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Maya M. Shmailov

Intellectual Pursuits of Nicolas Rashevsky

The Queer Duck of Biology

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To those who dare navigating the unknown
and making the world interesting to live in.

Preface

> Science is the creation of scientists, and every scientific advance bears somehow the mark of the man who made it.[1]
>
> Anne Roe, 1961

History of science is told through the endeavors, often heroic, of its primary characters. Historians tend to center on the heroes whose names and scientific accomplishments at times precede the disciplines in which they played a major role. Rarely is the history of a discipline "rewritten" by its practitioners to leave out the hero who indefatigably fought and strived toward its establishment. One such hero is Nicolas Rashevsky and mathematical biology the discipline he institutionalized.

The reasons behind this "rewriting" of history accompanied me on my journey of uncovering the intellectual identity of Nicolas Rashevsky. In what constitutes the first detailed biography of mathematical biologist Nicolas Rashevsky (1899–1972), spanning key aspects of his long scientific career, this book captures Rashevsky's ways of thinking about the place mathematical biology should have in biology and his personal struggle for the acceptance of his views. Through his character and his struggles, I set out to unearth all that was involved in establishing a new way of thinking in biology in the early twentieth century.

Nicolas Rashevsky is one of the unique cases in twentieth-century biology, who crossed over to biology with the aim of discovering and explaining all the properties of the living world in terms of fundamental principles and parameters that govern the life sciences and can lead to "laws of nature." While this book discusses the ways in which he succeeded and the ways in which he failed to reach his goal, it is his motivation, path, and struggles that are of particular interest, as these led to the establishment and institutionalization of a new discipline in biology: mathematical biology. Examining Rashevsky's intellectual life provides an invaluable facet in

[1] Roe, A. "The psychology of the scientist," in Obler, Paul C., and Herman A. Estrin eds. *The new scientist*. 1962, pg. 82–94.

discipline-crossing act that accounts for the source of significant innovation and the structure of modern biology.

Tracking Rashevsky's struggle for the acceptance of his dream by the social and political organizations that constitute science provides new insights into the dynamics of "outsiders" and "boundary crossers" in biology as promoters of innovative thinking. While looking forward to new groundbreaking developments in twenty-first-century biology which are and will continue to be introduced by innovative and unorthodox thinkers, Rashevsky's story allows us to observe and learn about the problem of introducing a novel way of looking at biology. *Errare humanum est*, here is to learning from past mistakes!

Seoul, South Korea
March 2016

Maya M. Shmailov

Nicolas Rashevsky, at the University of Chicago, ©*Special Collections Research Center, University of Chicago Library*, used with permission

Introduction

Over five rainy summer days of August 1961, a group of 102 participants hailing from dozens of countries around the world gathered at Western Carolina College in Cullowhee, North Carolina. The participants were roughly divided into "traditionally-trained biologists," mathematicians, statisticians, engineers, physicists, chemists, and a new variation of scientists—mathematical biologists. Sponsored by the National Institutes of Health and the Air Force Office of Scientific Research and organized by the prominent biostatistician Henry L. Lucas (1916–1977), the meeting was convened to address a central objective: the creation of an institutional and intellectual framework for what had come to be known as "mathematical biology."

At the time, "mathematical biology" was emerging from a period of isolation and incubation into a stage of rapid growth. As such, its history—present and future—needed to be discussed and plotted. In particular, the agenda was rich with important topics, primarily the subject of communication across disciplines, the transgression of disciplinary boundaries set by methodology, practices, perspectives, and attitudes as well as the training of scientists in the new discipline. The conference proceedings were published in a volume entitled "The Cullowhee Conference on Training in Biomathematics."[2]

The Cullowhee conference was one of a series of conferences in the 1960s that dealt with the application of physico-mathematical methods to biology. On some level, this conference marks the official recognition of a new discipline whose moniker, *Mathematical Biology,* was coined by a Ukrainian-born theoretical physicist named Nicolas Rashevsky. In fact, it even constitutes some sort of celebration. While during the conference participants employed the terms Biomathematics and "Mathematical Biology" interchangeably, Rashevsky repeated throughout the discussions that he was prejudiced toward the latter because he believed that the former did not do justice to its practitioners and their episteme. For Rashevsky,

[2]HL Lucas, *The Cullowhee Conference on Training in Biomathematics* (North Carolina State University, Raleigh, 1962).

practicing mathematical biology translated into biology being the subject and mathematics the tool to investigate it. His field of practice was neither Bio-Mathematics nor Theoretical Biology: it was Mathematical Biology. The primary reason for this configuration was that during the mid-1920s, Rashevsky—who was then in his mid-twenties—envisioned a new field of biology similar in structure and aim to *Mathematical Physics*.[3]

Rashevsky was a boundary crosser, both as a scientist and as a person. Having trained as a theoretical physicist, he decided to turn his attention to biological sciences and as such became an "outsider" in biology. When a scholar approaches the study of disciplinary boundary crossers, he or she must address the question as to why and how one decides to transgress his or her comfort zone into an unknown land. Is this a discrete act that recognizes and responds to a need and merely introduces a methodology, a set of concepts, or instruments from one discipline to another in order to tackle a specific problem? Or is it a tendency inherent in an "outsider" that may be observed repeatedly throughout his or her scientific life? In the case of Rashevsky, the latter seems to be the case. Rashevsky's outsiderness expressed itself in a wide range of disciplines, including biology, medicine, sociology, and psychology. "You name it; he had a theory on it," reminisced Alvin Weinberg, one of Rashevsky's first students.[4] Zigzagging from the problem of cell division to the challenges of automobile driving, Rashevsky's 45 years of scientific work provides more than its share of boundary crossing.[5] This fluid movement between disciplines coupled with Rashevsky's attempts to replace the biological problems presented by nature with mathematical investigations of simplified hypothetical cases antagonized biologists to the extent that they ultimately neglected the man who single-mindedly attempted to revolutionize their field.

Rashevsky's career as a mathematical biologist began while he was working as a mathematical physicist at the Westinghouse Research Laboratories in Pittsburgh (1924–1934), flourished at the Division of Biological Science at the University of Chicago (1934–1964), and dissipated with his resignation from the University of Chicago and move to Ann Arbor, Michigan, where he worked at the Mental Research Institute (1964–1970) until his retirement at the age of 70.[6] Rashevsky published more than 500 articles and seven books and was appointed to serve as an ex officio member on the board of University Publications at the University of Chicago. Rashevsky also established a journal for publishing research in the field of Mathematical Biology: the *Bulletin of Mathematical Biology*, used by

[3]N. Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology* (University of Chicago Press, 1938), pg. vii.

[4]Lou Gross and Alvin Weinberg, video recorded interview, May 15, 2004 (hereinafter "GWI, 2004"), courtesy of Lou Gross.

[5]For a comprehensive summary, see N Rashevsky, *Mathematical Biophysics Physico-Mathematical Foundations of Biology, Vol. 1 and 2* (Dover Publications, New York, New York, 1960).

[6]TH Abraham, "Nicolas Rashevsky's Mathematical Biophysics," *Journal of the History of Biology* 37, no. 2 (2004), 333–385.

mathematical biologists to this very day. In 1969, Rashevsky formed a nonprofit organization, *Mathematical Biology, Incorporated*, the precursor of the current *Society for Mathematical Biology,* which provided (and still provides) an institutionalized venue for research in the field.

Rashevsky's crossing over into biology was not motivated by the pursuit of a solution to a specific problem nor was it an attempt to mathematically evaluate a domain in biology. Rather, the tall, blue-eyed Ukrainian had a dream. Rashevsky had his mind set on the "building-up of a systematic mathematical biology similar in its structure and aims to mathematical physics."[7] He was in pursuit of fundamental laws governing life processes and began his quest convinced that only a persistent search for such laws employing physico-mathematical reasoning could eventually unravel the mysteries of life. Applying a physico-mathematical approach, his method was abstraction and approximation of the biological phenomena, which he believed would lead to insights into the processes governing the phenomena. As such, Rashevsky's program reflected two intersecting vocations: first, to establish a novel field of research in biology that would unveil its mystery, and, second, to demonstrate via the results of research in that field that mathematical biology can efficiently approach and engage biological problems in all their complexity.

While this study focuses primarily on Rashevsky's scientific career in the biological sciences, it also sheds some light on his attempts to introduce mathematical thinking into sociology and history. In his late forties, Rashevsky turned to sociological and historical pursuits, investing in the study of social behavior and the mathematics of history. While these studies did not evolve into his primary interest, they did play a role in the way Rashevsky was perceived by his colleagues in the biological sciences and affected his scientific occupations.

Although fellow biologists often viewed Rashevsky as a loser in their arena, he never viewed his scientific achievements as unaccomplished. Even when Rashevsky made a sharp turn from his previous methodology to a new and ambitious pathway in the mid-1950s, he still perceived his research in mathematical biology to be a promising and significant scientific field of research. He was quick to admit his failures, but these did not distract him from his path. He examined the reasons for the possible failure and came up with new, at times grander, solutions, adopting new agenda toward the realization of his dream.

Rashevsky's scientific biography positions him as an important figure in the history of science in general and in the history of the twentieth-century life sciences in particular. His colleagues and students recollected his attempts to connect mathematical reasoning to domains of biology, thereby establishing mathematical biology, yet tagged him as one who failed to successfully market and interest his experimentally oriented colleagues in the life sciences. Historians of science were quick to explain Rashevsky's failure, yet they did not seem to recognize that

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[7]N. Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology* (University of Chicago Press, 1938), pg. vii.

Rashevsky was the first to offer—and the first to attempt to establish—an ambitious program of mathematical biology that would encompass the entire spectrum of the life sciences. Although he is often accused of failing, researchers who adopted his approach placed mathematical biology on the landscape of the biological sciences as a discipline indispensable for answering complex questions on the nature of life.

Wherever the truth may lay, exploring Rashevsky's scientific biography enables us to examine a scientist's ability to transgress from his comfort zone into an unknown domain and construct a new hybrid within, constantly laboring to keep his dream alive. Current scholarship emphasizes the constraints imposed by disciplinary boundaries, characterizing disciplines as relatively closed intellectual structures. Rashevsky's story illuminates the problem of introducing a new view into biology. It shows how a separate institutional and professional niche is carved within an existing intellectual ecosystem via the differentiation of goals, methods, and an evolution of expertise in the newly carved scientific niche. It illustrates the strategies and motives of a particular outsider as well as the motivations and strategies of professional associations that sponsored or critiqued his activities. It tracks the difficulties an outsider encounters in trying to publish research and garner funding for research and teaching and the role played by peer reviewers and journals in changing the disciplinary organization of knowledge.[8] Examination of Rashevsky's intellectual biography helps to understand how an outsider's standpoint was developed and deployed in biology with the "insiders"—the biologists—rarely sharing the outsider's perspectives and methodology.

This inquiry is aimed at more than chronicling Rashevsky's scientific work. Rashevsky's biography is in fact the biography of the development of mathematical biology as a discipline.[9] Thus, this study also aims to sketch the dynamics of how and why a new scientific discipline took root, grew, flourished, and was eventually overtaken in a particular social and academic setting—the University of Chicago. Contributing to the academic study of the institutionalization of knowledge. I aim to answer the question: what are the changes that a field of practice experiences as it metamorphoses from being a disperse, sporadic area of research to a discipline with an intellectual and professional identity able to command its own techniques,

[8]For more information on disciplinarity, the reader is invited to review HH Bauer, "Barriers against Interdisciplinarity: Implications for Studies of Science, Technology, and Society (STS)." *Science, Technology, & Human Values* 15, no. 1 (1990); L Hunt, "The Virtues of Disciplinarity," *Eighteenth Century Studies* (1994); R.C. Post, "Debating Disciplinarity," (2009); D.R. Shumway and E. Messer-Davidow, "Disciplinarity: An Introduction," *Poetics Today* 12, no. 2 (1991); TF Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (University of Chicago Press, 1999); _____, "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists," *American Sociological Review* (1983); Timothy Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines* (Stanford University Press, California, 1997); TS Kuhn, *The Structure of Scientific Revolutions* (University of Chicago Press, Chicago, 1970).

[9]Lawrence Stark to Rashevsky, September 22, 1964, Box 10, Folder "Gordon Research Conference," NRP-SCRC.

methodologies, and intellectual orientations?[10] This study trains a spotlight on the academic, institutional, cultural, and political factors shaping the construction and definition of scientific knowledge and the development of a new discipline in the context of the early departments in which it emerged. In particular, this study illustrates how a new discipline is developed through the actions, struggles, successes, and failures of an outsider entering the inside with an ambitious dream of building a new hybrid from within the "inside."

Approaching Rashevsky as an "outsider" in biology, I ask three interconnected questions. First, what is the place of science in the realization of his dream? In particular, I examine the evolution of his scientific ideas and his approach toward the role physics and mathematics should play in biology. I discuss the dynamics of his research program and the attitude of the "insiders," the biologist toward his approach. Second, what role did his personality play in promoting his scientific agenda? I focus on his rhetoric in scientific publications, his correspondence with associates and administration, and his public relations skills in his attempts to craft an agenda and promote his dream. Third, what is the impact of the type of institution, be it the University of Chicago where he developed his agenda or government agencies that financially supported his program, on an "outsider's" research program? While my primary focus is on Rashevsky's interactions with the administration at the University of Chicago, his relations with the Rockefeller

[10]On institutionalization of knowledge and for discussions on discipline building and professionalization of science, see D. Riesman and C. Jencks, *The Academic Revolution* (Doubleday, 1969); J. Ben-David, *The Scientist's Role in Society* (Prentice Hall Englewood Cliffs, NJ, 1971); Shumway and Messer-Davidow, "Disciplinarity: An Introduction" H. Zuckerman and R.K. Merton, "Patterns of Evaluation in Science: Institutionalisation, Structure and Functions of the Referee System," *Minerva* 9, no. 1 (1971); RK Merton, "The Institutional Imperatives of Science," *Sociology of Science* (1972); A. Thackray and R.K. Merton, "On Discipline Building: The Paradoxes of George Sarton," *Isis* 63, no. 4 (1972); Rosenberg, "Toward an Ecology of Knowledge: On Discipline, Context and History." A. Oleson Voss, J.(Eds.) "The Organization of Knowledge in Modern America, 1860-1920," *Johns Hopkins University Press, Baltimore* (1979); R.E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (Cambridge University Press, 1982); P Abir-Am, "Beyond Deterministic Sociology and Apologetic History: Reassessing the Impact of Research Policy Upon New Scientific Disciplines (Reply to Fuerst, Bartels, Olby and Yoxen)," *Social Studies of Science* (1984); WO Hagstrom, "The Differentiation of Disciplines," *Interdisciplinary Analysis and Research: Theory and Practice of Problem-focused Research and Development* (1986); Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines*; Harold L. Wilensky, "The Professionalization of Everyone?," *The American Journal of Sociology* 70, no. 2 (1964); G Millerson, *The Qualifying Associations: A Study in Professionalization* (Routledge & Paul, 1964); E. Mendelsohn, *The Emergence of Science as a Profession in Nineteenth-Century Europe* (College Division [Bobbs-Merrill], 1964); Gieryn, "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists"; A Abbott, *The System of Professions: An Essay on the Division of Expert Labor* (University of Chicago Press, 1988); G Griffin, P Medhurst, and T Green, "Strep Comparative Report: The Relationship between the Process of Professionalization in Academe and Interdisciplinarity. A Comparative Study of Eight European Countries," (Hull: STREP Research Integration Project, 2005).

Foundation, the NSF, and the NIH are also examined. [11] I am especially concerned with the manner in which the administration at the University of Chicago perceived Rashevsky's person and scientific agenda, and the role the university's academic agenda played in promoting, or otherwise frustrating, Rashevsky's dream.

In the following pages, I trace chronologically Rashevsky's career in science, focusing mainly on his scientific research in mathematical biology, from 1926 to 1972. Chapter 1 provides a sketch of Rashevsky's life and reviews Rashevsky's scientific background. The chapter also observes the factors influencing crystallization of his interest in biology. It provides an account of Rashevsky's decision in 1926 to cross over from theoretical physics to biology and sets out his interests against the backdrop of the current attempts to mathematize biology, elucidating the contrast between his convictions regarding the applicability of mathematical biology and those of his peers. This chapter examines Rashevsky's initial attempts (1926–1933) to apply mathematical methods to the problems of cell division and nerve excitation while still working at Westinghouse. By examining the first arc of his intellectual trajectory, in particular his theory of nervous excitation and inhibition, this chapter demonstrates how Rashevsky's "outsiderness" unmasked the problem of acceptance of his approach by the insiders.

Chapter 2 examines Rashevsky's move to the University of Chicago and the institutional and academic conditions that facilitated this move. The focus is on factors that enabled Rashevsky to introduce his research agenda into his institution and to translate theoretical ideas into a research program. I explore the rationale behind the reorganization of the division of biological sciences at the University of Chicago, initiated by the university's president Robert Maynard Hutchins and implemented by the dean of the Biological Division, William Taliaferro, in an effort to foster an interdisciplinary approach in biological research. I examine how despite the supportive environment for interdisciplinary research, Rashevsky encountered problems finding a place in the department of physiology, chaired by a devout empiricist Anton Carlson, and was forced to transfer to the department of psychology. By examining the institutional and academic conditions, I try to delineate not only the problem of acceptance of his approach to biology by the insiders (the biologists) but also the challenge of institutional acceptance facing the "outsider."

In the section entitled "*An Experiment in Scientific Procedure: the Cold Spring Harbor Symposia on Quantitative Biology*," I provide an account of Rashevsky's first public encounter (1934) with leading scientists who were investigating the interplay between basic sciences and experimental physiology. In reviewing Rashevsky introducing his debut of methodology to his colleagues, I examine not only his exposition of the physico-mathematical approach but also the reaction to his approach. Rashevsky's presentation and the discussions that followed revealed a

[11] Detailed analysis of Rashevsky's relationship with the Rockefeller Foundation has been provided by historian of science Tara Abraham in "*Nicolas Rashevsky's Mathematical Biophysics*," 2004.

tension between the experientially minded biologists and those who believed in the possibility of mathematization of biology, and it sheds light on the divide between these two groups of scientists.

Chapter 3 explores the development of Rashevsky's scientific agenda highlighting the role of mathematical reasoning in biology and the first steps towards institutionalization of mathematical biology at the University of Chicago in 1935–1947. The first part of this chapter focuses on Rashevsky's studies in cell physiology, the central nervous system, and sociology applying his newly developed method of approximation. The second part of this chapter demonstrates Rashevsky's preoccupation with institutionalization of his program and efforts to garner support for his program. It focuses on his role of "outsiderness" played out in the process of institutionalization of his program. Surrounded by a cadre of young students, with common theoretical and methodological interests, Rashevsky designed a training program in mathematical biology. This chapter examines the factors that led the university's administration to form, in 1938, a separate "department" under the *Section of Mathematical Biophysics*, a precursor to the more solid and independent *Committee on Mathematical Biology,* established in 1948, to accommodate Rashevsky and his group. I further explore how Rashevsky, challenged by finding a suitable venue for disseminating his group's research results, garnered the support of Warren Weaver at the Rockefeller Foundation to establish in 1939 a journal dedicated to mathematical biology entitled *Bulletin of Mathematical Biophysics*.

Chapters 4 and 5 follow Rashevsky into the 1950s and 1960s. Chapter Four explores Rashevsky's scientific agenda between the years 1948–1960. It examines Rashevsky's search for formal principles that would advance development of a theory of complex biological phenomena. I detail the transformative period in his research agenda in 1948–1954. I discuss the two principles Rashevsky formulated and believed to constitute a part of the permanent foundation of mathematical biology: the principle of organic form and that of relational biology. The principle of relational biology was introduced by Rashevsky in 1954 when Rashevsky came to realize that the reductionist treatment of physiology had led him to lose sight of the organisms themselves. Rashevsky was now propagating "throw away the physics and keep the organization." He radically departed from the fold of mechanism and adopted the holistic approach to biology, while still highlighting the role of a mathematical approach to biology.

Despite the transformation of his scientific agenda, Rashevsky and his group were prolific in their intellectual and research output. However, the backbone of Rashevsky's dream—namely, his institution—was in danger. This chapter explores three factors that contributed to the feeling that Rashevsky's project was facing perilous times: (1) a change of administration within the division and the university; (2) the university's poor fiscal situation; and (3) the "Red Scare"—an anticommunist movement which was directed at un-American activities and affected Rashevsky's committee, as several of its members were believed to be pro-communism. The chapter further demonstrates that despite the scientific, institutional, and political hardships, Rashevsky was far from giving up on his dream.

While his program encountered difficulties, Rashevsky still regarded his research approach as promising significant scientific advance and vigorously fought to keep it alive. This chapter ends with the discussion of the role governmental agencies played in resurrecting Rashevsky's program at the University of Chicago, following its fall in the mid 1950s.

Chapter 5 follows Rashevsky into the 1960s, accounting for his untimely resignation in 1964 and his move to the Mental Health Research Institute at the University of Michigan until his retirement in 1969. The chapter primarily centers on the external, institutional, and social settings surrounding Rashevsky's mathematical biology rather than Rashevsky's scientific ideas. Discussion of these settings unfolds a detailed account of developments of "extrascientific" factors that dictated the future of Rashevsky's scientific ideas at the University of Chicago. I draw from correspondence in administrative records, correspondence in Rashevsky's archives, and personal interviews, to provide the reader with a fly on the wall perspective of the debates that emerged during the period leading to Rashevsky's resignation. I discuss the institutional settings and the political climate at the division of biological sciences and examine how personal and institutional elements achieve critical importance. The energetic debates between Rashevsky's proponents and the members of administration and leading figures at the division of biological sciences underline the particular focal features of not only intellectual but also political roots of the debate over the place mathematical biology should have at the University of Chicago. By providing a detailed chronology of events, I try to examine how the administration perceived Rashevsky's enterprise and the place it envisaged for his approach at the division.

In the final chapter of this book, I conclude by examining the implications of these findings. I discuss the theoretical findings of this research and question what lessons they might hold for the development of new scientific disciplines. I discuss how the definition and conception of mathematical biology as a discipline within biology resulted largely from Rashevsky's identity as an "outsider" and his efforts to secure resources to institutionalize his enterprise and legitimize its work.

Contents

List of Abbreviations

AAAS	American Association for the Advancement of Science
BMB	Bulletin of Mathematical Biology
CSHS	Cold Spring Harbor Symposia
NBC	National Broadcasting Company
NCSU	North Carolina State University
NIH	National Institutes of Health
NSF	National Science Foundation

Abbreviations for Archival Sources

NRP-SCRC	Nicolas Rashevsky papers, unsorted collection, Special Collections Research Center, University of Chicago
RLP-SCRC	Ralph Lillie papers, Special Collections Research Center, University of Chicago
HOP-SCRC	Office of the President, Hutchins Administration Records, Special Collections Research Center, University of Chicago
KOP-SCRC	Office of the President, Kimpton Administration Records, Special Collections Research Center, University of Chicago
BOP-SCRC	Office of the President, Beadle Administration Records, Special Collections Research Center, University of Chicago
VPO-SCRC	Office of the Vice-President, Records, Special Collections Research Center, University of Chicago
ARP-TUL	Anatol Rapoport's Papers, University of Toronto Archives Center
MBP-NCSU	Department of Biomathematics Papers, North Carolina State University Library
RAC	Warren Weaver Papers, Rockefeller Foundation Archive Center, Sleepy Hollow

Chapter 1
An Overview: Rashevsky's Mathematical Biology

Francis Crick once commented that "cosmologists are . . . less inhibited than chemists in regard to scientific speculations." When Rashevsky had a chance to discuss this statement with Crick in 1959, he jokingly pointed out to him that "mathematical biologists are much worse than cosmologists!"[1] Independent of whether he was being facetious, he had a point: it was precisely this speculative and abstract nature of mathematical biology that prompted others to habitually accuse Rashevsky and his fellow mathematical biologists of being entirely disconnected from biology.[2] Rashevsky set out to prove them wrong. Driven by his dream of establishing the counterpart to mathematical physics in biology, he was shaking the very core of biology and often found himself under attack by the biologists to whom his ways seemed unrealistic, overly theoretical, oversimplified and even arrogant. His strong personality played a major role in his incursion into biology as he continuously fought to turn his dream into reality. Following his dream through, he advanced his views aggressively, defended them when attacked by his peers and prominent biologists, manipulated and exploited available opportunities to receive funding to sustain his enterprise, and employed a mixed strategy of self-aggrandizing and self-deprecation to champion his cause. At times arrogant and domineering, Rashevsky had a sense of self importance and presumptuousness that led to alienation from his colleagues. Undoubtedly, his personality played a major role in the fortunes of his science.

[1]Correspondence with G. Gamov, June 26, 1959, Nicolas Rashevsky Papers (Hereinafter: NRP), Box 10, Folder G, Special Collections Research Center, University of Chicago, IL, (hereinafter SCRC).

[2]EF Keller, *Making Sense of Life: Explaining Biological Development with Models, Metaphors, and Machines* (Harvard Univ. Pr., 2003); Abraham, "Nicolas Rashevsky's Mathematical Biophysics" 2004.

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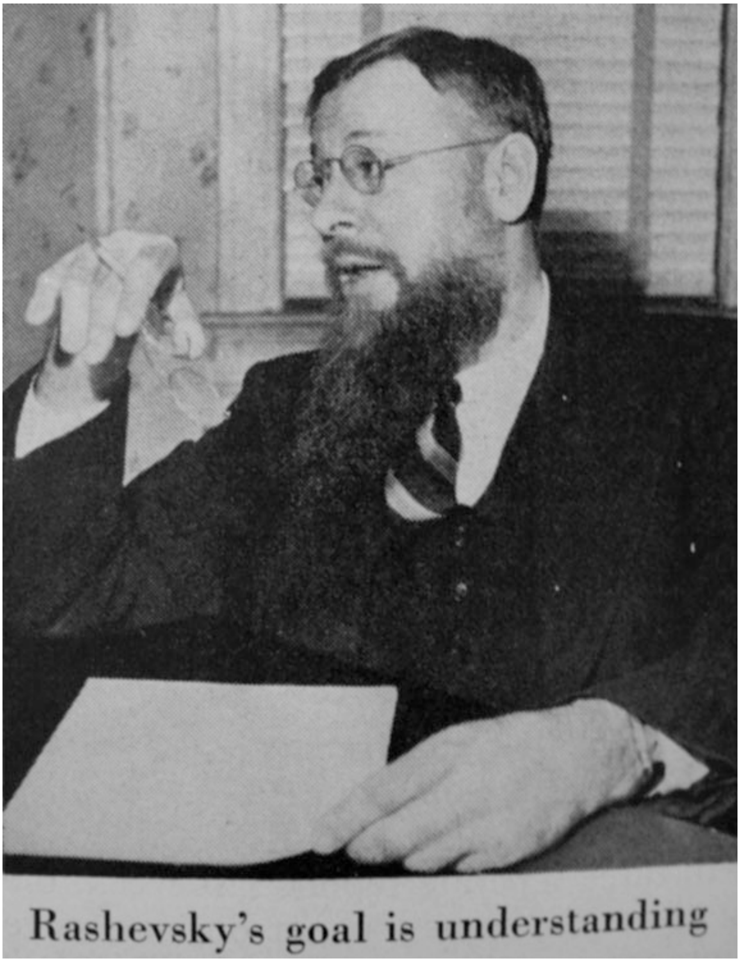
Rashevsky's decision to move from mathematical physics to biology occurred at a time when, as a commentator argued in 1925, "biology [was] fast approaching its scientific stage" if one considers "the amount of mathematical expression of a branch of science as a measure of how scientific that branch is."[3] Traditional methods of observation were being replaced by the experiment as biologists revolted against the theoretical speculative systems, embracing instead the empirical methods of the laboratory.[4] Still, there were exceptions to the trend. While biologists did opine that some biological problems were amenable to mathematical analysis, "fundamental physiological life processes did not…fall within the group".[5] With such assertions surrounding him, equipped with pencil and paper as his instruments, Rashevsky embarked on his life's journey to turn his vision into reality.[6]

[3]O.W. Richards, "The Mathematics of Biology", *The American Mathematical Monthly* 32, no. 1 (1925): 30–36.

[4]E.B. Wilson, "Some Aspects of Progress in Modern Zoology", *Science* 41, no. 1044 (1915), G.E. Allen, *Life Science in the Twentieth Century* (John Wiley & Sons, 1975); J. Maienschein, "Shifting Assumptions in American Biology: Embryology, 1890-1910", *J. Hist. Biol* 14, no. 1 (1981); ———, "Experimental Biology in Transition: Harrison's Embryology, 1895-1910", *Stud. Hist. Biol* 7(1983); R. Rainger, K.R. Benson, and J. Maienschein, *The American Development of Biology* (Rutgers Univ Pr, 1991); R. Creath and J. Maienschein, *Biology and Epistemology* (Cambridge Univ Pr, 2000); D.J. Kevles and G.L. Geison, "The Experimental Life Sciences in the Twentieth Century", *Osiris* 10(1995).

[5]R.G. Harris, "Mathematics in Biology", *The Scientific Monthly* 40(1935):504–510.

[6]"Paper and Pencil Biology", Research Reports, Vol.1, No. 5, 1950, University of Chicago, Nicolas Rashevsky Biographical File, SCRC.



Nicolas Rashevsky circa 1938–40, newspaper article clipping bearing no reference to its origins

In "Legitimation is the Name of the Game", Richard Lewontin elucidates the complexity of this venture: "to understand the problem of establishing a new view... is to understand the problem of introducing that view into a collective consideration and final acceptance by the social and political organization that constitutes science."[7] Lewontin accounts for four interlocking structural elements that enforce the scientific orthodoxy against which one must cope "if there is any hope of incorporating a heterodox view into the corpus of accepted scientific knowledge." These four elements are controlled by peers and comprise public

[7]RC Lewontin, "Epilogue: Legitimation Is the Name of the Game", in Harman and Dietrich eds., *Rebels, Mavericks, and Heretics in Biology* (2008), 372–380.

communication to a relevant scientific community, employment and promotion within the halls of science, professional descendents, and grants.[8] These elements in Rashevsky's career can be summarized thus:

- Rashevsky first published his work in *Protoplasma*, *Physics*, *Journal of General Physiology* and later established a new channel of information called the *Bulletin of Mathematical Biology* (BMB); he presented his views in *Nature* and *Science*; he was invited to various conferences and scientific meetings on the border between mathematics and biology, such as the Cold Spring Harbor Symposia and the Gordon Research Conference. He was invited by universities in the US, Europe, and Russia as a guest lecturer. He was hired as a consultant by the Federal Food and Drug Administration and was a member of AAAS, the Biometric Society, the Biophysical Society, and the International Brain Research Organization at UNESCO.
- Rashevsky's employment and promotion path at the University of Chicago began with the Rockefeller Fellowship (1934–1935), progressed to Assistant Professor (1935–1938), Associate Professor (1938–1946), and finally to Professor (1947–1964).
- Graduate students and postdoctoral fellows applied to work with Rashevsky, knowing that their chosen path was demanding and would require courses in both biological and mathematical sciences. Under Rashevsky's 'sponsorship', more than two dozen students received their Ph.D.s in Mathematical Biology. While some continued their scientific careers beyond the discipline of Mathematical Biology, several of Rashevsky's descendents continued his path at other institutions. For instance, John Hearon headed a research group in mathematical biology at the National Institutes of Health; James Danielly directed a center for theoretical biology at the University of Buffalo; George Karreman carved out a niche at the University of Pennsylvania and also founded the Society of Mathematical Biology (1972).
- Funding for Rashevsky's endeavors was granted by various privately held foundations and governmental agencies, such as the Rockefeller Foundation, the Lucius N. Littauer Foundation, the Morris Foundation, General Motors, the U.S. Air Force, in addition to grants from the United States Public Health Service, the National Institutes of Health, and the National Science Foundation.

