 7000 prisoners-of war across different camp sites—and, as will be shown, laid the groundwork for future anthropological traditions. Besides the collection of mostly physical data in accordance with the standard instructions by German anthropologist Rudolf Martin (1864–1926), Pöch outlined "continuous modifications and improvements in method and technique" as a main objective of the study.[58]

In spite of "rewarding" research conditions that promised such improvements, the prisoner-of-war camps considerably challenged Pöch's previous anthropological approach. The heterogeneous contemporary populations of Russia, India or North Africa no longer allowed for a direct documentation of "pure physical types". Referring to an earlier concept of spontaneous "de-miscegenation" (after Felix von Luschan), however, Pöch claimed that certain "racial types" would regularly resurface from the "mixed populations" under study and display an "original" combination of characteristic features, in spite of their segregated transmission predicted by "modern hereditary science".[59] This reveals a campaign under the banner of atavism—an early concept of evolutionary biology that referred to a reappearance of attributes typical of remote ancestors that have been lost in the course of evolution.[60] Under the term of *reversion* introduced by British naturalist Charles Darwin (1809–82) in his *On the Origin of Species* of 1859, atavistic concepts gained scientific popularity before the fundamental mechanisms of inheritance were established genetically. Connoted negatively, atavism was perceived as a visible recidivism into lowly evolutionary conditions—and was generally associated with fears of degeneracy, deviant behaviour or miscegenation.[61]

In such conjectural "returns" among the prisoners-of-war, Pöch ultimately anticipated the possibility of a "stricter differentiation of the races".[62] He called upon the anthropologist's "duty to recreate the image of the Ur-type"[63] and gave increasing priority to photography after the first year of the survey, whereas plaster casting was substantially reduced. The semi-profile portrait standard was now "laid down as a rule" and became mandatory, and the laboratory situation allowed for a full Bertillon set-up, including chair and corresponding fixed camera on a tripod. The camera's back was altered for the capture of what were now three standardized portrait views on a single glass plate. For an even result across the plate, the sitter was oriented according to the Frankfurt horizontal, and exposure times were determined to the beats of a metronome.[64] In the same year, stereo photography was introduced into the prisoner-of-war studies. The Imperial Academy of Sciences provided a Stereo Palmos camera and a Verant stereoscope developed for scientific and military purposes, both built by Carl Zeiss of Jena.[65] Supplementing single-plane photography, the stereoscopic method now likewise underwent standardization. A unified optical distance and a neutral background were introduced to establish comparability between serialized spatial images, while any cultural context like clothing was eliminated as far as possible. The African and Indian colonial soldiers, actually a minor group among the prisoners-of-war, were most thoroughly stereoscopically portrayed towards the end of the study.[66]

The handwritten survey sheets of the campaign show that the comparatively costly stereo photographic method was limited to those subjects intuitively chosen to represent "recurring types". Pöch additionally worked with a custom macro device to "record racial details of the face"[67] representing distributed "original constituent elements". These specific alterations of the stereo photographic apparatus clearly configured the medium as an evidentiary visualization of atavistic theory. Held by the Department of Anthropology at the University of Vienna, Pöch's archive of several hundred prisoners-of-war stereographs can be seen to form a seamless panorama of what could not be directly encountered in the field: a visible atavistic body. For these synoptic reconstructions, Pöch exploited a certain photographic capacity that can be observed to endow its object of representation "with the authenticity of a typological model arising from the depths of time".[68] In her broad study on atavism as a paradigm of obsolescence and return that modern scientific thought was oriented around, Dana Seitler has even likened photography to the condition of atavism itself: it accommodates "ostensibly disparate temporal frames" and forms "atavistic, composite spaces of perpetual return, sites of recurrence whereby the present is always also, instantly, an indication of the past".[69]

The Anthropological Study Commission's revivification of bodies from the depth of evolutionary time is not least reflected in the production of "naturally" coloured plaster busts of certain prisoners-of-war chosen to represent "original types". These busts featured vivid open eyes that had been manually sculpted after the stereoscopic template.[70] With their presentation at the popular War Exhibitions in Vienna's Prater amusement park in 1916 and 1917, the still-new discipline of anthropology certainly assumed an air of importance for war itself. It remains to be asked what role these representations served within controversies regarding war propaganda. After the war, Pöch became regular professor, holding the newly established Chair for Anthropology and Ethnography.[71] His unexpected death only two years later, in 1921, left Pöch's extensive anthropological "collections" orphaned. The now Austrian Academy of Sciences was bequeathed with considerable earmarked funds to organize their posthumous scientific evaluation, and published a twelve-volume *Rudolf Pöchs Nachlaß* series between 1927 and 1962. The survey's main assistant, Weninger, authored five of them—personally ushering his teacher's predominantly visual rasterizations into a new era of Viennese anthropology.

Hereditary Space: The German Enclave Marienfeld in Romania (1933–4)

Formerly indebted to evolutionary theory, the agenda of academic anthropology in interwar Vienna was recast to reflect the late Rudolf Pöch's claim that "the name of Darwin must give way to the name of

Mendel".[72] During these years, "hereditary biology" and "racial hygiene" became the priority of the Institute for Anthropology and Ethnography, and a method for legal "proofs of paternity" based on the heredity of different morphological features was introduced.[73] Eventually, the institute was divided and Josef Weninger, Pöch's former right-hand man, was appointed Professor of Physical Anthropology in 1929.[74] While initially hesitant about the methods of paternity diagnosis, Weninger soon adopted a practical stance and stepped up investigations into the then unknown patterns of heredity.[75] In 1931, he established a Working Group for Hereditary Biology, with narrowly specialized anthropologists, among them his wife Margarete Weninger (1896–1987).[76] The prisoners-of-war surveys had suggested a further "dissection of the facial field of observation",[77] and the morphological schemes after Pöch were subsequently broken down into ever smaller units. Weninger hoped to "approach the sources of human heredity" by recording entire family systems instead of "randomly chosen individuals".[78] Arguing that numbers would represent the "scaffolding, but not the completed building of an anthropological study",[79] he preferred a raised "awareness towards *gestalt*"—a direction that was subsumed under the term of "morphognosis", a portmanteau of morphology and physiognomics, by his first assistant Eberhard Geyer (1899–1942).[80] In an elevation of his own methods, Weninger referred to a Viennese School of Anthropology.

Ultimately, it was the institute's main project of the interwar years that became the testing ground for the so-called Viennese School: the Marienfeld expedition. In the winter of 1933–4, the Working Group for Hereditary Biology travelled to the village of Marienfeld (German for Teremia Mare) in the Romanian Banat region for a large-scale assessment among families of a German minority group.[81] Culturally well-isolated since their state-administered resettlement to the region 200 years earlier, these families promised to be of sufficient "racial purity" for the Viennese mission. In a reversal of the expansive thrust that usually directs expeditions towards the discovery of new unknown worlds, the scientists here set out on a quest to find their own "racial" centre of gravity. According to Maria Teschler-Nicola's thorough study of the project, the initiative for an anthropological campaign originated in the Marienfeld community itself. The locals not only suggested themselves as "objects" of exploration but also provided facilities for the research procedures as well as food and lodging for eight project workers over the course of the entire study. Funding came mainly from the German Research Foundation to cover travel costs and photographic materials, while the Institute for Anthropology provided the necessary equipment.[82]

During eight weeks in Teremia Mare, a total of 251 families with 1081 members were anthropologically as well as genealogically registered.[83] Modelling the procedure after Pöch's division of labour during the prisoner-of-war surveys, Weninger distributed different tasks across eight working stations operating simultaneously. Supervised by the respective specialist, five of the stations were designated to capture "morphognostic" details only—of the ear, the hand, the iris or the nose. The findings were recorded on 10,800 sheets, with data, mappings and drawings, and in 7890 single photographs[84] archived at the Department of Anthropology today. The Working Group for Hereditary Biology's "most bustling member", the ear specialist Eberhard Geyer,[85] was in charge of a working station that was entirely dedicated to stereo photography. Over 2000 stereo photographic glass plates of ears were taken in two standardized close-up views, isolating the single hereditary trait from the body, but also from the generation of its carrier. These stereo photographs most vividly illustrate the Viennese School's desire "to dismember and dissolve" the physical body into discrete complexes of features, and the stereoscopic method was given a whole new cast.

During research for his 1926 habilitation thesis on French prisoners-of-war from West Africa, published as the first volume of the *Rudolf Pöchs Nachlaß* series, Weninger had discovered that stereoscopic photography was of "eminent merit for evaluating the material".[86] While the semi-close-up stereographs had originally been taken to document certain atavistic traits, they were now used as a basis for the "elaboration of characteristic features or groups of features […] that the knowledge of heredity can be based on".[87] The new method of working directly from the stereo photograph was refined after observations that typical structures of the human iris were hereditary.[88] Under considerable economic pressure, Weninger acquired a new Zeiss stereo, or Raumbild camera for the Institute of Anthropology that was suitable for very close stereo shots of the anterior eye segment.[89] "Considering the importance the hereditary tracing of single biological traits has recently gained,"[90] the Austrian Academy of Sciences helped to pay for the new apparatus in 1932—just in time to deploy it afield during the upcoming Marienfeld survey.

It was in this expedition's context that the stereoscopic method was advanced from a means of visual onsite documentation to an instrument of analytical classification in the aftermath. Beyond "the plastic and spatially correct rendition of its objects" for morphological analysis,[91] stereo photography became an independent working instrument for identifying and experimentally grouping the unknown units of hereditary segregation. Meticulously arranged and rearranged according to genealogical trees, age charts and other tabulations, the fragmented stereographs were thus made to stand for an abstract whole—the principle of inheritance. In this, the tactile stereoscopic format can be seen to mimic an anatomical preparation's "material participation" in what it represents, suggesting permanence and reactivation.[92] To that effect, Geyer never referred to stereoscopy, but to the "2140 ears of my Marienfeld material".[93] His reordering of ears visually cut out of their physical context can thus be compared to the relocation of specimens in anatomical display cabinets. There, the new juxtaposition uncovers aspects that would not manifest otherwise, while the "reality of the collection itself" has been shown by Susan Stewart to "override specific histories of the object's production and appropriation".[94]

Contrary to the tight collaboration on site, the evaluation of the plethora of Marienfeld data became a protracted and politically contested issue. With the Nazi rise to power in Germany during the course of the expedition, paternity tests were drastically reframed there as "racial proofs of descent" under the terms of the Nuremberg Laws on Citizenship and Race after 1935.[95] Accordingly, the German Research Foundation predicated further funding to accelerate evaluations on the "political reliability" of the applying scientists.[96] The original mission of putting the comparative paternity test on a sound scientific footing was thus overtaken by political events that were soon to submerge Austria as well.

Total Space: The Prisoner-of-War Camps of the Second World War (1940–3)

The annexation of Austria into the Third Reich and the ensuing "alignment" policies fundamentally altered Vienna's university and museum structures after March 1938. With German racial legislation now effective on former Austrian territory, Josef Weninger was removed from his University Chair because his anthropologist wife Margarete was of Jewish descent. Professed Nazi Eberhard Geyer, Weninger's former first assistant, was appointed as his successor. Led to the "problems of family biology" by the Marienfeld expedition,[97] Geyer established the Institute for Anthropology as the foremost site for "clarifying questions of dubious Aryan descent".[98] The Museum of Natural History was reorganized within an alliance of Scientific State Museums in Vienna under the general direction of German ornithologist Hans Kummerlöwe (1903–95), a fierce Nazi propagandist.[99] The new orientation heralded for anthropology is best reflected in his editorial announcement of a close collaboration with the party, owing to "the significance of racial knowledge as a mainstay of National Socialist conviction".[100] Anthropologist Josef Wastl (1892–1968), an equally long-standing party member who had obtained his doctorate in 1925 on Bashkir prisoners-of-war data collected by Rudolf Pöch, was promoted to head the new Anthropological State Museum in 1938.[101]

Within weeks of the onset of the Second World War in September 1939, the plan to conduct anthropological explorations in prisoner-of-war camps resurfaced. Evoking the key arguments of Pöch's campaigns to establish such research during the First World War, the "practically unique situation" that would allow for systematic investigations on the "racial composition" of numerous nations, especially of those from the colonies, without costly travelling was appraised. Upon multiple initiatives of general director Kummerlöwe, the project received funding from the Reich Research Council in Berlin, the Reich Governor of Vienna and the Austrian Academy of Sciences.[102] The endeavour of a prisoners-of-war survey returned, however, in a version that was to play a pivotal role in the affirmative promotion and practical implementation of Nazi ideologies of race. Designated "expedition leader", Wastl anticipated the opportunity to "systematically subject members of different peoples to German racial science".[103] He had already distinguished himself in such research in September 1939 by leading an Anthropological Commission of eight museum members to the Vienna Prater Stadium, where over 1000 Jews of polish origin had been interned in the course of a Reich-wide campaign after the invasion of Poland.[104] Upon completion of this infamous "racial survey", all examinees were deported to Buchenwald, where two-thirds of them were put to death within days.[105]