This study helps to understand the struggle of the "outsider" and his coping with the elements identified by Lewontin as enforcing "scientific orthodoxy".[9] Tracking his struggle for the acceptance of his vision by the social and political organizations that constitute science provides new insight on "outsiders" in biology. Equipped with heterodox views, Rashevsky was able to propagate his influence within the realm of biology on a tortuous path of success and failure, with the academic world providing the institutionalized backdrop for his endeavors. He challenged

[8] Ibid.

[9] Ibid.

prevailing dogmas of biology and transformed a boundary-crossing event into a discipline in its own right. Identifying himself as a biologist rather than a mathematical physicist, Rashevsky did not seek acceptance from the "insiders"; rather, he was engaged with the design of a new kind of biologist. His battles within the halls of science that led to the establishment of the *BMB* and the first degree-granting program in mathematical biology illustrate a unique and valuable facet of the dynamics of "outsiders" transgressing boundaries. Yet Rashevsky's story shows how "outsiderness" can also haunt a transgressor and his scientific achievements. While driven by biological questions, Rashevsky's approach was—and still is—often lambasted, even by his own students, as "[having] nothing to do with the [biological] reality."[10]

Whereas he did establish a new discipline within biology, Rashevsky's legacy was summarized in retrospect by his student Robert Rosen as standing "in stark contrast to the fate of the man himself". Antagonism towards Rashevsky was expressed even by those who had never met him or followed his research. For instance, in 1970, the applied mathematician Richard Bellman asserted that "if Rashevsky knew what he was doing, he would have been a charlatan."[11] Rashevsky was perceived to be a Svengali propagating unorthodox views from within the realm of biology.[12] Perhaps his statuesque height, the notorious long beard, confidence, and strong voice lent credence to that sort of judgment.

Rashevsky, proclaimed by the participants of the Cullowhee meeting which took place in 1961 as the "first astronaut" orbiting into scientific space, had to battle not only the external forces exerted by the socio-political norms of science and the academe but also internal ones-the definitions of his scientific agenda. He shifted his attitude several times towards realizing his goal as he progressed through the domains of physiology, neurology, psychology, sociology, and even history.[13] By the mid-1950s Rashevsky would reach the conclusion that his attempts were not only unrealistic but on a certain level constituted an actual failure. He promptly shifted his research agenda from a purely quantitative one towards a more qualitative mathematical approach that would dominate his thinking from the mid-1950s through the 1960s. Yet the shift was not due to the encroacher Rashevsky being beaten down by "insider" experimentalists. Rather, he realized that his method, which he believed could be successful in separate and isolated areas in the domain of the life sciences, did not account for the organic world as a united entity—an entity that embodied all domains of the life sciences, including sociology. Rashevsky's constant search for universal mathematical principles in biology

[10] GWI, 2004.

[11] R. Rosen, *Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life* (Columbia University Press, 1991), pages 112–113.

[12] Personal communications with Mrs. Gwen Rapoport, widow of Anatol Rapoport, Rashevsky's student and a close friend (2010–2011), in author's possession.

[13] Lucas, *The Cullowhee Conference on Training in Biomathematics*, page 351.

akin to those of physics eventually sullied his reputation. He remains a notorious figure in the world of science in general and biology in particular.[14]

A Brief Sketch of Rashevsky's Life

Rashevsky's archival papers do not provide much information about his formative years or his personal life. Keenly protective of his private life, Rashevsky refused to share his biography unless it related to his scientific work. It was his firm conviction that "the only thing worth knowing about a man of science is his scientific work and his scientific publications".[15] While he was convinced that every scientific idea, no matter how small, should be shared by publication so as to allow others to learn from it and contribute to the accumulated knowledge and wisdom of science, he was equally convinced that a scientist's personal life was "entirely irrelevant to the qualifications of a scientist".[16]

I have pieced together the mosaic of his personality, private life and non-scientific biography through bits and pieces culled from miscellaneous correspondence in his archival records, the reminiscing of his students, and discussions with his granddaughter and friends. Since science provided the core of his sense of self, most of this book will deal with Rashevsky's scientific career as well as his administrative and institutional path towards realization of his vision. Nevertheless, the available personal information is shared; in order to understand Rashevsky's science and administrative decisions, it is instructive to know the person behind them. Science involves social collaboration, in particular when it comes to an outsider entering into unknown terrain. In order to understand the dynamics of carving a new niche in a relatively well-established domain, one needs to understand the personalities involved. After all, as C. P. Snow once said, "[scientists]'re all human, even if some of [them] don't look it".[17]

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[14]Keller, 2003, personal communications with late Lee Segel and Alvin Weinberg in 2004. Notes in the author's possession.

[15]Rashevsky to Jack Cattel, editor of American Men of Science, February 25, 1946, Box 8, Folder "Cattel", NRP-SCRC,. Rashevsky repeated statements in this vein repeatedly throughout his scientific life when approached by the editors of scientific directories in their requests to include his name. Rashevsky was so vehemently against publication of his biography that in one instance, he threatened the A.N. Marquis Company, publisher of the "Who's Who in America", that he would bring the matter to litigation should they dare to publish his biography.

[16]Ibid.; Rosen, "Autobiographical Reminiscences of Robert Rosen", pgs. 1–23, courtesy of Tara Abraham.

[17]In "The moral un-neutrality of science"; Speech delivered in 1960 to the American Association for the Advancement of Science; published in C.P. Snow and W. Cooper, *The Physicists* (Macmillan, 1981). pp. 180–188; see also C.P. Snow et al., "The Moral Un-Neutrality of Science", *Science* 133(1961).

Nicolas Rashevsky (*left*), his wife Emily, and Máximo Valentinuzzi, Sr., at the Conference on Biomathematics in Cullowhee, North Carolina, on 14–18 August 1961. © Max E. Valentinuzzi, used with permission

As discussed in the Introduction, Rashevsky's personality played a major role in his journey into the world of biology. His dedication to his dream and conviction that his way is the right way, the confidence with which he engaged the battles on the scientific, academic and political arenas can be best understood by acknowledging his strong personality.

In his role as an outsider propagating his views from within biology, his pretention to explain the complex biological phenomena employing the methodology of abstraction and oversimplification, his personality exhibits characteristics of persistence, aggressiveness, with a tendency to rhetorical manipulation, at times coupled with a tendency towards self-aggrandizing or self-deprecation. As a person and as a leader, Rashevsky was compassionate, a man of great patience, persistence, courage, and sincerity, who appreciated hard work; he was helpful to his students, staff, friends and colleagues and would go to great lengths to assist a colleague or a student in need. On questions other than science, Rashevsky rarely uttered any public comment of great length. Politics did not interest him in the slightest; in his words, the "scientist should keep completely away from any politics".[18] So too was the case for religion.

[18] Rashevsky to Edward Levi, July 3, 1953, Box 1, restricted Folder, NRP-SCRC.

His student, Anatol Rapoport remembered him as a man with a precious sense of humor who would recite "biting" satirical poetry.[19] He was a man of many idiosyncrasies. Perhaps his most "peculiar idiosyncrasy" was the lack of use of a first-person pronoun—I—in all of his scientific writings, books, and speeches, the pronoun used was "we" or very rarely "the author". This point was so important to him that towards the publication of one of his books, when the editors at MIT amended his manuscript by substituting "we" and "our" with "I" and "my", he was willing to absorb the costs of amending the type set in monotype, deducting it from his royalties.[20] Above all, he was proudly stubborn, a man of iron principles and integrity who adhered to his doctrines so strictly that towards the end of his career he was ready to sacrifice his life's work, the Committee on Mathematical Biology, in their name.

Nicolas (Nikolai) was born on September 20, 1899, to Peter and Nadejda Rashevsky in the small Ukrainian town of Chernigov. He was the eldest of three children. His parents owned several sugar refineries in South Russia along with an estate and other lands around Kiev. Born to a *bourgeoisie* family, Nicolas was well-educated and tutored to master Russian, English, Latin, German, and French; he was well versed in Russian literature and thanks to his impeccable memory was able to quote pages of Russian and Greek classics.

Considered a prodigy, Rashevsky was trained as a mathematical physicist and obtained his doctorate by the age of 19 at the University of Kiev where he was engaged as an instructor in physics until August 1919.[21] His early publications contributed to the relativity theory and then embryonic quantum theory.[22] As a young scientist he was categorized as a man of "unusual ability in physics and mathematics, capable of … original work and … a skilled and resourceful experimenter".[23]

When the Red Army forces invaded Ukraine in 1919, Rashevsky joined the Russian Revolution, fighting with the forces of the White army. Following the defeat of the Whites and after marrying in 1920 the physicist Emilie (Emily) Zolotareff, an orphaned princess from Vladekavkaz whom he had met at the

[19] A. Rapoport, *Certainties and Doubts: A Philosophy of Life* (Black Rose Books Ltd, 2000).

[20] Emily Rashevsky to C. Bowen, Director of MIT press, draft of a letter circa 1967. The book in question was N. Rashevsky, *Looking at History through Mathematics* (The MIT Press, 1968).

[21] "Reminiscences of Nicolas Rashevsky", Robert Rosen, n.d.; Nicolas Rashevsky Biographical File, SCRC.

[22] F.M. Snell and R. Rosen, *Progress in Theoretical Biology*, vol. 2 (Academic press, 1972). pp. xi–xiv.

[23] Letter of reference by Paul H. Dike, Box 12, misc. NRP -SCRC Rashevsky worked under Paul H. Dike's supervision at the Robert College in Constantinople. After completing his doctorate in physics at the University of Wisconsin in 1911, Dike taught physics at Cornell College in Iowa, the University of Missouri, and Robert College. Dike returned to the United States circa 1923 to teach at the Universities of North Carolina and Vermont before joining the Leeds and Northrup Research Department in 1925. A fellow of the American Physics Society, Dike was a specialist in pyrometry and "made important contributions to the development of precision resistors and of radiation-type detector for determining temperature". In "Paul H. Dike", *Physics Today* 9, no. 8 (1956).

university, it became difficult for Rashevsky to progress academically in Russia.[24] Rashevsky and Emily were forced to flee for a brief period to Constantinople, where he taught physics and higher mathematics at the Department of Physics at Robert College in Constantinople, Turkey.[25] However, with the Communist victory and the defeat of Turkey in World War I, the situation in Constantinople proved to be no better than that in the Ukraine. The young couple were again on the run.

By that time, Rashevsky's family had settled in Prague, and in 1921 Nicolas and Emily joined them.[26] While in exile, Rashevsky taught physics and supported himself and his wife by working as a research assistant. For 3 years Rashevsky worked at the University of Prague in the Department of Russian Studies and at the Polytechnical Institute of Prague, where he lectured on thermodynamics and the theory of electricity.[27]

His publications reflect his interests in photomagnetism (a subject that formed part of his doctoral thesis), diffraction of X-rays by pseudoamorphous bodies as well as electrodynamics and relativity theory.[28] A major part of his research was published in German in *Zitschrift Fur Physics* and included only one publication in English, in the prestigious *Physical Review* (1921).[29] After his secure world was destroyed by the revolution, Rashevsky spent his time in Czechoslovakia searching for an academic position in the United States, constantly corresponding with colleagues and friends who had enjoyed greater success than he in turning their lives around.[30] However, the flood of European scientists entering the gates of the United States made it difficult to secure a position across the Atlantic. Consider the words of one of Rashevsky's colleagues, physicist Paul H. Dike, who too was searching for a permanent position and shared this in a letter in 1923:

[24]A. Rapoport, *Certainties and Doubts: A Philosophy of Life* (Black Rose Books Ltd, 2000); Personal Communication with Rashevsky's granddaughter, Dr. Vibeke Strand. Emily's father, an officer in the army, was killed during WWI and the only other remaining family member was her brother, George. Her brother was sent with the French troops, and prior to her departure to the US, attempts to locate him and release him from service were unsuccessful. Not dated letter in French from Emily to Army officers in Folder "correspondence", Box 12, NRP-SCRC.

[25]Robert College was founded in 1863 by two Americans, philanthropist Christoper Rhinelander Robert and American school master Cyrus Hamli. Robert was a wealthy American industrialist who succeeded in establishing under the Ottoman Empire a modern university offering an "American-style" education with instruction in English. It was an American-sponsored school with benefactors that included John S. Kennedy, Olivia Stokes, and members of the Dodge and Huntington Families. In the early twentieth century, Robert college had evolved into a leading institution in the Middle East: R.H. Davison, "Westernized Education in Ottoman Turkey", *Middle East Journal* 15, no. 3 (1961).

[26]Ibid.

[27]"Biography of Nicolas Rashevsky", Folder "Miscellaneous", Box 11, NRP-SCRC.

[28]Folder "reprints", Box 11, NRP-SCRC.

[29]Folder "reprints", Box 11, NRP-SCRC; N. Rashevsky, "Light Emission from a Moving Source in Connection with the Relativity Theory", *Physical Review* 18, no. 5 (1921).

[30]Boxes 11 and 12, Folders "Correspondence", NRP-SCRC.

> A large part of my correspondence these days is in connection with positions in America, which do not seem to be very numerous, and most of those that are available are not such as I should choose. Perhaps the most favorable one that I have heard yet is at the University of Arizona, out in the Wild West where the cowboys come from. Others are in small colleges where the climate is worse than that of Prague.[31]

Some of the responses Rashevsky received from the U.S. imparted messages along these lines:

> If [you] have any sort of position in Europe, it is better to stay there rather than come here on an uncertainty . . . the life in America [is] very difficult and very expensive, so that there is a constant race between income and expenditure, with the latter always in the lead.[32]

By early 1924 Rashevsky was in close contact with the Russian Student Fund, Inc., and the Institute of International Education in New York, placing his name on their Bulletins of position-seekers.[33] Finally, by April of that year, Rashevsky was able to place his name on the waiting list for the position of research engineer at the research laboratories of Westinghouse Electric Company in Pittsburgh, Pennsylvania, thanks to the assistance of some of his friends. This spot on the Westinghouse waiting list was sufficient to secure a travel visa to the U.S. Having little funds of his own, Rashevsky's trip was made possible thanks to the financial help of his friends and colleagues including George Huntington, principal at the Robert Academy at the Robert College, Constantinople, Albert Staub, the director of the American headquarters for the Near East Colleges, prominent civil engineer Karl von Terzaghi and physicist Paul H. Dike.

Among Rashevsky's strongest benefactors was Karl von Terzaghi, who was an admirer of Rashevsky's work in theoretical physics and would remain a close family friend until his death. A renowned civil engineer, geologist and controversial figure in its own right, Terzaghi met Rashevsky at Robert College where he held a post after WWI. Following the publication of his *Erdbaumechanik* ("Soil Mechanics") in 1923, which is believed to have revolutionized the field of soil mechanics, Terzaghi was offered a position at the Massachusetts Institute of Technology (MIT) in Boston.[34] Having secured the position at MIT, Terzaghi was eager to help

[31] Paul Dike to Rashevsky, March 7, 1923, Box 11, Folder "correspondence", NRP-SCRC.

[32] Cited in P. Dike to Rashevsky, November 30, 1923, Box 11, Folder "correspondence", NRP-SCRC.

[33] With the rush of Russian émigrés to US in the 1920s, the Russian Student Fund was established to offer assistance in the form of loans and contacts assisting over some 650 persons. For more on the fund and the Russian émigrés, see A.R. Wiren, "The Russian Student Fund 1920-1945", *Russian Review* 5, no. 1 (1945), and T. Schaufuss, "The White Russian Refugees", *Annals of the American Academy of Political and Social Science* 203(1939).

[34] R. Boer, *The Engineer and the Scandal: A Piece of Science History* (Springer Verlag, 2005); R.E. Goodman, *Karl Terzaghi: The Engineer as Artist* (American Society of Civil Engineers, 1999).

Rashevsky by drawing on his connections in the U.S. through the assistant director of the American Headquarters of the Near East Colleges, Lawrence Moore, and the two made arrangements for Rashevsky's trip.[35]

Rashevsky's voyage to the US was not easy. Initial plans to emigrate as a family failed as Rashevsky was unable to secure visas for Emily and the two daughters, 3-year-old Emilie and 1-year-old Nina. Rashevsky boarded the ocean liner *Belgenland* alone on July 1, 1924, hoping to secure travel visas for his family in the US. Upon arrival, he was held for special inquiry and through his connections in the US was eventually released.[36]

In the US, Rashevsky was engaged to lecture at Washington Square College of New York University for the academic year of 1924–1925, while still waiting to hear from Westinghouse Electric.[37] When her husband left Prague, Emily and their two young daughters moved to Paris, France, until arrangements could be made to bring them to the United States a few months later.

Working at the university, Rashevsky lectured on the theory of relativity and published several articles in *Scientific American* on the subject of "the fourth dimension". In the United States, Rashevsky wrote and published in English, a language that was not alien to him.[38] Rashevsky's stay at the University of New York did not last for long, however; by December 1924 he ran into difficulty (the precise nature of which has not been documented) with a tenured faculty member, the physicist H.H. Sheldon.[39] Luckily, the Westinghouse Company finally offered him a position that paid enough for his family to live comfortably and for Rashevsky to pursue his research interests in thermodynamics and quantum physics.[40]

In early 1925 Rashevsky assumed the position of research physicist at the Westinghouse Research Laboratories, and the young family left New York to settle in Wilkinsburg, Pennsylvania. At Westinghouse he first worked as a theoretical physicist and later as an engineer in Biophysics and with the X-Ray Group in the Physics Division.[41]

Rashevsky also lectured on the theory of relativity at the Department of Physics at the University of Pittsburgh and translated scientific papers from German and

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[35]Paul H. Dike to Rashevsky, April 20, 1924, Lawrence Moore to K. Terzhagi, April 24, 1924, Box 11, NRP-SCRC.

[36]Records and ship manifests of Ellis Island Foundation. Inc.; Emily and the girls joined Rashevsky relatives in France and eventually arrived on the Aquitania on October 3, 1924.

[37]"Why we are Trying to Make Gold", *Scientific American* **131**, 389–389 (December 1924).

[38]"Scientific Notes and News", Science 60, no. 1547 (1924).

[39]The cause and nature of the disagreement has not been documented.

[40]"Minutes of the Washington Meeting April 23 and 24, 1926", *Physical Review* 27, no. 6 (1926). Correspondence with Paul Dike, 1925, Box 11, NRP-SCRC.

[41]Correspondence with Jerome Alexander, 1932, Box 11, NRP-SCRC; While no other records corroborate this fact, the title "engineer in Biophysics and X-Ray Group, under the Physics Division" appears under Rashevsky's signature in the correspondence.

Russian into English.[42] Nevertheless, he continued to search for a comfortable position in academia, writing to the Specialists' Educational Bureau in 1926:

> I am looking for a position in [sic] Physics Department . . .in order to devote more time for conducting research in pure Science and teaching.[43]

While working as a research scientist, he kept in close touch with "pure science", attending the meetings of the American Physics Society. His early works in mathematical physics dealt primarily with quantum mechanics and the theory of relativity, and towards 1925 he devoted himself to the problem of the thermionic effect and the thermodynamic properties of colloids and polydispersed systems. His papers were published primarily in *Zitschrift Fur Physics*, *Physical Review*, and *Physics*. As historian Tara Abraham has documented, between 1927 and 1929 Rashevsky published seven academic papers on the dynamics of colloidal particles with one of his first papers addressing the problem of size distribution of particles in a colloidal solution.[44] While for many theoretical physicists during the early twentieth century physical understanding took priority over mathematical understanding Rashevsky viewed many of the problems he dealt with as mathematical exercises. This sort of approach would also accompany his work in the realm of the life sciences.[45]

Crossing Boundaries: When Interest Crystallizes

With the remarkable discoveries affected by quantum mechanics, the mid-1920s were the beginning of an era of enormous intellectual upheaval in the vanguard of physics. "Theoretical physics has reached a terrible state, new methods have to be learned every week almost", reported theoretical physicist Earle Kennard from Gottingen in 1926 to his fellow physicist R.C. Gibbs. Young theoretical physicists were struggling in the shadow of a formidable array of talent.[46] Many theoretical physicists were turning to domains outside physics.

At this stage Rashevsky's research was devoted primarily to problems of industrial physics. As indicated above, Rashevsky published seven papers on the dynamics of colloidal particles, with one of his first addressing the analysis of the

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[42]Exchange in Box 11, NRP –SCRC indicates that Rashevsky made ends meet by translating articles and preparing English abstracts from the Russian Journal *Electrichestwo* for the journal *Electrical World*.

[43]Rashevsky to Robert Grant, President of the Bureau, May 1, 1926, Box 12, Folder "correspondence", NRP-SCRC.

[44]Abraham, "Nicolas Rashevsky's Mathematical Biophysics" (2004); N Rashevsky, "On the Size-Distribution of Colloidal Particles", *Physical Review* 31, no. 1 (1928).

[45]Ibid.

[46]D.J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Harvard University Press, 1995), pg. 201.

particles based on thermodynamic considerations involving volume, pressure, energy, and temperature.[47] Rashevsky was studying the spontaneous splitting of fluid drops into smaller droplets, basing his research on Max Planck's theory of ordinary dilute solutions. Specifically, Rashevsky found that such droplets became unstable past a certain critical size, namely, when surface tension became too weak to offset diffusional and other forces impinging on the droplets, and then spontaneously divided into smaller droplets. These studies in colloid physics/chemistry were directed at understanding the properties of dyes and glues, which formed an important branch of research at Westinghouse.[48]

During this period, Rashevsky's interest in biology began to crystallize.[49] Through his research, it occurred to him that similarities exist between the splitting of the fluid drops and the division of cells.[50] Rashevsky later told his student, Robert Rosen, that he had met a biologist from the University of Pittsburgh at a "social occasion." As Rosen recalled, "[Rashevsky] asked the biologist whether the thermodynamic mechanism on which he was working was the way biological cells divided. He was told that (1) nobody knew how biological cells divided and moreover, (2) nobody *could* know how biological cells divided, because this was biology." [emphasis in original].[51] Finding such a notion outrageous, Rashevsky was motivated to try to account for the process of cell division and set his horizons on developing differential equations for the process and expressing how the particular variables in the system are functionally related to one another and change over time.

The anonymous biologist's account of cell division could be understood by observing that cell biology and cytology in the first three decades of the twentieth century was dominated by a myriad of methodologies and motivations.[52] Cytologists were revolting from purely morphological investigation of cells and adopting experimentation utilizing chemical physics, biochemistry and physical instrumentation. As intimated in physiologist Geoffrey Bourne's (1941) statement in the preface to his Cytology and Cell Physiology:

> The phase of purely morphological investigation of cells is now changing into a period in which the interpretation of structure in terms of chemical composition and function is the aim of many cytologists. This ... means that the morphologist will need to work, not as before in a watertight compartment, nor even in a compartment which is covered with a semi-permeable membrane, but in one which will permit an intimate mixing of his

[47]Abraham, "Nicolas Rashevsky's Mathematical Biophysics"; e.g. Rashevsky, "On the Size-Distribution of Colloidal Particles."

[48]Abraham, "Nicolas Rashevsky's Mathematical Biophysics" (2004).

[49]Snell and Rosen, *Progress in Theoretical Biology*.

[50]N. Rashevsky, "Some Theoretical Aspects of the Biological Applications of Physics of Disperse Systems", *Physics* 1, no. 3 (1931), pg. 144.

[51]Rosen, *Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life*. Pg. 110.

[52]Cited in W. Bechtel, "Integrating Sciences by Creating New Disciplines: The Case of Cell Biology", *Biology and Philosophy* 8, no. 3 (1993).

> knowledge with that of the physicist, the biochemist, and the physical chemist: for so complex are cellular organization and function that the brain of no one man can hope to envisage their manifold complications.[53]

As historians of science studying general physiology at the turn of the twentieth century have observed, general physiologists employed the biochemical and physical methods in their experimental studies of the cell.[54] Physiologists who tackled cell division treated the cell as a physico-chemical system that existed in equilibrium with its chemical environment, and framed their experimental studies in terms of the permeability of the cell membrane and the reactivity of the cell to external stimuli. Those physiologists who focused on cell division expressed the problem in terms of surfaces, interfaces, tensions, osmosis, permeability, colloids, and dynamics. Review of the 1924 edition of *General Cytology* edited by EV Cowdry exposes a diversity of experimental studies of cell division.[55] Such experiments for example, often involved exposing cells in vivo to chemical or osmotic stimuli, using chemical solutions and electrical instrumentation to measure changes in potential across the cell membrane. Motivated to understand how cells divide, Rashevsky drew from his expertise as a mathematical physicist to idealize the cell and re-conceptualize the entities that played a role in cell functions.[56] He was not to view the cell as part of an experimental system, but rather viewed the cell as a physical system that can be explained and understood mathematically.

Rashevsky had his mind set on complementing experimental methods in biology with a methodology that would not necessarily lead to the solution of a specific problem yet would provide variations of possible solutions; at least one would then be proven correct by subsequent experimental works. Thus, while physiologists would say to one another "experiment, experiment, experiment!", Rashevsky was introducing quite the opposite approach: controversial speculative thinking.[57] As the physiologist Andrew Huxley (who would be awarded the Nobel Prize in 1963) indicated in 1950, speaking in retrospect, Rashevsky was "attempting over a wide field, a synthesis for which an adequate experimental basis [did] not exist."[58]

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[53]Cited in Ibid.

[54]For an account of the development of general physiology in the early 20th century, see e.g. P.J. Pauly, "'General Physiology and the Discipline of Physiology, 1890–1955", *Gerald L. Geison (ed.)* (1987).

[55]E.V. Cowdry, *General Cytology* (Univ. Chicago Press, 1924).

[56]Abraham, "Nicolas Rashevsky's Mathematical Biophysics."

[57]D'Arcey. Thompson, "Review: Nicolas Rashevsky, Mathematical Biophysics. Physicomathematical Foundations of Biology, Dr. Rashevsky has a way of his own" *Nature* 142, 1938, 931–932.

[58]Thompson, "Review: Nicolas Rashevsky, Mathematical Biophysics. Physicomathematical Foundations of Biology, Dr. Rashevsky has a way of his own"; A.F. Huxley, "Review: Nicolas Rashevsky, Mathematical Biophysics" *Nature* 165(1950). pg.292.

The historian of biology Garland Allen argues that from 1900 to the mid twentieth century, the mechanistic approach became the foundation of a "new biology".[59] The new biology sought to establish itself on the same solid and rigorous foundation as the physical sciences, including a strong emphasis on experimentation. According to Allen, in the context of the times this campaign was particularly aimed at combating the holistic, non-mechanical approaches into the life sciences (organicism, vitalism). During these decades there was a heated debate about the role of the mechanistic approach and about why it was or was not the best way to try to understand living organisms.[60] These debates according to Allen, influenced every area of biology from the then newly rediscovered Mendelian genetics to established fields such as physiology, cell biology and even evolutionary biology.[61] Biology was in a crisis.

As the German zoologist and proponent of theoretical approach to biology Julius Schaxel commented in 1922, "Modern biology is not in a position to display the results of systematic research in a system of concepts, or to represent the orderly behavior which is common to its objects in a general theory. The place of theoretical science has taken rather a heterogeneous multitude of facts, problems, views and interpretations. . ..such a state of affairs cannot be improved by the piling of new facts and opinions upon the old ones, but only by a fundamental reorganization after a process of careful sifting of those we already possess".[62] Other biologists in the early twentieth century gave parallel reasons for their invocation of the sense of crisis: Charles Minot observed various domains of biology as "sundry disciplines more or less separated from one another," Ludwig von Bertalanffy spoke of "abandonment of any comprehension of biological phenomena".[63]

Schaxel's and other theoretically minded biologists lament notwithstanding, true science was believed by experimental biologists to exist in the knowledge and accumulation of facts. Experimentation was hailed superior, and "the legitimate pride in the experimental approach implied aversion to 'theory'".[64] Yet the empiricists were forgetting that a mere accumulation of facts does not constitute science, just as a heap of bricks does not constitute a house. Schaxel argued that the empiricists, in their wish to be unencumbered by theory, were shifting from one theory to another, without adopting any one in particular and without fully

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[59] Allen, "Mechanism, Vitalism and Organicism in Late Nineteenth and Twentieth-Century Biology: The Importance of Historical Context."

[60] Ibid.

[61] Ibid.

[62] J. Schaxel, "Über Die Natur Der Formvorgänge in Der Tierischen Entwicklung", *Development Genes and Evolution* 50, no. 3 (1922). Cited L. Bertalanffy and J.H. Woodger, *Modern Theories of Development: An Introduction to Theoretical Biology* (Harper Torchbooks, 1962), pg. 2.

[63] Amidon, "Adolf Meyer-Abich, Holism, and the Negotiation of Theoretical Biology"; C.S. Minot, *Modern Problems of Biology* (Blakiston, 1913), Pg 113 Bertalanffy and Woodger, *Modern Theories of Development: An Introduction to Theoretical Biology*.

[64] Bertalanffy and Woodger, *Modern Theories of Development: An Introduction to Theoretical Biology*.

understanding any of them either.[65] A theoretical approach was apt to be labeled "mere philosophy" or–"metaphysics".[66]

At the same time Rashevsky grew interested in mathematisizing the physico-chemical aspects of life, "biology" as the general science of the phenomena of life was at its early genesis in the academic framework.[67] Rashevsky entered the world of biological investigation well aware of the crisis it was in.

The existence of biology in the first two decades of the twentieth century was characterized by the following aspects:

1. In the first decades of the twentieth century, biology was the in main dominated by the "mechanistic" world view and tried to model itself on the pattern established in physics, the mechanists forgetting at times that in physics, the paragon of science, theory and experiment were joined together in 'sacred marriage'. The scientific analysis of the component elements of living systems was how scientific explanation was seen. A mechanistic approach was associated with the methodology of reductionism to the lowest accessible level of organization. The parts were studied in isolation, and the whole was believed to be the sum of its parts.
2. Biology as an experimental science comprised two main branches: Genetics (although then very much removed from its present status) and developmental mechanics—physiology. The Mendelian theory, which was rediscovered in 1900, led the new school of geneticists to develop a strongly pragmatic and mostly experimental approach to biology. Drawing analogies from the physical sciences, geneticists extolled the value of experimentation and quantitative data and introduced them both into the previously descriptive area of biology. During that same period, in physiology an emphasis was placed on experimentation. Physiology was practiced with a distinctly materialistic and reductivist flavor.
3. Zoology and botany were essentially "morphology, systematic, microscopic anatomy and similar descriptive fields".[68]

Biology—hitherto purely descriptive and speculative—was heading towards adopting methods of the exact sciences, recognizing that "for permanent progress not only experiments are required but quantitative experiments".[69] How was biology adopting these methods? What areas of biology were perceived as amena-

[65]Schaxel, "Über Die Natur Der Formvorgänge in Der Tierischen Entwicklung."

[66]Bertalanffy and Woodger, *Modern Theories of Development: An Introduction to Theoretical Biology*.

[67]Ibid. pg. v.

[68]Ibid.; Allen, *Life Science in the Twentieth Century*.

[69]Ibid.

ble to physico-mathematical treatment? Who was introducing these methods and how?

The years circa 1920–1940 are often referred to as the "Golden Age" of mathematical biology.[70] While mathematical thinking had been introduced to biology as far back as eighteenth century, as pointed out by the Italian mathematician Vitto Volterra, prior to the twentieth century "attempts at mathematizing the life sciences were restricted to rare applications mainly following statistical or biometrical methods."[71] The process of mathematization, according to Volterra, takes place when a systematic attempt is made to determine relations or indeed laws expressed in mathematical terms.[72] It was during these two decades that the systematic quest for the relations and laws was commenced. One hypothesis that has been suggested to explain the explosion of attempts to mathematize biology in the first decade of twentieth century is that the mathematization of the non-physical sciences (e.g. biology, social sciences and economics) was guided by the notion of importing the concepts and methods of Newtonian science that met with great success in physics, such as that of material point, force and interaction, action and minimum action, and equilibrium.[73]

Starting in the 1920s, the mathematization of non physical phenomena and the attempts to unveil and express the laws and relations in mathematical terms began to develop massively. Evidence to only some of the developments during this period are population dynamics, mathematical epidemiology, population genetics, mathematization of many aspects of human physiology and pathology, models of economic equilibrium, game theory, not to mention applications in the field of engineering.[74]

Historian of Science Giorgio Israel, who has extensively studied this period and its complexities, argues that the rapid growth of modern mathematical biology can be characterized by two facts: (1) *invasion* of biology by mathematics as a conceptual tool rather than as a technical aid in making sense of data and (2) the fact that it was during the 1920s that "most determined attempts were made to apply deterministic conception, in particular mechanism, to biology."[75] The first decades of the twentieth century indeed witnessed important attempts to apply various mathematical methods and reasoning to areas of biology in a systematic way: in biometrics the school of Karl Pearson stands out; in studies of morphology (biogeometry and biomachanics) D'Arcy Thomson's work is a primary example;

[70] G. Israel, "On the Contribution of Volterra and Lotka to the Development of Modern Biomathematics", *History and Philosophy of the Life Science* 10, no. 1 (1988).

[71] Cited in Ibid.

[72] G. Israel, "A glance at the history of the mathematization of biological phenomena", lecture delivered at Bar Ilan University, Ramat Gan, 20.02.2006.

[73] Ibid.

[74] Ibid. Millán Gasca, "Mathematical Theories Versus Biological Facts: A Debate on Mathematical Population Dynamics in the 1930s".

[75] Israel, "On the Contribution of Volterra and Lotka to the Development of Modern Biomathematics."

in population genetics Ronald A. Fisher, J.B.S. Haldane, and Sewall Wright were introducing mathematical approach, and in population dynamics Alfred Lotka, Vladimir Kostitzin and Vito Volterra were developing their mathematical work on species interaction in populations of organisms, although their works extend further than that.[76] In the following, I limit myself to the brief discussion of the above listed main attempts to introduce mathematical thinking into biology.[77]

Karl Pearson (1857–1936) was trained in mathematics and his application of statistics to human populations was related to his aim of subjecting evolutionary concepts to quantitative analysis. [78] In his biometric research program, developed at the turn of the twentieth century in England, Pearson developed the fundamental methods of statistical analysis of populations. It was a technique used to assess present populations, to determine the rate of change in a species and thus provide an aid to prediction.[79] Pearson founded the journal *Biomerika* in 1901.

Although predominantly descriptive, biogeometry and biomechanics are additional fields which were developed in the first decades of twentieth century.[80] These subjects used physics and mathematical methods to describe the living entities. They comprise the theory of organic form and those aspects of function which are distinctly mechanical. One of the works considered to be classics in biogeometry is that of the Scottish zoologist D'Arcey Thomson published in 1917.[81] In his book *On Growth and Form* Thomson moved away from the contemporary approach of zoology having the tendency to investigate organic forms in terms of comparative anatomy and evolutionary theory.[82] Thompson, a "lonely wolf", recommended an approach based on mathematics; in his opinion every biological problem could be described and understood mathematically.[83] Thompson's "fresh point of view" was

[76]S.E. Kingsland, "Mathematical Figments, Biological Facts: Population Ecology in the Thirties", *Journal of the History of Biology* 19, no. 2 (1986); Millán Gasca, "Mathematical Theories Versus Biological Facts: A Debate on Mathematical Population Dynamics in the 1930s".

[77]For a more comprehensive discussion of the attempts and the complexities of introducing mathematical thinking see, e.g. Israel and Millán Gasca, *The Biology of Numbers: The Correspondence of Vito Volterra on Mathematical Biology*; and further publications mentioned in footnote 42.

[78]SE Kingsland, *Modeling Nature* (University of Chicago Press Chicago, 1995); Abraham, "Nicolas Rashevsky's Mathematical Biophysics."

[79]Kingsland, *Modeling Nature*. Abraham, "Nicolas Rashevsky's Mathematical Biophysics"; K. Pearson, "On the Fundamental Conceptions of Biology", *Biometrika* 1, no. 3 (1902); E.S. Pearson, *Karl Pearson: An Appreciation of Some Aspects of His Life and Work* (CUP Archive, 1938).

[80]A. Rapoport, "Beachheads in Mathematical Biology", n.d. circa 1950, Anatol Rapoport's Papers, University of Toronto Archives Center (ARP-TUL).

[81]D Thompson and JT Bonner, *On Growth and Form* (Cambridge University Press Cambridge, 1942, second edition first published in 1917).

[82]Millán Gasca, "Mathematical Theories Versus Biological Facts: A Debate on Mathematical Population Dynamics in the 1930s" page 352.

[83]Ibid. page 352.

not based on reductionism or any quantitative ideal but was rather a presentation of a qualitative view of mathematical representation of morphogenesis.[84]

Having no experience in experimental work, Thompson was satisfied with a mathematical description or physical analogy.[85] In the introduction to his magnum opus *On Growth and Form*, Thompson cites Sir John Herschel to support his views that “numerical precision is the very soul of science, and its attainment affords the best, perhaps the only criterion of the truth of theories and the correctness of experiments”. He takes up the problem of showing how non-living and living conform to the same mathematico-physical equations, by a kind of analogy. He views his task as one of correlation of biological phenomena with physical ones. This correlation being not a causal one, but rather consisting of identifying one single mathematical form that would equate both types of phenomena as instances of the same law. For Thomson “the problems of forms are in the first instance mathematical problems, their [cell and tissue, shell and bone, leaf and flower] problems of growth are essentially physical problems” and the solution to these problems is taught by physical science –which he considered as the “only teacher and guide”.[86] It was Thomson’s view that what the biologist learns from the physicist is a point of approach, a set of quantitative methods, and certain natural restraints upon the application of physical laws.[87]

For Thompson, form was primarily a mathematical concept, growth a physical one, although the two have common boundaries and grounds. Thus, the form of an object is defined when we know its magnitude, actual and relative, in various directions, while growth adds the ‘dimension’ of time.

The three men prominently associated with erecting theoretical constructs to “classical” population genetics between the years 1918 and 1932 are mathematician Ronald Aylmer Fisher (1890–1962) and physiologist (with some mathematical training) John Burdon Sanderson Haldane (1892–1964) in England and biologist Sewall Wright (1889–1988) in the United States.[88] It is customary to consider their work together.[89] The work of these scientists was stimulated by the controversy over the continuity of evolution and the efficacy of natural selection.[90] Fisher and Haldane, both trained in mathematics and used statistical analysis as well as differential and integral calculus (usual techniques in mathematical physics) in their approaches to the problem. Wright, who identified himself primarily as a

[84]Bonner, in editor’s view in Thompson and Bonner, *On Growth and Form*.

[85]Ibid.

[86]Ibid. pp. 7–8.

[87]Ibid.

[88]Provine, “The Origins of Theoretical Population Genetics”; editor’s introduction, Sarkar, *The Founders of Evolutionary Genetics: A Centenary Reappraisal*.

[89]Sarkar, Ibid.

[90]Abraham, “Nicolas Rashevsky’s Mathematical Biophysics.”

developmental geneticist, having no formal training in mathematics, invented novel, and sometimes "unbelievably cumbersome" techniques as he went along.[91] His published works present "immensely complicated" diagrams of the interactions of genes of coat color in the subjects of his research-the guinea pigs.[92]

Working independently, the three men systematically explored the mathematical consequences of Mendelian inheritance and provided mathematical models for hereditary change in a population of organisms, reconciling Mendelian heredity with natural selection, a contribution that would play an important role in the emergence of the "evolutionary synthesis".[93] Their models analyzed distributions of gene frequencies expected from a large, randomly breeding population, and analyzed changes in these frequencies from generation to generation, the population being exposed to such factors as selection, dominance, linkage, and mutation. It was a "straightforward case" of theoretical reduction, of biometry to Mendelian heredity.[94]

The general methodology employed by Fisher, Haldane, and Wright was to develop hypotheses about the relationship between variables and would introduce simplifications in order to enable mathematical analysis and explore general possibilities. Following this, they would develop "simplified descriptions" which had "testable consequences" in natural populations. [95] They then returned to particular cases, after which the mathematical model could be modified. For historian of science and Sewall Wright's biographer, William Provine, their theories complemented existing field research and stimulated new research, entered a somewhat controversial field and solved several existing problems, and lent a firm theoretical basis to Darwinian natural selection.[96]

The theories were highly influential but they were not without their critics. Among its critics, Ernst Mayr referred to population genetics in the 1960s as "beanbag genetics"- with population geneticists treating evolution as mere changes in gene frequency, as if evolution was nothing more than the "adding and removal of beans from a bag".[97] Richard Lewontin has called the achievements of the population geneticists "minimal deductive programs", viewing these as reductionist, purely analytic, idealized statements about interactions of variables such as population size, mutation etc. in populations represented strictly in terms of gene

[91] S. Sarkar, "The Founders of Evolutionary Genetics: A Centenary Reappraisal", *Boston studies in the philosophy of science* (142(1992); R.C. Lewontin, "Theoretical Population Genetics in the Evolutionary Synthesis", *The Evolutionary synthesis: perspectives on the unification of biology*, no. 787 (1980); Harman, *The Price of Altruism: George Price and the Search for the Origins of Kindness* (2010); pg. 59–85.

[92] Ibid.

[93] Ibid. Millán Gasca, "Mathematical Theories Versus Biological Facts: A Debate on Mathematical Population Dynamics in the 1930s."

[94] Sarkar, "The Founders of Evolutionary Genetics: A Centenary Reappraisal."

[95] Provine, "The Origins of Theoretical Population Genetics."

[96] Ibid.

[97] E. Mayr, "Where Are We?" (1959).

frequencies.[98] On the influence of the theory on biologists, Lewontin notes that much of the theoretical work of the three men was not in fact incorporated into the thinking of biologists at the time their work was developed. Lewontin states that "all biologists are either scornful, hostile, or fearful of mathematical theory" and adds, "sometimes with good reason."[99] While the theoretical works in population genetics could have provided a tremendous amount of understanding to biologists, Lewontin asserts that the insights in these works were "unavailable to most biologists for reasons of literacy".[100]

Another direction in the mathematical theory of populations was observed by the Italian mathematician Vito Volterra (1860–1940), and the mathematician and physical chemist Alfred Lotka (1880–1949), with parallel approaches developed by the Canadian entomologist William Robin Thompson, Russian experimental ecologist Georgii Gause and Vladimir Kostitzin in the late 1920s and 1930s.[101] They were prompted to explore the biological phenomena mathematically, realizing that purely descriptive methods could not easily cope with the complexity observed in nature. According to Sharon Kingsland, those studying interactions of organisms in a population had both practical and theoretical motivations for adopting the mechanistic, mathematical approach in their work. On a practical level, they wanted to understand the fluctuation of populations related to agriculture, fisheries and fur trade. On the theoretical level they were motivated to submit the "struggle for existence" to the methods of physics and to create "biological analogue to mechanics", raising the status of ecology to the level of the physical sciences. As will be discussed primarily with reference to Lotka and Volterra, their motivations went beyond raising the status of ecology to the level of the physical sciences, as these scientists had their mind set on systematic mathematization of biology and development of mathematical biology.

Perhaps the most important chapter in the mathematization of biological sciences was developed by two prominent pioneers: world renowned Italian mathematician Vito Volterra (1860–1940), and the American mathematician and physical chemist Alfred Lotka (1880–1949). The works of these scientists are considered to be the beginning of mathematical ecology, a field highly active today and largely

[98]RC Lewontin, "What Do Population Geneticists Know and How Do They Know It", *Biology and epistemology* (2000).

[99]Lewontin, "Theoretical Population Genetics in the Evolutionary Synthesis", pg. 58.

[100]Ibid.

[101]See e.g. Kingsland, *Modeling Nature*; Israel and Millán Gasca, *The Biology of Numbers: The Correspondence of Vito Volterra on Mathematical Biology*.; Francesco M Scudo, "Vito Volterra and Theoretical Ecology," *Theoretical Population Biology* 2, no. 1 (1971); Francesco M Scudo and James R Ziegler, *The Golden Age of Theoretical Ecology, 1923-1940: A Collection of Works by V. Volterra, VA Kostitzin, AJ Lotka, and AN Kolmogoroff,* (Springer-Verlag, 1978); Francesco M Scudo, "The 'Golden Age' Of Theoretical Ecology; a Conceptual Appraisal," *Revue europé enne des sciences sociales*(1984).; Francesco M Scudo and James R Ziegler, "Vladimir Aleksandrovich Kostitsin and Theoretical Ecology," *Theoretical Population Biology* 10, no. 3 (1976). Kingsland, *Modeling Nature*; Israel and Millán Gasca, *The Biology of Numbers: The Correspondence of Vito Volterra on Mathematical Biology*.

inspired by their works.[102] However, their heritage in the history of mathematical biology, especially that of Vito Volterra, extends much further than in mathematical ecology or population dynamics, and is considered to be the first systematic development of mathematical biology.[103] It can also be noted that some of the physical and mechanical analogies applied in mathematical biology and that have shaped many of its concepts, derive from the works of Volterra and Lotka.

Volterra believed in the usefulness of mathematical applications outside the field of physics and, as far back as 1900, expressed his views on the importance of "transporting the classical methods that have produced such significant results in the mechanical and physical sciences to the new unexplored fields with equal success".[104] Volterra viewed favorably the progress and product attained by the mechanistic mathematization in economics largely due to Léon Walras and Vilfredo Pareto, who followed an explicitly mechanistic approach based on the methods of mathematical analysis.[105] Contrary to this success, Volterra deemed the state of mathematization of biology in the first decade of twentieth century to be lagging behind, as, a few exceptions notwithstanding, it was supported by statistics and probability calculus, branches of mathematics which he deemed as less important and non accurate. Despite his strong belief that the programme of mechanistic mathematization should be pursued, Volterra would engage directly in its practice and contribute to the programme only in the mid 1920s. Volterra's direct involvement in the programme was stimulated by his scientific relations with his future son in law, the zoologist Umberto D'Ancona (1896–1964). In 1925 D'Ancona was puzzled by the results of a statistical survey of fish populations from the Adriatic when he noticed some curious increases in the numbers of predators during the war, when fishing had almost stopped. D'Ancona suggested that the break in fishing activities during the war was the cause of the increase in the number of predators, and he asked Volterra if he could provide mathematical analysis of the data to prove his assumption. Volterra threw himself into the question and came up with a description of the interaction between prey and predators based on mathematical equations, which have since become famous and are known as the 'Lotka–Volterra equations'. Through his work to verify D'Ancona's assumption, Volterra also drew from it and worked out a much more extensive range of models to describe the interaction between any number of animal species in competition among themselves. Volterra was clearly interested not only in providing D'Ancona with the sought after mathematical proof but rather in giving form to the programme he set out in 1900, i.e. to introduce, at least for one branch of biology, a mechanistic

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[102] G Israel, "The Scientific Heritage of Vito Volterra and Alfred J. Lotka in Mathematical Biology," *La matematizzazione della biologia. Storia e problematiche attuali, P. Cerrai, P. Freguglia (eds), Quattro Venti, Urbino*(1999).

[103] Ibid.

[104] G. Israel, "A glance at the history of the mathematization of biological phenomena"; Israel, G. "Vito Volterra, Book on Mathematical Biology (1931)", Chapter 73 in Grattan-Guinness, Ivor, ed. *Landmark writings in Western mathematics 1640-1940*. Elsevier Science, 2005.

[105] Ibid.

approach based on the methods of mathematical analysis. Volterra published his results in several articles and aroused widespread interest not only among his fellow mathematicians but also among biologists. One of the articles publishing his results appeared in *Nature*, which not only disseminated his work to a wide scientific audience but also brought forth friction with Alfred Lotka, who was working on a similar subject published in 1925 in a book titled *Elements of Physical Biology*, work which was not known to Volterra.[106] Despite the overlapping nature of the work of two scientists, Volterra easily showed that his work had a much more ambitious aim than Lotka's and put forward a much more general system of equations.

As indicated above, Volterra brought a mechanistic approach to his work on the predator prey systems. Volterra borrowed his ideas directly from physics. His equations relied on the kinetic gas theory model. Volterra developed his equations based on a physical analogy between the collision of gas molecules in a closed container and the interaction of two species; he referred to his method as the "method of encounters". Volterra used a rigidly reductionistic and mechanistic approach to treat his problem. In designing his mathematical equations, Volterra made several, sometimes unrealistic, simplifying assumptions: that the prey is only destroyed by being eaten, and that the predator only eats one prey species.[107] Volterra constantly sought to demonstrate the empirical validity of his results, as he was convinced that an applied mathematical model had no value unless "a satisfactory empirical proof was available."[108] He conducted a vast number of contacts with biologists in an attempt to verify and justify his mathematical theory in practice.[109]

In 1928, Volterra was invited by the mathematician Emile Borel to Paris, to give a series of lectures in the winter of 1928–1929 at the new Henri Poincaré Institute on the subject of mathematical theory of biological fluctuations. By 1929, Volterra made a decision to compile the text of these lectures and publish a book. Three possible titles were considered by Volterra: 'Principes mathématiques de la lutte pour la vie' ('Mathematical principles of the struggle for life'), 'Théorie mathématique de la lutte pour la vie' ('Mathematical theory of the struggle for life'), and 'Principes mathématiques de biologie' ('Mathematical principles of biology'). After consulting with D'Ancona and following his suggestion, Volterra chose the second option and the book *Leçons sur la théorie mathématique de la lutte pour la vie* was published in 1931. The two other titles and in particular the third option were rejected by D'Ancona as Volterra's studies involved only part of one branch of biology—ecology. However, according to Giorgio Israel, Volterra's

[106]Ibid.

[107]Ibid.

[108]Israel, "On the Contribution of Volterra and Lotka to the Development of Modern Biomathematics". pg 12.

[109]Ibid. Israel and Millán Gasca, *The Biology of Numbers: The Correspondence of Vito Volterra on Mathematical Biology*.

suggestion of the third title is indicative of his ambition towards a general mathematization of biology.[110]

As of 1925, when Volterra began his research on population dynamics, he set out to construct a mechanics of biological associations "parallel to the mechanics of inanimate material bodies".[111] As argued by Giorgio Israel, this becomes apparent in the three-tier structure of Volterra's research programme developed over a period of 15 years. The first phase—*rational mechanics* of biological associations—is similar to the rational mechanics of the material point and systems. The second phase—*analytical mechanics* of these associations—is similar to the Lagrangian and Hamiltonian mechanics of material systems and is based on a variational principle analogous to Hamilton's principle and expressed in a canonical form. The third phase was, according to Volterra, the *applied phase* aimed at the empirical verification of the theory.[112]

Concurrently with Volterra's work in Europe, a biomathematical movement in the United States was inaugurated independently by Alfred Lotka. Lotka's research was published in 1925 in his *magnum opus* titled *Elements of Physical Biology*—subsequently republished in 1956 under the title of *Elements of Mathematical Biology*.[113] The predator-prey example, independently developed by Volterra, was included in this book as one of the many examples used by Lotka to illustrate his methods.

Viewing the natural world as a system, Lotka used the framework of physical chemistry and thermodynamics to treat the kinetics and dynamics of living systems.[114] Lotka had been working on a physical interpretation of biological processes, which he hoped to broaden into an entirely new discipline of physical biology, akin to physical chemistry, and based on the principles of thermodynamics. Lotka's application of physical principles to biological systems, in his own opinion, was distinct from the biophysics of the time, which in his view studied the morphology and physiology of the individual organism. Lotka expressed the relations of organisms in terms of energy and matter, using thermodynamic principles, and aimed to find a law of the evolution for biological systems with a degree of generality like that of the second law of thermodynamics. Lotka described the interactions between predator and prey species as a set of differential equations, based on the method used for the mathematical description of the dynamics of chemical reactions.

A review of the contents of Lotka's book might give a false impression of mechanistic schema, as it has the layout of a handbook of classical mechanics

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[110]Israel, G. "Vito Volterra, Book on Mathematical Biology (1931)", chapter 73 in *Landmark Writings in Western Mathematics 1640–1940* (2005).

[111]G. Israel, "A glance at the history of the mathematization of biological phenomena."

[112]Ibid.

[113]AJ Lotka, *Elements of Physical Biology* (Williams & Wilkins company, 1925); AJ Lotka, *Elements of Mathematical Biology* (Dover Publications, New York, 1956).

[114]Abraham, "Nicolas Rashevsky's Mathematical Biophysics"; Kingsland, *Modeling Nature*.

divided into four sections: "general principles", "kinetics", "statics" and "dynamics". However, this could not be farther from the truth. It has been suggested by Giorgio Israel that the explanation to Lotka's inclusion of such a conventional schema for an unconventional collection of boldly mingled topics is to be found in the epigraph to Chap. 2 citing the biochemist Gustav von Bunge: "Nature must be considered as a whole if she is to be understood in detail".[115] Thus, according to Israel, Lotka distances himself from key principle of reductionism (whole is sum of its parts) and pursues the opposite approach: only by considering nature as a whole can one achieve an understanding of the behavior of the parts. Such an approach clearly differs from Volterra's rigidly reductionistic and mechanistic approach to biology and makes him the true mechanist of the two.[116] According to Kingsland, despite the differences between Lotka and Volterra, and there were many, they had the same general objective, "…to show that theoretical, mathematical approaches had a place in biology…that theory could guide experiment and research, and that it was not worth waiting until all the facts were in before engaging in speculation with the help of mathematical models."

The mathematical theory and equations developed independently by Lotka and Volterra are widely used by mathematical biologists today and "Lotka-Volterra" equations are listed as one of the ten equations that changed biology, alongside with Fisher's equations of natural selection, Haldane's function for genetic mapping, and Hodgkin-Huxley equations for natural membrane potential.[117] The mathematical models in ecology were, however, highly criticized at the time they were introduced, primarily because of the distance between formal representations and biological reality. Among the critics was the entomologist William R. Thompson who in fact started out in the 1920s as one of the pioneers of mathematical modeling in ecology but ended up in the 1930s as one of its most stern critics.[118] As Kingsland points out, Thomson's main objection was to the presumption that real populations behave as an entity governed by mathematical laws, which were developed and applied only to ideal mathematical entities.[119] Thompson argued that the relationships within populations could only be discovered by laborious observation and inductive generalization. The theoretical work both in ecology and in population genetics appeared to Thompson as a signal of an unfortunate move in science away from a direct concern with reality.[120]

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[115]G Israel, "The Scientific Heritage of Vito Volterra and Alfred J. Lotka in Mathematical Biology".

[116]Ibid.

[117]J.R. Jungck, "Ten Equations That Changed Biology: Mathematics in Problem-Solving Biology Curricula", *Bioscene* 23, no. 1 (1997); L Edelstein-Keshet, *Mathematical Models in Biology* (Society for Industrial and Applied Mathematics Philadelphia, PA, USA, 2005).

[118]Kingsland, "Mathematical Figments, Biological Facts: Population Ecology in the Thirties"; ———, *Modeling Nature*.

[119]Ibid.

[120]Ibid.

Rashevsky's approach to biology and the role mathematics should play in it did not significantly differ from those described above. His ambitious dream, conceived in 1926, was to make biology an exact science, dream that as discussed above he shared with others.[121]

His systematic approach was a quest for general mathematical principles applicable to the entire realm of biology. Mathematical biology was "to stand in the same relation to experimental biology as mathematical physics stands to experimental physics."[122] Using primarily principles of physics, the name originally given to the field by Rashevsky was mathematical *biophysics*, a name which his program carried until 1948.[123] As will be further discussed and explained, the name was changed to *"mathematical biology"* in 1948 for two reasons: academic and administrative. Towards the mid 1940s, Rashevsky and the group of students and associates working in his program would use purely mathematical models which had little to do with the principles of physics. The formal mathematical models they developed differed from the physical ones and as pointed out by Rashevsky, these were in the form of "a purely mathematical concept or structure which possessed the properties of a given biological phenomenon".[124] In search of a broader name to his program, the term *mathematical biology* was chosen. The administrative reason behind the name change was to distinguish it from the Institute of Radiobiology and Biophysics which was being established at the University of Chicago, where Rashevsky developed his program.

How did Rashevsky relate to the above discussed systematic attempts to develop mathematical biology? What relation did his work bear to these works? While Rashevsky never discussed in length the relation between his own work and that of others systematically applying mathematical methods to biology, Rashevsky was aware at least of the seminal works of D'Arcy Thompson, Vito Volterra and Alfred Lotka and considered these works to be fundamental. A first discussion of these works by Rashevsky appears in 1938 in the Preface and Explanatory Remark to his *magnum opus Mathematical Biophysics*. There is no discussion of these works or even a reference in his previous publications or correspondence on foundations of mathematical biophysics, which might imply that he wanted to distance and differentiate his program from those of his peers.[125] In the Preface, Rashevsky collectively acknowledges the successful systematic application of mathematics by both Volterra and Lotka "on the interaction of biological species in a population of organisms". He further acknowledged that between his own work and that of Volterra and Lotka "a relation does exist", although he found such a relation to

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[121] Rashevsky, "Foundations of Mathematical Biophysics". Pg 196.

[122] ———, "Organismic Sets: Some Reflections on the Nature of Life and Society". Pg. 2.

[123] ———, "Foundations of Mathematical Biophysics."

[124] ———, "Organismic Sets: Some Reflections on the Nature of Life and Society."

[125] E.g. N. Rashevsky, "Foundations of Mathematical Biophysics," *Philosophy of Science* (1934); N. Rashevsky, "Physico-Mathematical Methods in Biological Sciences," *Biological Reviews* 11, no. 3 (1936).

gain "actual importance only upon further development of mathematical biology".[126] Rashevsky analogized the relation between his work and those of Volterra and Lotka to "somewhat as does the molecular theory in physics to thermodynamics", the latter "deal[ing] with large bulks of matter with relatively gross phenomena". He asserted that it "may be developed without the introduction of any hypothetical elements, solely on the basis of a few accepted postulates, based on direct experimental evidence". Rashevsky asserted that the works of Volterra and Lotka, dealing with the "organic world as a whole", postulate, on the "basis of direct observation, certain relations between organisms, and therefrom develop a mathematical theory of…phenomena involving such relations". These works, however, do not "go into the consideration of the detailed structure of each individual organism or of the relations of the fundamental parts of this organism to the physical inorganic world" as does his own work. His work being analogous to that of a the physicist pushing his "curiosity farther" to gain "a deeper insight into the 'nature of things'" by interpreting "the thermodynamical quantities in terms of atomic concepts, introducing herewith a large element of hypothesis but at the same time enlarging considerably the field of application of his theory". According to Rashevsky, as these two branches of mathematical biology will develop further, they will "go hand in hand, as have the developments of thermodynamics and of atomic physics".[127]

Despite the differences pointed out above, in response to a question posed by Lotka's associate of 20 years, Dr. Mortimer Spiegelman, on the place of Lotka's work in Rashevsky's enterprise, Rashevsky admitted that his general method of approach is in fact identical to that of Lotka. The difference, although not a great one, as Rashevsky stated it, lay in that he and his group "put . . .emphasis on physico chemical theories of individual cells, individual organs or individual organisms" whilst Lotka "studied in his work the organic world as a whole".

In this letter, Rashevsky in fact attributed the trigger to his vision of mathematical biology and inspiration for his work to Lotka's *Elements of Physical Biology* (1925).[128] It is reasonable to assume that the book's fifth chapter, entitled "The Program of Physical Biology", sparked Rashevsky's vision of developing his program in mathematical biophysics. In this chapter, Lotka introduced his program of Physical Biology. Like Rashevsky, he had a dream of discipline building.[129] Physical Biology, as conceived by Lotka, is a "branch of the greater discipline of the General Mechanics of Evolution, the mechanics of systems undergoing irreversible changes in the distribution of matter among the several components of such

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[126] Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*.pg. vii.

[127] Ibid. pages vii–viii.

[128] Correspondence with Mortimer Spiegelman, February 28, 1950, Box 8, Folder S, NRP-SCRC; N. Rashevsky "Birds eye view at Mathematical Biology" in Lucas, *The Cullowhee Conference on Training in Biomathematics*. AJ Lotka, Elements of Physical Biology (Williams & Wilkins company, 1925).

[129] Abraham, "Nicolas Rashevsky's Mathematical Biophysics."

system".[130] Lotka expected his Physical Biology to develop by gathering data through methods of observation in natural conditions, and observation under experimental (laboratory) conditions. Laws would be established by the method of induction, with the assistance of statistical technique. Nevertheless, Lotka argued for first placing an emphasis upon deductive methods of mathematical analysis to be applied either to data furnished by observation, or to "unknown" quantities', ready for numerical substitution whenever concrete data became available.[131]

Whereas according to Rashevsky "Lotka's interest lay . . .in. . . [the] theory of the interaction of different species, he was more interested in the living world as a whole". Rashevsky became interested in developing a "general method of approach" that would cover the entire field of biology while discovering the "physical mechanisms which underline the functioning of the individual organism and its parts" that would be presented using mathematical language and would start with the cell.[132] He viewed his work as differing from that of Lotka and in fact of Volterra in that his work represents "the first attempt at systematic development of mathematical theories of the basic physicochemical phenomena which underlie the working of an organism". [133] Expressing his hopes in the Preface to his *magnum opus,* he stated that "in the future the relations between individual organisms, as postulated in the works of A. Lotka and V. Volterra, will be derived from the fundamental biophysical properties of these organisms" studied in his own works.

For Rashevsky, Lotka presented a program using only principles of thermodynamics, while he did not confine himself to a specific branch of physico-mathematics. More accurately, Rashevsky on the whole was dealing with microscopic phenomena, whereas Lotka was dealing with macroscopic phenomena. As Rashevsky saw it, Lotka was developing a mathematical theory of interactions and relations between organisms while his own program was to take into consideration the detailed structure of "each individual organisms or of the relations of the fundamental parts of this organism to the physical inorganic world."[134]

Rashevsky's Mathematical Biologist

In order to understand Rashevsky's relationship with the experimental community, we must understand his stance regarding mathematical biology and who should be deemed its practitioners. The cornerstone of Rashevsky's approach positioned

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[130] Lotka, *Elements of Physical Biology*.