In January 1940, the mass surveys on prisoners-of-war were resumed. The Anthropological Commission under Wastl travelled to the camp of Kaisersteinbruch, where thousands of Polish soldiers were interned. The war campaign for France brought additional thousands to Kaisersteinbruch, including soldiers from the French colonies in Africa and Indochina. Until the summer of 1941, Wastl led up to eleven commission members to racially survey a further 4700 prisoners-of-war in Kaisersteinbruch, while smaller explorations were carried out in the Wolfsberg camp in 1942, during which the anthropometric data of another 1100 prisoners-of-war were collected.[106] Like the Marienfeld project before them, these expeditions were largely based on Pöch's research design developed for the prisoner-of-war campaigns during the First World War. For "reasons of consistency", each member of the Anthropological Commission was again in charge of one specialized survey task. Every investigated person had to pass through a set of up to ten stations, and the first statistical evaluations were worked out on site with the assistance of prisoners-of-war, as previously established by Pöch. The anthropometric *parcours* yielded thousands of data sheets, hand- and footprints, hair samples and hundreds of plaster cast masks.[107]

While the details registered again largely remained within the framework of instructions given by Rudolf Martin, now in their 1928 edition,[108] the scope of photographic and stereo photographic documentation was substantially expanded and taken to a new methodological level. Wastl personally operated a working station for scientific photography where nearly 60,000 photographs were taken, among them 25,000 stereographs.[109] Each examinee was photographed in the three portrait and naked full-body standards after Pöch, and towards the end of the studies in up to six positions. Stereoscopic portrait and body standard views were also expanded to include six positions, now mostly taken in "natural colours", and up to seven additional close-ups.[110] The subjects were photographed on a Bertillon chair in front of a millimetre grid, with the Frankfurt horizontal drawn right across their faces. To cancel out perspectival distortions, Wastl introduced lenses of very long focal length that necessitated additional artificial lighting.[111] For the stereographs, different custom constructions that coupled two regular cameras were worked out, and an additional Stereotar outfit for single cameras was used from 1942 on.[112] Wastl had considerable difficulties in acquiring these photographic materials as their distribution was strictly regulated by the German war economy.[113]

In a lecture to "confidants" of the Reich Federation of German Civil Servants in 1941, Wastl presented the rationale behind these efforts: to establish photography as "a permanently evaluable document" on which anthropological measurements could directly be performed.[114] While crediting Pöch's photographic standards for basic evaluability, Wastl had already stressed in 1933 that only a stereoscopic image produced under defined circumstances would allow for a precise measuring of its subject.[115] He subsequently promoted the newest stereo photogrammetric devices, originally deployed for topographic mapping, among them the

Pulfrich stereocomparator, with built-in reference marks for an accelerated extraction of spatial data.[116] The prisoners-of-war now offered a sufficient statistical basis to substantiate a method that promised a remote mass inventory of man. What seems significant here is Wastl's reconciliation of two methodical strands that anthropology had hitherto been dealing with inconclusively: subjective visual (stereo) typing and objective measuring. This comes full circle, linking with the 1857–9 *Novara* expedition, during which photography was still refrained from in favour of mathematical drawings. In the practical reciprocation of photographic impression and numerical evaluation, the "photogrammetric race masks" taken during the Second World War prisoner-of-war campaigns resolved this long-standing contradiction in such a way as to anticipate digital imagery.

In his stereoscopic "racial science" project, Wastl was also eager to connect to the popular culture of National Socialism. The Third Reich had rediscovered the long-standing stereo photograph as a key propaganda medium and metaphor for its *Raum* politics.[117] Wastl repeatedly wrote articles for the party-affiliated stereoscopic journal *Das Raumbild* from 1935 on. In 1941, he pointed to the future possibility of a "plastic projection" of his prisoner-of-war stereographs[118] that would make these images sculpturally available for broader public viewing and further authenticate his views on man in the service of a totalitarian regime.

After the war had taken an unfavourable turn for the German Reich with its Russian campaign, all activities relating to colonialist ambitions were cancelled after February 1943.[119] Accordingly, the Viennese prisoner-of-war surveys soon came to a halt. Wastl, who had taken up issuing "certificates of descent" for the Reich Office of Genealogy in 1941, now expanded this work.[120] After 1945, he was rated a "lesser National Socialist offender" and suspended from museum service.[121] As late as in 1959, Wastl tried to summarize his prison camp explorations under the heading of "constitutional and racial anthropology" for the *Proceedings of the Anthropological Society.* The manuscript never progressed beyond proof stage, and his monstrous anthropometric imagery subsequently disappeared into the vaults of Vienna's Museum of Natural History.

Double Take: A Mutual Space?

Starting around 1980, the role of photography in the creation of anthropology's knowledge has come into question.[122] These critical accounts have focused on photographs from the heyday of colonialism, between

1870 and 1930, and emphasized the controlling and destructive effects of the medium on its indigenous subjects.[123] John Tagg was the first to relate anthropological photography to the seminal writings on discipline and surveillance of French philosopher Michel Foucault, and to identify how the status of this imagery varies with "the power relations which invest it".[124] While travelling anthropologists purported to collect data under natural conditions, they can indeed be seen to have photographed in increasingly rationalized and confined, hence modern, environments.[125] These in fact represent an unresisted scientific gaze at a disciplined foreign Other—and thus increased the divide between the explorer and the explored, the subject and the object of representation.[126]

The last decade has seen a re-evaluation of anthropological photography that is still in its early stages. Moving past Foucault-inspired readings that invoke surveillance and objectification,[127] the medium is increasingly negotiated in its capacity to transcend this configuration. The fact that the camera always captures more than the photographer is in control of encourages the notion that anthropological photographs have a performative quality: their visual "spill over" can be seen to exceed their original contexts of construction[128] and thus unsettle global accounts of the world. The formerly hermetic anthropological archive, constrained by the dominant and asymmetrical frame of interpretation along with racial and colonial ideologies[129] is cracking open. Digital access has opened anthropological collections to indigenous communities who were once the focus of study and who now engage with photography as part of their own history, and to contemporary artists who critically intervene in such imagery.[130]

Major exhibitions and lavishly illustrated studies with iconic titles such as *Surviving the Lens*[131] reflect this contemporary shift in focus. Stereoscopic glass plates taken by Leo Frobenius (1873–1938) on his first Congo expedition between 1904 and 1906 were recently adapted for a three-dimensional display at Frankfurt's Museum der Weltkulturen. The accompanying exhibition catalogue underlines photography's "special capacity to give presence to the Other".[132] This refers to Johannes Fabian's analysis of presence versus representation in ethnography, which defined its highest aim as transforming "into a praxis capable of making the Other present".[133] Is it conceivable to showcase the stereo photographs that Viennese anthropology produced during the first half of the twentieth century in a similar way? Can their destructive conditions of production and inappropriate "racial" abstractions be reframed? Can they even become a "globally disseminated and locally appropriated medium"?[134]

In its simulation of spatial vision, the stereoscopic arrangement is one of great immersive depth and brings into play a new corporeal and reciprocal relationship between viewer and photographic subject. Whereas single-plane photography always presents a discernible testimony of the past within a certain viewing context, the stereoscopic gaze shuts out all external reference. Suggesting "palpable proximity",[135] the plastic stereograph reinforces the appropriating character of the photographic project,[136] and the anthropological subject experiences, as it were, a voyeuristic incorporation by the viewer. At the same time, stereo photographic immersion blurs the boundaries between representational and perceptual space.[137] The viewer is no longer inspecting an anthropological motif, but rather is physically implicated or "enfolded" in a delusional image now mutually invading the spectator.[138] In his musings on a future Virtual Reality anthropology, Christopher Pinney delineated total immersion as an expedition without a departure or a return. Whereas these constituted the historical "frame" of the strange Other, the frameless distance-travel through immersive media blurs and ultimately obliterates the traveller and their subject.[139] The immersive agency of stereo photography can indeed be seen to have anticipated such a future transgression.

Scientific photography would become redundant if its results were under strict control and thus foreseeable. Instead of opting for or against the controlling impact of the photographer, Peter Geimer underlined the necessity of facing both "the construction and the necessary unpredictability of the constructed, the intentional and the incident, the representation and its possible interruption".[140] It seems sensible to keep these experimental modes firmly in view when dealing with the stereo photographic legacy of anthropological expeditions in the future. While the three-dimensional portraits can indeed be seen to oscillate between representation and presence, they also remain suspended indeterminately between trauma and promise.

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CHAPTER 10

Traditions, Networks and Deep-Sea Expeditions After 1945

Peder Roberts

Introduction

The aim of this chapter is to examine a series of expeditions that illuminate a period of profound change in deep-sea research during the fifteen years after 1945. In recent years historians have devoted considerable attention to exploration of various spaces.[1] Technologically sophisticated space programmes such as the Apollo missions involved dense and powerful networks linking government, industry, the media and the military in addition to scientists and engineers (not to mention astronauts), an example of what is widely known as "Big Science".[2] Dependent upon ships and the money and personnel needed for extended voyages, ocean expeditions have long exemplified aspects of this phenomenon. Expeditions to the deepest reaches of the oceans were products of the geopolitical and economic as well as scientific and technological contexts of their times.

I will consider two dimensions of what it means to be an expedition that I suggest are complementary. I use the term "expedition" to describe a venture marked by travel to and investigation of a remote location—in this

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case the deep ocean—located within traditions that validate the individual expedition's aims and made possible by networks that provide the resources for both the expedition itself and the processing of its subsequent results. The term "cultures of exploration", coined by Felix Driver,[3] neatly captures the importance of considering expeditions as expressions of social ambitions, anxieties and assumptions that inform structure as well as content. Expeditions charged with investigating the deep seas were no exception. Moreover, the notion of the expedition as the unit of exploration accentuated the status of each voyage as a singular event in addition to a process, a discrete probing of the boundaries of nature. This perspective offers a complement to the conceptualization of knowledge production from field expeditions put forward by Bruno Latour. While Latour's model has proven influential for studies ranging from the early modern period to the present,[4] it necessarily de-emphasizes the form of the singular expedition in order to privilege its function—that of accumulating data to be converted into knowledge at a "center of calculation".[5] The broader programme of actor-network theory (ANT), in which Latour has been a key figure, is nevertheless useful because it conceives scientific and technological processes in terms of actors mobilizing resources (material and otherwise) to build the networks that allow work to be done and debates to be won.

To obtain the funds to conduct exploration was to provide an answer that satisfied patrons, from scientific foundations to wealthy philanthropists to militaries, but also to the publics for whom exploration continued to constitute a spectacle. In cases where reaching a distant point was contingent upon the deployment of novel technology, the mere fact of the expedition's occurrence—its traverse of space—could function as a form of experiment in its own right. I suggest there is an instrumental value to thinking in terms of networks because it focuses attention on patronage and community-building, and on the justifications advanced for the importance of each expedition. How was the deep sea constructed as a space in need of exploration? What role did nationalism play? How were international connections woven into the structure of expeditions (and the analysis of their data)? And to what extent was the act of encountering the deep sea an experiment in its own right, the validation of the transport technology the end rather than the means?

I have chosen a selection of expeditions to bear out the point without claiming comprehensiveness. The chapter is divided roughly into two halves. The first considers the Swedish *Albatross* expedition (1947–8, led by Hans Pettersson) and the Danish *Galathea* expedition (1950–2, led by Anton Bruun), which perhaps represented the final flourishes of a tradition that began with the British *Challenger* expedition (1872–6).[6] My concern is to deconstruct each expedition as an articulation of specific values and commitments—in cultural as well as intellectual terms—and to consider the role of the individual expedition within the more general emergence of deep-sea research as a discipline its own right. The second half of the chapter is primarily concerned with the bathyscaphe, a deep-sea submersible developed by the Swiss engineer and physicist Auguste Piccard. The bathyscaphe fitted within a different tradition of exploration, concerned more with individual dives than with longer cruises, but it too raised questions about the international dimensions of deep-sea expeditions and exactly what such expeditions ought to achieve. In setting records for the greatest depths ever reached by a human, the bathyscaphe also became a public relations asset for its sponsors—the navies of both France and the United States.

Continuing a Tradition of Deep-Sea Expeditions

The study of the ocean depths began seriously around the 1840s.[7] Much of this early interest focused on obtaining depth measures for individual points that could be aggregated to produce a topographic picture of the sea floor. The instruments were not particularly sophisticated—simply ropes with weights attached, in many instances—until acoustic sounding methods became widespread from the 1920s. As Sabine Höhler has argued, the increasing trust placed in this these instruments helped to make the ocean floor into a space that could be reliably known without direct personal observation.[8]

During the 1930s a series of expeditions to remote or little-known corners of the ocean returned with new data bearing upon both general problems related to the deep oceans (such as how water masses circulated around the globe) and more specific problems (such as locating the breeding grounds of eels). The German *Meteor* expedition (1925–7), the British Discovery Investigations (1925–38, and again 1946–51),[9] the Danish *Dana* expeditions (1920–30),[10] and others helped flesh out the physical, chemical and biological composition of the oceans in addition to the topography of the seabed. The *Meteor* employed an early form of sonar imaging to reveal under-sea topography, but most biological research continued with time-honoured instruments such as trawls and dredges. The most important element in obtaining new knowledge of the deep seas remained access to a ship that could take the expedition to a site where research could be conducted and data obtained.

The Second World War brought increased appreciation for the military applications of physical oceanography—the study of waves, tides, currents, and particularly acoustics (which could help detect submarines).[11] Oceanographers in the United States benefited from increased funding and support during the war, and by its end their British counterparts were optimistically building a new state-backed oceanographic institution.[12] To Scandinavian oceanographers the wartime transformation was less easy to perceive. From his home in Gothenburg in neutral Sweden, Hans Pettersson spent the war years plotting a global expedition to investigate the deep seas, with a particular focus on sediments from the deep ocean floor. In Copenhagen the zoologist Anton Bruun pondered a continuation of his earlier work on the *Dana* expeditions, which included the recovery of an outsized eel larva that Bruun speculated might be a very young sea serpent—prime fodder for summer news even in 1941.[13] Both men shared a common belief that once hostilities ceased in 1945 the grand, nationally flagged expeditions of earlier years would resume.