[131] Ibid.

[132] N. Rashevsky, Mathematical Approach to Fundamental Phenomena of Biology, n.d., Box 9, Folder "Misc", NRP-SCRC.

[133] N. Rashevsky, Mathematical Approach to Fundamental Phenomena of Biology, n.d., Box 9, Folder "Misc", NRP-SCRC.

[134] Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*.pg. viii.

mathematical biology as a counterpart to mathematical physics. Just as mathematical physicists were not concerned with the statistical evaluation of experimental results but rather with *fundamentals* of the physical phenomena, the same guiding principle ought to apply to mathematical biologists. A mathematical biologist, by Rashevsky's own definition, "is *not* interested in finding an empirical equation which best fits a given set of experimental points. He is interested in *deriving from a set of assumptions* . . . an equation which will fit the set of experimental points."[135]

Rashevsky's mathematical biologist was to work from *inside* the world of biologists rather than *outside*; the latter searches for correlation between measured parts whereas the former embeds biological thought within mathematical schemes.[136] The notion of working from inside rather than outside does not refer to him adopting the methods of biologists, nor does it mean that he strived to become a biologist. Throughout his career Rashevsky was striving to promote within the world of biologists "inexhaustible powers of human thought" just as the great theoretical physicists Heisenberg, Einstein, Dirac and Schrodinger did in physics. His vision was to guide the biologists from the purely empirical, fact seeking science, into a definitely rational science, where the use of physics would not be merely empirical but rather would be exercised through "application of rational, mathematical methods".[137] His top-down approach was to transform biology from the descriptive, inductive stage to a stage where experimentalists are governed by "deductively-formulated theory." When Rashevsky introduced his stand to the wider scientific community in 1935, he characterized his methodology as "first studying . . . oversimplified cases, which may even perhaps have no counterpart in reality" and only later examining "realistic" cases.[138] Simplification was used to predict *trends* rather than exact values, modeling this standard practice after what was customary in physics and applied mathematics.[139] Rashevsky believed that that sort of methodology would facilitate seeing through the *complexity* of all biological phenomena and making it an "exact science".[140] Rashevsky did not bother himself with the attitude of the skeptical biologist that would view his approach as "an intellectual curiosity".[141] For Rashevsky viewed and compared his approach to those of great theoretical physicists such as Clark Maxwell and Max

[135]N. Rashevsky, Mathematical Approach to Fundamental Phenomena of Biology, n.d., Box 9, Folder "Misc", NRP-SCRC.

[136]Ibid.

[137]N. Rashevsky, "Physico-Mathematical Methods in Biological Sciences", *Biological Reviews* 11, no. 3 (1936).

[138]N. Rashevsky, "Mathematical Biophysics", *Nature* 135, (1935), 1938.

[139]P. Cull, "The Mathematical Biophysics of Nicolas Rashevsky", *Biosystems* 88, no. 3 (2007), 178–184.

[140]Rashevsky, "Foundations of Mathematical Biophysics", pg. 176–196.

[141]Ibid. pg. 176–196.

von Laue, whose use of rational, mathematical methods resulted in "listening to music over the radio" and "X- ray analysis", respectively.[142] Rashevsky asserted that while many have tried and failed to use mathematical methods in biology, it was not the method to be blamed but rather those who used it holding "only a superficial idea of theoretical science *par excellence*, mathematical physics".[143] He had set his mind to "try the one thing hitherto not tried in biology, namely the building of a 'system of mathematical biology', similar to mathematical physics".[144] In the first decade of his research, Rashevsky was disregarding the fact that the great theoretical physicists he so admired did not work in isolation from experimental physics, but in fact based their theoretical analyses on experimental data. Maxwell's field theory in electro-magnetism for example is based on the work of an experimentalist, Michael Faraday, and might not have been possible without it. Faraday had raised a new hypothesis pertaining to electro-magnetism, experimented heavily and had made it intuitively plausible. Faraday however had not shown that it was the only theory that would take care of his experimental data. Maxwell theoretically analyzed Faraday's hypothesis and inductive experimental findings and accompanied it with a deductive logical analysis which validated Faraday's intuitive concept as the only theory which is adequate for electro-magnetism.

Rashevsky had no intention of performing experiments as basis for his theoretical studies. Rather, he first developed a theory and later sought for experimental confirmation. In the first decade of his path he relied on the experimental data in available publications and later, on the experimental results of his experimentally minded collaborators. While he did not intend to perform experiments to support and examine his hypothesis, recognizing the importance of comprehending the domain that he wished to transform while still working at Westinghouse, Rashevsky commenced by familiarizing himself with the ways of the 'insider'. Arguing that "even for a theoretician, familiarity with laboratory work was essential, in biology as well as in physics," Rashevsky studied biological literature, wetting his hands by doing 'informal' laboratory work while still at Westinghouse with Davenport Hooker, a professor of anatomy, and with the physiologist C.C. Guthrie from Pittsburgh University as well as the biologist Everett Kinsey, with whom he studied techniques of animal operations.[145] According to Robert Rosen, one of Rashevsky's students and collaborators at Chicago, Rashevsky was so driven at this stage to educate himself in experimental biology that he "brought a

[142] Ibid; N. Rashevsky, "Physico-Mathematical Methods in Biological and Social Sciences", *Erkenntnis* 6(1936).

[143] Rashevsky, "Physico-Mathematical Methods in Biological Sciences."

[144] ———, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*.

[145] Abraham, "Nicolas Rashevsky's Mathematical Biophysics", 2004; History of the Committee, (1963), Box 2, NRP-SCRC.

human brain back with him to the Westinghouse labs, to the consternation of the night-watchman".[146]

Correspondence in Rashevsky's archival papers presents evidence that Rashevsky did spend some time working with the bacteriologist Ralph R. Mellon at the Institute of Pathology at Western Pennsylvania Hospital in Pittsburgh.[147] Rashevsky was apparently working with Mellon on an outline of a "Program in Biophysics". The tentative outline presents subjects such as "absorption spectra of invisible bacteria cultures", "investigation of the shape of the invisible microorganisms", "optical characteristics of various constituents of living cell", and "radiation emitted by living tissues"; each was followed by a brief review of the work performed in the field and the apparatus that might be required for further research.[148]

Nevertheless, Rashevsky never conducted experiments to support his research in mathematical biology. He solemnly believed that just as Lord Kelvin and James Clerk Maxwell in physics never experimented with actual physical models but rather investigated the problem mathematically, so too should the work of mathematical biologists be mathematical and *in abstracto*.

1st Arc of Intellectual Trajectory

Cell as a "Sphere"

Trying to make initial inroads towards unveiling the complexity of biological phenomena led Rashevsky to the "fundamental living unit": the cell.[149] Inspired by his research on thermionic devices involving problems of spontaneous splitting of fluid drops, he realized that similarities might exist between the splitting of the drops and the division of cells.[150]

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[146]Robert Rosen, "Reminiscences of Nicolas Rashevsky", n.d.

[147]Rashevsky to RR. Mellon, 10, April, 1928, Box 11, Folder "correspondence", NRP-SCRC; affirmation to the collaboration with RR Mellon is also found in "Photograph onion light; rays stimulate growth," New York Herald Tribune, 28 December 1928, p. 7.

[148]Ibid. It is interesting to note in this program's outline that Emily Rashevsky was also involved in the program, doing some work on the radiation emitted by living tissue. While Emily Rashevsky's name does not appear in any of the scientific papers or exchange in the NRP, this record indicates that at least at the beginning, Rashevsky's interest in biology was shared by his wife; her name was indicated as one of the scientists to execute a section of the outlined program. Her name would appear in his books as a contributor to editing and preparations of miscellaneous appendixes, but no scientific contributions to the new discipline have been recorded.

[149]R. Rosen, Life itself, *1991*; D'Arcey Thompson, "Review: Nicolas Rashevsky, Mathematical Biophysics", 1938.

[150]US Patent US1,840,130 and British patent GB 271885 listing Rashevsky as an Inventor, 1926; R. Rosen and D.P. Agin, eds., *Foundations of Mathematical Biology* (Academic Press New York,1972).

While "numerous suggestions had been made by biologists as to the possible mechanism" governing the division of the cell, none of the theories represented "rigorous mathematical theories" that, according to Rashevsky, would lead to "verifiable quantitative conclusions". While still working at Westinghouse, the methodology Rashevsky introduced was that of "idealization" and "abstraction" studying the problem *in abstracto*. The underlying assumption was that as many as possible "conceivable mechanisms" of cell division should be examined, and that this examination would eventually enable them to "decide which of the conceivable mechanisms is likely to be actually in operation in the living cell".[151]

Faced with the complexity of the actual cell and the fact that "no two cells are exactly the same", Rashevsky abstracted the fundamental unit by approximating its structure to either a sphere or an ellipsoid. The living cell was abstracted to the point where it was analogous to a small sphere suspended in a solution containing "food substances."

This theoretical cell, Rashevsky argued, would hold its spherical shape and would not divide until a disturbance was imposed thereupon, e.g., by interaction between the cell's interior and the surrounding substances. Based on this scheme, Rashevsky developed mathematical equations, what he called "pencil and paper models," stating that these models have a value greater than actual "experimental" models.[152] His explanation for this was yet again analogy to scientists in theoretical physics, where theory triumphs over experiment. Defending his views he often referred to theoretical physicist Max von Laue as never having performed any experiments, doing all his research by "paper and pencil method". Laue discovered on paper the diffraction of X-rays by crystals, a discovery which eventually led to experimental studies which revealed the fine structure of matter. [153]

After developing initial equations, Rashevsky then looked into the commonalities between all cells. One of these was the electrical charges on the cell. For several years Rashevsky borrowed from his field of expertise, theoretical physics, to turn to the problem of the possible effects of electrical forces on the mechanism of cell division. Cells were known to carry an electric charge. Charges of the same sign were known to repel one another. It was only "natural", as Rashevsky reminisced several decades later, to "assume that the electrically charged parts of the cell repel one another, causing the cell to divide". Knowing the magnitude of the electric charges in the cell and their mechanical strength led to calculations of possibility of these forces affecting the division of the cell. The result was "an emphatic *no*".[154]

Cell division studies at the turn of the twentieth century were dominated by experimentation rather than a theoretical approach and involved exposing cells

[151]Rashevsky, "Some Theoretical Aspects of the Biological Applications of Physics of Disperse Systems", pg. 143–153; Rashevsky, "Foundations of Mathematical Biophysics."

[152]Ibid.

[153]Rashevsky, "Foundations of Mathematical Biophysics."

[154]N. Rashevsky, "From Mathematical Biology to Mathematical Sociology", ETC., *A Review of General Semantics* (1951).

in vivo to chemical stimuli, using chemical solutions and electrical instrumentation to measure changes in potential across the cell membrane. To the extent that mathematics entered the picture in these studies, it was mostly to arrange experimental data in quantitative terms. The early work of physiologist Ralph S. Lillie was typical of this approach.[155] Lillie examined the physical and chemical conditions for initiating cell division in unfertilized sea urchin or starfish eggs and the factors governing division in fertilized eggs. As observed by historian of science Tara Abraham, for Lillie, the cell was in equilibrium with its environment, and his studies focused on how cells reacted to a changing environment. Lillie described the experimental results one would expect based on the hypothesis of the cell as a drop of viscous fluid based on the idea that cell division is connected to changes in surface tension. He then performed experiments to test this hypothesis. Lillie's method was to form a hypothesis based on experimental results, and perform a large number of measurements to test the hypothesis. Following the presentation of data and calculations on the data, Lillie would draw conclusions about the nature of cell division and the role of chemical stimuli in the process.[156]

The methodology Rashevsky employed was borrowed from the discipline that he was trained in—mathematical physics. The method was that of idealization and abstraction; just as a physicist speaks of ideal fluids and perfectly rigid bodies, so did Rashevsky speak of idealizing the living unit and treating it as a metabolizing system in the form of a sphere. In search of a property that was common to all cells, he found it in the physico-chemical reactions. Thus the first theory that he tried to explore was "a general physico mathematical theory of metabolizing systems".[157] The mathematical methods employed by Rashevsky in his studies of the cell were primarily the use of linear algebraic equations and ordinary differential equations. However, Rashevsky also used the diffusion equation, a partial differential equation which he was able to solve only in highly oversimplified cases with symmetrical

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[155]Abraham, "Nicolas Rashevsky's Mathematical Biophysics."

[156]Ibid. R.S. Lillie, "The Physiology of Cell-Division.—I. Experiments on the Conditions Determining the Distribution of Chromatic Matter in Mitosis", *American Journal of Physiology—Legacy Content* 15, no. 1 (1905); ———, "The Relation of Ions to Contractile Processes.—I. The Action of Salt Solutions on the Ciliated Epithelium of Mytilus Edulis", *American Journal of Physiology—Legacy Content* 17, no. 1 (1906); ———, "The General Biological Significance of Changes in the Permeability of the Surface Layer or Plasma-Membrane of Living Cells", *Biological Bulletin* (1909); ———, "The Physiology of Cell-Division.—Ii. The Action of Isotonic Solutions of Neutral Salts on Unfertilized Eggs of Asterias and Arbacia", *American Journal of Physiology—Legacy Content* 26, no. 1 (1910); ———, "The Relation of Stimulation and Conduction in Irritable Tissues to Changes in the Permeability of the Limiting Membranes", *American Journal of Physiology—Legacy Content* 28, no. 4 (1911); ———, "Increase of Permeability to Water Following Normal and Artificial Activation in Sea Urchin Eggs", *Amer. J. Physiol* 40 (1916); ———, "The Physiology of Cell Division. Vi. Rhythmical Changes in the Resistance of the Dividing Sea-Urchin Egg to Hypotonic Sea Water and Their Physiological Significance", *Journal of Experimental Zoology* 21, no. 3 (1916).

[157]Rashevsky, "Physico-Mathematical Methods in Biological and Social Sciences", *Erkenntnis* 6 (1936).

boundary conditions. Due to the complexity of the mathematical calculations involved in solving partial differential equations, Rashevsky would later develop an approximation method that would allow him to reduce the partial differential equations to ordinary ones and to express the forces in terms of parameters amenable to measurements. Further discussion of the approximation method is provided under Chap. 4. Rashevsky's mathematics was never sophisticated or groundbreaking. His purpose was to make his equations easy to follow and simple enough to solve and provide an adequate to his theory solution.

On a conceptual level, Rashevsky's treatment of diffusion rates and chemical reactions was not dramatically different from that of Lillie and other cell physiologists. He was well aware of these works and often cited Lillie's work and the works of other physiologists. Methodologically speaking, Rashevsky's treatment was distinct.[158] In contrast to the empirical studies of physiologists, Rashevsky was not relating to specific cases, but rather to idealized mathematical systems. His method was to idealize the cell, stripping it from any properties he believed were not pertinent to the problem at hand. Rashevsky would then develop an equation relating various variables such as osmotic pressure, volume, forces of attraction and repulsion between chemical molecules, and rates of reaction. He would then solve the equations, interpret its solution, and draw conclusions: for example, how one variable will vary with respect to another variable according to the mathematical equations under consideration. If possible, he would then compare the results to available experimental data and conclude on the appropriateness and accuracy of his initial assumptions.

Exciting Nerves

By 1933 it seemed that Rashevsky's approach of employing the methods of "abstraction" was sprinting down the express lane to success when his paper and pencil methods led him to the phenomenological "two factor" theory of nervous excitation and inhibition. Parallel to Rashevsky's interest in physiology of the cell, he became interested in nerve excitation. By the end of the nineteenth century attempts were made by physiologists to mathematically express the effect stimuli had on excitation of nerves. In 1899 the theoretical chemist Walther Nernst, inspired by Jacques Loeb's work, introduced mathematical theories that described the effect the electric current had on electric excitation in living tissue. The connection between the intensity of the electric current, its duration, and the concentration of ions became a focal point of several theories developed on nerve excitation. Towards the end of the 1920s and throughout the 1930s Rashevsky published several papers on the subject.[159] Yet it was in 1933 that Rashevsky

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[158] Abraham, "Nicolas Rashevsky's Mathematical Biophysics", 2004.

[159] Abraham, "Nicolas Rashevsky's Mathematical Biophysics", 2004.

proposed a novel phenomenological theory ("two-factor theory") that he called "an essentially new point of view", in an attempt to embrace the "whole field of [nervous excitation and inhibition] phenomena" unaccounted for by any of the previous attempts. Rashevsky published it in *Protoplasma*, with the physiological community as his primary audience.[160]

Rashevsky considered that an electrical stimulus applied to the axon would have two effects: it would cause a rise in an "excitatory" process and a simultaneous rise in an "inhibitory" process. Each process increases at a rate proportional to the current flowing through the nerve and decays at a rate proportional to its own magnitude. Rashevsky's equations read thus:

$$\frac{de}{dt} = KI - k(e - e_0)$$
$$\frac{di}{dt} = MI - m(i - i_0)$$

where K, k, M and m are constants, I is the current, *e* the excitatory process, and *i* the inhibitory one. Action occurs whenever *e* equals or exceeds *i* in value.

What made this theory particularly compelling was the fact that less than 3 years later, the neurophysiologist and Nobel Laureate Archibald Hill introduced a similar theory at which he arrived, apparently, without being aware of Rashevsky's work.[161] Whereas Hill described the excitation by an equation similar to that of Rashevsky's, he thought of inhibition, which he termed "accommodation," as related not to the stimulating current per se but rather as emerging as a result of the rise in the excitatory process *e*. Again, excitation occurs when *e* exceeds *i* in value. Hill's equations (using Rashevsky's notations, which differ from Hill's, for the sake of comparison) read thus:

$$\frac{de}{dt} = KI - k(e - e_0)$$
$$\frac{di}{dt} = M(e - e_0) - m(i - i_0)$$

The main difference between Rashevsky's and Hill's works lay in methodology; specifically, it was rooted in the fact that Hill performed extensive experimental studies on the subject through which he designed his "model" whereas Rashevsky

[160]N. Rashevsky, "Outline of a Physico-Mathematical Theory of Excitation and Inhibition", *Protoplasma* 20, no. 1 (1933), 42–56.

[161]AV Hill, "Excitation and Accommodation in Nerve", *Proceedings of the Royal Society of London. Series B, Biological Sciences* 119, no. 814 (1936):305–355.

made assumptions, built a model, and verified it with available experimental data.[162] It seemed that the experimental "verification" which Hill had for his theory was not, in fact, any better for his model than for Rashevsky's.[163]

An Outsider's Sad Lot

As was noted later by one of Rashevsky's first students, Alvin Weinberg, Rashevsky's phenomenological theory of nerve excitation was the "most solid predecessor" of the Hodgkin and Huxley model of action potential propagation, published in 1952.[164] Hodgkin and Huxley won the 1963 Nobel Prize for their study. Nonetheless, Rashevsky's theory was now being cast aside by insiders: in the fifth edition of "Recent Advances in Physiology" published in 1937, the two-factor scheme was in fact described as Hill's theory "without any qualifications," as the University of Rochester neuro-physiologist Henry Blair pointed out to Rashevsky later that year.[165] However, an analysis of the two theories had already been suggested by the University of Chicago's prominent neuro-physiologist Ralph Gerard. Gerard was very familiar with Hill's work, having studied with him for several years; therefore he suggested to his student, Franklin Offner, that he undertake the task of examining the two theories. Offner proposed to investigate which model corresponded best to the experiment. He discovered that solutions to the mathematical equations proposed by Rashevsky and Hill lead to exactly the same predictions and fit the experimental data equally.[166]

In lieu of this conclusion, in 1936 Gerard and Offner submitted a short paper to the editor of *Nature*, who rejected it "on account of lack of space."[167] Hill never discussed the matter publicly nor did he respond to "some embarrassing questions" raised by one of his graduate students, Donald Scott.[168] Blair sent a copy of Rashevsky's paper to Scott with the hope of receiving answers as to how Hill arrived at his theory without being aware of Rashevsky's work. Six month prior to

[162] C. Hodson and LY Wei, "Comparative Evaluation of Quantum Theory of Nerve Excitation", *Bulletin of Mathematical Biology* 38, no. 3 (1976):277–293.

[163] F. Offner, "Excitation Theories of Rashevsky and Hill", *The Journal of General Physiology* 21, no. 1 (1937):89–91.

[164] GWI, 2004.

[165] Scattered correspondence with H.A. Blair, 1933–1939, Boxes 6 and 8, NRP-SCRC.

[166] F.F. Offner, "The Excitable Membrane-Biophysical Theory and Experiment", *Bulletin of Mathematical Biology* 35, no. 1 (1973):101–107.

[167] The letter was eventually published in 1937 as an article by Offner, "Excitation Theories of Rashevsky and Hill."

[168] Correspondence with H.A. Blair 1933–1939, Box 8, NRP-SCRC.

submitting his own paper, after all, Hill was corresponding with Rashevsky on Rashevsky's 1933 article on nerve conduction published in *Physics*.[169] When Blair wrote to Rashevsky about the "embarrassing questions" posed by Scott and unanswered by Hill, Rashevsky responded stating that Hill "is [not] a man to be embarrassed by such trifles" and decided to "take the matter philosophically."[170]

What factors could account for Rashevsky's "failure" to receive recognition from the *insiders*? Why was his theory neglected whereas Hill's theory was placed squarely on the map of physiological research? Was it the fact that Hill based his theory on experimentation and Rashevsky based his on "speculations"? Was it because Hill was a Nobel laureate and had the kind of institutional standing that Rashevsky lacked? Was it because Rashevsky proposed his theory as an outsider or an intruder, while still working as a theoretical physicist at Westinghouse?

It is instructive to quote the biologist and founder of *The Quarterly Review of Biology*, Raymond Pearl, in his review of Rashevsky's 1938 magnum opus *Mathematical Biophysics*:

> Somewhat unfortunately the pioneers in [Mathematical Biology] have a hard and discouraging row to hoe. The reaction of the biologists—including both those who are able to understand the mathematical procedures and the much larger number who are not—is apt to be that the initial postulates are always too much simplified to have any *significant* relation to biological reality as they know it (and the mathematicians do not)."[emphasis in original].[171]

Yet our outsider's failure to have an impact cannot be attributed solely to his inability to grasp biological reality. Presenting his views while still at Westinghouse, Rashevsky and his work were losing their power for the experimentalists due to his assertion of "the theorist's independence."[172] As he approached biology from the perspective of a mathematical physicist, Rashevsky was not attempting to *trade* knowledge with physiologists or become one of them; he was using his perspective as a mathematical physicist to *dictate* how biology should work.

From the very beginning Rashevsky presented his work to insiders, admittedly "quite intentionally," as he wrote in a letter to a colleague, making presumptuous statements, which "might irritate <u>some</u> biologists."[173] He continuously advocated the abstract theoretical approach to biology, comparing his work and vision to those of Kepler, Newton, and Einstein in physics.[174] An examination of his published

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[169] N. Rashevsky, "Some Physico Mathematical Aspects of Nerve Conduction", Physics 4, no. 9 (1933):341–349.

[170] Correspondence with H.A. Blair 1933–1939, Box 8, NRP-SCRC.

[171] R. Pearl, "Review: Nicolas Rashevsky, Mathematical Biophysics. Physicomathematical Foundations of Biology", *Bulletin (New Series) of the American Mathematical Society* 45, no. 3 (1939): 223–224.

[172] Huxley, "Review: Nicolas Rashevsky, Mathematical Biophysics".

[173] Rashevsky to Weaver, September 24, 1936, RG 1.1, Series 216D, Box 11, Folder 147, RAC.

[174] e.g. N. Rashevsky, "Foundations of Mathematical Biophysics", *Philosophy of Science* (1934).

works reveals a rhetorical style filled with overstatements and exaggerations. He wrote the following example in 1936:

> ...although we consider the development of mathematical [biology] ... of greatest importance for interpretation of empirical biology, we do not consider this "utilitarian" aim as the principal driving motive for our study. ... mathematical [biology] has a right to [an] existence of its own, and its interest lies not merely in the number of empirical facts which it can explain but in its ....mathematical beauty. As a consolation for the "fact-seekers" we have many times pointed out that usually such pure theoretical studies bear...practical fruits. But this to us is really beside the point.[175] [emphasis added]

Rashevsky did not advocate the use of the theoretical tools to explain empirical facts; rather, he asserted the independence of mathematical biology. With practical biologists seeking to unveil the mysteries of life, Rashevsky was advocating for mathematical beauty and speculations that may or may not lead to practical results. Either due to his lack of command of English or to his intentional attempts to irritate biologists, his tendency to pretentiousness managed to alienate and antagonize a fair share of 'insiders'.[176]

In reviewing Rashevsky's early work, it quickly becomes apparent that he was not seeking acceptance by 'insiders'; rather, he was trying to design a new kind of *biologist*, one that would work from *within* biology with a new mathematical approach. For him, mathematics was not to be made a "mere handmaiden of the experimentalists"; he was constructing a new discipline that would require expertise at the intersection between mathematics, physics, and biology.[177] Rashevsky's "outsiderness" soon unmasked not only the problem of the reception of his science by insiders, but also the challenge of institutional acceptance.

[175]———, "Mathematical Biophysics and Psychology", *Psychometrika* 1, no. 1 (1936).

[176]Weaver to Rashevsky, September 19, 1936, RG 1.1, Series 216D, Box 11, Folder 147, RAC.

[177]N. Rashevsky, "Mathematical Biophysics: Physico-Mathematical Foundations of Biology", *Bull. Amer. Math. Soc. 45,* 2(1939), 223–224.

Chapter 2
Chicago Experiments in Mathematical Biology

With the crash of the stock market in 1929, the Great Depression hit the United States, severely crippling employment in science. Scientists feared for their jobs in industrial laboratories as well as at universities. The Bureau of Standards fired more than 50 % of its personnel, and equal numbers were furloughed by General Electric and AT&T. The Westinghouse Electric Company also laid off its researchers. In 1931 physicist Samuel Goudsmit reported that the spring meeting of the American Physical Society looked "much more like an employment agency than a scientific gathering".[1] Money had run out and a moratorium was imposed on physical research in the United States.[2]

In April 1934 Rashevsky was fired from his position as a research physicist at Westinghouse. Concurrently, Rashevsky's application for applying methods from the physico-mathematical sciences to domains of the natural sciences attracted the interest of Warren Weaver, director of the Natural Sciences Division at the Rockefeller Foundation (1932–1955). Following the collapse of the stock market, retrenchment was the order of the day at Rockefeller Headquarters. In the realm of Natural Science, Weaver was guided by a cluster of convictions. One of these was that the Rockefeller Foundation ought to concentrate its resources not on ordinary disciplines but on selected fields of scientific interest.

The choices were dictated by two criteria: ripeness for significant intellectual development and the likelihood that the field would contribute to the "welfare of mankind". The latter, Weaver believed, "depends . . . on man's understanding of himself and his physical environment. Science has made magnificent progress in the analysis and control of inanimate forces, but science has not made equal

[1]Kevles, *The Physicists: The History of a Scientific Community in Modern America*. Pgs 250–251.
[2]Ibid.

M.M. Shmailov, *Intellectual Pursuits of Nicolas Rashevsky*, Science Networks. Historical Studies 55, DOI 10.1007/978-3-319-39922-5_2

advances in the more delicate, more difficult, and more important problem of the analysis and control of animate forces."[3]

Weaver's agenda was "to bring to reality a change in the . . . biological research that would open up if some of the most imaginative physical scientists turned their attention . . . to the examination of biological problems."[4] For Weaver, the fields of biology that were likely to exploit physics and chemistry were ripe for advance. As he summarized in 1933, ". . .hope for the future of mankind depends in basic on the development in the next fifty years of a new biology and new psychology."[5] Weaver was in search of ideas that would produce "the intellectual ferment characteristic of much of the work in the physical sciences."[6] Rashevsky, it seemed, was just the physicist that he was looking for.

Although Rashevsky was largely isolated from the scientific community while employed at Westinghouse, he attended the meetings of the American Physics Society and published in scientific journals such as *Protoplasma, Psychometrica, and Journal of General Physiology,* as well as the prestigious *Physical Review* and *Physics*. He was not unknown in the scientific arena. The exchange in his archival papers suggests that he was in close contact with the prominent physiologist Ralph Lillie with whom he communicated to gain insight into the world of physiology.[7] Lillie was not Rashevsky's only contact at Chicago. Rashevsky was friends with Otto Struve, a Ukrainian astronomer and director of the University of Chicago's Yerkes observatory. Rashevsky visited Struve and the University of Chicago several times and attended social and scientific gatherings. Rashevsky was not incognito on the Chicago campus and was apparently successful at promoting his point of view on the integration of physico mathematical methods into the biological sciences. Indeed, he soon received a fellowship to develop his views at the University of Chicago.

There was an intersection of forces that led to Rashevsky finding a niche for realizing his aspirations in the Department of Psychology at the University of Chicago. With Weaver in search of a person to develop a new biology and psychology, through the efforts of Louis L. Thurstone, Chairman of Psychology at the University of Chicago and other prominent scientist from Chicago, including physiologist Ralph S. Lillie, the geneticist Sewall Wright, physicist Arthur H. Compton, developmental psychologist W. Harkness, and experimental psychologist Karl S. Lashley, Rashevsky found a home for his endeavors.[8]

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[3]Ibid.

[4]M. Rees, "Warren Weaver, 1894–1978", *Biographical Memoirs of Members of the National Academy of Sciences* 57(1987): 493–529.

[5]Cited in Kevles, *The Physicists: The History of a Scientific Community in Modern America*.pgs. 247–248.

[6]Rees, "Warren Weaver, 1894–1978."

[7]Letter from Rashevsky to Lillie November 9, 1931, Box 2, Folder 9, RLP-SCRC, University of Chicago.

[8]HD Landahl, "A Biographical Sketch of Nicolas Rashevsky", *Bulletin of Mathematical Biophysics* 27(1965).

Bearing in mind that Rashevsky's primary interest during the early 1930s was in cell division and conduction in nerves and that the University of Chicago was a center of neurophysiological research in some respects, Rashevsky's association with Chicago is unsurprising.[9] Thus, on April 5, 1934, Rashevsky was afforded a 1-year fellowship by the Rockefeller Foundation to develop an adventuresome project applying physico-mathematical methods to biological problems at the University of Chicago.[10] The Foundation supported Rashevsky for an additional period of 3 years when it entered into a cooperative fellowship with the University of Chicago. In 1935 the University chose to retain Rashevsky on its staff and appointed him to an assistant professorship' after the fellowship grant was exhausted.

In Search of a "Queer Duck"

The institutional venue for this interdisciplinary project was not coincidental. On November 19, 1929, Robert Maynard Hutchins was inaugurated as the fifth and youngest president of the University of Chicago (1929–1945), later on changing his title to University Chancellor (1945–1951), heading the university's public relations and political affairs rather than its administrative affairs. Hutchins' inauguration coincided with the drastically changed social and economic climate in the United States, assuming the position only 3 weeks before the Great Depression set in on the heels of the stock market crash. During his time as president, Hutchins developed ideas of his own as to what the university ought to be and tried to induce those around him to act in accordance with his convictions.

During the Robert Hutchins presidential era, the University of Chicago was unique in having an administrative mechanism for promoting interdisciplinary studies.[11] Hutchins had promoted cross-disciplinary work from the start of his presidential tenure as a means to counter the increased departmental specializations and increasing division between scientific pursuits and ethical considerations.[12] The center of attention at the University of Chicago during the late 1930s and 1940s was the ongoing efforts of Hutchins to recreate the American university as a moral and

[9]B.E. Blustein, "Percival Bailey and Neurology at the University of Chicago, 1928-1939", *Bulletin of the History of Medicine* 66, no. 1 (1992); Abraham, "Nicolas Rashevsky's Mathematical Biophysics"; Pauly, "General Physiology and the Discipline of Physiology, 1890–1955."

[10]Abraham, "Nicolas Rashevsky's Mathematical Biophysics."

[11]R.B. Emmett, "Specializing in Interdisciplinarity: The Committee on Social Thought as the University of Chicago's Antidote to Compartmentalization in the Social Sciences", *History of Political Economy* 42, no. Supplement 1 (2010): 261–287.

[12]M.A. Dzuback, *Robert M. Hutchins: Portrait of an Educator* (University of Chicago Press, 1991), pg. 211.

cultural bulwark against the gathering storm he believed to be threatening the foundations of western society.[13]

In Hutchins' ruminations on modern society, he often referred to science as a quest for knowledge that had lost its moral foundation. Unsurprisingly, many of the natural and social scientists at the University of Chicago felt threatened by Hutchins' remarks and his program for university reform.[14] The ensuing battle played out on the matter of reorganizing undergraduate education, decisions regarding personnel and human resource policy, and the division of responsibility and power between faculty and administration.[15]

Undergraduate education was concentrated at the College of the University and followed a curriculum influenced by Hutchins' interest in an integrated approach to knowledge. During Hutchins' presidency, the graduate departments were grouped into four divisions, each headed by a dean who reported to the president. Established in 1930, these were: Division in Physical Science, Division in Biological Science (included the medical and the biological sciences), Division in Social Science, and Division in Humanities.

This reorganization resulted in a streamlined chain of command whereby a coven of four of the academic deans headed the divisions, with each presiding over a faculty divided into departments; the dean of students and comptroller presided over the student affairs and university finances, respectively, and all reported to the president. Nonetheless, this grouping—much to Hutchins' chagrin—resulted in fragmentation and departmentalization of learning in the divisions which prescribed the education of the undergraduates and graduate students over the long run.

During the 1930s the first deans of the humanities, biological sciences, and social sciences were respected elder members of the faculty who did not intend to use their new authority to alter the pattern of graduate training to which they had grown accustomed.[16] Thus by 1935 Hutchins had appointed youthful deans to head three out of the four divisions, individuals who were keen on reforming the departments entrusted to their jurisdiction. One was William H. Taliaferro (1895–1973) who presided over the Division of Biological Sciences (1935–1944) and would later become an advisor to Chancellor Hutchins (1944–1947).[17] With Taliaferro in the role of Dean of Biological Division, interdisciplinarity was

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[13]WH McNeill, *Hutchins' University: A Memoir of the University of Chicago, 1929-1950* (University of Chicago Press, 1991).

[14]Emmett, "Specializing in Interdisciplinarity: The Committee on Social Thought as the University of Chicago's Antidote to Compartmentalization in the Social Sciences."

[15]McNeill, *Hutchins' University: A Memoir of the University of Chicago, 1929-1950*; Emmett, "Specializing in Interdisciplinarity: The Committee on Social Thought as the University of Chicago's Antidote to Compartmentalization in the Social Sciences"; A. Levine, "The Remaking of the American University", *Innovative Higher Education* 25, no. 4 (2001).

[16]McNeill, *Hutchins' University: A Memoir of the University of Chicago, 1929-1950*, pg. 33.

[17]D.W. Talmage and V. Portsmouth, "William Hay Taliaferro", *Biographical memoirs* 54, (National Academy of Sciences, 1983). pg. 386.

fostered with the notion that science should not be constrained by a demand for immediate application of its findings.[18] Thus, it is not surprising that Rashevsky’s school grew and prospered during Hutchins’s presidency and Taliaferro’s deanship of the Division of Biological Sciences.

Taliaferro’s views on science differed from his predecessors, helping to foster the interdisciplinary approach to biology. In Taliaferro’s address delivered at the 231st Convocation on December 19, 1947 at the University of Chicago, he defended the pursuit of pure science. That kind of science possesses a “gradual spectrum of interest starting with fundamental science, whose votaries try to understand and explain natural phenomena without regard to practical value, and extending to developmental science, whose adherents attempt to apply basic science to the needs of mankind”.[19] Profiling the basic pure scientist, Taliaferro asserted the following:

> The basic scientist, to a greater extent, defines his goal in terms of interest and is largely dependent on lucky guesses (inspiration, if you like) and often just plain fumbling. For this reason, the basic scientist is much more of <u>a lone wolf</u> than the applied variety. His work <u>cannot be directed</u>, because he must be allowed to change his goal as he works and because his best ideas are *unorthodox* and are only too often known to be *impractical by his famous colleagues* who would be his most likely directors. It is the abstract, atypically brilliant individual, considered peculiar by the practical man, who most often provides the keystone to the arch of accumulated scientific evidence that makes possible the formulation of broad, often sweeping generalizations.[20] [emphasis in original]

Defending pure and not readily applicable scientific research, Taliaferro contended:

> No man can guess what knowledge will be practically applied next. . . . To put it another way, if we support only work which the wisest men believe promises practical application, we shall miss, almost by definition, new and revolutionary discoveries. . . In part, however, they [universities and nonprofit research organizations] are plagued by a lack of understanding of the nature of basic science and by confusing it with applied science. . . Yet it is true that basic science has always had to depend a great deal on fanatics or “queer ducks,” and I am sure it will continue to do so. To those who belong to this peculiar group and who are willing to continue in university work, there are compensations for the flesh pots of his life payable in the joy of teaching, in the advantage of close contact with scholars in other disciplines, and in real freedom and independence in intellectual pursuits.[21]

As a countermeasure or antidote to the fragmentation and increasing specialization of the disciplines, Hutchins also promoted a new type of academic structure at the University: the Committee. A Committee customarily comprised professors with appointments in other departments, but also could include faculty with appointments only in the Committee. It was generally much smaller than a

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[18] W.H. Taliaferro, “Science in the Universities”, *Science* 108, no. 2798 (1948): 145–148.

[19] Address delivered at the 231st Convocation, University of Chicago, December 19, 1947. published under Ibid.

[20] Ibid.

[21] Ibid.

department. Some Committees existed only to offer interdisciplinary courses whereas others were degree-granting organizations. A student's program generally comprised of some Committee courses as well as a selection from the regular course offerings in the cooperating departments. For example, a Committee on Information Science (the forerunner of the department of Computer Science) was established, and the members of this Committee had appointments in the Physics Department, the Mathematics Department, the Library School, and the School of Business. Chicago's famous Committee on Social Thought had members from a wide variety of departments in the humanities, the social sciences, as well as in law and religion.

A Forward-Looking Policy in the Division of Biological Sciences

In 1930, with the reorganization of the University, the Division of Biological Sciences was set up as an administrative unit with Frank Lillie (1870–1947) as its dean (1931–1933). The aim was to unite all of the biological interests at the university into one single endeavor in education and research. This vision had its problems; it was challenging to integrate new and strong departments concerned with the actual practice of medicine with the older and more veteran university departments with pure academic interests and traditions, uniting their educational and research policies in the new Division.

The administration believed the union to be timely "because an outstanding feature of the development of the biological sciences during the present century has been a breaking down of barriers that had been built up during the nineteenth century in a period of very intense specialization within various biological sciences."[22] One of the consequences was interdependence to the extent that the fields of applied biology in clinical departments leaned on the theoretical biological disciplines for aid in solving their problems. The interdependence was so great that a medical school that lacked direct affiliation with theoretical biology was destined to become "an anachronism".[23] Between 1931 and 1932 more than ten senior academic members retired or resigned, assuming positions outside the University. The primary reason was the cut in faculty salaries and incomes, making it extremely difficult for the University to fill the vacancies.

The 1933 report of the Dean of Biological Sciences recognized that "a conspicuous feature of the progress of biological research in recent years has been the

[22]Deans' periodical report on the Division of Biological Sciences for the years 1930–1933, from Taliaferro to Hutchins, August 1, 1935, Box 385, Folder 5, Hutchins Administration Records, Special Collections Research Center, University of Chicago Library (hereinafter: HOP-SCRC).
[23]Ibid.

breaking down of departmental boundaries and even of divisional boundaries."[24] This dissolution of borders was believed to be well-exemplified in genetics, biochemistry, the study of infectious diseases, neurological matters, psychological problems, etc. Looking into the future, the University was planning to establish several new programs within the division, such as the institute for genetic biology; the establishment of the institute of Hygiene and Bacteriology and the study of Infectious Disease; and modern laboratories for Anatomy, Botany, Psychology and Zoology. In general, the university administration was striving to repair the broken fences brought about by the interdisciplinary program through filling vacancies left gaping during the depression by increasing salaries, and by 'very carefully" considering personnel for upcoming new projects.[25]

In accordance with Hutchins' vision, the University was taking the necessary steps to foster interdisciplinary cooperation in biological research. The newly appointed Dean Taliaferro submitted his report for the time period between 1934 and 1937, articulating that "...the entire history of science has been largely the history of strong departments led by outstanding men. I believe that it is necessary to continue the development of strong departments. Such a development of discrete entities will, however, no longer serve the broad interests of science."[26]

The new divisional organization was intended to provide a better fit for the development of strong departments on the one hand and interdepartmental cooperation on the other. Although Taliaferro believed that "no method of administration can force cooperation of individual investigators", the administration could provide the facilities and encourage such collaboration.

The university administration was looking for young promising blood to come over to develop a program that would meet its expectations. In this constellation, it is not surprising that Rashevsky was granted a position when his fellowship ended in 1935.

The Scientific Pathfinder

With the supportive environment of the Hutchins' presidency and Taliaferro's deanship, Rashevsky's vision was—at least institutionally—on its way to becoming a reality. While he was developing an intellectual identity geared towards establishing and institutionalizing mathematical biology, he was concurrently on the path of creating its professional identity. The first decade of Rashevsky's intellectual trajectory would prove to be fruitful. During the Hutchins presidency, two of Rashevsky's major accomplishments were the establishment of the first

[24]Ibid.

[25]Ibid.

[26]Dean's periodical report on the Division of Biological Sciences for the years 1934–1937, Box 386, Folder 7, HOP-SCRC.

journal devoted to mathematical biology and the first program to award doctorates, initially in the form of a Section in the Department of Physiology and later on as an independent Committee in the Division of Biological Sciences.

During this period of reorganizing the Division, Rashevsky was granted a place to pursue his vision and lay the first stones towards establishing a new discipline, equipped with its own methodology, publishing venue, and training program. Rashevsky's scientific, political, and academic skills suggested that he was headed for a bright future. Between 1934 and 1938 he built a scientific program that laid the foundations for realizing his vision. Its contours were first presented in 1934 in *Foundations of Mathematical Biophysics* published in the first volume of the journal *Philosophy of Science* founded by the logical empiricist Herbert Feigl and others[27] The program as he laid it out would occupy him throughout his scientific career and even his lifetime.

Rashevsky argued that his vision of mathematical biology differed from other attempts to apply mathematics to biological problems. The key distinction was that the efforts of his predecessors dealt with the occasional application of mathematics to some specific *ad hoc* problems rather than with developing a systematic mathematical biology. He consistently argued that the methodology employed by his predecessors, e.g., Lotka and Volterra, differed from his. According to Rashevsky, Lotka and Volterra postulated on the basis of direct observation and general relations between organisms, thereby developing a mathematical theory of various phenomena involving such inter-individual relations.[28] This kind of theory did not consider the detailed structure of an organism nor did it consider the relations of the fundamental parts of the organism to the physical inorganic world. These considerations constitute the backbone of Rashevsky's own research methodology.

His mathematical biology was not merely the use of mathematics to describe biological systems. Rashevsky aimed at developing a mathematical biology that

[27] Rashevsky, "Foundations of Mathematical Biophysics."

[28] An extensive historical review of the scientific agenda developed by Lotka and Volterra is found in the works of Giorgio Israel, Ana Millán Gasca and S. Kingsland: see e.g. G Israel, "On the Contribution of Volterra and Lotka to the Development of Modern Biomathematics", *History and Philosophy of the Life Sciences* 10, no. 1 (1988); ———, "Volterra's' Analytical Mechanics' of Biological Associations", *Archives Internationales d'histoire des Sciences* 41, no. 126 (1991): 57–104 and no. 127: 306–351; G Israel "The Two Faces of Mathematical Modelling: Objectivism Vs. Subjectivism, Simplicity Vs. Complexity", *The Application of Mathematics to the Sciences of Nature. Critical Moments and Aspects* (2002); G Israel and Millán Gasca, *The Biology of Numbers: The Correspondence of Vito Volterra on Mathematical Biology*, Science Networks-Historical Studies, Vol. 26 (Basel-Boston-Berlin, Birkhäuser Verlag, 2002); G Israel, "The Science of Complexity: Epistemological Problems and Perspectives", *Science in Context* 18, no. 03 (2005); ———, "The Emergence of Biomathematics and the Case of Population Dynamics a Revival of Mechanical Reductionism and Darwinism", *Science in context* 6, no. 02 (2008); A. Millán Gasca, "Mathematical Theories Versus Biological Facts: A Debate on Mathematical Population Dynamics in the 1930s", *Historical studies in the physical and biological sciences* 26, no. 2 (1996); SE Kingsland, *Modeling Nature* (University of Chicago Press Chicago, 1995).

was a precise analogy to the use of mathematics in "molecular theory in physics", whereas Lotka's and Volterra's approaches, respectively, were according to Rashevsky analogous to the use of mathematics in thermodynamics. Rashevsky believed that curiosity should be pushed further rather than concentrate on the large bulks of material with relatively gross phenomena. These previous approaches were according to Rashevsky characterized by the development of theory based solely on the basis of a few accepted postulates, direct observation and experimental evidence. "Molecular physicists", in Rashevsky's view, dealt with atomic concepts rather than "gross phenomena". Rather than study the "general relations" between organisms, Rashevsky's mathematical biology addressed the details of organisms.[29] Rashevsky was preoccupied with the grandiosity of his program. It was to be grander and perhaps better than that of his predecessors or approaches developed in parallel, e.g. Lotka, Fisher, Wright, etc.

In his work, Rashevsky continuously sought *physical interpretation* of biological phenomena. It was "in line with the desire to *unify* all natural sciences", laying the first stone in the foundations of mathematical biology.[30] Moreover, Rashevsky acknowledged on several occasions that Lotka "came closer than anyone before him in an attempt to encompass the whole field of biology in a mathematical study".[31] However, Lotka's attempts were limited to one biological problem, namely, the theory of the interaction of species. Thus, while Lotka and other contemporaries attempted to apply a mathematical approach to "special branches of biology", these efforts were viewed by Rashevsky as providing only a glance into the available opportunity of integrating mathematics and biology.

Contrary to what he perceived as Lotka's approach, Rashevsky intended to construct a more systematic approach, starting off with the smallest of biological entities and gradually moving forward on the scale to study the whole field of biology via a physico-mathematical approach. Rashevsky believed it to be "worthwhile to try the one thing hitherto not tried in biology, namely the building of a 'system of mathematical biology', similar to mathematical physics. This task is not a small one, and one hardly could expect any spectacular achievements in a short time. It took two centuries of efforts of the best mathematicians to bring mathematical physics to its present perfection. Yet somebody has to start, no matter how difficult the task and how slow the progress."[32] Rashevsky's vision was nurtured by the success of physicists who employed mathematical analysis. Trying to prove his point, he referred to Carl Friedrich Gauss, indicating that Gauss "by mathematical calculation alone" found the orbit of the "asteroid" Ceres when efforts of other

[29] Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*. Preface, 1938.

[30] Ibid.

[31] Ibid.

[32] N. Rashevsky, "Physico-Mathematical Methods in Biological Sciences", *Biological Reviews* 11, no. 3 (1936).

astronomers failed.[33] Rashevsky asserted that mathematical biologists would "play a similar role in the study of …biological problems when the efforts of many experimenters have failed". The goal would be accomplished by using a fundamental rule of gradual approximation exactly as has been done in physics. While Rashevsky never identified himself as *the* mathematical biologist that would lead to great discoveries in biology using mathematical analysis, his constant analogies to great physicists such as Maxwell, Laue, Gauss, Dirac, Einstein etc. and their works portray him as person preoccupied with a notion of self-importance, sense of arrogance and a belief of being perhaps unique enough to reach achievements similar to those of the great physicists he so admired. It was also his way to promote and defend his methodology to biologists, perhaps hoping to convince biologists that successes achieved in physics using similar methodologies are achievable in biology if they bear with him. Rather than centering on his actual achievements in mathematisizing biology, he continuously stated the goals of his program and the potential it harbored to lead to great discoveries in biology.

Rashevsky's concept was to design a program that would eventually combine theory *and* experiment. He was to unite theoretical physics and biology, suggesting paths where experiments had yet to tread. At the core, his vision was to build "a new science" and to "make biology an exact science".[34] For Rashevsky, mathematical bio-physics is a "new-born babe [sic]", undeveloped, but "contains in itself, in an embryonic stage, all its future qualities and characteristics".[35] However, it would take several decades before such a combination would be successfully achieved. His outlook was that of a theoretical physicist and mathematician.

As a pure scientist who had been in close contact with industrial research for several years, he was well aware that experimental biologists would be wholeheartedly enthusiastic about the mathematization of biology only when practical use of his theories would be achieved as "the evaluation of any research still remained its practical use".[36] The exposition of his program began with an analogy to the domain that he had only recently left behind: industrial research:

> Mathematical methods in biology occupy a somewhat peculiar position, and the attitude of many biologists toward them is similar to that of many practical engineers toward what is called pure scientific research. The modern progressive engineer recognizes the value of pure science, which seeks for truth regardless of any possibility of practical applications; yet he still frequently shows a definite dislike towards such investigations… The ultimate

[33]Ibid. (pg. 354) Ceres is considered to be one of the largest asteroids in the main asteroid belt. However, the classification of Ceres has changed more than once, and in 2006 it was classified as a "dwarf planet" by the International Astronomical Union.

From Rashevsky's statement a false impression might be received that other astronomers tried to determine the orbit of Ceres by observation. This however was not the case. Gauss succeeded not because he used "mathematical calculations alone" but rather because his calculations were more correct than those of others.

[34]Rashevsky, "Foundations of Mathematical Biophysics."

[35]Ibid.

[36]Ibid.

> criterion in the evaluation of any research still remains its practical use....Many men of science may feel tempted to revolt against such an attitude. And yet such a revolt would be unwise, because the above attitude is rather deeply rooted in human psychology and its parallel is found even within the domain of pure science itself.
>
> The attitude of the practical man towards pure science in general resembles that of the pure scientist who is an experimentalist towards the more mathematical branches of his files. The experimental scientist recognizes the value of the mathematical science. He knows that mathematical investigations which at first glance looked like mental gymnastics without any connection whatever to reality, have led subsequently to formulae that predicted new phenomena...but he [empirical scientist] will not appreciate the investigation whole heartedly unless he sees some immediate connection between the mathematics and the experiment; unless he is given a formula which he can at once proceed to check by means to a set of thermometers, respirometers, galvanometers, etc.[37]

Rashevsky was aware of the fact the "biologists approve of mathematics only when they lead to simple formulae which can be easily tested experimentally".[38] What would mathematical biophysics contribute?

He responded with these words:

> Mathematical biophysics studies all physically conceivable possibilities of what may happen in a biological system. It studies these without regard to whether the possibility in question furnishes *the* explanation of a given, biological phenomenon. It studies all possible explanations. And only after such a study has given us a clear insight into all possibilities, can experiment decide which possibilities are found in nature [emphasis in original].[39]

For Rashevsky one purpose of theory and mathematization was to indicate to the experimental biologists in which direction to look when "hunting for facts" and enable the experimentalists to see through the complexity of the biological phenomena.

Rather naively he continued to state:

> True, biological phenomena are *perhaps* more complex than ordinary physical ones. But even the latter are on their face so complex, that their complete mathematical treatment may appear impossible. And yet it is just the mathematical method of approach that enables us to see through that complexity. The important thing in the mathematical method is to abstract from a very complex group of phenomena its essential features and thereby to simplify the problem. The more complex features are then taken care of gradually, according to the degree of their importance and complexity, as second, third, and higher approximations.

Rashevsky rigorously defended his approach, stating that the "characteristic of mathematical method is that it is applied to a scientific problems for its own sake, regardless of immediate contact with reality" and further stated that "experimentally useless" mathematical treatment should not be considered a failure of the mathematical method but rather a prerequisite to a method that has more contact with the reality.

[37] Ibid.

[38] Ibid.

[39] Ibid.

Rashevsky's mathematical biophysics was to study all physically conceivable possibilities of what may happen in a biological system. It was to study these "without regard to whether the possibility in question furnishes the explanation of a given, biological phenomenon." It was to study all possible explanations, ". . .and only after such a study has given us a clear insight into all possibilities, can experiment decide which of the possibilities are found in nature."[40]

Rashevsky's aim was to examine the fundamental structure of the parts of organisms and the relation of these parts to the physical, inorganic world. The first and primary object of study for mathematical biophysics during the 1930s was the cell.[41] And in justifying this approach, Rashevsky again referred to the mathematical methods of physics:

> Following the fundamental method of physicomathematical sciences, we do not attempt a mathematical description of a concrete cell, in all its complexity. We start with a study of highly idealized systems, which at first may not even have any counterpart in real nature. This point must be particularly emphasized. The objection may be raised against such an approach, because systems have no connection to reality; and therefore any conclusions drawn about such idealized systems cannot be applied to real ones. Yet this is exactly what has been, and always is, done in physics. The physicist goes on studying mathematically, in detail, such nonreal [sic] things as "material points," "absolutely rigid bodies" "ideal fluids," and so on. *There are no such things as those in nature*. Yet the physicist not only studies them but applies his conclusions to *real things*. And behold! Such an application leads to practical results—at least within certain limits. This is because within these limits the real things have common properties with the fictitious idealized ones! Only a superman could grasp mathematically at once the complexity of a real thing. We ordinary mortals must be more modest and approach reality asymptotically, by gradual approximation [original emphasis].[42]

One can see that Rashevsky outlined the fundamental aspect of his project and provided a clear justification for a theoretical approach to biology. Complex phenomena in biology are ubiquitous, and it is through simplification or idealization that one may begin to understand them.[43] That sort of approximation may be achieved through the use of mathematics, as was successfully achieved in physics. Rashevsky applied this method to biological processes such as cell division, cell respiration, cellular growth, kinetics of diffusion, rates of reaction, and the processes of excitation, inhibition and conduction in nerve cells.

His initial outline of the project inferred that biological systems are to be abstracted and translated into physical systems before any analysis of the complexity of the former could be made. His approach was to transform biology from the descriptive, classificatory, inductive stage to "deductively-formulated theory".

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[40]Ibid.; N Rashevsky, "The Relation of Mathematical Biophysics to Experimental Biology", *Acta Biotheoretica* 4, no. 2 (1938).

[41]Rashevsky, "Physico-Mathematical Methods in Biological Sciences."

[42]———, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*.

[43]———, "Foundations of Mathematical Biophysics", pg 178.

An Experiment in Scientific Procedure: The Cold Spring Harbor Symposia on Quantitative Biology

One of the crucial events in Rashevsky's early career was the second meeting of the Cold Spring Harbor Symposia (CSHS) on Quantitative biology in 1934. All major scientists who were investigating the interplay between basic sciences and experiment attended that meeting.

Beginning in 1933, at the initiative of geneticist Reginald Harris Director of the Biological Laboratory at Cold Spring Harbor, the Cold Spring Harbor Biological Laboratory held a meeting every summer devoted to fostering a "closer relationship between biology and basic sciences". These meetings were considered to be "an experiment in scientific procedure" and were called Cold Spring Harbor Symposia on Quantitative Biology.[44] The meetings spanned over a month, with participants conducting experiments in the laboratories and giving talks in the meetings. The first meetings exemplified Harris's belief that a quantitative approach to biology was the way forward, and that the older descriptive approaches were inept at revealing the true workings of organisms.[45]

Each summer the Laboratory would invite a group of mathematicians, physicists, chemists, and biologists who were actively interested in a specific aspect of quantitative biology, or in methods and theories applicable to it, to participate in the symposia. It was the object of the meeting organizers that every contributor to the final outlay should be "an expert in his field".[46] Moreover, the meetings lasted for weeks with no time-limit imposed on discussions following the presentation of formal papers. The number of scientists presenting papers was limited in order to stimulate discussion, and all of the participants in a discussion helped with its revision; thus, in a sense, the discussions as published in the end represented the best considered thought of the group on the subject.

The subjects of the meetings were determined based on topics in which rapid advancement had recently occurred along quantitative lines. While some of the papers were a review of certain phenomenon, the majority engaged a presentation of specialized and even controversial aspects of a subject. The organizers realized that a probable result was that the volumes would be outdated within relatively few years; nevertheless, they believed that "to research workers such a disadvantage is outweighed by each volume presenting the state of the subject as it exists at the moment, and presenting not only what is known, but what is still speculative or undetermined."[47]

At the first meeting in July 1933 that dealt with surface phenomena, Harris made the following opening remarks explaining the choice of invitees:

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[44] Introduction by Harris to the 1934 (second), Cold Spring Harbor Symposia of Quantitative Biology.

[45] Harris, "Mathematics in Biology."

[46] Introduction by Ponder to the 1936 Cold Spring Harbor Symposia of Quantitative Biology.

[47] Introduction by Ponder to the 1936 Cold Spring Harbor Symposia of Quantitative Biology.

> The officers of the Laboratory are interested in the development of an institute in which biologists, chemists, physicists and mathematicians will cooperate in the further opening, and beneficial use, of the vast territory of quantitative biology...
>
> The present meeting is the inauguration of a plan whereby each summer a group of mathematicians, physicists, chemists and biologists, actively interested in a specific aspect of quantitative biology, or in methods and theories applicable to it, will be *invited* to carry on their work, to give lectures and to take part in symposia at the Laboratory. A given group in residence here will necessarily be *relatively small*, but members of the group will be *chosen* with the aim that every important aspect of a particular subject is adequately represented from the physical and chemical, as well as from the biological point of view; and that the whole span of a subject, from theories of physics to application to medicine, is covered...
>
> It is expected that many advantages will be secured through the operation of the plan. Outstanding among these is the value of the meetings to the men who form the group... [the] summer laboratories ...should be centers of growth and dissemination of *new methods and ideas in biology*.[48]

Harris encouraged participants to grant "special consideration to theoretical and controversial aspects" of the topics in their lectures. Because large attendance would interfere with the unique advantages of these symposia, Harris made arrangements for the papers and discussions to be available as soon as possible to the greater community of biologists.

By the summer of 1934 Rashevsky's work in mathematical biophysics reached Harris. Perhaps due to its controversial nature, Rashevsky was invited to the second meeting at the CSHS that dealt with aspects of Growth. Rashevsky presented a paper entitled "Physico-mathematical aspects of cellular multiplication and development".[49]

Harris explained his rationale for choosing as a topic the phenomena of growth:

> Growth is a very complex phenomenon. In general, the more complex the problem, the more clearly mathematicians, physicists and chemists may see the enormous difficulties surrounding biologists who are conducting research in what we have chosen to call quantitative biology. Similarly, the more complex the problem, the more the biologist must use mathematics, physics and chemistry, and the more valuable *cooperation* with representatives of these several sciences becomes. An indication of the truth of this is to be found in studies of growth in even relatively simple organisms.[50]

The presentation at the CSHS was Rashevsky's first public lecture introducing his methodology. It was Rashevsky's chance to introduce his Mathematical Biology and to get a feel for what more experimentally oriented colleagues might think of it. Yet the lecture did not end as Rashevsky hoped, with scientists embracing his theories and methods. His attempt to persuade biologists of the potential effectiveness of his mathematical approach to the fundamental biological problem resulted in failure. Hostility was quick to follow. Nevertheless the lack of success was not

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[48]Opening remarks by Harris to the 1933 Cold Spring Harbor Symposia of Quantitative Biology.

[49]N. Rashevsky, "Physico-Mathematical Aspects of Cellular Multiplication and Development" (1934).

[50]Introduction by Harris to the 1934, second, Cold Spring Harbor Symposia of Quantitative Biology.

due to inadequate analysis or comprehension of the subject matter on Rashevsky's part. The lack of success was primarily due to a lack of sufficient data and measurements of biology upon which Rashevsky's work could be examined and verified. Rashevsky's presentation and the discussion that followed revealed a tension between the experientially minded biologists and those who believed in the possibility of mathematization of biology, and it sheds light on a divide between these two groups of scientists.

Rashevsky's exposition of the physico-mathematical aspect of cellular multiplication and development opened with this introduction:

> We know now a great deal about viscosity of the protoplasm and its changes during different phases of the life of the cell; we know a great deal about the electrical properties of the cell. And yet, in spite of all this progress, our knowledge of the fundamental and ultimate causes of one of the most important phenomena of the life of the cell, namely that of the multiplication, remains as unsatisfactory as it was. . .it is simply a logical necessity, free of any hypothesis, that some physical force or forces must be active within the cell to produce a division of the latter. . . [if] we entertain the hope of finding a consistent explanation of biological phenomena in terms of physics and chemistry, this explanation must of necessity follow logically and mathematically from a set of well-defined general principles. The collection of experimental facts gives us a lead for the establishment of the general principles. But the question as to whether a phenomenon. . .follow[s] from a certain experimentally established principle is in general beyond the reach of the experiment. . .the answer to such questions belongs to the domain of deductive sciences.[51]

In Rashevsky's introduction, he drew the conclusion that the dearth of knowledge on the fundamental causes of biological phenomena was due to the fact that in biology nobody was employing deductive mathematical methods. He argued that theoretical research "will have to go hand in hand with the experimental, and ask of the latter information . . .for which the experimental scientist would even not have looked."[52]

Admitting the complexity and diversity of the cell, Rashevsky proposed disregarding all properties and phenomena that were not common to all cells. Placing himself among biologists Rashevsky posed the following question: "Do *we* need to assume some special independent mechanisms, which produce at a certain stage of the cellular life a division, or are those mechanisms merely the consequences of a more general phenomena [sic], which *we* know occur in all cells?" [emphasis added] The answer to this question according to Rashevsky lay in investigating mathematical consequences of all general phenomena to see if the process of division is found among such consequences. In case it is not found, a search for yet undiscovered general properties of cells should be made. Since the task at hand was investigation of general and exceptionless phenomena, common to all cells, Rashevsky, argued that the complexity of a cell and the almost infinite variety of different kinds of cells made the task easier than one would assume.

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[51] Rashevsky, "Physico-Mathematical Aspects of Cellular Multiplication and Development".
[52] Ibid.