Pettersson's most famous achievement, the 1947–8 *Albatross* expedition, was in many ways emblematic of the pre-1939 age. Funds came from the wealthier members of Gothenburg society, continuing a tradition of civic patronage that supported Swedish polar exploration in the early twentieth century and established an Oceanographic Institute in the town in 1930—local contributions to a wider trend of using science as a means to express national commitment to civilized values.[14] While the expedition included foreign researchers during and especially after the voyage, the funding was entirely Swedish (as was the ship), and the expedition epitomized the continued strength of a nation that had long held a leading role in the marine sciences. Newspaper accounts of the expedition, many written by Pettersson himself, created a sense of adventure that both reflected expectations—oceanographic expeditions were encounters with exotic lands as much as interrogations of seas—and directed them, by noting the expedition's status as a proudly Swedish contribution to expanding human knowledge.[15]

The expedition was conceived within a longer tradition of exploration to which it remained faithful in both organization and practice. Pettersson consciously saw himself following a path laid down by the *Challenger* expedition. He had been interested in the radioactive qualities of the *Challenger's* deep-sea sediment samples since before the First World War, when he first encountered both the samples and the man who oversaw their collection, the great British oceanographer Sir John Murray.[16] On at least one occasion Pettersson ordered that stations be taken at the same locations as the *Challenger*, ostensibly to check the accuracy of individual samples—as though the *Albatross* had identified a specific point on the ocean floor as a controlled site for experimentation. That being said, the sentimental significance of following in the *Challenger*'s wake ought not to be dismissed, particularly given the admiration for the expedition and its guiding spirit that shines from Pettersson's papers.[17]

The difference between the *Albatross* expedition and the *Challenger* expedition lay in the methods deployed to make sense of the data recovered, rather than the basic idea of a single ship traversing the ocean and collecting samples under a national flag. Analytic methods had advanced considerably during the intervening thirty-five years, in part thanks to Pettersson's own earlier work in nuclear physics,[18] and new techniques continued to be developed to analyse the material that the *Albatross* brought back. Those samples were in turn obtained by a new instrument that would become a mainstay of submarine geology in the years to come—a piston corer developed by Börje Kullenberg, who worked under Pettersson at the Oceanographic Institute in Gothenburg.[19] This device was able to return long sediment cores from great depths, ultimately permitting historical analysis of changes at a given site through time, including large-scale climate change. The piston corer became the most notable achievement of Kullenberg's career, not least because it permitted the *Albatross* expedition to interrogate the same deep sea that other ships had traversed or even charted in a substantially different manner.

Might we consider this confluence of new technology and new techniques as the framework of an experiment? Possibly so, especially as the piston corer and new geochemical techniques permitted a far more detailed picture of the geological history of the deep oceans. But the decisive obstacle to conducting research remained the difficulty of obtaining a ship. Indeed, in the years that followed, Pettersson continued to lobby wealthy private benefactors who might appreciate research for its own sake (notably the new Prince of Monaco, who Pettersson and others hoped might continue the lavish patronage bestowed upon oceanographers by his grandfather).[20]

Similar to Pettersson, Bruun drew heavily upon local benefactors to finance his own round-the-world oceanographic expedition, and consciously located it within a longer tradition. Whereas Pettersson looked to the *Challenger* as his cultural lodestar, Bruun named his ship *Galathea* in emulation of a Danish expedition that had circumnavigated the world a century prior, investigating shallower waters. (A third *Galathea* expedition, conducted from 2006–7, met with rather less success.)[21] As Kristian Hvidtfelt Nielsen has skilfully demonstrated, Bruun constructed his new venture as an articulation of post-1945 Danish ambitions and anxieties.[22] An expedition was an expression of how its organizers viewed the world in addition to how they wished to explore it. In the case of the *Galathea*, the project of constructing the expedition as a media event went further than ever before, including press representatives as a permanent part of the ship's complement.[23]

The research conducted by the *Galathea* reflected Bruun's own interests and style of working, just as the *Albatross* did Pettersson's, in this case with a dominant focus on biological rather than physical sciences. Bruun spent much of his career at the Zoological Museum of Copenhagen. His doctoral work was a study of flying fish, and he retained a fascination for the taxonomic classification of exotic fishes. I think it fair to characterize his interest in the deep seas as informed by a basic curiosity about a region of the planet about which very little was known, with a concomitant possibility of discovering strange new life-forms. To the modern eye there is something almost charming about Bruun's work, a fascination for discovering and ordering new species allied with a sense of pleasure in establishing a worldwide network of friends and contacts. Charming people on shore was an integral component of the *Galathea*'s mission in geopolitical terms—waving the Danish flag in a manner befitting Denmark's tradition of marine science as well as marine commerce. But it was also an expression of Bruun's personal and professional worldview. His archives are filled with correspondence from around the world, testifying to an eagerness to make and maintain friendships and to build a global network of colleagues.

Bruun's international-mindedness became a powerful asset as the 1950s progressed and little Denmark could no longer support grand expeditions in the style of the *Galathea*.[24] As noted gravely on the inside front cover of the *Galathea* expedition's official book, released in 1953, "Deep-sea research is a costly science and expeditions rare."[25] Bruun became a key figure in the rapidly expanding ecosystem of international oceanographic organizations and a valued source of information for scientific and even military bodies in the United States, who in turn could fund oceanographic cruises as the cost and complexity of such ventures increased. While Pettersson was reduced to pleading with individual patrons whom he thought benevolently disposed to the cultural value of science, and grumbling when his efforts failed,[26] Bruun was able to get the United States military-industrial complex to work in his favour.

The main focus of the *Galathea* was on the creatures that lived in the deep sea, though the expedition also took advantage of its presence in remote spaces to do other work. Whereas the *Albatross* pioneered the Kullenberg piston corer, the most important instrument that the *Galathea* employed was an excellent winch, which permitted the collection of trawls from greater depths than hitherto possible. Raising the trawl to the surface inevitably created anticipation among the expedition members, for it was only when the trawl arrived at the ship that the strange deep materialized. Perhaps the most striking of these creatures was the deep-sea angler fish *Galatheathauma axeli* that graced the cover of the expedition book (dramatically luring a crustacean to its doom), the single specimen recovered in the tropical eastern Pacific. The size of the trawls placed limitations on the range of fauna that might be retrieved, and precluded any guarantee that the specimens returned were representative. As Bruun teasingly noted in the expedition book, "still no one has caught the Great Sea Serpent".[27] Knowledge of deep sea ecologies was correspondingly more difficult to piece together—though even individual specimens could be read for clues to the broader environment.

The *Albatross* expedition was far less concerned with deep sea fauna, but was similarly concerned with using data recovered from the deep ocean to reconstruct environments that could not be directly experienced. In Pettersson's case the impediment was time as well as space. His description of the seabed as an archive capable of revealing secrets from earth history, notably of climatic changes, in turn required the deployment of radiological and other methods that could translate geological samples into knowledge of past environments. The world revealed by the expedition was one requiring arcane interpretation, and not immediately available to the public—who were likely more intrigued by the crew's descriptions of exotic tropical lands (and their inhabitants).[28]

Obtaining and analysing the sediment cores that revealed the history was a complicated task that required a wide range of human and material resources. Lowering and raising the piston corer successfully took three to four hours and required calm seas, but the cores were up to 20 metres long—considerably better than the 3 metres hitherto possible, as Pettersson bragged to his peers.[29] Once the expedition returned, core samples were sent to facilities across Europe and the United States.

The marine ecologist Fred Phleger, who accompanied the *Albatross* for part of its cruise, used the presence of fossil foraminifera (microscopic shelled organisms) in the cores to illuminate past environmental conditions. Phleger carried out much of this work at the Scripps Institute of Oceanography at La Jolla, California. Back in Sweden, the young geochemist Gustaf Arrhenius performed similar analyses while also using spectroscopy to reveal the chemical composition of core sediments (including a pet interest of Pettersson's concerning the meteoritic origin of some sea-floor minerals, such as nickel). Arrhenius did much of the work at his father's ecological laboratories at a farmstead in Kagghamra, around 35 kilometres south-west of central Stockholm, producing results sufficiently good that he was invited to take a temporary position at Scripps—where he stayed and eventually became a full professor. Pettersson's diaries and correspondence reveal enthusiasm for the role American institutions played in working up the results of his expedition, but also frustration at how many of his best colleagues in Sweden headed across the Atlantic for better opportunities, to Scripps but also to newer institutions such as the University of Miami's marine science centre, where resources were more plentiful.[30]

Both the *Galathea* and the *Albatross* expeditions played important roles in establishing deep-sea oceanography as a field of study. The traffic in individual researchers both during and afterwards drew upon a network of individuals on both sides of the Atlantic that coalesced into a community, including a short-lived Joint Commission on Oceanography formed under the auspices of the International Council of Scientific Unions, with the aim of fostering discussion and cooperation among researchers with deep-sea interests. In 1953 a journal was founded, *Deep-Sea Research*, which remains active to the present day. Right up to his death in 1961 Bruun played key roles in a range of organizations and networks that established deep-sea research as a regular field of inquiry rather than the irregular fruit of distinct national expeditions.[31]

Bruun eagerly embraced the status of the ocean's depths as unknown spaces, capable of supporting beasts as remarkable as the legendary sea serpent, but also an array of odd-looking creatures that confirmed that life did indeed exist at great depths—and in strange forms. His almost romantic fascination with the underwater world paralleled that of the French naval officer and marine engineer Jacques Cousteau, who made a name for himself during the 1950s with books and films depicting the world up to 200 metres beneath the waves, made accessible through the aqualung (which he helped invent).[32] Cousteau took readers and later viewers on a personal journey to a space that was familiar—in the sense that the animals and objects that inhabited it were known—but exotic in the sense that the environment had never been encountered first-hand. Like others, he yearned to explore the deepest oceans and nurtured an interest in submersible technologies, including the bathyscaphe, a portmanteau term marrying the Greek words for "deep" and "ship". The bathyscaphe permitted a new form of deep-sea expedition that compressed the act of exploration into a comparatively short window—a dive rather than a cruise—with the promise of first-hand observation of the deep ocean, albeit balanced by fewer opportunities for data collection in other ways. Although the *Albatross* and *Galathea* expeditions relied upon their ship as a platform, in a sense very little had changed since the *Challenger*: it was the corers, winches and people on the boats that made the difference, not any shift in naval architecture. The bathyscaphe was different. The act of transporting a person to the deep sea depths was the experiment.

Going Up, Going Down

The bathyscaphe was the brainchild of the Swiss physicist and inventor Auguste Piccard (1884–1962). During the 1930s Piccard had pioneered high-altitude ballooning, using balloons and capsules of his own design to set new records for travel into the stratosphere (much of the technology was also used by his United States-based twin brother Jean).[33] Following 1945, he developed and oversaw the construction of the bathyscaphe, a device that drew on many of the same principles—a reinforced gondola attached to a flotation source—to instead go to the depths of the sea. Like a balloon, the bathyscaphe was also limited in its capacity for horizontal movement, being largely restricted to travelling up or down (at least in its early incarnations).

Piccard was not the first to design a strengthened cabin capable of transporting an observer to the deep seas. The American Otis Barton devised and built a "bathysphere", a spherical steel cabin attached by thick steel cable to a ship on the surface. First used in 1930, the bathysphere gained fame when Barton and the marine biologist William Beebe used it to explore the waters off Bermuda in a series of dives chronicled widely in the media, notably by *National Geographic Magazine*.[34] As a 1930 article in the magazine *Popular Mechanics* put it, the bathysphere provided "authentic information of the ocean depths" at depths of more than 400 metres below the surface,[35] as though the quality of authenticity could not attach to information obtained through trawling or sounding. In a subsequent book, Beebe enthused about the importance of direct observation for deep-sea biology (because the minimal light made the sensitive human eye essential for observing the subtle light changes and almost imperceptible marine fauna).[36] The bathyscaphe dives were public spectacles—the vehicle had the names of its sponsors (the National Geographical Society and the New York Zoological Society) painted on its side—and one dive was even broadcast live on radio. The drama packed into each descent more than compensated for the limited duration of each dive—and the fact that the two men were doomed if the cable snapped only added to the excitement. Picking out anything in the dark was a difficult task, even though the bathysphere was equipped with spotlights, but even the most basic observation of a realm hitherto invisible to human eyes could be depicted as the conquest of a frontier.

While Beebe and Barton gained fame for their feats, their descents did not constitute major scientific expeditions in the sense of those of the *Meteor* or the *Dana*, let alone the long-running Discovery Investigations. Each relied upon a ship and crew that sailed across the oceans and investigated the deep sea through instruments lowered across its side (in this case a bathysphere compared to a trawl). The key difference lay in the emblematic value attached to the instruments, as if the *Albatross* and its piston corer had swapped roles, with Kullenberg rather than Pettersson the key figure. The subject of the experiment was the instrument, not the data it returned.