As in his previous research on the subject and influenced by his work on the colloidal particles, Rashevsky presented his theory of cell division based on an analysis of the simplest of cases, an idealized system of a spherical cell, comprising one homogeneous phase. The general property Rashevsky investigated was that of "taking in some kind of substance [by the cell] from the surrounding medium, to metabolize them and to give off into the surrounding medium some products of its metabolism".[53] Yet again he drew from his expertise as a physicist and asked his audience to "consider a physical system, which is liquid, like a cell…".[54] He consistently argued throughout his own presentation and in the discussions that ensued that such a system and the quantitative analysis performed thereon would not apply to actual cells with any degree of precision. However, it could provide a "general quantitative picture of various possible phenomena and yield also at least the order of magnitude of the effects which occur in more complex cases."[55] Rashevsky asserted that the cause for the division of the cell was the forces of repulsion acting within the cell between each element of its volume. His presentation was filled with mathematical equations and theoretical analysis, his method was formal and deductive and stood out amongst other presentations in the volume. Whilst others presented quantitative measurements to arrange their data or applied mathematical formulae on experimentally accumulated data, Rashevsky's studies had no references to specific cases, only to idealized "cell systems". Rashevsky presented his equations relating variables such as pressure, concentration, volume, forces of attraction and repulsion between molecules, and coefficients of diffusion. He then "solved" the equations, interpreted the solution, and drew conclusions (e.g. this variable will vary with respect to this other variable according to this mathematical expression).

In the discussion that followed, Rashevsky was bombarded with questions: "What is the nearest example in nature to this theoretical case?" "What is the effect of the cell wall around the cell?"[56] Rashevsky thought quickly on his feet and responded immediately providing examples from the biological world. To the first question his response was that while it is difficult to answer, the closest case in nature to the idealized system was in his opinion bacteria such as cocci. As to the second question, Rashevsky responded that the forces due to the presence of the cell-wall are included in his consideration although not discussed during the presentation. An interesting discussion ensued between Rashevsky and the physical chemist L.G. Longsworth. Longsworth shared his impression that Rashevsky's approach of cell division was promising. However, he postulated that it did not take into consideration factors that might bear influence on the division, such as gravitational forces. Such gravitational forces would destroy the spherical symmetry upon which Rashevsky based his computations. Rashevsky responded again,

[53] Ibid. In the discussion that followed the paper.

[54] Ibid.

[55] Ibid.

[56] Ibid.

repeating "I am perfectly aware of the presence of those other factors [gravity currents]...As I explicitly stated in several publications, I am choosing the case of spherical symmetry only as the mathematically simplest case with which to begin".[57]

Perhaps the harshest criticism lodged against Rashevsky's presentation was that of the eminent biologist, leading spokesman for eugenic research, previously director at the Biological Laboratory and one of the most influential biologists of his time, Charles Davenport[58]:

> I think the biologist might find that whereas the explanation of the division of the spherical cell is very satisfactory, yet it doesn't help as a general solution because spherical cell isn't the commonest of cell. The biologist knows all the possible conditions of cell form before division; cases where cells increase enormously without dividing, and divide without increasing in size. There doesn't seem to be in any general way a relationship between the form or size in connection with the cell division. In the special cases of egg cells and cleavage spheres, this analysis may prove very valuable. But after all, these are only special cases.[59]

In an attempt to fight the criticism leveled against his approach, Rashevsky responded rather aggressively, feeling himself cornered to repeat that "the results presented...[were] only the first steps in the development of mathematical biology," repeating that it would be a "misunderstanding of the spirit and methods of mathematical sciences should we attempt to investigate complex cases without preliminary study of the simpler ones". He proceeded to opine rather arrogantly that in his view "it is already ...a progress that a general physico-mathematical approach to the fundamental phenomena of cellular growth and division...has been shown to be possible." He further predicted that it would take "twenty five years of work by scores of mathematicians to bring mathematical biology to a stage of development comparable to that of mathematical physics".[60] Such a prediction was not only unreasonable but insulting to biologists. It illustrates Rashevsky's disregard (and even ignorance) to the complexity of the biological sciences asserting it would take only 25 years for mathematical biology to reach the stage which mathematical physics struggled to reach for two centuries.

Reviewing the volume of the proceedings of the second meeting, it is perhaps intentional that the paper that follows Rashevsky's paper was that of the prominent physiologist Edwin B. Wilson, who did not attend the meeting but submitted his paper for the published proceedings. Wilson's paper is in a way a continuation of the discussion of the effectiveness of mathematical analysis in biology and is not directed at Rashevsky *per se*. In Wilson's short paper "Mathematics of Growth" he

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[57]Ibid.

[58]J.A. Witkowski, "Charles Benedict Davenport, 1866–1944", *Davenport's Dream: 21st Century Reflections on Heredity and Eugenics* (2008).

[59]Ibid.

[60]Ibid.

shared his views on the place mathematics should have in biology and in particular in the studies of growth via five "axioms or platitudes"[61]:

1. Science need not be mathematical.
2. Just because a subject is mathematical it does not mean that it is necessarily scientific.
3. Empirical curve fitting may be without other than classificatory significance.
4. Growth of an individual should not be confused with the growth of an aggregate of individuals.
5. Different aspects of the individual, or of the average, may have different types of growth curves.

Wilson concluded that for mathematics, individual cases would not be a good study case, even though it might be helpful to the study of populations. Davenport expressed his cordial agreement with Wilson. This is not surprising as at least the last two points are based on Davenport's work and conclusions which are also presented in the second volume of the proceedings (1934) and follow Wilson's brief paper.[62] Another respondent to Wilson's paper, membrane biophysicist Eric Ponder (who was to succeed Harris as the director at the Cold Spring Harbor laboratory after Harris' untimely death in 1936) concurred. Ponder commented that "one point upon which there seems to be pretty general agreement is that there is little relation between the amount of work which has been done on the mathematics of growth and the clarification of the subject which has resulted".[63] Ponder used the discussion following Wilson's paper as an opportunity to articulate strongly his conclusions related to the mathematics of growth written in differential equations, as in the case of Rashevsky's work. Ponder remarked:

> I think there is a general agreement that these investigations have not been very successful. I am far from being opposed to biomathematics, but I feel that it is futile to conjure up in the imagination a system of differential equations for the purpose of accounting for facts which are not only very complex, but largely unknown, and the fact that the resulting expressions are not at variance with the observed data really says little for them, unless they are used for descriptive and appreciate purposes only. It is said that if one asks the right question of Nature, she will always give you answer, but if your question is not sufficiently specific, you can scarcely expect her to waste her time on you. . ..What we require at the present time is more measurements and less theory. . ..more experimental analysis of phenomena and less integration.[64]

Ponder was not the only one who held this opinion. The bacteriologist Stuart Mudd of the University of Pennsylvania accorded, stating that "at the present time our need for accurate measurements is greater than for theoretical expressions".[65] In the clash between the two view points, the experimentally minded biologists had

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[61]EB Wilson, "Mathematics of Growth" (1934).

[62]C.B. Davenport, "Critique of Curves of Growth and of Relative Growth" (1934).

[63]Ibid. In the discussion that followed Wilson's paper.

[64]Ibid.

[65]Ibid.

the upper hand. The prevailing ethos among the experimentally minded biologists was to seek for more data through measurements, more experimental analysis of the growth phenomena and less of the mathematical speculations of cases rarely presented in nature.[66]

Yet it was Harris who stressed the importance of theoretical work: "new training and new viewpoints would unquestionably be brought to biology by mathematicians developing a science of theoretical biology."[67] Harris was not shy in expressing his "strong" opinion on the utility of exact sciences to biology. Following the 1934 meeting and prompted by the discussions that followed Rashevsky's presentation and Wilson's paper, Harris presented his opinion in *The Scientific Monthly* in 1935.[68] According to Harris, the review of the CSHS proceedings which in fact centers on quantitative biology will paint a "depressing" and an ironic picture of a "wide spread disappointment in the results of the use of mathematics in the study of growth".[69]

To balance Wilson's skeptical attitude towards the mathematization of biology, Harris presented his own axioms:

1. "Mathematics cannot [sic] produce valuable generalities, laws or formulae in biology when the data which it uses are insufficient."
2. "Mathematics is of value in even very limited areas in which sufficient data are at hand."
3. "Mathematical expression of biological findings in terms of laws or equations, gives significance to so-called negative findings."
4. "Mathematics may serve as a valuable measure of the state of completeness of knowledge of a science or a part of a science."[70]

Harris contended that "one may expect sufficiently valuable returns from a theoretical biology, based on mathematics, to justify its birth and controlled nurture; this in spite of the fact that there are plenty of examples of the failure of such a procedure in the past."[71] Harris was a strong advocate of Rashevsky's approach and argued that it "should receive some attention as a definite part of biology". He went as far as suggesting that "half a dozen of chairs for theoretical biologists [like Rashevsky] be established at biological laboratories". The holder of such chair should be devoted to "deduction" and explore further the "possibilities of theoretical biology, and to be in a position to become the chiefs of staff if and when recruits are needed". He ended his article by suggesting that a fair and friendly test be given

[66]EB Wilson, "Mathematics of Growth" (1934); Keller, *Making Sense of Life: Explaining Biological Development with Models, Metaphors, and Machines*. Pg. 84.

[67]Ibid.

[68]R.G. Harris, "Mathematics in Biology", *The Scientific Monthly* 40(1935).

[69]Ibid.

[70]Ibid.

[71]Ibid.

to “a new branch of the service”.[72] Based on the above, and despite the critique expressed by the experimentalists against Rashevsky’s approach, it is not surprising, that Rashevsky’s experiment at the University of Chicago was not shut down but rather further promoted, at least institutionally.

The Queer Ducks: The University of Chicago Group of Mathematical Biologists

Fortuitously, the first public debacle did not affect Rashevsky’s academic prospects, and he was promoted to assistant professor in July 1935 with his salary partially paid by the Rockefeller Foundation. With strong supporters such as Harris, Weaver, Thurstone, Lillie, Compton and others, as well as the administration’s positive attitude towards interdisciplinary studies, Rashevsky’s experiment in mathematical biology at the University was far from over. Nevertheless, despite the positive attitude, Rashevsky initially had a hard time finding a place for his research at the biological laboratories. Although he was a member of the division of biological science, he spent his first year working under the auspices of Karl Lashley at the Department of Psychology. By the end of 1935, Rashevsky was dealing primarily with physiological subjects and was thus moved to the Department of Physiology. This transfer happened despite the vociferous objection of the department chair, physiologist Anton J. Carlson, who was a devoted empiricist. With Rashevsky boldly promoting theoretical work over experimentation, his clash with Carlson was inevitable.

Carlson “actively disliked and mistrusted” Rashevsky and ultimately forced the administration to move Rashevsky back to the Department of Psychology in 1936.[73] On some level, Carlson’s attitude was “self-defeating,” as Taliaferro would later indicate to him; pushing Rashevsky out of his department forced the administration to “set R[ashevsky] up as a separate Department,” encouraging Taliaferro to provide Rashevsky with an institutionalized venue to pursue his ‘science’ within the division of biological sciences.[74]

In the years after establishing his program, Rashevsky advanced on two fronts: further expansion of his intellectual persona and establishing his professional identity. Rashevsky constantly promoted his own views about the methodology that would best unveil the complexity presented in biology. The nature of the product resulting from the application of his methodology was less important than the extent and ease of manipulating the studied phenomena through application of mathematical reasoning.. Essentially, it was the potential of the approach that counted: “The value and fate of mathematical bio-physics does not depend on

[72] Ibid.

[73] Weaver Interviews, January 19, 1939, RG1.1, Series 216D, Box 11, Folder 148, RAC.

[74] Ibid.

such outcomes. It is an attempt to make biology an exact science."[75] Rashevsky focused on his own research, and was not directly dependent on the studies of others. He was preoccupied with the power and the potential success his methods harbored.

The administration was on his side. "The final importance of current research cannot be immediately evaluated because frequently seemingly unimportant investigations may form the keystone in some new work," stated Taliaferro in his periodic report to the president.[76] It was the Dean's underlying assumption that the importance of scientific investigations could be evaluated according to these parameters:

(1) Whether the investigator has a well-formulated plan which he pursues for a long term of years,
(2) Whether he originates or develops or leads his field, and
(3) To what extent his work is recognized by other scientists who work in his field.[77]

Rashevsky was named one of the pathfinders that the "university is lucky to have", listed alongside A.J. Carlson, George Dick, Ralph Gerard, James Herrick, William Taliaferro, Louis Thurstone, and Sewall Wright, who was working on "mathematical analysis of the method of evolution".[78]

It did not take long for Rashevsky's professional identity to develop, and he soon attracted young students who showed an interest in his approach and became disciples of his intellectual identity. In 1935, while still under the protective wing of the Department of Psychology, two students came to work with him: the physicists John M. Reiner and Gaylord J. Young. No formal training program in mathematical biology existed at the time. As long as no training program was available, his graduate students were willing to undergo training by pursuing the regular curriculum in either the physics or mathematics department and attend courses suggested by Rashevsky. Rashevsky insisted that his students take various courses in biology, including laboratory courses in physiology and anatomy.

The small group was soon joined by Alvin Weinberg, Herbert Landahl, and Alston Householder. The latter already had a PhD in mathematics when he came to Chicago as a Rockefeller Fellow in Mathematical Biophysics. Nonetheless, just like other members of the group, Householder took courses in biology, including laboratory work.[79]

Promoted to associate professor, Rashevsky was no longer a lone wolf; he was now working with a cadre of young and promising men. This group formed what

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[75]Rashevsky, 'Foundations of Mathematical Biophysics."

[76]Deans' periodical report on the Division of Biological Sciences for the years 1934–1937, Box 386, Folder 7, HOP-SCRC.

[77]Ibid.

[78]Ibid.

[79]History of the Committee, (1963), Box 2, NRP-SCRC.

Rashevsky called "a permanent nucleus" around which the work in mathematical biology was crystallizing.[80] While there are no records as to why the young physicists came to study with Rashevsky, presumably this was due to his publications. Most of his publications at this stage were in *Physics* and presented a program encompassing cellular biology, neurophysiology, psychology and even sociology. While his reputation amongst biologists might not have been positive, to physicists his vision seemed promising. His program was after all the first to provide an institutional venue for a physicist to deal with biological complexity other than ecology and population biology which was relatively more established at this stage.[81] It allowed the young scientists to explore the range of applications of mathematical methods outside the physical sciences.[82]

While the group conducted their research under the division of biological sciences, they were physically isolated from its other members. Rashevsky and his team were given quarters by the administration at the outskirts of the University, away from the insiders. Despite the fact that they were physically and academically isolated, his students teasingly pleaded to let them don white coats to at least look like the 'scientists'.[83] Rashevsky refused, insisting on "a special niche for mathematical biology" with the conviction that it would someday attain a status comparable with that of mathematical physics.[84] That sort of physical and academic isolation was characteristic of Rashevsky throughout his career at Chicago. Rashevsky's refusal further illustrates that while the physical isolation might have been imposed on him and his group by the administration, the academic isolation was something he was in a way striving to claim a place for his mathematical biology. He believed his project to be unique and filled with a sense of self entitlement thought it deserved a special, separate niche of its own.

The point of contact for Rashevsky's group with the "outside" community was via Friday afternoon seminars organized by Rashevsky.[85] The students nicknamed the seminars the "samovar" meetings, alluding to Rashevsky's antique samovar

[80]Rashevsky's letter to Weaver, March 26, 1938, RG 1.1, Series 216D, Box 11, Folder 148, RAC.

[81]Keller, *Making Sense of Life: Explaining Biological Development with Models, Metaphors, and Machines*, pg. 81; Kingsland, *Modeling Nature*; Millán Gasca, "Mathematical Theories Versus Biological Facts: A Debate on Mathematical Population Dynamics in the 1930s."

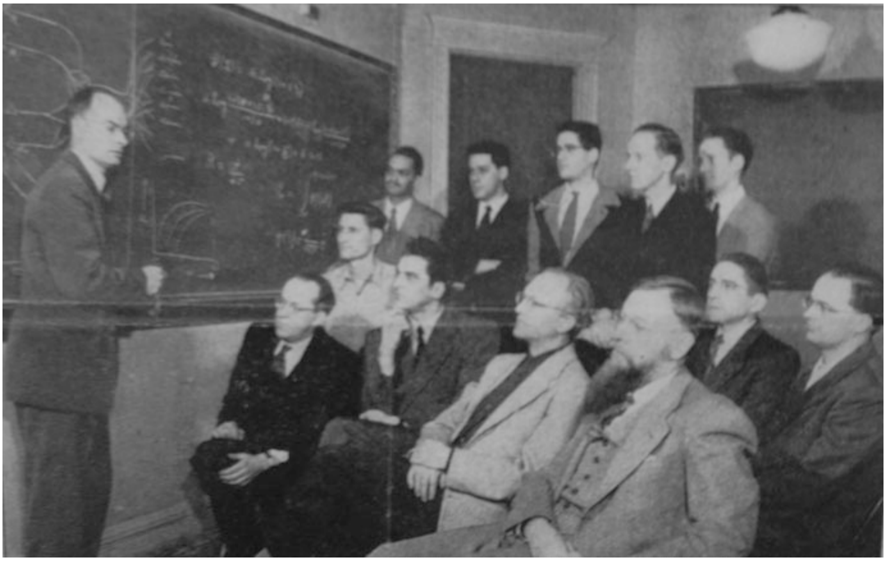
[82]A. Rapoport, *Certainties and Doubts: A Philosophy of Life* (Black Rose Books Ltd, 2000), pg. 89.

[83]The stereotype of a "scientist" is typically a person wearing a white coat and working in a laboratory. It was a plead to Rashevsky to let them at least look like the experimental biologists-the insiders.

[84]Rapoport, *Certainties and Doubts: A Philosophy of Life*; pg. 90.

[85]Ibid. Over the years the "Mathematical Biophysics Seminars" were renamed the "Mathematical biophysics meeting" for purely administrative reasons. As seminars were considered part of the regular courses, administrative regulations mandated from 1944 onward they could not announce them in the University of Chicago weekly calendar. Since the administration recognized the importance of the wide publicity of the meetings, the name was changed.

from which tea was dispensed.[86] These seminars were devoted to presentations on current research in biology, physics and mathematics. It was Rashevsky's way to establish contact with members of other departments, off-campus, and out-of-town experimentalists and theoreticians. The group was exposed to research ranging from theoretical to experimental studies and had an opportunity to create long-lasting liaisons to support their research agendas.



Samovar Meeting, 1952. From a newspaper clipping, bearing no reference to its origins

In its early years, invited lecturers included the neuro-physiologist Ralph Lillie, who gave a talk on "General Parallels between the Phenomena of Activation and Transmission in Passive Iron Wires and in Living systems".[87] The geneticist Sewall Wright lectured on "The Genetics of Melanic Pigmentation of the Guinea Pig". The physiologist Melvin H. Knisely lectured on "Normal and Pathological Capillary Circulation in the Malarial Infected Monkey". Neuroanatomist Gerhardt von Bonin, lectured on "Functional Organization of the Cerebral Cortex". Psychologist Ernest R. Hilgard lectured on "Stimulus-Substitution and the Law of Effect". Physicist Carl Eckart lectured on "The Theory of Irreversible Processes". Sociologists Samuel A. Stouffer lectured on "Intervening Opportunities: A theory Relating Mobility and Distance".[88]

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[86]Ibid., pg. 65.

[87]R.S. Lillie, "The Passive Iron Wire Model of Proto-Plasmic and Nervous Transmission and Its Physiological Analogues", *Biological Reviews* 11, no. 2 (1936); R.W. Gerard, "Ralph Stayner Lillie: 1875-1952", *Science* 116, no. 3019 (1952).

[88]List of papers presented at the Seminar are contained in Box 3, NRP-SCRC.

Interest in the Friday-afternoon *samovar* meetings was exhibited by many scientists from diverse disciplines and institutions. The mailing list for the seminar notices included LL Thurstone from Social Sciences, S.A. Stouffer from Sociology, Ralph Gerard and Ralph Lillie from physiology, Sewall Wright and Ralph Buchsbaum from zoology, Carl Eckart from physics, Professor G.D. Gore from the mathematics department at Y.M.C.A College, Warren McCulloch from the Neuro-Psychiatric Institute, Physicist James Bartlett from the University of Illinois, and others.

The group's scientific developments included Young's research on the application of the plastic flow to cell division, the work of Weinberg and Young on models of nerve excitation, Householder's work on a discrimination mechanism for localizing different stimulus intensities within the nervous system, Landahl's work on cell respiration and his work on psycho-physical discrimination.[89] The work of the group was published in *Growth, Physics and Psychometrika,* but was often refused for publication as the biological journals viewed the works to be too mathematical and the *Physics* journal viewed them as too biological. This problematic situation will be remedied, as will be discussed ahead, by the establishment of the group's organ journal—*Bulletin of Mathematical Biophysics*.[90] *As* there was no degree in Mathematical Biology at that time, the students were awarded their degrees by other departments. For example, Weinberg got a PhD in 1937 from the Department of Physics, although his thesis was on the mathematic-biophysical topic "periodicities in cells" and was performed under the supervision of Rashevsky and Professor Carl Eckart of the Department of Physics.[91]

In Taliaferro's annual administrative report written in 1937, he reported on Rashevsky's work thus:

> ...in the past biology has used mathematical biology as a descriptive tool. With the appointment of Nicolas Rashevsky we are experimenting with the development of a theoretical biomathematics which may eventually serve biology in the same way that theoretical physics serves the science of physics. <u>Such a development of theoretical biology will probably be useless unless it eventually serves to formulate and develop biological experimentation.</u> Furthermore, its development is necessarily slow because of the great complexity of biological phenomena. Dr. Rashevsky's work <u>is an extremely interesting experiment but it is impossible to predict how far he can gain the confidence of the experimental biologists and get them to test out his conclusions and to assist in the general development page</u>.[92] [emphasis added]

[89]History of the Committee, (1963), Box 2, NRP-SCRC; Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*; N. Rashevsky, "Advances and Applications of Mathematical Biology", *Bull. Amer. Math. Soc. 47 (1941), 7.* 2(1941).

[90]Weaver Interviews, July 3, 1938, RG 1.1, Series 216D, Box 11, Folder 148, RAC.

[91]A.M. Weinberg, *The First Nuclear Era: The Life and Times of a Technological Fixer* (Copernicus Books, 1994).

[92]Deans' periodical report on the Division of Biological Sciences for the years 1934–1937, Box 386, Folder 7, HOP-SCRC.

The concern that Taliaferro raises regarding contact or collaboration with experimental biology was important and would cast a shadow on Rashevsky's mathematical biology throughout its development. As a first stage in reconciling the divide that was formed between the abstract theoretical treatment and the experimental approach, Rashevsky aired his view on the subject.[93] In his article on "*The Relation of Mathematical Biophysics to Experimental Biology*" published in *Acta Biotheoretica* in 1938, Rashevsky further elucidates his approach:

> . . .before we attempt to find any relations between already-known facts, we must possess a sufficiently large array of already-known facts. Thus the experimental discovery of phenomena of necessity preceeds [sic] any attempt at theorizing. And it not only proceeds [sic], but also follows it. . .Thus working hand in hand, the experimental and theoretical scientists move together towards new knowledge. In order however to bring a theory to such a stage at which it can be of actual use to the experimenter, it is frequently necessary to do a great deal of preliminary work, which may have nothing or very little to do with actual experimental data, but which is entirely unavoidable. Shortcuts are of no avail in such cases.[94]

[93] Rashevsky, "The Relation of Mathematical Biophysics to Experimental Biology."
[94] Ibid.

Chapter 3
Scientific Experiment: Attempts to Converse Across Disciplinary Boundaries Using the Method of Approximation

The intellectual trajectory of Rashevsky and his group focused on three main subjects which constituted the core of their research for more than a decade. The group adopted Rashevsky's method of approximation and followed it. The approximation method was developed by Rashevsky in 1937 and was presented in an appendix to his *magnum opus, Mathematical Biophysics,* published in 1938.[1]

The method which Rashevsky believed to be "rather crude but powerful" was developed following his realization that biological systems are too complex to be described in detailed mathematical equations.[2] While such equations would theoretically provide exact solutions, such as concentration of a certain substance as a function of time at any point inside and outside the cell, such results were "practically unobservable".[3] According to Rashevsky, "what the biologist actually observes is the average concentration of a substance inside or outside the cell". With cells in nature holding various shapes, and realizing that unlike in physics, no two studied subjects, e.g. cells, organisms etc, are "quantitatively exactly alike", his new method was to take into account not the "exact quantitative solution of a ... problem, but its general functional character".[4] His new method, he believed was "more useful biologically than the more exact method used hitherto". With the approximation method, instead of trying to find various concentrations in a cell, the equations were developed using "average concentrations, average gradients, etc." Rashevsky was thus able to proceed with his mathematical analysis of the complex biological systems without falling into difficulties of unsolvable differential equa-

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[1]Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*.

[2]Ibid; ———, "Organismic Sets: Some Reflections on the Nature of Life and Society."

[3]Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*; ———, "Organismic Sets: Some Reflections on the Nature of Life and Society."

[4]Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*; ———, "Organismic Sets: Some Reflections on the Nature of Life and Society."

M.M. Shmailov, *Intellectual Pursuits of Nicolas Rashevsky*, Science Networks. Historical Studies 55, DOI 10.1007/978-3-319-39922-5_3

tions and to provide results he considered to be eventually "useful biologically".[5] Using the new methodology, he was no longer constrained to only spherical cells or those holding ellipsoid shape. He used vague terminology such as "approximate length and approximate width" for generally oblong cells and "approximately radial" for more symmetrical shapes. Exact shapes and dimensions were no longer relevant as these had no affect on the functional relations involved in the problem.

Cell Division and Cellular Aggregates

As discussed in earlier chapters, the first problem Rashevsky tackled was the problem of cell division, which he continued to pursue into the 1940s. While "numerous suggestions have been made by biologists as to the possible mechanism" governing the division of the cell, none of the theories represented "rigorous mathematical theories" which according to Rashevsky would lead to "verifiable quantitative conclusions".[6] The methodology Rashevsky introduced, at first alone and later on with his group, was that of "approximation", studying the problem *in abstracto*. The underlying assumption was that as many "conceivable mechanisms" of cell division as possible should be examined and this plethora of possibilities would eventually enable them to "decide which of the conceivable mechanisms is likely to be actually operating in the living cell".[7]

Faced with the complexity of the actual cell and the fact that "no two cells are exactly the same", Rashevsky abstracted the fundamental unit by approximating it to either a sphere or ellipsoid.[8] He then explored the commonalities between all cells. One was the cell's electrical charges. For several years Rashevsky drew from his field of expertise, theoretical physics, and addressed the problem by looking into the possible effects of electrical forces. Cells were known to carry an electric charge. Charges of the same sign were known to repel one another. It was only "natural", as Rashevsky reminisced several decades later, to "assume that the electrically charged parts of the cell repel one another, causing the cell to divide".[9] Knowing the magnitude of the electric charges in the cell and their mechanical strength led to calculating the possibility of these forces affecting the division of the cell.[10]

Another possibility was proposed by Rashevsky by suggesting that all cells exhibit another property that leads to their division. In reviewing possible causes

[5]Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*; ———, "Organismic Sets: Some Reflections on the Nature of Life and Society."

[6]Rashevsky, "From Mathematical Biology to Mathematical Sociology."

[7]Ibid.

[8]Ibid.

[9]Ibid.

[10]Ibid.

for cell division, attention was given to a "diffusion of metabolites into and out of cell", a function Rashevsky found to be common to all cells manifesting signs of life.[11] Explaining the logic that led to the new theory, Rashevsky wrote: "every living cell metabolizes so long as it shows signs of life. There is a constant flow of different substances, the metabolites. . .the medium through which any substance diffuses offers a resistance to the diffusion flow, a resistance expressed by the finiteness of the coefficient of diffusion. According to Newton's third law, the diffusing substance must exert a corresponding force on the solvent".[12] Rashevsky's conclusion was "that every living metabolizing cell and its immediate surroundings represent. . ..'diffusion drag' forces".[13]

The new theory led to three mathematical problems that had to be solved. The first was to calculate the forces in terms of diffusion forces acting within the cell. Next, calculations were made for the distribution of the diffusion flows for a cell of a given shape and size which, together with the first set of calculations, imbued one with knowledge as to the magnitude and distribution of the forces within and around the cell. Finally, calculations were made to determine whether the found forces could deform the cell and eventually lead to division. At first the calculations took into account only one substance produced as a result of cells metabolism at a certain rate. The calculations showed that the cell will divide under the action of diffusion drag forces when its radius exceeds a critical value, or when the radius *r* of the cell satisfies the inequality:

$$r \geq \left(\frac{3M(2D_i + D_e)\gamma}{RT\mu q} \right)^{1/3}$$

Where M is the molecular weight of substance, D_i is its coefficient of diffusion inside the cell, D_e is its coefficient of diffusion in the external medium, γ is the surface tension of the cell, R is the gas constant per mol, T is the absolute temperature and μ is a constant, characteristic of the colloidal structure of the cell. The role of q is played by the average rate of metabolism, based on the rate of oxygen consumption.

The "approximate" conclusion was that "when certain inequalities between different constants characterizing a cell are satisfied, the cell becomes mechanically unstable under the action of the diffusion drag forces, if it exceeds a critical size. Assuming that the average size of the cell is 3×10^{-3} cm, the size should decrease

[11]Ibid.; ———, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*; N. Rashevsky, "Advances and Applications of Mathematical Biology", *Bull. Amer. Math. Soc. 47 (1941), 7*, no. 9904 (1941); Rashevsky, *Mathematical Biophysics Physico-Mathematical Foundations of Biology, Vol. 1 and 2*.

[12]Ibid.

[13]Ibid.

with increasing rate of metabolism. In plotting cell size against rate of metabolism on a logarithmic scale, the slope would be $(-1/3)$".[14]

When the critical size of the cell is exceeded, it becomes unstable, starting to elongate. The cell eventually constricts in the equatorial plane, the radius of the constriction gradually decreasing to zero until the cell is divided. This phenomenon was known and described qualitatively before Rashevsky even approached the problem. It was known that de-membraned *Arbacia* eggs elongate during the process of division assuming the shape of the dumbbell. A constriction then occurred in the equatorial plane, as a result of which it was believed that the cell divided. However, nobody performed any measurements of the phenomenon, thus the time course of elongation and constriction was unknown.

Apparently, New York University's physiologist Robert Chambers had accelerated micro films of the phenomenon that he was known to present at his lectures.[15] Rashevsky obtained and examined the films, subsequently writing to Chambers to ask about the time intervals. Chambers replied that the intervals were not recorded and "were most likely non-uniform".[16] Rashevsky, with the assistance of his first doctoral student Herbert Landahl and Gaylord Yong, had successfully developed the quantitative description of the phenomena that suited the observed one; however, data was missing to verify the theoretical conclusions. Rashevsky and Landahl contacted Ralph Buchsbaum, a physiologist in the University of Chicago's Zoology department and Woods Hole Laboratory, with whom he collaborated on several projects between 1939 and 1944.