Designing and constructing a vehicle capable of taking a human to an inaccessible space was what Auguste Piccard had in mind when he described exploration as "the sport of the scientist".[37] The travel of an individual to the stratosphere or the ocean depths did not necessarily advance science (although the vehicle could certainly provide a platform for instruments). But the feat drew legitimacy from an existing discourse that located exploration of the earth's more remote regions as an act with scientific gravitas—particularly when technology rather than courage was the determining factor. While Beebe protested that he was entirely unconcerned with depth records, the simple yardstick of who had descended deepest provided a validation of engineering skill—something that Piccard embraced with more relish.

Piccard was a physicist by training, educated at the prestigious Swiss Federal Institute of Technology (ETH) in Zürich, and he spent most of his career as professor of physics at the Free University of Brussels. Yet his legacy was defined by feats of engineering. The time of Beebe and Barton's descents and Piccard's stratospheric ascents was marked also by a rise in feats of heavier-than-air aviation, such as Charles Lindbergh's crossing of the Atlantic in the *Spirit of St Louis* (1927) and Richard Byrd's polar flights (1926 and 1929). These feats were similarly limited in time and heavily defined by technology, like the bathysphere and balloon ventures. As Marionne Cronin has argued, the construction of the environment as an obstacle to be overcome through technology permitted the individual explorer to retain a sense of masculine triumph despite their reliance upon a vehicle to perform their task.[38] Such dynamics were not entirely new: after all, the Norwegian naval architect Colin Archer gained a measure of fame as the designer of the ship *Fram*, which permitted Fridtjof Nansen's epic 1893–96 Arctic voyage.[39] Cronin is nevertheless right to suggest that the adoption of the aircraft as a vehicle for geographical exploration led to the renegotiation (though not the replacement) of traditional narratives of personal struggle against nature, with mastery of technology now an essential component of—and not a diminished alternative to—the demonstration of mastery over nature.

To generations of deep-sea researchers, success or failure was attributed more to those on board (or those responsible for the route and planning) than to the designers of the ship. Not all research vessels were created equal; the *Discovery II* was particularly admired during the interwar years. James Cook's three great expeditions are not chalked up to the brilliant design of the *Endeavour*, although it was certainly a suitable vessel, any more than Bruun viewed the *Galathea* expedition as a product of the ship. The *Galathea* was valuable because it was available and because it had been equipped with a fine winch. In much the same way, the *Albatross* was the fungible component of the expedition when compared with the Kullenberg piston corer. This was not the case for the bathyscaphe. Piccard proposed a different model of deep-sea expedition in the tradition of Beebe and Barton, with the vehicle the decisive element.

The design of the bathyscaphe required assiduous attention to detail in addition to knowledge of a range of materials and techniques. *In Balloon and Bathyscaphe*, the 1956 book that Piccard wrote for a popular audience about his experience with the bathyscaphe (prefaced by a brief section on his ballooning), was heavy on technical and engineering detail, replete with diagrams and jargon that must have taxed the ability of most readers to follow.[40] While apparently consistent with Piccard's character—the curious engineer who took obvious pride in his ingenious achievements—this style also performed rhetorical work by emphasizing the design and construction of the bathyscaphe as the key aspect of the feat of exploration. As he made painstakingly clear in the text, Piccard viewed the actual manned dives as comparatively routine events, the very quality of being unexciting a testament to the rigor of the bathyscaphe's design.[41]

The first bathyscaphe that Piccard designed and built was named the *FNRS 2*, a nod simultaneously to the Belgian national research agency that funded it, as well as the balloon that used many of the same principles (and which bore the title *FNRS 1*). Its construction phase drew upon Belgium's modern history as both an industrial and a colonial nation, with steel milled in Wallonia and checked for impurities through analysis involving radium from the mines in Katanga, Belgian Congo.[42] The *FNRS 2* successfully dived to a depth of 1388 metres off the coast of West Africa in 1948, with Piccard at its controls, breaking Beebe and Barton's previous record of 929 metres. Each dive required considerable infrastructure, including iron ballast pellets (which could not be reused, as they were expelled into the ocean in order to make the bathyscaphe rise again) and a support ship to carry the staff needed to keep the bathyscaphe in a condition ready for diving.

During the African dives, Piccard and his team received the active support of the French Navy, and when the FNRS hesitated to invest more money in the expensive programme the Navy agreed to support the programme on the condition that it assumed ownership of the *FNRS 2*, retaining Piccard as an adviser. The Navy already possessed a thriving Undersea Research Group based in Toulon, on the Mediterranean coast, founded in 1945 by Jacques Cousteau. Primarily concerned initially with aqualung diving—the field Cousteau had pioneered—the group, known by its French acronym GRS, expanded its horizons when Piccard approached Cousteau in 1948 to assist with the *FNRS 2*'s dive programme. Cousteau's biographer Brad Matsen notes that "several of the scientists in the Underwater Research Group cautioned him against allying with a man considered to be a daredevil showman" (a sentiment apparently shared by Cousteau's wife).[43] The promise of diving far beyond the range of a scuba diver ultimately proved irresistible. But Cousteau's dismissive description of Piccard's emergence from the bathyscaphe after its first dive—"clutching a patented health drink with the label squarely presented to the cameras"[44]—reinforced a sense that the sport had surpassed the science in Piccard's persona, reducing the experiment to a demonstration.

The agreement between Piccard and the Navy soon broke down. Competing narratives emerged concerning the value of the 1948 dives, and who deserved credit for subsequent developments of the *FNRS 2*. The French rebuilt the *FNRS 2* into the *FNRS 3*, improving upon Piccard's basic design without changing any of its fundamental principles (a point the Piccards made clear in their own books). Depicting the 1948 dives as "ill-fated"[45] and dangerous allowed the GRS to claim credit for successful innovation by categorizing the original Piccard design as fatally flawed. Matsen argues that Cousteau himself soon lost interest in the bathyscaphe, frustrated by its inability to manoeuvre horizontally and the fact it was "far too clumsy for filmmaking", leading him to develop alternative technologies, such as the diving saucer that could not dive as deep but do much more once there.[46]

While the Navy worked on the *FNRS 3*, Auguste Piccard and his son Jacques procured funds from Italy to construct a new bathyscaphe, named the *Trieste*, in honour of the city where it was constructed (apparently following a suggestion from the town's mayor to Jacques Piccard).[47] The city remained disputed between Italy and Yugoslavia, with a faction supporting independent status as a free city,[48] and the bathyscaphe offered a prestige project in addition to a practical benefit for the depressed local shipyards. In August 1953 the *Trieste* established a new record depth of 3167 metres off the coast of Ponza, a small island in the Tyrrhenian Sea. Under the command of Georges Houot (Cousteau's successor at the GRS) and Pierre Willm (the engineer responsible for its redesign), the *FNRS 3* descended to 4050 metres off the coast of Dakar in February 1954. While these expeditions never captured global attention to the same degree as the Space Race, they relied upon the bathyscaphe performing a very similar role to the manned space capsules that went progressively further into space and for longer, as artifacts that valorized the network of engineers and scientists behind them as much as the occupant inside.

The scale of resources necessary for constructing and operating a bathyscaphe, combined with its status as an emblem of technological sophistication, helps explain why United States military agencies began to take an interest in the *Trieste* after its first series of dives in 1954. The cost of conducting each dive dictated a degree of logistical support that proved too much for the French, and which the Piccards found difficult to replace in Italy, although the Italian Navy offered some assistance. Meanwhile, the United States Office of Naval Research (ONR) had become an enormously generous sponsor of oceanographic research,[49] and its liaison officer in

Europe—Robert S. Dietz—held a particular interest in under-sea technology, from Cousteau's aqualung to the bathyscaphe. Dietz opened negotiations with Jacques Piccard on purchasing the *Trieste* and sponsoring a new series of dives in the Mediterranean and eventually the Pacific, home to the deepest trenches of all, after it had been modified to descend to depths of over 10,000 metres.

How did Dietz persuade the United States authorities—and more specifically, the Navy Electronics Laboratory at La Jolla, California—to invest in this vehicle? Already in 1954 the Piccards had approached the US National Science Foundation to sponsor a series of dives in the deep Atlantic, citing the unexplored nature of the deep ocean rather than any specific goal, but the proposal was rejected.[50] A year later Dietz filed a report for the ONR that succinctly assessed the bathyscaphe's military value as "nil" while stressing the importance of basic oceanographic research and insights that might aid the design of future deep-water submarines.[51] Aided by personal testimony from Dietz and Jacques Piccard, a panel of six well-respected figures in American oceanography signed a resolution calling for the United States to invest in bathyscaphe technology, citing the potentially significant benefits of possessing a craft capable of reaching deep waters.[52] Thus was a justification established: the technical expertise developed could be used for future vehicles, in addition to the potential benefits to oceanographic research from having an observation platform in the deep oceans.

But despite a general belief that possessing a vehicle capable of reaching the deep oceans must per definition be useful, the bathyscaphe was a problematic platform for most oceanographic research. Houot held out hope in 1955 that the *FNRS 3*, having proved its ability to attain great depths, would become "a really efficient undersea observatory" provided that an appropriate suite of instruments was devised (and advances made in "the complex problems of underwater photography").[53] When the ONR took over sponsorship of the *Trieste* programme, it piously announced that the 1957 dive programme was related to the International Geophysical Year (IGY), a massive episode of international cooperation in which observations were coordinated from around and beyond the globe.[54] "It is not the purpose of these dives to establish any records", stated the press release. "It is rather to make scientific observations and to maintain the orderly and safe development of a craft which promises eventually to make 'inner space,' i.e., the vast depths of the ocean, accessible to man."[55] Fidelity to aims of the IGY was part of an attempt to place the bathyscaphe in a different tradition from that of record-setting vehicles (most notably aircraft and balloons). That, in turn, required evidence that it could furnish useful scientific data in addition to completing a feat of travel, but as the press release suggested, the ultimate justification for sponsoring the programme rested with the development of the vehicle itself.

The nature of the bathyscaphe, designed to transport people, meant that the kinds of instruments that augmented human eyes were restricted to those it was possible for the observer to carry on board and use within the cabin, such as a light meter. Houot seized on a problem identified by Hans Pettersson (who would have been horrified at Houot's misidentification of him as Danish) concerning the inability of trawls to retrieve samples of exceptionally small plankton, suggesting that the observations made by humans could provide an answer, and even speculating (based on ideas of the submarine geologist Jacques Bourcart) that deep-sea plankton distribution might help identify rich fishing grounds closer to the surface.[56] Yet that same question of how to quantify and spatially describe the productive capacity of the oceans—with the overall goal of increasing fishing and thus helping to feed the world's population—was already the subject of extensive discussion at bodies such as the United Nations Food and Agriculture Organization, and research by oceanographers such as Bruun's associate Einer Steemann Nielsen.[57] None of this required a bathyscaphe; indeed, much of Steemann Nielsen's pioneering work on ocean productivity was derived from work he conducted during the *Galathea* expedition, notably through using carbon-14 isotope ratios to measure marine photosynthesis.[58] Bruun's initial view of the bathyscaphe as "a little passé" reflected the fact that, without any means of personally reaching the ocean depths, he and many others had accumulated sufficient data to fuel a thriving scholarly debate on the nature and distribution of deep-sea fauna, and their relation to the surrounding geomorphology.[59]

Under the title of Project Nekton, the *Trieste* achieved the ultimate feat in deep-sea travel when it descended to the bottom of the Mariana Trench on 23 January 1960, a depth of 11,521 metres, with Jacques Piccard at the controls along with US Navy lieutenant Don Walsh. The subsequent ONR press release noted the dive's record-breaking nature while asserting its value to acquiring "scientific knowledge of sunlight penetration, underwater visibility, transmission of man-made sounds, and marine geological studies".[60] During the years that followed, a number of studies were indeed made from bathyscaphes on subjects such as observations of benthic (sea-bottom) fauna[61] and analysis of sediments collected from deep waters,[62] some conducted by the *Trieste* and its successor *Trieste II*, and many others by the *Archimède*, the successor to the *FNRS 3* designed by Houot and Willm.[63] The bathyscaphe proved useful in limited instances, but it remains synonymous today with the setting of a record that demonstrated mastery of marine technology more than interrogation of the earth's phenomena.

It is telling to note that at the same time that the *Trieste* reached the high (low?) point of its career, a major research programme known as the International Indian Ocean Expedition (IIOE) was getting underway, resulting in a comprehensive series of surveys involving tens of ships and thousands of personnel over six years. This "expedition"—which Bruun had been involved with organizing before his sudden death—was an evolution of the tradition that he had helped to build. A coordinated programme, involving personnel and ships from a range of nations, worked in concert toward a systematic exploration of the deep oceans. The IIOE represented a new vision of deep-sea oceanography that mirrored the geopolitical template laid down by IGY. Just as the IGY ushered in a new era of Antarctic research in which long-term, state-sponsored missions replaced discrete expeditions, the IIOE represented the new face of deep-sea oceanography, one in which singular achievements took a back seat to the coordinated acquisition of huge datasets.

If the IIOE had its antecedents in the expeditions of Pettersson and Bruun, the dives of the *Trieste* drew upon the legacies of Lindbergh and Byrd. They resonated more with the manned space missions that so captured public attention in the 1960s than the ship-borne expeditions of the 1950s. The individual vessel was central, the technological artifact whose subjection to the ocean depths constituted the experiment more than any particular scientific accomplishment. While the *Trieste* remains a well-known name, I challenge any reader not intimately familiar with the IIOE to name a single one of its vessels.

Conclusion

The act of exploring the deep seas could be conducted through numerous different methods—from personal observation to sediment cores and trawling—with a range of backers and practitioners. Such differences highlight the cultural work involved with constructing particular expeditions as emblematic examples of deep-sea research. But they also draw attention to the discrete expedition as a site of data accumulation, to the broader networks within which that data would become valuable knowledge, and to the contrasting values placed upon penetration of the deep sea as a means to an end or an end in itself. Yet each format performed a particular geopolitical as well as research function.

If the *Albatross* and the *Galathea* collected samples and specimens that could be analysed at centres of calculation, they nevertheless represented more than simply vehicles for data collection. Pettersson's veneration of the *Challenger* expedition articulated a view of his own discipline's history and of the *Albatross* expedition's place within it: a continuation of a proud tradition to which he paid tribute through the process of exploration. The analysis of the *Albatross* samples through sophisticated geochemical methods further linked the practice of oceanographic data collection to the intellectual tradition to which Pettersson belonged. Each record-setting dive of the *FNRS 2*, the *FNRS 3* and the *Trieste* was an occasion in its own right, an extension of previous feats that culminated in the Marianas Trench dive, but the bathyscaphe programmes were not located within specific intellectual traditions in the same way. While I would not want to dismiss the bathyscaphe entirely as a means of obtaining data, the emblematic goals consisted of completing a journey, in contrast to Pettersson's ambition to read the archives of the ocean floor or Bruun's quest for the sea serpent. The vehicle, not the ocean or its floor, was the subject of the experiment.

Today the term "expedition" retains its long-standing definition as a unit of research activity defined by travel, with all the cultural connotations that entails. National oceanographic expeditions, particularly in large ships such as icebreakers, are geopolitical performances that articulate the interest of the state in both investigating and controlling the space being investigated. Networks are more important than ever, from marshalling the resources to build impressive vessels to paying for specialized staff and their sophisticated instruments and ensuring that data can be analysed and converted into knowledge.

And yet, the individual act of deep-sea exploration retains a distinctive sense of expanding the limits of human endeavour in a manner that goes beyond science. Film-maker James Cameron descended to the bottom of the Mariana Trench in March 2012 in a bright green submersible that he helped design himself. As the publicity material surrounding the expedition made clear, the technical wizardry required to design the submarine was the real novelty, permitting travel that could potentially aid all manner of research.[64] The venture was specifically marketed as an expedition inspired by curiosity and the drive "to promote exploration and scientific discovery" that built on the achievements of the *Trieste*.[65] The tradition in which Bruun and Pettersson located themselves clearly continues to coexist with another that includes Cameron and the Piccards.

Notes

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2. Peter Galison and Bruce Hevly, eds., *Big Science: The Growth of Large-Scale Research* (Stanford: Stanford University Press, 1992).
3. Felix Driver, *Geography Militant: Cultures of Exploration and Empire* (Oxford: Blackwell, 2001).
4. See for instance David Philip Miller, "Joseph Banks, Empire, and 'Centres of Calculation' in Late Hanoverian London", in David Philip Miller and Peter Hanns Reill, eds., *Visions of Empire: Voyages, Botany, and Representations of Nature* (Cambridge: Cambridge University Press, 1996), 21–37; D. Graham Burnett, *Masters of All They Surveyed: Exploration, Geography, and a British El Dorado* (Chicago: University of Chicago Press, 2000).
5. The term is introduced and outlined in Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Milton Keynes: Open University Press, 1987), 215–57.
6. Each expedition produced an official account. See respectively Hans Pettersson, *Westward Ho with the* Albatross (New York: Dutton, 1953), and Anton Bruun, Svend Greve, Haakon Mielche and Ragnar Spärck, eds., trans. Reginald Spink, *The Galathea Deep Sea Expedition 1950–1952: Described by Members of the Expedition* (London: George Allen and Unwin, 1956).

7. See for instance Helen Rozwadowski, *Fathoming the Ocean*: *The Discovery of the Deep Sea* (Cambridge MA: Belknap, 2005); Susan Schlee, *The Edge of an Unfamiliar World*: *A History of Oceanography* (New York: Dutton, 1973).
8. Sabine Höhler, "Depth Records and Ocean Volumes: Ocean Profiling by Sounding Technology, 1850–1930", *History and Technology* 18(2) (2002): 119–54.
9. I have discussed the Discovery Investigations at some length in Roberts, *The European Antarctic*: *Science and Strategy in Scandinavia and the British Empire* (New York: Palgrave Macmillan, 2011). See also Mills, *The Fluid Envelope* on the Physical Oceanographic Work of the Investigations and D. Graham Burnett, *The Sounding of the Whale*: *Science and Cetaceans in the Twentieth Century* (Chicago: University of Chicago Press, 2012) on its whaling research.
10. Torben Wolff, "The Danish *Dana* Expedition, 1928–30: Purpose and Accomplishments, Mainly in the Indo-Pacific", in Benson and Rehbock, eds., *Oceanographic History*, 196–203.
11. Hamblin, *Oceanographers and the Cold War*; Gary E. Weir, *An Ocean in Common*: *American Naval Officers*, *Scientists*, *and the Ocean Environment* (College Station, TX: Texas A&M University Press, 2001).
12. Sam Robinson, *Between the Devil and the Deep Blue Sea*: *Ocean Science and the British Cold War State*, PhD diss., University of Manchester, 2015.
13. Nielsen, *På jagt efter søslangen*, 11.
14. Lennart Olausson, "The Göteborg Civic Culture at the Turn of the Century 1900: A Cultural Heritage of Entrepreneurial Management and Liberal Education", in Aant Elzinga, Torgny Nordin, David Turner and Urban Wråkberg, eds., *Antarctic Challenges*: *Historical and Current Perspectives on Otto Nordenskjöld's Antarctic Expedition 1901–1903* (Gothenburg: Royal Society of Arts and Sciences, 2004), 41–54. Robert Marc Friedman uses this term in his analysis of Norwegian geophysical research in "Civilization and National Honour: The Rise of Norwegian Geophysical and Cosmic Science", in John Peter Collett, ed., *Making Sense of Space*: *The History of Norwegian Space Activities* (Oslo, Stockholm, Copenhagen, Bonn: Scandinavian University Press, 1995), 3–39.
15. A collection of newspaper stories written by expedition members (and about the expedition) is kept in the Hans Pettersson collection at the Gothenburg University Library (hereafter GUL).
16. Pettersson frequently mentioned the *Challenger* in his newspaper articles before and during the *Albatross* expedition. A particularly good example is "Atlantens botten", *Göteborgs Handels- och Sjöfartstidning*, 30 August 1947.
17. Hans Pettersson, "I H.M.S. Challengers spår", *Göteborgs Handels- och Sjöfartstidning*, 6 December 1947.

18. On this period of Pettersson's career, see for instance Maria Rentetzi, *Trafficking Materials and Gendered Experimental Practices: Radium Research in Early 20th-Century Vienna* (New York: Columbia University Press, 2008).
19. While Pettersson played a role in the development of the corer, and it would not be right to ascribe the device's invention and development entirely to Kullenberg, the corer became synonymous with Kullenberg to a far greater extent.
20. See for instance the records of the Joint Commission on Oceanography meeting in Monaco held on 23–5 September 1952. A copy is kept in the Scripps Institute of Oceanography Archives (hereafter SIO), Roger Revelle Papers MC 6 23/1.
21. See, for instance, http://videnskab.dk/kultur-samfund/galathea-ekspeditionen-var-naivt-skruet-sammen, accessed 29 April 2014.
22. Kristian Hvidtfelt Nielsen, "Postcolonial Partnerships: Deep Sea Research, Media Coverage and (Inter)national Narratives on the *Galathea* Deep Sea Expedition from 1950 to 1952", *British Journal for the History of Science* 43(1) (2009): 75–98.
23. Kristian Hvidtfelt Nielsen, "In Quest of Publicity: The Science–Media Partnership of the *Galathea* Deep Sea Expedition from 1950 to 1952", *Public Understanding of Science* 18 (2009): 464–80.
24. Peder Roberts, "Intelligence and Internationalism: The Cold War Career of Anton Bruun", *Centaurus* 55(3) (2013): 243–63.
25. *The Galathea Deep Sea Expedition*, inside front dust jacket cover.
26. Pettersson's strategy for approaching the wealthy American patron of oceanography Allan Hancock to fund a research vessel was particularly striking, as recorded in Pettersson's diary entry for 28 April 1955. GUL Pettersson collection H:212/9. I am very grateful to Hans Pettersson's son Rutger Irgens for granting access to his father's diaries, and to Anders Larsson, manuscript librarian at the University of Gothenburg, for facilitating this permission.
27. Bruun in *The Galathea Deep Sea Expedition*, 177.
28. For a particularly good example see "Med Albatross i Söderhavet", *Aftonbladet*, 13 March 1948. A copy of this, and many other articles, is held in Pettersson's papers at GUL.
29. The process of obtaining and analysing the cores is described in overview in Pettersson, "The Swedish Deep-Sea Expedition, 1947–48", *Deep-Sea Research* 1 (1953): 17–24.
30. The extent of this collaboration is documented in Pettersson's papers at the GUL and related papers at SIO, and Pettersson's sometimes bitter feelings come through clearly in diary entries and correspondence, particularly with highly regarded assistant Fritz Koczy.

31. Roberts, "Intelligence and Internationalism".
32. Jacques Cousteau, *The Silent World* (New York: Harper, 1953).
33. On Auguste Piccard's ballooning career, see most notably David DeVorkin, *Race to the Stratosphere: Manned Ballooning in America* (New York: Springer, 1989).
34. On the bathyscaphe dives, see Carol Grant Gould, *The Remarkable Life of William Beebe: Explorer and Naturalist* (London: Shearwater Books, 2004); and Brad Matsen, *Descent: The Heroic Discovery of the Abyss* (New York: Pantheon, 2005).
35. "Three Hundred Fathoms Beneath the Sea", *Popular Mechanics* (October 1930), 579.
36. William Beebe, *Half Mile Down* (London: John Lane The Bodley Head, 1935).
37. Auguste Piccard, trans. Christina Stead, *In Balloon and Bathyscaphe* (London: Cassell, 1956), xii.
38. Marionne Cronin, "Polar Horizons: Images of the Arctic in Accounts of Amundsen's Polar Aviation Expeditions", *Scientia Canadensis* 33(2) (2010): 99–120.
39. The importance of the *Fram* to Nansen is discussed in Roland Huntford, *Nansen: The Explorer as Hero* (London: Duckworth, 1997).
40. Piccard, *In Balloon and Bathyscaphe.*
41. Even the title nodded to this point by referring to the vehicle rather than how far it travelled: contrast with Jacques Piccard and Robert S. Dietz, *Seven Miles Down: The Story of the Bathyscaphe* Trieste (New York: Putnam, 1961), and Georges Houot and Pierre Willm, trans. Michael Bullock, *2000 Fathoms Down* (New York: Dutton, 1955).
42. Piccard, *In Balloon and Bathyscaphe*, 42.
43. Brad Matsen, *Jacques Cousteau: The Sea King* (New York: Pantheon, 2009), 98.
44. Cousteau, *The Silent World*, 101.
45. Houot and Willm, *2000 Fathoms Down*, 18.
46. Matsen, *The Sea King*, 145.
47. Piccard and Dietz, *Seven Miles Down.*
48. On the post-war dispute over Trieste, see for instance Bogdan C. Novak, *Trieste, 1941–1954: The Ethnic, Political, and Ideological Struggle* (Chicago: University of Chicago Press, 1970).
49. Of the many works on the ONR, see for instance Harvey M. Sapolsky, *Science and the Navy: The History of the Office of Naval Research* (Princeton: Princeton University Press, 1990).
50. A translated copy of this application is held in the Dietz collection at SIO, MC 28 11/19.
51. Dietz, ONR Technical Report ONRL 71–55, 10 August 1955, 9. Copy held in SIO Dietz papers, MC 28 11/19.

52. E. H. Smith, A. Vine, J. D. Isaacs, R. Montgomery, R. Fleming and W. Bascom, “Resolution Submitted to the Symposium on Aspects of Deep Sea Research”, 10 March 1956. Copy held in SIO Dietz papers, MC 28 11/19.
53. Houot, *2000 Fathoms Down*, 181.
54. On the IGY and oceanography, see for instance Hamblin, *Oceanographers and the Cold War*, chapter 3.
55. Press release issued 27 February 1957 by the United States Naval Forces Public Information Office Eastern Atlantic and Mediterranean in London. A copy is held in the papers of Håkon Mosby at the Geofysisk Institutt in Bergen, Norway, folder IGY IV.
56. Houot, *2000 Fathoms Down*, 181.
57. Extensive records on this subject are held in Bruun’s personal papers, especially RAD 06768, box 26.
58. See for instance Einer Steemann Nielsen, “The Use of Radioactive Carbon (C14) for Measuring Organic Production in the Sea”, *Journal du Conseil/Conseil Permanent International pour l’Exploration de la Mer* 18 (1952): 117–40.
59. Andreas Rechnitzer to Bruun, 29 November 1957, RAD 06768, box 15. Although Bruun appears to have come round to the vehicle’s merits by late 1958, I can find no record of a specific scientific task he thought it might be useful for (although he did express optimism about its potential in general terms). He did however tell a friend in the shipping business that it might aid the development of submarines capable of transporting freight underneath the Arctic Ocean. Bruun to Direktør O. Christiansen, Nakskov, 3 February 1959, RAD 06768, box 15.
60. A copy is archived at http://www.onr.navy.mil/focus/ocean/vessels/submersibles11.htm
61. E. G. Barham, N. J. Ayer Jr and R. E. Boyce, “Macrobenthos of the San Diego Trough: Photographic Census and Observations from Bathyscaphe, Trieste”, *Deep-Sea Research* 14 (1967): 773–84.
62. C. Boulouard and H. Deleuze, “Analyse palynoplanctologique de sédiments prélevés par le bathyscaphe ‘Archimède’ dans la fosse du Japon”, *Marine Geology* 4 (1966): 461–6.
63. See for example the accounts by Robert Ballard and Don Walsh in *They Lived to Tell the Tale*: *True Stories of Adventure from the Legendary Explorers Club* (Guilford, CT: Explorers Club, 2008).
64. See the expedition website: http://deepseachallenge.com. Accessed 29 April 2014.
65. See: http://deepseachallenge.com/the-expedition/. Accessed 29 April 2014.