The first set of experiments was designed with algae. Regrettably, Buchsbaum reported that the measurements had failed due to an "unapparent reason". The group then proposed working with sea urchin eggs—*Arbacia*. Buchsbaum, working with the assistance of Robert Williamson, performed the experiments and periodically sent Rashevsky the films. Rashevsky guided Buchsbaum through numerous experiments, indicating the time periods and stages at which the pictures should be taken as well as the substances that should be used.[17] The results of the experiments performed based on this plan were remarkably compatible with the theory.

Rashevsky's *in abstracto* treatment was progressively graduating into more realistic cases. For a while, it seemed that his "speculations" on cell division were even propelling mathematical biology of the cell to its final stage of development, where theory predicts reality. Yet the fortunes of the new field remained precarious due to occasional slips. For example, in 1949, the Swedish geneticist Gunnar Östergren noted to Rashevsky in a private correspondence that his theory of

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[14] Ibid.

[15] N. Rashevsky, *Some Medical Aspects of Mathematical Biology* (1964); R. Chambers, "Structural and Kinetic Aspects of Cell Division", *Journal of Cellular and Comparative Physiology* 12, no. 2 (1938). pg. 6.

[16] Rashevsky, *Some Medical Aspects of Mathematical Biology*; Chambers, "Structural and Kinetic Aspects of Cell Division", Pg. 6.

[17] Correspondence with Buchsbaum, June 11, 1940, Box 8, Folder "Buchsbaum", NRP-SCRC.

cell division overlooked mitosis that takes place in the nucleus during cell division, and Rashevsky was forced to admit that his new theory was "inadequate."[18] Such gross oversights presented setbacks that led to severe criticism of Rashevsky's approach and forced him again beyond the boundary of the world of experimentalists.

Thus the theory was abandoned in terms of its relation to cell division, yet adapted by Landahl to explain and predict other phenomena, such as the rate of oxygen consumption of the cell from the surrounding medium.

Central Nervous System (CNS)

The first half of the twentieth century was hailed a golden age for American neurophysiology. Fittingly, Rashevsky exhibited an interest in the peripheral and central nervous systems.[19] Mathematical treatment of the CNS was a natural extension of the hitherto treated fundamental unit—the cell, "in as much as the cell remains the fundamental unit even in the central nervous system".[20] The brain's tremendous complexity was considered to be outside the reach of mathematical biology and "seemed to make any mathematical approach hopeless".[21] Thus to simplify the problem, using the method of approximation, Rashevsky postulated the existence of certain units which he termed interchangeably "nerve elements" and "neurons".[22] The next step was to deduce mathematically the "simplest possible laws of interaction" between such abstract "neurons" in order to understand their behavior.[23] The mathematical tool employed was differential equations.

One of the postulates was the existence of two types of "nerve elements": excitatory and inhibitory. The next postulate was that the propagation of excitation is via local bioelectric currents; and finally, the "all-or-nothing" law: any nerve possesses a finite threshold, and the intensity of stimulus must exceed this threshold in order to produce excitation. Once produced, the excitation proceeds independently of the intensity of the stimulus and depends only on the physico-chemical nature of the nerve.[24] The all- or-none principle was not new in the field. It emerged in response to an accumulation of experimental evidence during the first quarter of

[18] G. Ostergren to Rashevsky, December 5, 1949, Box 8, Folder "O", NRP-SCRC; Rashevsky, Mathematical Approach to Fundamental Phenomena of Biology, n.d.

[19] Kevles and Geison, "The Experimental Life Sciences in the Twentieth Century."

[20] Rashevsky, "Mathematical Biophysics and Psychology."

[21] Rashevsky, Mathematical Approach to Fundamental Phenomena of Biology, n.d.

[22] Which did not necessarily correspond in their properties to the actual neurons.

[23] Rashevsky, Mathematical Approach to Fundamental Phenomena of Biology, n.d.

[24] Rashevsky, "Mathematical Biophysics and Psychology."

the twentieth century. Some works that bear mention are the studies of physiologists Edgar Adrian and Keith Lucas.[25]

In Rashevsky's first paper on the subject, he indicated that "since we are building a purely theoretical science, we do not consider any of the above assumptions as having a counterpart in reality." [26] Again he reiterated that he was "investigating all possible cases, and merely for the sake of definiteness we begin with this one".[27] The system he was building was "in width rather than in depth", on some level laying the first stone towards a systematic development of the mathematical biology of the CNS.[28] Rashevsky never did develop the system in depth. Such abandonment of a project was characteristic to Rashevsky. With a dream of systematically building mathematical biology that would cover all fields of biology, he moved from one project to another without bringing it to a state where it could be confirmed or contested. Committed to his dream of "mathematical biology", he could not commit to just one field of biology, nor even to one problem.

The mathematical biology of the CNS—in the form in which it was initially developed by Rashevsky and later by H. D. Landahl, A. S. Householder and others—showed in some instances considerable success.[29] The theory has at the time quantitatively correctly represented such diverse phenomena as reaction times, psycho-physical discrimination, discrimination of intensities, color vision, conditioning and learning and even esthetic perception. Phenomena such as the perception of abstract relations and of Gestalt-invariance were also treated. Rashevsky and his group were becoming pioneers in the field of peripheral and central nervous system modeling. Using the differential equations as the mathematical tool for quantitative analysis, the two-factor theory was, in fact, a continuous theory of excitation, examining thresholds, and electrical currents.

What started as an exercise with the "purpose of merely showing that rather complicated phenomena. . .can be in principle treated mathematically" proved to be much more.[30] The models constructed by Rashevsky and his students represented several actual phenomena, showing promise when compared to empirical data. However, the situation was "puzzling". The neurophysiologists were making progress in their understanding of the interaction between neurons, and it soon became clear that it differed from the laws of interaction as presented by the "abstract

[25]Tara Abraham, Doctoral Dissertation, University of Toronto, 2002 and R.G. Frank, "Instruments, Nerve Action, and the All-or-None Principle", *Osiris* 9(1994).

[26]N. Rashevsky, "Outline of a Physico-Mathematical Theory of the Brain", *The Journal of General Psychology* 13, no. 1 (1935).

[27]Ibid.

[28]Ibid.

[29]AS Householder, "Mathematical Biophysics and the Central Nervous System", *Acta Biotheoretica* 8, no. 1 (1946); N. Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology* (Dover publications, 1960); Vol. II.

[30]Ibid.

neurons".[31] Following the all-or-none law, the interactions of the actual neurons were in fact discontinuous.

The abstract theory became unrealistic and even obsolete in lieu of the far more realistic discontinuous one presented in 1943 by the neurophysiologist Warren McCulloch and Rashevsky's youngest protégée, Walter Pitts. In their epoch-marking paper, McCulloch and Pitts—both influenced by the tradition of Rashevsky's mathematical biology—presented their model of the activities in the CNS formulated by applying logical calculus to a living system. This paper is considered to constitute the "birth" of cybernetics.[32] Based on the "all-or-none" character of nervous activity, McCulloch and Pitts describe neural events and their relations using Boolean logic. This approach led them to approximate and abstract neurons as on-off devices that either "fired" or did not. Connecting this to the "true–false" nature of propositions in logic, McCulloch and Pitts constructed hypothetical networks of excitatory and inhibitory neurons, with varying patterns of connection, and demonstrated an isomorphism with these hypothetical arrangements of neurons and the "logic of propositions."[33]

The "failure" of the continuous theory became apparent to Rashevsky immediately and in a series of papers he and H. D. Landahl tried to connect the two theories by solving the resultant paradox.[34] The paradox was eventually solved when it was found that "in all psychological experiments, such as those dealing with the reaction times, psychophysical judgments, discrimination of intensities etc, a very large number of actual neurons is involved".[35] This finding meant that although the activity of each neuron was discontinuous, over a large number of neurons, i.e., neural pathways, the activities assume a continuous form. Rashevsky's reconciliation of the continuous and discontinuous theories is based on the circumstance that a very large number of discontinuous events produce the effect of a quasi-continuity, that is, the ensemble of discontinuous elements behaves like a continuous system. For these events, the hitherto-developed differential equations were perfectly suited.

Applying the original theory, Rashevsky turned his attention to developing a theory of aesthetic perception of simple geometrical shapes and patterns. To check his theory and its predictions, Rashevsky collaborated with Virginia Brown. The

[31]Rashevsky, "Mathematical Approach to Fundamental Phenomena of Biology", n.d. pg, 244.

[32]A detailed account of McCulloch and Pitts paper and its role in the birth of cybernetics is given by Tara Abraham in "(Physio) Logical Circuits: The Intellectual Origins of the Mcculloch-Pitts Neural Networks", *Journal of the History of the Behavioral Sciences* 38, no. 1 (2002); Abraham, "From Theory to Data: Representing Neurons in the 1940s."

[33]Abraham, "(Physio) Logical Circuits: The Intellectual Origins of the Mcculloch-Pitts Neural Networks"; N. Rashevsky, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*. Vol. II.

[34]Rashevsky, "From Mathematical Biology to Mathematical Sociology"; N. Rashevsky, "Some Remarks on the Boolean Algebra of Nervous Nets in Mathematical Biophysics", *Bulletin of Mathematical Biology* 7, no. 4 (1945).

[35]Ibid.

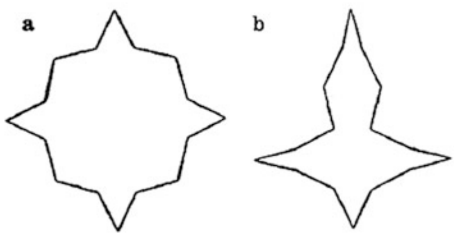

Fig. 3.1 **a**, **b**, **c** illustrate different shapes of *polygons*

difference between the theoretical values and those obtained by experiment was 5 %. Bearing in mind that the theory was developed over a year before the experiments, the results were astonishing—suggesting that the case reflected a "real prediction".[36]

Rashevsky suggested that the visual perception of aesthetic values governed by mathematical relations could be examined experimentally by measuring aesthetic values of polygons in which lengths and positions of straight lines in the plane do not vary, but angles do. This data was obtained by permuting the sides of a polygon, which left the direction and length of each side invariant. Thus, starting with the 16-sided polygon shown in Fig. 3.1a, by permutation of the sides, different shapes of polygons could be obtained as exemplified in Fig. 3.1b and c. A total of 77 polygons obtained by permuting the sides of Fig. 3.1a were used in an experimental study of their aesthetic values by the rank order method. All 77 polygons had a vertical axis of symmetry, but not all had a horizontal. All the polygons were printed on square cards, 12 × 12 cm. The length of each side was 1.2 cm. A total of 109 subjects were used. The linear relation was thus confirmed experimentally.

While the theory did provide description and prediction of certain behaviors governed by the CNS, its next natural step was to deal with interacting individuals. This natural progression led Rashevsky and his group to broach yet another field, "human relations".[37]

[36]Rashevsky, "From Mathematical Biology to Mathematical Sociology", pg. 106.

[37]N. Rashevsky, "Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomena", *Bull. Amer. Math. Soc. 55 (1949), 722–724*; ———, "Outline of a Mathematical Theory of Human Relations", *Philosophy of Science* 2, no. 4 (1935); ———, "Further Contributions to the Mathematical Theory of Human Relations", *Psychometrika* 1, no. 2 (1936); ———, "Studies in Mathematical Theory of Human Relations. Ii", *Psychometrika* 4, no. 4 (1939); ———, "Studies in Mathematical Theory of Human Relations", *Psychometrika* 4, no. 3 (1939); ———, "Contributions to the Mathematical Theory of Human Relations. Iv", *Psychometrika* 5, no. 4 (1940); ———, "Contributions to the Mathematical Theory of Human Relations Iii", *Psychometrika* 5, no. 3 (1940); ———, "Contributions to the Theory of Human Relations: Vii. Outline of a Mathematical Theory of the Sizes of Cities", *Psychometrika* 8, no. 2 (1943); ———, *Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomena* (Principia Press, 1947).

Mathematical Biology of Human Relations: Laying Down the Foundation for Mathematical Sociology

As early as 1935, Rashevsky's mathematical biology led him into the world of human behavior or sociology. Rashevsky believed that a true mathematical biologist ought to engage in the problems of social organization.[38] As he explained in his introductory paper on the subject "Outline of a Mathematical Theory of Human Relations", which was published in the second volume of *Philosophy of Science*, the rationale was thus:

> After having established a physico-mathematical theory of the fundamental properties of the cell, we have studied the interaction of several cells. This led us into two different fields. On the one hand we studied such interactions of cells, which determine the form of cellular aggregates, constituting multicellular organisms. On the other hand we studied different types of functional interactions, which deter-mine [sic!] the reactions of the aggregate as a whole to different environmental changes. While the first field leads us eventually to the theory of organic form, the second brings us to the physico-mathematical theory of behavior... The reaction of any organism is determined by its surroundings at a given moment as well as on the variations of the surroundings in its past. But the environment of an organism is in itself constituted of two parts: first of the inorganic environment and then of other organisms. Hence any mathematical theory of organismic reactions and behavior must of necessity include the study of interaction between various individual organisms.[39]

Again paying tribute to the works of Lotka and Voltera on the interactions of different species, Rashevsky emphasized the novelty of his studies as relating to "the study of interaction of organisms of the same species, and consider the more complex problem of psychological interaction, involving complex behavior."[40] Stated simply, Rashevsky was attempting no less than "a mathematical theory of human relations".

Mathematical Sociology was by no means new. The notion had popped into the minds of many predecessors. Rashevsky was well aware of such works, including a study called *Mecanique Sociale*, published back in 1910 by a Romanian scientist and politician Spiru Haret (1851–1912), and those of civil engineer of Cuban-Spanish origin Antonio Portuondo (1845–1927).[41] Underscoring that his mathematical treatment would differ from those, he indicated that he did not "intend to draw any merely formal analogies from physics in the manner of the so-called 'social mechanics'" as was done in for example in first name Haret's treatment of the subject.[42] In Rashevsky's opinion, such works exhibited a "complete lack of

[38]R. Rosen, "Anticipatory Systems in Retrospect and Prospect", *General systems yearbook* 24, no. 11 (1979), pgs. 11–23 cited in AH Louie, "Essays on More Than Life Itself", *Axiomathes* (2011).

[39]Rashevsky, "Foundations of Mathematical Biophysics."

[40]———, *Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomena*.

[41]Wilson, "Consilience: The University of Knowledge."

[42]S.C. Haret, *Mécanique Sociale* (Paris and Bucharest, Gauthier-Villars, 1910).

understanding of the true spirit of the mathematical methods in science."[43] Spiru Haret's approach entailed the application of equations from classical mechanics to social phenomena, introducing artificial concepts such as "social mass" and social inertia.[44]

Rashevsky opined that there was no reason whatsoever "why social phenomena should be described by the same equations as mechanical phenomena."[45] He believed that just as in biology, it was "[possible to have] a common physical basis" that are *sui generis* and should be described "by mathematical laws of their own", so too in social phenomena. Commensurate with his views regarding the biological phenomena, Rashevsky believed that social phenomena should be first studied in the abstract, even if there was a counterpart in reality, as this practice was the mathematical method. To those sociologists questioning his approach, he responded thus:

> If questioned as to the value of such purely abstract speculations, we shall answer as before: such has been the way of development of all mathematical sciences. In order to have a mathematical theory of concrete, real phenomena, we must first possess the abstract structure.[46]

Abstract treatment of the phenomena characterized most of his work on the subject. In an attempt to receive input on his theories, Rashevsky corresponded with the Russian–American sociologist, Pitirim A. Sorokin (1889–1968). Sorokin, then the chair of the newly established department of sociology at Harvard University (1930–1944), is considered one of the most original, important, and controversial figures in American sociology.[47]

Between the years 1930–1941, Sorokin published several volumes of his magnum opus, *Social and Cultural Dynamics,* which Rashevsky read eagerly.[48] In his attempts to transform his theories from mere abstract formulation to more realistic conjectures, Rashevsky corresponded with Sorokin to request various data.[49] Sorokin was very impressed with Rashevsky's treatment of the social behavior

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[43]Rashevsky, *Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomena*.

[44]Ibid.

[45]Ibid.

[46]Rashevsky, "Foundations of Mathematical Biophysics."

[47]Johnston, *Pitirim A. Sorokin: An Intellectual Biography*.

[48]P.A. Sorokin, "1937–1941. Social and Cultural Dynamics", *New York: American Book Company*; ———, "Social and Cultural Dynamics (New York, 1937)", *Vol. II*; ———, *Social and Cultural Dynamics: Vol. 3: Fluctuation of Social Relationships, War, and Revolution* (American Book Company, 1937); ———, *The Crisis of Our Age: The Social and Cultural Outlook* (Dutton New York, 1941); ———, *Social and Cultural Dynamics: Basic Problems, Principles, and Methods*, vol. 4 (American Book Company, 1941); ———, *Social and Cultural Dynamics: A Study of Change in Major Systems of Art, Truth, Ethics, Law, and Social Relationships* (Transaction Publishers, 1957).

[49]There is no evidence to suggest that Rashevsky knew Sorokin and their correspondence is mainly professional.

and deemed it "a very interesting work".[50] After reading the first three volumes of "Social and Cultural Dynamics", Rashevsky wrote in 1939, "I was particularly pleased to notice in reading your volumes, that some of these purely abstract relations which I have recently worked out. . ., do possess a certain analogy with some of the actual relations which you discuss in your treaties".[51] He continued in the letter to request "quantitative data. . .on the various social phenomena. The fact that I do not find any more of these data in your volumes makes me strongly suspect that such data are as yet not available at all. . ..on the other hand it is possible that in my ignorance I am asking you rather foolish and impossible questions".[52]

The type of data Rashevsky was looking for was "the relative and absolute sizes of different social classes and groups. . .for as long span of time as possible. . .say 800 B.C. to the present time"; "the ratio of the total urban to the total rural population and its variation for the same span of time"; "the number and average size of cities for different periods"; "incidents of crime, especially political crimes". . .etc.[53] Just as Rashevsky presumed, Sorokin responded that such data was not readily available and perhaps did not even exist for the periods Rashevsky was interested in studying. Rashevsky continued with his work on the phenomena of human relations and it seemed to be leave an impression, at least on Sorokin. "I found it exceedingly interesting and valuable as an abstract theory of the main types of social relationships and shifts among postulated conditions and groups. . ..translating your mathematical formulas into non-mathematical language, I think that many of them are of a great help and value even for purely empirical study of the respective social phenomena".[54]

Sorokin was impressed with Rashevsky's work to the extent that he included it in the curricula of his courses, bringing it to the attention of his students and colleagues.[55] By 1948 Rashevsky had compiled all his articles on the subject of human relations into a book entitled *Mathematical Theory of Human Relations: an approach to a mathematical biology of social phenomena*.[56] Sorokin considered the book to be "the most important contribution to a mathematical study of social phenomena for the last few decades".[57] In a personal missive Sorokin indicated that the book would be used as "a text and reference book".[58] In the same letter, Sorokin

[50] Correspondence with P. Sorokin, January 10, 1936, translation from Russian is by me. Box 8, Folder "Sorokin", NRP-SCRC.

[51] Correspondence with P. Sorokin, Box 8, Folder "Sorokin", NRP-SCRC.

[52] Correspondence with P. Sorokin, April 9, 1941, Box 8, Folder "Sorokin", NRP-SCRC.

[53] Correspondence with P. Sorokin, Box 8, Folder "Sorokin", NRP-SCRC.

[54] Correspondence with P. Sorokin, October 5, 1939, Box 8, Folder "Sorokin", NRP-SCRC.

[55] Correspondence with P. Sorokin, April 14, 1941, Box 8, Folder "Sorokin", NRP-SCRC.

[56] Rashevsky, "Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomena."

[57] Correspondence with P. Sorokin, January 24, 1948, translation from Russian, MMS. Box 8, Folder "Sorokin", NRP-SCRC.

[58] Ibid.

indicated to Rashevsky that a few months prior, during departmental considerations of possible candidates, he had suggested Rashevsky as a professor in sociology at Harvard. Sorokin reported that "all members, unanimously agreed that [Rashevsky] was top in his field and that it would have been a privilege to have him."[59] However, as they all believed that Rashevsky was "so well situated at Chicago with his institute, that there was no hope in luring him in" and just out of curiosity asked Rashevsky what would have been his response should they make the proposition.

Rashevsky's response was laconic, thanking Sorokin, saying that he was "honored and extremely flattered by the high opinion" Sorokin and his colleagues had of him.[60] Sorokin, understood this as a confirmation of the prevailing opinion that Rashevsky was well-situated at Chicago and suggested that in the future a course based on Rashevsky's studies "… may be taken perhaps jointly by this and biology departments".[61]

In the meanwhile, the department of social relations at Harvard was considering the development of a mathematical approach to the study of sociology "along the lines of [Rashevsky's] pioneering work".[62] At the time, while quite original in his work, Rashevsky was not the only one interested in developing a mathematical approach to sociology. In the mid 1940s it was becoming a trend, although practiced by few. As one contemporary critic noted: "The surge of mathematical effort in the social sciences can be a frightening thing to the unequipped. Summer institutes in mathematics for social scientists have increased the mathematical training of a number of young scholars. College mathematics teachers are planning revisions in mathematical curricula, and these revisions are influenced to some extent by the needs of social scientists, not just the needs of engineers and natural scientists."[63] But mathematical work on human relations was scarce. While statistical methods were widely used in all social sciences, these were usually employed for descriptive purposes rather than as mathematical models, suggested by Rashevsky.[64] Impressed with Rashevsky's work and interested to learn more about it, Sorokin made arrangements with his colleagues statistician and sociologists Fredrick Mosteller, Samuel Stouffer (director of the Laboratory of Social Relations at Harvard), and Talcott Parsons (Chairman of the Department of Social Relations) who shared his opinion, to extend to Rashevsky an invitation to come to Harvard as a visiting scholar for the 1948–1949 academic year. Moreover, efforts were made by Sorokin, Frederick Mosteller and others at Harvard to establish a

[59]Ibid.

[60]Ibid.

[61]Correspondence with P. Sorokin, January 29, 1948, translation from Russian, MMS. Box 8, Folder "Sorokin", NRP-SCRC.

[62]Ibid.

[63]Review by F. Mosteller, "General and Theoretical: Mathematical Thinking in the Social Sciences. Paul F. Lazarsfeld", *American Anthropologist* 58, no. 4 (1956).

[64]F. Mosteller, "Review of Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomenon by N. Rashevsky", Journal of the American Statistical Association; Vol. 44, No. 245 (Mar., 1949), pp. 150–155.

research institute for “altruism and ethics”, with a budget of millions of dollars available endowed by Eli Lilly who was a strong supporter of Sorokin[65]; Sorokin pointed out that Rashevsky’s participation in the work of such an institute would be “highly desirable”.[66] In 1949 Sorokin established the Harvard Research Centre in Creative Altruism, which he directed until his full retirement in 1959. The founding of the Centre was a product of Sorokin’s professional beliefs, with roots in Christianity and intuitivism.[67] Yet Rashevsky, who stayed in touch with Sorokin and visited the Centre at times, did not join Sorokin at Harvard.

Reviews of Rashevsky’s tome lodged criticism that was shared by many. Rashevsky’s choice to treat the subject matter in “width” rather than “in depth” was particularly criticized. As Mosteller observed in his review of the book, “The continuity is more one of method than of subject matter. The casual reader will find that the topics dodge around rather rapidly. Indeed, the book is something of a hodge-podge. It contains many early thoughts not very thoroughly worked out but apparently put down quickly as they came to mind by a rather prolific but not very elegant writer...One has the feeling that the problems were made to fit the mathematics with which the author has been successful in treating other problems, rather than making the mathematics suitable to the problem.” [68] When compared with works of others, such as Rashevsky’s friend and colleague, the physicist Lewis F. Richardson in *Generalized Foreign Politics* published 1939, Rashevsky’s in “width” rather than “in depth” treatment stands out.[69] Richardson studied the theory of stability of peace between two or more nations largely by studying the behavior of linear differential equations. He studied conditions which might lead to war and did not attempt to say when war will occur, nor when one side or the other will be defeated, nor how the action will be carried out; while Rashevsky was presumptuous enough to make attempts of such a nature. It is worth noting the contrast between the work of Richardson and that of Rashevsky, as one man takes a single topic and works it very extensively, while the other prefers to handle many topics thinly. This was characteristic of Rashevsky not only in his treatment of human behavior but as discussed above, also in biology. As observed by Tara Abraham, Rashevsky seemed to have carried such an approach from his early days in physics where in a relatively short period of time of few years he tackled a fairly broad range of topics including relativity theory, electrodynamics, photomagnetism,

[65]B.V. Johnston, *Pitirim A. Sorokin: An Intellectual Biography* (University Press of Kansas Lawrence, KS, 1995).

[66]Correspondence with P. Sorokin, April 14, 1948, translation from Russian, MMS. Box 8, Folder “Sorokin”, NRP-SCRC.

[67]I. Ponomareva, “Pitirim a Sorokin: The Interconnection between His Life and Scientific Work”, *International Sociology* 26, no. 6 (2011).

[68]Frederick Mosteller, “Review of: Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomenon by N. Rashevsky”, (Journal of the American Statistical Association; Vol. 44, No. 245 (Mar., 1949), pp. 150–155).

[69]L.F. Richardson, *Generalized Foreign Politics: A Study in Group Psychology* (The University Press, 1939).

thermionic effect and many others.[70] As one of his students Alvin Weinberg commented, Rashevsky's "canvas was so broad that he could hardly carry any of his models to crucial test, to a point of Popper's 'falsification'".[71] Such an approach certainly made it not only difficult to follow Rashevsky's scientific endeavors but also sullied his reputation to a point where his studies were rarely taken seriously. His attempt to be in a way a "super-scientists" with mathematics as his superpower certainly must have aggravated his peers.

Yet the reviews of Rashevsky's work on human relations also proved that Rashevsky at least partially succeeded in his agenda. Rashevsky's work was on some level a source of inspiration and Rashevsky a pathfinder in the field. As Mosteller further observed in his review:

> The most important thing is that a book has appeared which tries to treat a variety of social problems by means of mathematical models. That the attempts have met with varying degrees of success is not too important. The results given are certainly successful enough to encourage others to make further attempts. Indeed, some of the basic material presented here is worth extending along the lines indicated by the author and worth supplementing with practical numerical examples drawn from data.[72]

Another reviewer, the statistician and sociologists Daniel O. Price, critiqued the treatment in the journal *Social Forces* as rather broad and shallow, then offering praise for its methodology: "the methods of the book should be of interest to every sociologist whether he knows mathematics or not. It should be of special interest to social theorists."[73]

Rashevsky's next step in this direction was presented in his book *Mathematical Biology of Social Behavior*, which was published in 1951.[74] In this book, the research was based on equations derived from the theory of the nervous system governing interaction of several individuals. While in the first volume the theory was based on formal postulates, these postulates had no connection to the neurobiological mechanisms. Rashevsky's own criticism of the book was that "no matter how inadequate the treatment, the author [Rashevsky] actually uses mathematics everywhere and does not merely talk about using it. His results are, even though lacking in elegance, always specific enough to suggest explanations and understanding of different social phenomena. The expressions derived are, at least in principle, verifiable by long-range sociological observations and suggest such

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[70] Abraham, "Nicolas Rashevsky's Mathematical Biophysics", pg. 337.

[71] Weinberg, *The First Nuclear Era: The Life and Times of a Technological Fixer*. pg. 7.

[72] Frederick Mosteller, "Review of :Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomenon by N. Rashevsky", (Journal of the American Statistical Association; Vol. 44, No. 245 (Mar., 1949), pp. 150–155).

[73] D.O. Price, "Mathematical Theory of Human Relations. An Approach to a Mathematical Biology of Social Phenomena. By N. Rashevsky. Bloomington, Indiana: The Principia Press, Inc., 1947" *Social Forces* 27, no. 2 (1948).

[74] N. Rashevsky, *Mathematical Biology of Social Behavior*, vol. 256 (University of Chicago Press Chicago, 1951) D.O. Price, "Mathematical Biology of Social Behavior. By Nicholas Rashevsky. Chicago: The University of Chicago Press, 1951", *Social Forces* 30, no. 1 (1951).

observations. In many instances the road to possible improvements is specifically indicated; even where no direct reduction to neurophysics is made, specific suggestions as to how to do so are given here and there. The author hopes that many problems discussed here will find a much more adequate treatment in the hands of others."[75]

In a way, the Preface to the book was a preemptive response to the reviewers of the previous book-*Mathematical Theory of Human Relations*. To Mosteller's review, he responds: "in a newly developing branch of science there is nothing more dangerous than to fall into a rut by concentrating on one 'pet' hypothesis or method of approach. Such a procedure. . .is likely to lead to stagnation rather than to progress".[76] The book deals with phenomena such as imitative behavior, learning, motivational behavior, altruism and egotism, hedonism and their interconnections, and social classes and hierarchies and forces behind their formation.

Rashevsky realized that the domain of social sciences differs greatly from biology or physics. The ultimate evaluation of any mathematical theory is based on its ability to describe quantitatively the observed data and its ability to predict. While his work in the domain of social sciences provides an explanation, it was challenged by meeting the second criterion. Whereas in the case of mathematical biology and physics, it was possible within certain constraints to verify the theory, in social sciences the circumstance is different. As one commentator argued, "it may be necessary to wait for several generations until verification becomes possible".[77] However, in the book's final paragraph Rashevsky reminds its reader that "as scientists we are interested only in the "why" and the "how" of events. When we raise the question of what ought to be, we approach the problem not so much as scientists but as political leaders or, if a more dignified word is desirable, as 'social engineers'".[78]

Growing Up and Making a Name

As challenging as it is to establish a new academic program in a recognized area, it proved to be ever so much more so to "pioneer" the first program in a newly developing area. By its very nature, mathematical biology is an interdisciplinary arena. Successful practice in this area requires the collaboration of researchers skilled in various biological arenas ranging from physiology to zoology, scientists

[75]Ibid. Preface viii.

[76]Ibid.

[77]D.O. Price, "Mathematical Theory of Human Relations. An Approach to a Mathematical Biology of Social Phenomena. By N. Rashevsky. Bloomington, Indiana: The Principia Press, Inc., 1947" *Social Forces* 27, no. 2 (1948).

[78]Rashevsky, *Mathematical Theory of Human Relations: An Approach to a Mathematical Biology of Social Phenomena*. pg. 237.

knowledgeable in physical and chemical modeling, and scholars adept in mathematical techniques from algebra to bifurcation.[79]

Starting in 1935, Rashevsky designed and taught a sequence of courses in Mathematical Biophysics that were attended by his graduate students as well as students from various other departments on the campus. There was no teaching material suitable for his the courses and thus the reference material in these courses included reprints of Rashevsky's research and the projects of the students in his young group.[80] The deficit in teaching material, coupled with a large volume of Rashevsky's own studies and the projects of his group members encouraged Rashevsky to compile a book. Rashevsky's *magnum opus* was published in 1938 by the University of Chicago Press, entitled *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*. The book was the first systematic presentation of the field that Rashevsky envisioned; it contained over 300 pages of his research. The book will be printed in two more editions in 1948 and 1960 each being a compilation of the scientific research material of the group.