CHAPTER 11

It Had to Be Us: Geological Practice, Scientific Authority and Politics in the Expedition to Goa (1960–1)

Teresa Salomé Mota

Introduction

On the 22 February 1960, at 8.15 a.m., the members of the Brigade of Geological Studies of the State of India left Lisbon by plane heading to Goa. After a stop in Karachi, they flew from Pakistan on the morning of the 26 February and arrived in Goa the same day around 4 p.m.[1]

The Brigade was organized by the Portuguese Board for Overseas Research and aimed to "undertake the survey and studies required to know the geology and mineral resources of the territory [Goa, Damão and Diu] in view of its better use".[2] Funding came from both the second Development Plan[3] and the Board; the geological expedition to the State of Portuguese India[4] was planned to last four years.[5]

The Brigade's first campaign[6] took place between February and July 1960, and the second between November 1960 and July 1961. The third and last campaign began in November 1961 and ended abruptly on the morning of 18 December with the invasion of Goa by Indian troops.[7]

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From the eighteenth century onwards, European nations promoted the organization of scientific expeditions. They confronted the Europeans with the wild, the wilderness and the exotic, their aim being the description and inventory of natural objects with the ultimate purpose of classifying them, but utilitarian aspects, such as the economic exploitation of natural resources, were also part of their goals.[8] During the nineteenth century, the characteristics of scientific expeditions changed in a context of growing professionalization and specialization in Western science. A new perception of the role of European nations emerged and they began to base their colonial enterprises on the concept of the *mission civilisatrice*, which was largely structured around techno-scientific parameters. Scientific expeditions were an expression of this process.[9]

Scientific expeditions sometimes had a geological component; at other times, they were utterly dedicated to the geological reconnaissance of a certain region, an enterprise that is very much based on fieldwork. In effect, fieldwork was decisive to the emergence of geology between the late eighteenth and the beginning of the nineteenth century: fieldwork allowed the gathering of data that was used in the resolution of a bewildering variety of geological problems.[10] Thus, travelling and fieldwork became quintessential activities of geological practice, the field being perceived as a kind of "laboratory" where geological knowledge is constructed.[11]

Today, historians of geology are keener to accept that the role of the real laboratory and experiment in the development of geology as a scientific discipline was far greater than has usually been considered.[12] Yet, experiments in geology raise important methodological questions: the magnitude of spatial and temporal scales and the multitude of variables involved in geological processes make it impossible to "reproduce" them in the laboratory and to unmistakably prove that a certain cause–effect relation occurred.[13] Perhaps a suitable way to look at the laboratory and the field in geology is to perceive them not as opposite spaces but as a *continuum* through which various elements of geological practice circulate.[14]

This particular aspect of geological practice becomes evident in the present chapter, which deals with the circumstances surrounding the geological expedition to Goa made by the Brigade of Geological Studies of the State of India during the Estado Novo (literally, New State), the dictatorship that ruled Portugal from 1930 to 1974. The expedition is here presented as an "experimental space", where scientific methodologies and techniques were tried for the first time and an entirely new network of social relations was experienced and put to the test. Thus, in addition to geological practice, questions of scientific authority will be addressed, as well as the web of scientific and social relations established by the members of the Brigade. This is also an interesting case study from the point of view of the instrumentalization of science. It shows how a dictatorial regime, the Estado Novo, used geological knowledge to assert its role as a colonial power, and that geologists did not have a naive attitude towards this process: they were able to take advantage of the situation by using the geological expedition organized by the Board as a strategy to assert the importance of the Portuguese geological community.

Setting the Stage

The Board for Overseas Research was created in 1936 with the mission of coordinating and promoting scientific work in the Portuguese colonies. Conducting scientific expeditions addressing geographical, geological, botanical, zoological, anthropological and ethnographical questions was one of the strategies adopted by the Board as a means to achieve the *effective occupation* of Portuguese colonies.[15] The principle of *effective occupation* arose following the Berlin Conference in 1884/85. Consequently, during the last two decades of the nineteenth century, the monarchy was compelled to intensify the occupation of the Portuguese African colonies and promote their development, in order to counteract the impending threat of possession and partition by other European colonial powers. The principle was maintained by the ensuing republican regime that overthrew the monarchy in 1910, and gained a systematic character during the subsequent Estado Novo, when a new phase of colonial administration with particular nationalist and centralist overtones was established.

The Estado Novo was a conservative dictatorial regime established in Portugal in the 1930s, as a response to the incapacity of the previous republican regime to deal with the political, economic and social changes that took place in the country in the early twentieth century.[16] The dictatorship was able to keep its main characteristics almost unchanged for nearly half a century until its end, in 1974.[17]

When the Brigade of Geological Studies of the State of India was created in 1960, the Estado Novo was going through a critical and difficult period, in particular with respect to its colonial policies and international relationships. After 1947, when it became independent from Great Britain, India claimed that the Indian territories under Portuguese administration should be reintegrated into India. The Estado Novo, however, stubbornly refused to relinquish them and, throughout the 1950s, the tension between Portugal and India increased. In 1954, two continental enclaves, Dadrá and Nagar-Aveli, were annexed by India. Meanwhile, between backstage diplomatic games and other less smooth strategies,[18] Portugal tried to implement the *effective occupation* of Indian territories. But as the 1960s approached and the dictatorship came uncer increasing criticism from the international community, it became clear that it was impossible to maintain its policies regarding India; in 1961, the territories of the State of Portuguese India (Goa, Damão and Diu) became part of the Indian Union.

The 1960s were also a critical period for the consolidation of the Portuguese geological community. Since 1940, when the Geological Society of Portugal was created, geologists had claimed their place in society and struggled for the recognition of their scientific and professional status. They joined the Portuguese scientific community in arguing that scientific knowledge was a prime means to both the *effective occupation* of the colonies and economic and social progress; eventually, this strategy proved fruitful. The creation of the Brigade of Geological Studies of the State of India can be considered a tour de force on the part of the geological community in gaining recognition from the Portuguese ruling power.

Arriving and Settling

The Brigade of Geological Studies of the State of India was composed of eight members: five geologists and three field assistants. Geologist Gaspar Soares de Carvalho (1920–2016), recently contracted by the Board, headed the Brigade; António Morais Romão Serralheiro (b. 1927) and Ilídio António da Assunção Godinho were also geologists from the Board, the first being assistant geologist of the Brigade; José Ávila Martins (1917–96) was a geologist from the Board for Nuclear Power, and Carlos Teixeira (1910–82) a professor of geology at the Faculty of Sciences of the University of Lisbon. The field assistants were Joaquim César Lopes and Francisco Bento Vale, both from the same faculty, and Henrique Vieira Dias.[19]

When the members of the Brigade arrived at the capital of Goa, Pangim, they were received by representatives of the Portuguese authorities in the colony: the Governor, General Manuel António Vassalo e Silva (1899–1985), and the director of the Economic Bureau, Major Ricardo Ferreira.[20] However, the stay in Goa did not start under the best auspices: the

Governor did not prove particularly welcoming since he had not been informed of the arrival and purposes of the Brigade.[21] He even advised the members of the Brigade not to count on him in finding accommodation. They ended up being lodged in different hotels and guesthouses in Pangim according to their respective income. The cost of accommodation was high; in the case of junior geologists and field assistants the amount almost equalled their salary, making it more difficult to send money to their families in the metropolis, Lisbon.[22]

Eventually, the relationship between the members of the Brigade and the Governor improved with time. He provided them accommodation in Caranzalem, a small coastal village on the outskirts of Pangim, but the house had no running water or electricity. To begin with, the members of the Brigade settled in tents and only after some time were they accommodated in another house. Caranzalem became the headquarters of geological fieldwork but campsites were also set up when necessary. Samples collected during the surveys were stored in an old garage in Pangim, courtesy of the Directorate of Public Works in Goa.[23]

Conducting Geological Fieldwork in Goa

Geological fieldwork started in Goa; yet during the first campaign, the Brigade also surveyed Damão and Diu. Geologists and field assistants worked in the field for six or seven hours, from early morning to early afternoon on a daily basis. Surveys began in the north and extended gradually to the south, and from the coast to the hinterland, according to the division of the territory into three zones: one situated north of the Mandovi River, another between the Mandovi and Zuari rivers, and a third one south of the Zuari River. Overall, the Brigade surveyed the entire county of Bardez and part of the counties of Bicholim and Satari.[24] Locally contracted people assisted the members of the Brigade during fieldwork, but they also had to perform other tasks, like driving vehicles, setting up campsites or transporting equipment.[25]

The members of the Brigade conducted geological fieldwork under Teixeira's guidance, in accordance with methods commonly used in Portugal[26]: first, they carried out a general survey of the territory in order to identify the main geological units and structures. Subsequently, they decided which itineraries should be followed during more detailed surveys; typically, geologists chose routes where the probability of finding good outcrops was greater.[27]

However, when detailed surveys began, the members of the Brigade realized the serious difficulties they would face in completing their task. One of the most relevant concerned the use of topographic maps provided by the section of mines of the Economic Bureau in Goa. Part of the geological information obtained during surveys—namely the extent and configuration of geological units—is represented on topographic maps with an appropriate scale, which requires them to be as precise as possible.[28] This was not the case with topographic maps in Goa. In order to get around this problem, the chief geologist of the Brigade, Soares de Carvalho, suggested the use of aerial photography to his superiors in Lisbon and proposed the purchase of two collections of aerial photographs: one to be used in fieldwork and another for cabinet work.[29]

In Portugal, the use of aerial photography in geological surveying was fairly recent: only the Board for Nuclear Power had systematically used it, in the 1950s. Aerial photography is only relevant in the absence of topographic maps or when they are inaccurate. It is a means to avoid lengthy descriptions when geographical references are not reliable. Using aerial photographs instead of topographic maps enables geologists to mark outcrops and fieldwork stations and to represent geological units, just as if using a map. But the greatest advantage of aerial photography in geological surveying concerns cabinet work: it enables the recognition of geological features that cannot be perceived in the field, like faults and tectonic alignments, and thus speeds up the geological reconnaissance of a region, particularly in areas where access is difficult or even impossible.

The second campaign of the geological survey of Goa was conducted using aerial photographs at the approximate scale of 1:15,000. Ávila Martins, the geologist who came from the Board for Nuclear Power, was the only member of the Brigade who was familiar with this new technique, which he introduced to his colleagues, with the exception of Teixeira.[30]

To avoid wasting time, each day geologists prepared aerial photographs before heading to the field. A transparent screen was placed over the photographs and the hydrographical network was copied onto it using blue China ink; routes were marked in red. In the field, geologists identified and marked the boundaries between geological units on the transparent screen, which was fixed with tape on the left side of the photograph, so geologists could mark directly on the photograph field information, when necessary.[31] Geologists marked sampling locations or registered other significant geological information both on the screen and in notebooks. In the afternoon, using the indoors collection of aerial photographs and a stereoscopic view, they checked the geomorphology of the surveyed region and marked geological boundaries and the location of sampling stations with black China ink; they also marked the places that seemed to correspond to the continuation of the already identified geological boundaries. Geological information was then transposed onto a provisional map drawn from a mosaic made of aerial photographs. At the end of the day, geologists prepared the next fieldwork journey, which always began with checking the region again to confirm or correct geological information gathered the day before.[32]

While conducting the survey of Goa, the Brigade's geologists also engaged in the discussion of more general theoretical questions: hypotheses about the origin of geological units were put forward, correlations between geological outcrops in different points of the territory were established, and cross-sections were sketched. There was the general feeling among the Brigade members that important contributions to the study of geological and geomorphological problems could be achieved once the Portuguese territories in India were satisfactorily mapped.[33] Although the comprehensive geological survey of Goa was never completed, nearly two dozen publications addressing various aspects of the geology of the State of Portuguese India were published during the 1960s.[34]

Field assistants were in charge of collecting and processing geological samples. These were provisionally numbered, referenced and located on aerial photographs during fieldwork. Later on, in the cabinet, samples were cleaned, labelled, recorded and packed, and, sooner or later, sent to the Board in Lisbon by boat, since sending them by plane was too expensive. Although carefully packed, every once in a while some samples arrived broken or were even lost. Whenever possible, the Brigade collected three samples of each rock: one for the Brigade's own collection, another to be sent to Lisbon and another to the Mining Directorate of Goa. In the case of more typical rocks, additional samples were also collected and sent to Goa's secondary school.[35]

Besides conducting geological surveys, the Brigade was also entrusted with the study of mineral deposits and pedological processes, the location of useful materials for public works, the detection of radioactive minerals, and hydrogeological surveying with a view to supplying water to the population of Goa.[36]

At the end of each campaign, the members of the Brigade returned to the metropolis where the chief geologist purchased the necessary equipment to carry out the next campaign, outlined working plans and proposed new members for the Brigade; geologists wrote scientific reports and articles.[37] The composition of the Brigade varied slightly in the course of the three campaigns. In the second, Soares de Carvalho gave up the leadership of the Brigade on the grounds that, in early 1961, his contract with the Board would come to an end and he would become professor of geology at the University of Porto.[38] In fact, there was a confrontation between Soares de Carvalho and Teixeira who, despite being scientific adviser to the Brigade, often behaved as its chief. The privileges given to Teixeira by the Board[39]—certainly due to Teixeira's position in the academic and scientific Portuguese *milieu* and his long scientific collaboration with the institution[40]—definitely contributed to the confrontation between the two geologists.

Both the second and third campaigns were headed by Ávila Martins and integrated a new field assistant from the Board for Nuclear Power, Joaquim de Lima, who replaced Henrique Dias. During the third campaign, the Brigade also included a mining engineer, Luís Aires Barros (b. 1932), another geologist, Francisco Gonçalves (1926–97), and another field assistant, António Joaquim Caraça Valente. The new members, as well as the Brigade's leader, were suggested by Teixeira,[41] who kept his position of adviser but actually never returned to India.

The third campaign ended abruptly after the invasion of Goa by Indian troops, thus making the completion of a thorough geological study of the State of Portuguese India impossible.

Making Things Work

The difficulties faced by the Brigade in conducting geological fieldwork in Goa went far beyond the lack of suitable base cartography. The number of outcrops was almost negligible, given the vast areas covered with vegetation and lateritic soils. The only visible and well-defined outcrops were situated along the border with the Indian Union. At one point, the Governor, who was constantly kept abreast of the movements and activities of the Brigade's members, authorized their movements by train to the border to check for outcrops. Escorted by armed Portuguese soldiers, they could do no more than carry out fleeting observations since they were not allowed to step off the train because of the political tensions between Portugal and the Indian Union.[42]

The soil in Goa was rough and therefore there were few passable roads and paths were difficult to walk. Furthermore, the Brigade's members had to be aware of the quirkiness and dangerousness of the Indian flora and fauna, and the merciless climate led to the fast deterioration of groceries, compromising fieldwork. Monsoons determined the location and timing of geological surveys: flat areas with little forestation at the end of the monsoon, and mountains and forests during the dry season.[43]

The good relationships established by the Brigade's members with both representatives of the Portuguese State in Goa and local people played a decisive role in the success of their operation. The captaincy of Goa provided maritime transport whenever needed, for example, when it was necessary to carry out observations on the shoreline, or travel to the island of Angediva to undertake its geological survey. Commodities were received from and sent to the metropolis through the Customs of Mormugão Harbour.[44] The poor state of the roads often damaged the jeeps used by the Brigade, which were repaired thanks to the Finance Warehouse of Goa. The General Director of Finance and Accounting managed the budget allocated to the Brigade and took charge of the related paperwork.[45]

The contracting of locals to assist the Brigade was particularly complex. Most individuals were dismissed during the monsoon, when the Brigade interrupted the surveys, and it was hard to find and enrol them again when fieldwork was resumed. It was necessary to select and contract new individuals time and again, who had to learn to perform their tasks from scratch.[46] The Brigade benefited from contacts with private entities, such as local geologists, engineers and owners of mining concessions. Many of them were acquainted with the geology of Goa and one or two of them even joined the Brigade during fieldwork, offering geological advice and therefore allowing its members to spare some time.[47]

But the work undertaken by the Brigade in Goa also depended on a number of agents and circumstances in the metropolis. Part of the required equipment came from Lisbon, not only technical apparatus like the aerial photographs, but also more ordinary items such as campaign tents and mattresses impossible to find in Goa.[48] It was also in Lisbon that the evaluation of the photographs and movies made during the surveys in Goa was carried out, as well as the editing of the latter in the film laboratories of the Shell Oil Company.[49] As for the rock samples, they were readily studied in the Laboratory of Petrological and Palaeontological Studies of the Board, in Lisbon. Once they had arrived, the samples were renumbered definitively with numbers that differed from the ones originally used during fieldwork.[50] Then, thin sections intended for mineralogical and petrographic studies were made and, eventually, also crystallographic sections. The latter were held by the Laboratory of Physical and Chemical Techniques Applied to Geological Sciences, which was also part of the Board. Once the samples were analysed, the information was forwarded to the members of the Brigade in Goa, helping them to continue geological surveying in a quick and convenient way.[51]

Assertion Manoeuvres by the Portuguese Geological Community

In 1953, the Ministry for Overseas Territories had already organized the Mining and Geological Brigade of the State of India (from now on, the Mining Brigade), headed by a mining engineer from the General Directorate of Mines and Geological Survey and including a German geologist who worked for the Portuguese Oil Company, Gerhard Oertel (b. 1920).[52] The Mining Brigade conducted a geological survey of Goa from December 1953 to September 1955 whose main purpose was to find mineral deposits of economic and strategic interest, especially radioactive minerals, and to make an evaluation of their future profitability. Moreover, the Portuguese government was convinced that iron and manganese stocks were particularly valuable and therefore coveted by India.[53] But the geological survey of Goa conducted by the Mining Brigade also had military purposes: the geological description of the north-east headland of the Mormugão peninsula was made having in mind the installation of coastal batteries.[54] The results achieved by the Mining Brigade were not particularly encouraging concerning the discovery of new profitable mineral deposits and radioactive minerals. Only iron mining was profitable and had good prospects for future exploitation since the existence of high stocks was estimated. As for manganese, it could only become cost-effective if mining companies solved the many problems associated with their own operation.[55]

So why was a new geological expedition to Goa organized in 1960, when less than six years before the Mining Brigade had conducted a geological survey of the territory? The official version was that the Mining Brigade never completed the geological survey of Goa, so the Brigade was created to finish it. But the true story is that the political tension between Portugal and India was at its highest so the new geological expedition was also a reassertion of the presence of Portugal in Goa, Damão and Diu, facing the growing threats of integration by India. When the Board was organizing the Brigade, several official documents were issued that emphasized the need to send its members to Goa as soon as possible due to "the great national interest" underlying the geological expedition.[56] During the twentieth session of the International Geological Congress, held in Mexico City in September 1956, representatives from India had already presented some geological studies concerning Goa, Damão and Diu during the meeting of the Standing Commission of the Geological Map of the World. The members of the Portuguese delegation who were present "immediately argued for the interests of Portugal as a colonial nation" and stated that a geological map of Goa would be presented at the meeting of the Commission to be held in Paris in 1958.[57]

But the geological expedition to Goa in 1960 can also be perceived as a strategy of assertion by the still recent geological community in Portugal. The Board was the main institution responsible for the scientific knowledge of overseas territories and some of the most significant Portuguese geologists at the time worked there. Some of them even held pre-eminent positions in the institution; this was the case of the director of the Board, João Carrington Simões da Costa (1881–1982). He certainly did not like being overridden by the Mining Brigade regarding the first geological survey of Goa. Besides, the Mining Brigade was headed by a mining engineer belonging to the General Directorate of Mines and Geological Survey, an institution that the Portuguese geological community bitterly criticized due to the subordinate role geologists played there. Furthermore, the geologist of the Mining Brigade, Gerhard Oertel, was German, and the national geological community did not look favourably upon foreign geologists who came to work in Portugal.

Final Remarks

The geological expedition to Goa made by the Brigade of Geological Studies of the State of India between February 1960 and December 1961 is a revealing case when dealing with various aspects of scientific expeditions. It allows us to identify and characterize scientific practices; to recognize divisions of scientific labour and unveil related questions of scientific authority; to trace the establishment of scientific and social relations; and to grasp the often less obvious reasons that lay behind their organization. It highlights something that historians of science have known for some time: that it is not possible to disentangle the scientific and the social, the natural and the cultural, human agency and non-human agents, because an intricate network of relations is established between all of them.[58]

The expedition can be perceived as an "experimental space" because a new technique, aerial photography, and associated innovative methodologies were tested in the context of geological practice. Success resulting from their use allowed data and information to continuously circulate between the field, in Goa, and the laboratory, in Lisbon. However, the expedition was equally an "experimental space" from the social point of view: the recently established Portuguese geological community tested not only the power relations between some of its members but its own status in the context of the Estado Novo.

From the beginning, the members of the Brigade realized that the natural features of Goa made carrying out a geological survey by using the traditional methodology almost impossible, so they resorted to a different one involving the use of aerial photography. Having a geologist in the Brigade who had mastered the use of this technique proved providential. The use of aerial photography associated with geological surveying was not a common practice in Portugal at the time and its use had not been anticipated: the presence of Ávila Martins in the Brigade had been mainly due to his knowledge of the genesis and deposits of radioactive minerals, the existence of which the Portuguese government wanted to clarify once and for all.

Geological surveying is a complex and sophisticated process; methods and techniques used for the gathering, recording and interpretation of information involve different degrees of skill and geological knowledge. During fieldwork, specific issues concerning the practices, language, moral and material economy, criteria of scientific authority and social status of the participants all play a significant role.[59] Therefore, fieldwork usually leads to the establishment of a division of labour, and the expedition to Goa was no exception. While geologists were responsible for geological practices that were considered to involve a higher degree of theoretical knowledge and skills, such as drawing geological cross-sections and writing scientific papers, field assistants carried out practices considered conceptually and technically less sophisticated, like rock sampling and processing. Tasks which were only circumstantially associated with scientific work, such as driving vehicles and transporting equipment, were carried out by locally contracted workers.

Thus, the division of labour inside the Brigade reflected the existence of a scientific hierarchy but it also implied a social one; it was the interplay between those two aspects that led to problems around authority. During the second and third campaigns, Ávila Martins replaced Soares de

Carvalho as the head of the Brigade because the latter came into conflict with Teixeira, given the situation of ambiguous authority that arose in the Brigade during the first campaign. Although Soares de Carvalho was the official head of the Brigade, the Board granted Teixeira a number of privileges and responsibilities that led the former to feel challenged and even surpassed in his position. Other circumstances certainly led to the confrontation between the two geologists, such as their own personalities and the fact that Teixeira had a scientific and institutional authority that was not matched by Soares de Carvalho. Ávila Martins was one of Teixeira's *protégées* and, even if Teixeira did not return to Goa, he continued to influence the Brigade's subsequent work, from the choice of its members to the responsibility for the compilation of scientific works.

Teixeira was a full professor at the Faculty of Sciences of the University of Lisbon where he headed a research school in geology. Besides being a professor, he also held significant positions in other state-led institutions like being an adviser and member of the Board for Nuclear Power, head of the Petrologic and Palaeontological Laboratory of the Board for Overseas Research, and a scientific collaborator of the Portuguese Geological Survey. Teixeira's unquestionable scientific and institutional authority must be understood in the context of the Portuguese dictatorship, when some members of the geological community, most belonging to academia, achieved considerable institutional power. Teixeira was not an enthusiastic supporter of the Estado Novo, but he managed to take advantage of some specific circumstances to reinforce his own agenda, such as the regime's enthusiasm for the nuclear programme or for furthering scientific knowledge of overseas territories.[60]

The work of the Brigade was constrained by more than natural conditions and authority issues. The extended web of relationships established in Goa was crucial for the Brigade to achieve its goals and proved to be extremely useful when Indian troops invaded Goa on the morning of the 18 December 1961.[61] That web of relations extended beyond Goa to scientists working in the metropolis in the Laboratory of the Board, the Faculty of Sciences of the University of Lisbon, and even a private company: Shell Oil.

The creation of the Brigade of Geological Studies of the State of India and of the former Mining and Geological Brigade show that the Estado Novo used geological knowledge to assert its status as a colonial power in the international scene. Geological expeditions conducted by both Brigades did not only have explicit scientific goals—the geological survey of the territories of Portuguese India—but also hidden political ones: the search for radioactive mineral deposits and of appropriate locations for the pursuit of military offensives. Besides that, the geological expeditions took place at two moments when the tension between Portugal and the Indian Union reached its highest level: in 1953, the year before the annexation of the enclaves of Dadrá and Nagar-Aveli, and in 1960, more or less one year before the occupation of Goa. Thus the Portuguese dictatorship tried to secure the territories in India at least in part through the instrumentalization of geological knowledge.

The geological expedition to Goa must also be understood as a strategy by the still relatively recently established Portuguese geological community to assert and strengthen its scientific, professional and social status. The historical circumstances, and the scientific objections raised concerning the work of the previous Mining Brigade—many of which were made by geologists who were later involved in the creation of the Brigade—gave Portuguese geologists the chance to use the policy of *effective occupation* pursued by the Estado Novo to their own advantage. They pressured the Portuguese government to allow the organization of a new geological expedition to Goa in 1960, and they were successful. The Portuguese state had little interest in the geological knowledge of its Indian colonies but geological mapping, with all its symbolism in terms of territorial domination, was understood as a guarantee of the integrity of the colonial empire, a major ideological premise of the Portuguese dictatorship but also a matter of its own survival.[62]

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Notes

1. *Carlos Teixeira's Fieldwork Notebook Goa I*, Historical Archive of the National Laboratory of Energy and Geology, Lisbon.
2. Ordinance 17 581, 6 February 1960, 1.

3. The Estado Novo implemented Development Plans in Portugal after the Second World War intended to foster the economic development of the country.
4. The State of Portuguese India or the Portuguese State of India was the designation given to territories situated in India, which were part of the Portuguese colonial empire. In 1960, those territories were Goa, Damão and Diu.
5. Ordinance 17 581, 2.
6. From the start, it was foreseen that the geological expedition to Goa would have three phases; each phase was named a *campaign*.
7. N/a, *Estudos Geológicos Referentes à Índia Portuguesa*, *Volume I* (*1960–1962*) (Lisboa: Ministério do Ultramar/Junta de Investigações do Ultramar, 1963).
8. Ana Simões, Ana Carneiro and Maria Paula Diogo, eds., *Travels of Learning: A Geography of Science in Europe* (Boston: Kluwer, 2003).
9. Simões, Carneiro and Diogo, *Travels of Learning*. For the role of techno-scientific knowledge in the European colonial enterprise, see for instance John McKenzie, ed., *Imperialism and the Natural World* (Manchester: Manchester University Press, 1990); Patrick Petitjean, Catherine Jami and Anne Marie Moulin, eds., *Science and Empires*: *Historical Studies about Scientific Development and European Expansion* (Boston: Kluwer Academic Press, 1992); David Philip Miller and Peter Hanns Reill, eds., *Visions of Empire*: *Voyages*, *Botany and Representations of Nature* (Cambridge and New York: Cambridge University Press, 1996). For the Portuguese case, see Maria Paula Diogo and Isabel Amaral, eds., *A Outra Face do Império*: *Ciência*, *Tecnologia e Medicina* (Lisboa: CIUHCT/Edições Colibri, 2013).
10. Rudwick explores the emergence of geology from other areas of knowledge, for instance in Martin Rudwick, "Minerals, Strata and Fossils", in Nicholas Jardine, James Secord and Emma C. Spary, eds., *Cultures of Natural History* (Cambridge: Cambridge University Press, 1996), 266–86.
11. On geological travel and expeditions, see Simões, Carneiro and Diogo, *Travels of Learning*; and Peter Wyse Jackson, ed., *Four Centuries of Geological Travel*: *The Search for Knowledge on Foot*, *Bicycle*, *Sledge and Camel* (London: Geological Society, 2007). For the Portuguese case, see Maria das Dores Areias, "Expeditions in the African Colonies during the 19th Century: Geological Contributions from Portuguese Travellers", *Comunicações do Instituto Geológico e Mineiro* 88 (2001): 347–54; and Maria das Dores Areias, *Viagens e Expedições Científicas dos Portugueses ao Continente Africano durante o século XIX. Contributos para o Conhecimento da Geologia Africana*, unpublished Master's dissertation (Lisboa: Faculdade de Ciências e Tecnologia da Universidade Nova de Lisboa, 2004).
12. See, for instance, Sally Newcomb, *The World in a Crucible*: *Laboratory Practice and Geological Theory at the Beginning of Geology* (Boulder, CO: Geological Society of America, 2009); and Teresa Salomé Mota, "A

Bursting Landscape in the Middle of Portugal: Theories and Experiments by Georges Zbyszewski", *Centaurus* 53, special issue, edited by Ana Carneiro and Marianne Klemun (2011): 146–63.

13. Naomi Oreskes, "From Scaling to Simulation: Changing Meanings and Ambitions of Models in Geology", in Angela Creager, Elizabeth Lunbeck and Matthew Norton Wise, eds., *Science without Laws*: *Model Systems, Cases, Exemplary Narratives* (Durham and London: Duke University Press, 2007), 93–123.
14. Kohler argues that the field and the laboratory in biology represent different sides of a border through which many elements of scientific practice pass. Robert Kohler, *Landscapes and Labscapes*: *Exploring the Lab–Field Border in Biology* (Chicago: University of Chicago Press, 2002). In geology, the image of a *continuum* seems more elucidative than that of a border since the *migration* of elements of geological practice between the field and the laboratory/cabinet is permanent.
15. For a more detailed history of the Board for Overseas Research, see: Ana Lobato, *Da Commissão de Cartographia* (*1883*) *ao Instituto de Investigação Científica e Tropical* (*1983*). *100 Anos de História* (Lisboa: Instituto de Investigação Científica e Tropical, 1983); Teresa Albino, *Viagens e Missões Científicas nos Trópicos*: *1883–2000* (Lisboa: Instituto de Investigação Científica e Tropical, 2010); and Claúdia Castelo, "Investigação Científica e Política Colonial Portuguesa: Evolução e Articulações, 1936–1974", *História*, *Ciências*, *Saúde—Manguinhos* 2 (2012): 391–408.
16. José Pais, "A Crise do Regime Liberal Republicano: Algumas Hipóteses Explicativas", in *O Estado Novo—das Origens ao Fim da Autarcia* (*1926–1959*), vol. I, org. António Costa Pinto (Lisboa: Editorial Fragmentos, 1987), 129–44.
17. Fernando Rosas, *Portugal entre a Paz e a Guerra*, *1939–1945* (Lisboa: Imprensa Universitária/Editorial Estampa, 1990).
18. For a history of the invasion of Goa by Indian troops in 1960, see Maria Manuel Stocker, *Xeque-Mate a Goa. O Princípio do Fim do Império Português* (Lisboa: Texto Editora, 2011).
19. Information 62/775/60, from João Carrington da Costa (1881–1982), President of the Board for Overseas Research, to the Minister of Overseas Territories, suggesting the members of the Brigade, 30 January 1960, File 775, BEGEI, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon; António Morais Romão Serralheiro oral testimony, 25 January 2010, interview by Cláudia Castelo, researcher responsible for the project "Scientific Heritage: Collections and Memories", hosted by the Institute for Scientific and Tropical Research in Lisbon.
20. *Carlos Teixeira's Fieldwork Notebook Goa I.*
21. *Carlos Teixeira's Fieldwork Notebook Goa I*; Official Letter 18/1960, from Soares de Carvalho to Carrington da Costa, asking that the Governor be

sent the names of the members of the Brigade, 28 March 1960, File 775, BEGEI, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon.

22. *Carlos Teixeira's Fieldwork Notebook Goa I*; Serralheiro oral testimony.
23. *Relatório da campanha de 1960/1961 da Brigada de Estudos Geológicos do Estado da Índia*, File 775, BEGEI, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon.
24. *A Brigada de Estudos Geológicos do Estado da Índia* (*BEGEI*) *e a Geologia do Território*, unpublished written document by António Morais Romão Serralheiro, courtesy of Claúdia Castelo; N/a, *Estudos geológicos Referentes à Índia Portuguesa*, 1; *Relatório*, 10; Information 406/775/60, File 775, BEGEI, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon.
25. *Relatório*, 4.
26. On geological fieldwork in Portugal, see for instance: Teresa Salomé Mota, "Spending Some Time in the Field: Fieldwork in the Portuguese Geological Survey during the Twentieth Century", *Earth Sciences History* 33 (2014): 201–13; Ana Carneiro, Teresa Salomé Mota and Vanda Leitão, *O Chão que Pisamos*: *a Geologia ao Serviço do Estado* (*1848–1974*) (Lisboa: CIUHCT/ Edições Colibri, 2014); Ana Carneiro and Teresa Salomé Mota, "The Geological Survey of Portugal (1857–1948), an Overview", *Earth Sciences History*, special issue, edited by Pietro Corsi, 28 (2007): 85–96.
27. *Carlos Teixeira's Fieldwork Notebook Goa I.*
28. *Relatório*, 11.
29. Official Letter 35/1960 from Soares de Carvalho to Carrington da Costa, 28 March 1960, suggesting the purchase of two collections of aerial photography, File 775, BEGEI, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon. During the First World War, aerial photography made with cameras especially designed for airplanes replaced sketching and drawing by aerial observers. The conflict was responsible for some major improvements in the quality of the images obtained, but, following the end of the war, aerial photography was used for non-military purposes. Aerial photography was shown to have civilian uses and became commercially successful because aerial surveys were faster and less expensive than ground ones. Roy Stanley, *World War II Photo Intelligence* (London: Sidgwick & Jackson, 1981).
30. *A Brigada de Estudos Geológicos do Estado da Índia* (*BEGEI*); Serralheiro oral testimony.
31. Serralheiro oral testimony.
32. *Relatório*, 9; Serralheiro oral testimony.
33. *Carlos Teixeira's Fieldwork Notebook Goa I* and *II*.
34. Some of those works are gathered in N/a, *Estudos geológicos Referentes à Índia Portuguesa*.

35. *Relatório*, 12 and 13; Serralheiro oral testimony; *A Brigada de Estudos Geológicos do Estado da Índia* (*BEGEI*).
36. Information 406/775/60.
37. *Relatório*, 14–18.
38. Letter from Soares de Carvalho to José Palmeirim, assistant secretary of the Board, 22 December 1960, Soares de Carvalho Personal File, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon.
39. Teixeira was considered chief of the Brigade, so he was permitted to fly to Goa first class. Dispatch by the Ministry of Overseas Territories issued on the 8 February 1960. The budgets of the Brigade were deposited in two accounts of the National Overseas Bank: Soares de Carvalho was the holder of one and Teixeira of the other. Carlos Teixeira Personal File, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon.
40. Teixeira was a scientific collaborator of the Board from when he finished his PhD in 1940. Information 196, 30 December 1946, Carlos Teixeira Personal File, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon.
41. Official Letter from Teixeira to Carrington da Costa, suggesting the new members of the Brigade, 17 August 1961; Information 348/775/61, 11 October 1961, File 775, BEGEI, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon.
42. *Relatório*, 2; Soares de Carvalho oral testimony to the author.
43. Serralheiro oral testimony; *A Brigada de Estudos Geológicos do Estado da Índia* (*BEGEI*).
44. *Relatório*, 3.
45. *Relatório*, 2 and 3.
46. *Relatório*, 4 and 5.
47. *Carlos Teixeira*'*s Fieldwork Notebook Goa I* and *II*.
48. *Relatório*, 6; Soares de Carvalho oral testimony to the author.
49. The Shell Oil Company was perhaps the only institution in Portugal that had the knowledge and expertise to deal with geological film shooting at the time. The movies were made on 16 mm colour film, the only kind considered suitable for the purpose. The cine-camera belonged to the Centre for Studies of Pure and Applied Geology from the Faculty of Sciences of the University of Lisbon. *Carlos Teixeira*'*s Fieldwork Notebook Goa I* and *II*.
50. The numbering of samples during fieldwork is important because sometimes it is necessary to check data in fieldwork notebooks.
51. *Relatório*, 17.
52. An agricultural expedition, in 1955, and a geographic expedition, in 1956, had already been sent to Goa, AOS/CO/UL-23C, P1, 12th subdivision, sheets 73–76, Salazar Archive, Torre do Tombo, Lisbon.

53. *Relatórios de 1954 da Brigada Geológico Mineira do Estado da Índia* (*BGMEI*), 8–84, Historical Archive of the National Laboratory of Energy and Geology, Lisbon; *Relatório do Secretário Geral do Movimento de Goa e de pró-libertação da Índia*, *António da Fonseca*, AOS/CO/UL-28B, P4, 7th subdivision, sheets 333–355, Salazar Archive, Torre do Tombo, Lisbon.
54. *Relatórios de 1954*, 166.
55. *Relatórios de 1954*, 8–84.
56. Official Letter 404, 67/60 from Carrington da Costa to the Minister of Education, 22 January 1960; Information 49/775/60 from Carrington da Costa to the Minister of Overseas Territories, 2 February 1960, File 775, BEGEI, Overseas Historical Archive, Institute for Scientific and Tropical Research, Lisbon.
57. António Castello Branco and Fernando Silva, *Relatório dos representantes da Direcção Geral de Minas e Serviços Geológicos à XX^{a} sessão do Congresso Geológico Internacional*, Historical Archive of the National Laboratory of Energy and Geology, Lisbon.
58. See Bruno Latour, *Science in Action* (Cambridge: Harvard University Press, 1987).
59. For questions related to the practice of scientific fieldwork, see, for instance, Henrika Kuklick and Robert Kohler, eds., *Science in the Field. Osíris*, Special Issue 11 (1996); Kohler, *Landscapes and Labscapes.*
60. Teresa Salomé Mota and Ana Carneiro, "A Time for Engineers and a Time for Geologists: Scientific Lives and Different Pathways in the History of Portuguese Geology", *Earth Sciences History*, special issue, edited by David Oldroyd, 32 (2013): 23–38.
61. Most members of the Brigade left Goa in a vessel belonging to a company from the Vaticano, the *Searaven*, which was anchored in Mormugão Harbour shipping haematite. The *Searaven* took the members of the Brigade to Karachi, Pakistan, where they took a plane to Lisbon; Serralheiro oral testimony.
62. During the Estado Novo, a vast and comprehensive process was held with the intention to implement a colonial mentality throughout the Portuguese society. Some historians defend that the maintenance of the Portuguese dictatorship relied in part on the maintenance of the colonial empire. For more information on the question of the Portuguese colonial empire during the Estado Novo, see, for instance, Fernando Rosas, *O Estado Novo nos Anos 30, 1928–1938* (Lisboa: Imprensa Universitária/Editorial Estampa, 1986); Valentim Alexandre, "O Império Colonial", in *Portugal Contemporâneo*, coord. António Costa Pinto (Lisboa: Dom Quixote, 2004), 67–86; Nuno Gonçalo Monteiro and António Costa Pinto, "A Identidade Nacional Portuguesa", in *Portugal Contemporâneo*, coord. António Costa Pinto (Lisboa: Dom Quixote, 2004), 51–65.

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