Securing a suitable venue for disseminating the group's research results proved to be challenging, further isolating them from the insiders. While Rashevsky's earlier papers were published, as indicated above, in the *Zeitschrift fur Physic, Physics, Protoplasma, Psychometrica, and Journal of General Physiology*, he was moving "from one journal to another as difficulties . . .developed."[81] Rashevsky's research was slipping between the cracks. As Weaver wrote in 1936 in support of a grant for Rashevsky so that he could publish his work:

> Rashevsky has had to submit his papers either to physics journals or to biological journals. . .In the physics journals he has had to suppress the biological interpretation and application. . .Conversely, when publishing in biological journals he is required to eliminate a large share of the mathematical. . .argument. Thus. . .his researches have never been adequately presented in a form which gives the proper emphasis to the intimate relationship between the physico-chemical analysis and the biological problems.[82]

It seemed as though the journal *Protoplasma* provided a "relatively satisfactory" outlet for publishing; however, when the cellular physiologist Robert Chambers was appointed editor in 1938, it became increasingly clear to Rashevsky and Weaver that "[the referees] did not really understand the analytical arguments in the papers of mathematical biologists" and that the editor was "not qualified to edit these papers," as summarized in one of Weaver's interviews.[83] These factors led to the formation of the *Bulletin of Mathematical Biophysics* (BMB) in March 1939, with Rashevsky as its editor. The bulletin quickly became "a classical journal in

[79]Cull, "The Mathematical Biophysics of Nicolas Rashevsky."

[80]Rapoport, *Certainties and Doubts: A Philosophy of Life*; History of the Committee, (1963), Box 2, NRP-SCRC.

[81]Weaver Interviews, July 3, 1938, RG 1.1, Series 216D, Box 11, Folder 148, RAC.

[82]Grant in Aid, October 14, 1936, RG 1.1, Series 216D, Box 11, Folder 147, RAC.

[83]Weaver Interviews, July 3, 1938, RG 1.1, Series 216D, Box 11, Folder 148, RAC.

mathematical biology and served as the principal publication outlet" for mathematical biologists.[84]

During the first decade of its existence, almost all contributions to the journal were submitted by Rashevsky and his students. The BMB was printed by Dentan Printing Co., at a cost which Rashevsky acknowledged were "ridiculously low . . . of something like $2.25 per printed page". The circulation was very limited, comprising 50 subscribers, primarily libraries and institutions. In its first year of existence, Rashevsky assumed many hats: editor, copyreader, proofreader, accountant, and treasurer. By the following year, the University of Chicago Press took over the publication of the journal, releasing Rashevsky from the bureaucratic duties and leaving him in the roles of editor and copyreader. Thanks to the low printing costs, it was easy to establish an annual subscription at a most reasonable US $2.50. Slowly but steadily, Rashevsky's efforts to market the journal succeeded. Within 10 years, the number of subscriptions reached more than 500. By the late 1950s, there were nearly 500 annual subscriptions sold, spreading from the east to the west coast of the United States, South America, Europe, Russia, Japan, and Australia.

By 1950 articles were submitted by more than 50 scientists from the United States and about a score from abroad.[85] With Rashevsky as its editor, BMB was considered a controversial journal by those beyond the pale of his group and close circle of followers. Rashevsky's student Robert Rosen recalled that Rashevsky passionately believed that anonymous refereeing was practically a consummate evil; thus if somebody was asked to referee a manuscript for the BMB, his comments and name were transmitted verbatim to the author. This policy created more than one embarrassing "faux-pas" and arguably created a situation in which referees outside the group would not agree to referee if their name was to be disclosed to the authors.[86]

Most of the reviewers were well-known to scientists who wanted to publish in the BMB; the short list included Landahl, Rashevsky's right-hand man, who was later joined by Rapoport and Ernesto Trucco. Referees outside the group were chosen on occasion to evaluate various papers, depending on their nature. During the journal's first decade, less than a dozen contributors were either graduates of Rashevsky's program or mathematical physicists interested in his approach.

With the university providing institutional means for supporting his research, Rashevsky was—by 1940—already a well-established figure. He was working primarily on the problems of cell division, cellular growth, cancer, nerve conduction, and the central nervous system. Starting with the physico-mathematical theory of a single cell and ending with the complex system of the brain, which Rashevsky believed to function "due to the interaction of billions of cells", he was now considering interactions between various individuals which in turn compose a

[84]PK Maini, S Schnell, and S Jolliffe, "Bulletin of Mathematical Biology—Facts, Figures and Comparisons", *Bulletin of Mathematical Biology* 66, no. 4 (2004).

[85]Lucas, *The Cullowhee Conference on Training in Biomathematics*. pg. 14.

[86]Rosen n.d, Weinberg Alvin-personal communications, 2004.

large group. This latter consideration led him into the domain of social sciences.[87] His primary objective concerning the latter was not to examine superficial analogies but rather to establish and formulate mathematically different type of relations that govern social units.

Rashevsky received two more invitations to present at the Cold Spring Harbor Symposia on quantitative biology; he was solicited to discuss his mathematical theories of excitation and conduction in nerves (1936) as well as the permeability of cells (1940). Rashevsky and his group were in close contact with on-campus and off-campus experimentalists, discussing biological problems, gathering data, guiding their experiments, and verifying the mathematical theories. This collaboration intensified to such an extent that Carlson not only came to terms with Rashevsky, but in fact, as Rashevsky proudly reported to Weaver, "became quite an enthusiastic backer of Rashevsky's group".[88]

On the institutional front, Rashevsky was advancing. Despite Carlson's gusto, he made it clear that Rashevsky did not have a place in his department, where experiments came before theory. Thus, by late 1938 Taliaferro was indeed forced to form a separate "department" under the Section of Mathematical Biophysics to accommodate Rashevsky and his group. Although the Section acted as an independent body within the division, it was officially under the auspices of Carlson's Department.[89] At the faculty meeting of the Biological Sciences Division on December 7, 1939, "the Dean [Taliaferro] announced that Mathematical Biophysics would be administered by the Department of Physiology. The Dean also announced that on recommendation of the Divisional Committee he would appoint a committee to advise the Department of Physiology on the curriculum and requirements for [a] degree in Mathematical Biophysics".[90]

By now Rashevsky's mathematical biology had "grown out" of what he viewed as the first abstract stage of development that he argued "must of necessity remain on [a] purely theoretical level, without any apparent contact with actual data."[91] Having laid down what he believed to be the "theoretical foundations", he envisioned his field heading towards the "most important stage of development of a theoretical science."[92] This stage, according to Rashevsky, occurs when the

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[87] N. Rashevsky, "Physico-Mathematical Aspects of the Gestalt-Problem", *Philosophy of Science* 1, no. 4 (1934); Rashevsky, "Foundations of Mathematical Biophysics"; ———, "Mathematical Biophysics and Psychology"; ———, "Physico-Mathematical Methods in Biological and Social Sciences"; ———, *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*.

[88] Weaver Interviews, June 18, 1940, RG1.1, Series 216D, Box 11, Folder 148 RAC.

[89] By the beginning of the 1938–1939 academic year, the department of psychology was shifted from the Division of Biological Science to the Division of Social Sciences, and a new place had to be found for some of its members, including Rashevsky.

[90] BSD Division Minutes recited in a note dated 15.07.1964 in Beadle Papers, Box 327, Folder 4, BOP-SCRC, University of Chicago Library.

[91] This approach was reflected in the first edition of Rashevsky's magnum opus "Mathematical Biophysics: Physico-Mathematical foundations of Biology", University of Chicago Press, 1938.

[92] Rashevsky to R. G. Gustavson, September 24, 1945, Box 214, Folder 6, HOP-SCRC.

science "not only explains already known phenomena, but mathematically *predicts* new ones, and suggests ways for new experimentation".[93]

With Rashevsky and his group making "considerably greater contact with the laboratory" than it had in the past, Rashevsky felt compelled to publish a summary of their work in *Advances and Applications of Mathematical Biology* in 1940 to present to insiders the progress Rashevsky's research was making.[94]

Into the 1940s, Rashevsky's *in abstracto* treatments of various phenomena were progressively graduating into more realistic cases. Thus, in the case of cell division, his "spheres" were now incorporating some physiological characteristics of the living cell and accounting for the roles of 'cytoplasmic streaming' and catalysts. Rashevsky was now working with experimentalists, designing and guiding their experiments and at times leading them to inspiring breakthroughs in their research.[95] For a while, it even seemed as though his "speculations" on cell division were going to bring mathematical biology of the cell to its final stage of development, where theory can indeed predict reality.

In 1940 the Faculty of the Division of Biological Sciences, headed by Taliaferro approved a special training program toward a PhD degree in Mathematical Biophysics. As summarized in the minutes of the meeting of May 16, 1940: "the Dean announced that this program for the PhD had been approved by the Department of Physiology through which this degree will be granted. It was moved and seconded that this report be accepted. The motion was carried".[96]

The training program brought together scientists, primarily graduate and postdoctoral students, who were interested in doing research in mathematical biology. Rashevsky managed to develop a disciplined approach for honing the competency of interdisciplinary researchers. The program was designed to provide students with proficiency and competence in both biology and the physico-mathematical sciences. Students were required to complete a heavy curriculum that entailed at least 1 extra year of study. The program was designed for 18 quarters for those who did not have a bachelor's degree; those graduates would receive the bachelor's degree and then complete general coursework totaling at 54 courses. Those entering the program with a Bachelor's degree were required to take a total of 36 courses and at times even more, as compared to the 27-course requirement in the Division's other departments. Proficiency in mathematics or theoretical physics was a prerequisite for those interested in entering the program.[97]

[93]Ibid.

[94]N. Rashevsky, *Advances and Applications of Mathematical Biology*. Chicago, Univ. press (1940); A.S. Householder, "Review: Advances and Applications of Mathematical Biology", vol. 15, *National Mathematics Magazine*, (1941): 384–386.

[95]One example is Rashevsky's work with biologist F.C. Besic on the effects of corrosion of the teeth enamel by various acids.

[96]BSD Division Minutes recited in a note dated 15.07.1964 in Beadle Papers, Box 327, Folder 4, BOP-SCRC.

[97]Discussion with Don Makuleky, December 23, 2012.

The list of courses available to the students included 22 courses in Biological Sciences: these included requirements such as the Chemistry of Cell Constituents; The Concept of Organism; Life Processes; Evolution and Maintenance of Species; Cellular Functions; Integration within the Organism; General Physiology; Electrical Aspects of Cell Structure, and The Neuron. Other courses required by the program fell under Mathematical Biology: Elements of Mathematical Biology; Mathematical Biophysics of Cell Growth and Multiplication; Mathematical Biophysics of the Central Nervous System; and Mathematical Biophysics of the Central Nervous System: Applications. Students were required to take a course in Introductory Psychology and a course on Vertebrate Zoology. Students could choose additional courses in Anatomy, Bacteriology, Botany, Physiology, and Biochemistry.

Additional 23 courses in Physical Sciences, including the following ones, were required, too: Qualitative Analysis; Qualitative Analysis for Students in Biological Sciences; Elementary Physical Chemistry I and II; Chemical Thermodynamics I and II; Mathematical Physics I and II; Mechanics I, II, and III; Statistical Mechanics; Electricity and Optics I and II; Quantum Mechanics and Atomic Structure I, II, and III; and finally Thermodynamics and Statistical Physics I, II and III, or any equivalent of these courses taken in Chemistry. In Mathematics the required courses were Calculus I, II, and III; optional courses were Differential Equations and Introduction to the Theory of Functions. Finally, a student also had to take nine research courses in Mathematical Biophysics.

As Rashevsky documented, "at the faculty meeting at which the program was approved, doubts were voiced whether any student would wish to take such a heavy program of studies, which entailed at least one extra year".[98] Rashevsky responded thus: he "[was] not interested in quantity production of PhD's but rather in their quality". Rashevsky believes that "if a person intended to devote his lifetime to work in Mathematical Biophysics, he would not mind an extra year or two of study to make himself ready".[99] This program was the first to offer a degree in mathematical biology and would remain such up until the 1960s. Interest in the program was expressed by young scholars from around the world, including South America, Australia, Holland and Switzerland. Indeed, the program granted over 26 PhD's under Rashevsky's mentoring. In 1941 the first student to receive a PhD in mathematical biology was physicist Herbert (Herb) D. Landahl. He received a master's in physical sciences at the University of Chicago, where he first became acquainted, and later on enchanted, by Rashevsky's program. Fascinated by Rashevsky's approach, Landahl joined Rashevsky and stayed on his team first as a student and later on as a colleague and friend for nearly three decades.

At this juncture, it seemed that Rashevsky's endeavors were on stable ground. He had assembled a group of brilliant students pursuing his research and methodology; he had established a publishing body, had published two books as well as

[98] Memorandum on "History of the Committee", (1963), Box 2, NRP-SCRC.
[99] Ibid.

dozens of papers; and he had an organized institutional venue under the Section of Mathematical Biology to solidify his standing in the field and that of his group. Yet, a tide of political turmoil would pull out the solid ground from under his feet.

By the end of 1941, the United States was mobilizing for war. Hitler attacked the Soviet Union in June of that year, the Japanese attacked Pearl Harbor on December 7, and 3 days afterwards, Germany declared war on the United States. Hutchins was compelled to declare the university an "instrumentality of total war".[100] It became the university's duty to contribute to the war efforts and as Hutchins recognized, "it made it hard, perhaps very hard, perhaps impossible to carry out our basic duties".[101]

Despite the disruption, the University of Chicago made valuable contributions to the war effort. While it accommodated thousands of sailors and soldiers assigned to specially designed training courses, it also changed the face of its Division of Physical Sciences and of the Section on Mathematical Biophysics. Chicago became a principal center of research and experimentation in the field of nuclear energy. Initially Chicago played a marginal role, and the first contract with the federal government ran from January to August 1941.[102] The contract was to investigate beryllium for enhancing the pace of atomic reaction. The group of scientists involved in the project—Carl Eckhart, Samuel Allison, Robert Millikan, Arthur Dempster, and William Zachariasen—was considered to be the "very top of their respective fields".[103] Within about a year all groups working on the chain reaction were congregated in Chicago under Arthur Compton's leadership. Compton, a Nobel prize laureate in Physics, decided to bring together all forces working on the chain reaction into the University's Metallurgical Laboratory (Chicago's code name for the atomic project, also known as MetLab or Met). A dazzling array of talent was gathering in Chicago. Some of the sub-groups were led by Enrico Fermi, Eugene Wigner, John Wheeler, Ed Cruetz, Edward Teller, and a few others. In addition, Robert Oppenheimer visited from time to time. Oppenheimer and his group at UC Berkeley were working on the design of the bomb with Chicago until the Chicago headquarters were transferred to Los Alamos in November 1942.

The MetLab took over Eckhart Hall, which was the mathematics building at the University. Physicists and mathematicians were absorbed like a sponge. The Met project engaged the campus physicists so completely that they withdrew from their ordinary campus activities between 1941 and 1945.

[100]R.M. Hutchins, *The State of the University, 1929-1949* (1949):Public Relations Office, University Archives, Pg 13–14; and McNeill, *Hutchins' University: A Memoir of the University of Chicago, 1929-1950*. Pg. 102.

[101]Cited in H.S. Ashmore, *Unseasonable Truths: The Life of Robert Maynard Hutchins* (Little, Brown, 1989), pgs 222–223 and McNeill, *Hutchins' University: A Memoir of the University of Chicago, 1929-1950*. Pg. 102.

[102]WH McNeill, *Hutchins' University: A Memoir of the University of Chicago, 1929-1950* (University of Chicago Press, 1991).

[103]Weinberg, *The First Nuclear Era: The Life and Times of a Technological Fixer*.

Among the physicists engaged by Eckhart and Compton were members of the Rashevsky group. Eckhart and Compton were very familiar with the abilities and training of the group members, choosing to recruit them to perform calculations in which they were well versed. Rashevsky's group diminished in size considerably during the war. Weinberg, Young, and Bloch were recruited to work on the atomic bomb project and never returned. Landahl was called in to work on the Toxicity Project to understand the way inhaled chemical agents are distributed throughout the body, and Householder and Weinberg were recruited for secret projects at Oak Ridge and eventually stayed there. Rashevsky deemed these men to be "war casualties".[104]

Weinberg, Young, and Bloch were in the service of the country investigating the use of beryllium as a possible moderator for a uranium chain reactor. Trained to build and solve differential equations dealing with various phenomena of diffusion, the men seemed to be invaluable to the efforts. However, this time the diffusion was not of metabolites in the cell but rather of neutrons. The mathematics of the two was identical; the only difference was scale. Whereas the distances to collision in the cell were in angstroms, the situation with neutrons was on a larger scale; they travel several centimeters before colliding with another atom.[105]

In the 1940s Rashevsky began to set aside all of his affiliation with the physico-mathematical world. By 1942 he had distanced himself from the mathematics and physics community and even resigned from the American Physical Society, deeming it no longer appropriate to present his work to the physics community. Insisting that he be approached as a mathematical biologist, he considered his work as "too biological" and out of place amongst papers in modern physics.[106] Yet the physicists viewed Rashevsky's resignation as "a highly important matter", in the words of the Treasurer of the American Physical Society, who described it in this fashion in a letter to Karl Darrow, President of the American Physics Society. Rashevsky was characterized by Darrow as "an extraordinarily able person who has created a whole new domain of Bio-Physics through his applications of mathematics to biological problems". Darrow was urging the Society Treasurer to induce Rashevsky to remain with the Society for "he would be a great strength to in the post war years when we can expect heavily[sic] numbers of Bio-Physical papers".[107] Although the physico-mathematical world mourned the loss of its associate, Rashevsky was not well-received by the biological community.

[104] History of the Committee, (1963), Box 2, NRP-SCRC.

[105] Weinberg, *The First Nuclear Era: The Life and Times of a Technological Fixer*. pg. 9.

[106] Correspondence with Arthur Compton, 1942, Box 8, NRP-SCRC.

[107] Ibid.

Making “an Honest Woman” of Mathematical Biology

Rashevsky’s ambitions and political and administrative skills became apparent when he decided to emancipate the Section from the Department of Physiology and convert it into an independent entity within the Division of Biological Sciences during the mid-1940s. This step was concurrent with administrative changes at the Division of Biological Sciences and the University as a whole. In 1944 Taliaferro advanced to the post of advisor to Chancellor Hutchins and the new dean-elect (1944–1947) of the Division of Biological Sciences. Taliaferro was then promoted to the Dean of Faculties (1947) and the post of vice president was occupied by microbiologist Roland Wendell Harrison. Paleographer Ernst Colwell (1901–1974) was elected President in charge of administrative affairs and chemist/pharmacologist R.G. Gustavson acted as his vice president.

Rashevsky, now a tenured professor, was set on taking additional steps to turn his vision into an independent, organized entity, and a profession. On several occasions he pleaded with the University administration to establish an *Institution* for Mathematical Biology. Despite the fact that the Section was acting under the Department of Physiology, it was already an independent unit with its own budget In terms of the administrative infrastructure, Rashevsky was satisfied, or so he claimed.[108] However, consistent with his grandiose sense of self-importance and preoccupation with power and recognition, he was clamoring for “de jure” *recognition* of the field, which in his opinion had been established “de facto” for almost 10 years.[109] Rashevsky was requesting to set an institute for mathematical biology. This request was inspired by the steps taken towards institutionalizing and professionalizing theoretical physics in Continental Europe when its official recognition manifested itself, amongst others in a setting of separate Institutes for the experimental and the theoretical approaches.[110] The function of the institute for mathematical biology would be “to train research workers in this field [mathematical biology] so that when other institutions become interested in it, we could supply well trained mathematical biophysicists”.[111]

To consider Rashevsky’s request for the institute, the administration needed to understand his vision. In 1945 Rashevsky was requested to submit a brief statement of accounts. This memorandum—entitled “The Organization of Research in Mathematical Biophysics” and submitted to Gustavson on September 24, 1945—intended to provide a survey of the field and present Rashevsky’s vision of the profession and the Institution. Rashevsky opens the memorandum with a general survey of the field, giving credit to Lotka, Volterra, Haldane and others who had contributed to the purpose of developing “a rational theory of biological phenomena”. This brief acknowledgement of the works of others was followed by a defense

[108]Rashevsky to R. G. Gustavson, September 24, 1945, Box 214, Folder 6, HOP-SCRC.

[109]Ibid.

[110]Rashevsky to R.W. Harrison, December 27, 1945, Box 214, Folder 6, HOP-SCRC.

[111]Rashevsky to R. G. Gustavson, September 24, 1945, Box 214, Folder 6, HOP-SCRC.

of the work performed by the University of Chicago group which Rashevsky asserted, merited "credit for the first successful attempt at a systematically developed theory, which covers a very wide range of biological phenomena".[112] He continued to explain and defend his mechanistic and deductive methodology:

> In its first stages such a development must of necessity remain on a rather purely theoretical level, without any apparent contact with actual data. In order to understand the ultimate hidden mechanism which underlies a given directly observed phenomenon, we must first investigate purely theoretically all conceivable mechanism. A comparison of the quantitative mathematical deduction of such a study with available data will then show which of the many possible mechanisms actually is at work.[113]

This first abstract theoretical stage remained practically a solo act from 1927 until 1938, with Rashevsky as its main participant and contributor. As Rashevsky explained, during that time "the theoretical foundations were laid down sufficiently solidly and appropriate mathematical methods were developed".[114] The method he was referring to was that of approximation which he developed in 1938, published as an appendix to his *Mathematical Biophysics*.

This period was followed by the second stage, in which mathematical investigations could be directly compared with experimental data. Citing the works of Landahl and Householder, Rashevsky presented results of the comparison of theory and experiment. In one of the figures presented in this memorandum Rashevsky illustrated Landahl's research on the variation of oxygen consumption of a cell with oxygen pressure.[115]

The third stage of development of a theoretical science, argued Rashevsky, was when the science "not only explains already known phenomena, but mathematically predicts new ones, and suggests ways for new experimentation", a more advanced stage that according to Rashevsky had been reached by mathematical biophysics. In connection with the third stage, Rashevsky mentioned Landahl's study of equations for the rates of elongation and constriction of freely-dividing cells. Landahl completed his theory that preceded the experimental data; after the completion of the work, biologist Ralph Buchsbaum of the Department of Zoology performed the experiments, measuring over 50 cells. The results showed a correlation with the theoretical predictions. Also presented was Householder's theoretical work on enzyme activity which was verified experimentally by Dr. G. Gomori of the Department of Medicine.[116]

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[112] Ibid.

[113] Rashevsky to R. G. Gustavson, September 24, 1945, Box 214, Folder 6, HOP-SCRC.

[114] Ibid.

[115] More on this work, note Landahl, H. D. "Mathematical Biophysics of Cell Respiration." *Growth* 1 (1937): 263–77; and Landahl, H. D. "Mathematical biophysics of cell respiration II." *The bulletin of mathematical biophysics* 1, no. 1 (1939): 1–17.

[116] Householder, Alston S., and George Gomori. "The kinetics of enzyme inactivation." *The bulletin of mathematical biophysics* 5, no. 3 (1943): 83–90.

Rashevsky continued to present the state of mathematical biophysics and describe the steps taken thus far to establish the field, including the formation of the Section and founding the *BMB*. In terms of the academic endeavors Rashevsky mentioned the Friday Afternoon Seminars. He underscored that discussions at the seminars "concern a great variety of experimental fields, and are intended to establish contact between our work and experimental fields, and to serve the general purpose of integration of science".[117]

The final point in the memorandum concerned the budget. With the University supporting Rashevsky's research, the Section depended on the budget of the department of physiology. He had to go through Carlson to receive funds to hire assistants and associates. Turning the section into an autonomous body would entail a separate budget that would "enable [the group] to handle long range problems" and "expand the permanent personnel".[118] It would also emancipate the Section and provide Rashevsky with the power to make his own decisions, steering his program in the direction of interest to him without constraints which might be imposed by the department of physiology and Carlson as its chair.

After reading the memorandum, Colwell wrote this to Gustavson on December 10, 1945:

> To the non-scientist Rashevsky makes a convincing case. I think it should be well to submit this [the memorandum] to Harrison for an extended comment; and that we should then decide whether to make *an honest woman out of mathematical biophysics*.[119]

The memorandum was submitted to Harrison. Harrison was thus far not convinced. Unfamiliar with Rashevsky's work, the dean needed to be persuaded as to the utility of the type of research performed by the group and in particular, its applicability and connection to experimental work. Later that month Rashevsky responded to Gustavson, this time enclosing yet another memorandum entitled "Relation of Mathematical Biophysics to Experimental Work" as requested by Dean Harrison. Rashevsky wrote the memorandum after a couple of conversations with Harrison on the subject.

In that missive, Rashevsky explained:

> All natural sciences begin with observation of facts. Later on the observation is made under controlled sometimes artificial conditions, thus leading to the development of the experimental method. As our knowledge of facts obtained through observation and experiment accumulates, we begin to ask not only for the facts as such, but also for the relations, especially causal relations, between those facts. The answer to those questions is the function of theoretical science…and rudiments of theoretical thought are to be found at the very beginning of every natural science. With the development of a science, the theoretical end of it becomes more and more important… The usefulness of the theoretical approach has been proved beyond any doubt… A natural science can develop fully only when theory and experiment go hand in hand. It is important to emphasize that theory is not merely subservient to the experiment, but that it has a standing of its own.[120]

[117]Rashevsky to R. G. Gustavson, September 24, 1945, Box 214, Folder 6, HOP-SCRC.

[118]Ibid.

[119]Colwell to Gustavson, December 10, 1945, Box 214, Folder 6, HOP-SCRC.

[120]Rashevsky to R.W. Harrison, December 27, 1945, Box 214, Folder 6, HOP-SCRC.

Rashevsky continued to argue that it is "true, theoretical science cannot exist without experimentation".[121] However, he believed that a purely experimental science cannot develop without theory, asserting that "the experimental and theoretical methods are equal partners in the development of every natural science."[122] Rashevsky believed that the questions in biology had become complicated and reached a stage where one person could not deal with both the theoretical and experimental side of a problem; as such, cooperation between the theoretician and the experimentalist was crucial.

In Rashevsky's words:

> In the early days when experiments were relatively simple and the mathematics involved not too complicated, a scientist would work on both experimental and theoretical aspects of his problem. In the last decades a certain division of labor took place, and usually a scientist devotes himself to either the experimental or the theoretical study of his problem.[123]

Rashevsky explained that "even when a scientist chooses to specialize in one of the two methods, it does not mean that he should remain aloof from the other" and that "*a cooperation between experimentalists and theoreticians is essential to the development of science*. An experimenter should be guided by theory and for that purpose he needs at least some knowledge of the latter. A theorist should keep in touch with experiment and in order to fully appreciate the latter, should have at least some training in it. *Specialization in experiment and theory does by no means prevent cooperation between both*" and that "a really full cooperation is secured only through personal contact". That sort of contact can be achieved only when an experimental scientist and a theoretical scientist become "spontaneously… interested in the same problem to secure the best benefit of cooperation". To this end Rashevsky strongly believed that "the work of the theoretician should not be required to adhere to problems which can immediately be verified experimentally" because that sort of limitation would "handicap…if not completely [wreck]" mathematical biologists and would lead to a situation where experimental science would suffer as much as the theoretical would. Rashevsky concluded thus: "limiting in any way the freedom of theoretical research of any member of this group who does independent work would defeat the very purpose of organizing this group as such".[124] Filled with a sense of entitlement, and seizing the opportunity, Rashevsky used his highly persuasive rhetorical skills to plead his case with the administration, which obviously had little understanding of his scientific program. In a way he was taking advantage of this lack of real understanding to promote his agenda of establishing an independent organization, where he would have the power to make decisions and promote his agenda.

After considering the memorandums, Gustavson voiced the administration's decision that it was not the "time to organize a separate department of Mathematical

[121] Ibid.

[122] Ibid.

[123] Rashevsky to R.W. Harrison, December 27, 1945, Box 214, Folder 6, HOP-SCRC.

[124] Rashevsky to R.W. Harrison, December 27, 1945, Box 214, Folder 6, HOP-SCRC.

Biophysics."[125] Echoing Rashevsky's views on Mathematical Biology, Gustavson stated that Mathematical Biophysics should not exist separately from other departments' because "just as Mathematical Physics is attached to the Department of Physics, so at least for the time being, Mathematical Biophysics should be attached to one of the departments of Biology if for no other reason than to give the department of Mathematical Biophysics the opportunity to have contact with experimental work".[126] In contrast to the opinion Rashevsky had formed of Gustavson, the latter reassured him that he did not have any concerns "about the fact that Mathematical Biophysics does not carry out any experimental work" and stated that he believes "that this experimental work can be carried out through other departments".[127] Gustavson added that the current decision did not preclude the possibility that the administration would change the status of Mathematical Biophysics "a year from now."[128]

And thus it was. On February 26, 1948, the faculty of the Division of Biological sciences voted to create an interdisciplinary departmental Committee on Mathematical Biology with the power to grant PhD's in the Division of Biological Sciences. This Committee was to:

> . . .supersede . . . the Section on Mathematical Biophysics of the Department of Physiology. It is the understanding that the personnel and the activities of the new committee will be the same as that of the old [Section]. The Divisional action merely recommends (1) the establishment of the committee as a semi-autonomous unit directly under the Division of Biological Sciences rather than directly under the Department of Physiology; and (2) a change of title from "Mathematical Biophysics" to "Mathematical Biology". It is felt that the latter title will more accurately reflect the continuing activities of the committee and will distinguish it from the Institute of Radiobiology and Biophysics. It is my understanding that this action must now secure the approval of Central Administration before Dr. Rashevsky can be informed that the Committee on Mathematical Biology has indeed been authorized as a semi-autonomous unit at the Division of Biological Sciences.[129]

The motion was carried out, indicating that "the faculty was aware that this committee is already in existence; and that since its activities have been in effect autonomous, it would be more realistic to recognize its autonomy as an interdivisional committee of the Division of the Biological Sciences."[130] The motion was seconded and passed (by voice vote) without dissent.

Rashevsky viewed this decision as a receipt of "a final divorce decree from the department of physiology".[131] The name change from *mathematical biophysics* to *mathematical biology* was related to two key factors: it seemed more appropriate than mathematical biophysics because, as Rashevsky asserted, "the central nervous

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[125]Gustavson to Rashevsky, March 28, 1946, Box 214, Folder 6, HOP-SCRC.

[126]Ibid.

[127]Ibid.

[128]Ibid.

[129]Merle Coulter to Harrison at the central administration on February 28, 1948. HOP-SCRC.

[130]Ibid.

[131]Rashevsky to Bennet, BOP-SCRC, University of Chicago Library.

system work is doubtful biophysics but unquestionably mathematical biology"; and the second reason was to "avoid confusion with the institute of radiobiology and biophysics".[132]

By the late 1940s, Rashevsky and his department were attracting considerable attention as the only place "where the student is free to study mathematics and biology."[133] The Committee's primary function was "to train research workers in this field so that when other institutions become interested in it, we could supply well-trained mathematical [biologists]".[134] It seemed that as the administration recognized the group as an independent unit within the division, Rashevsky was on his way to realizing his vision.

By the time the committee was formed, Rashevsky's equations spun webs over cellular biology, dealing with problems of cellular metabolism and division, neurophysiology dealing with problems including the excitation and conduction of nerve impulses, the structure and the function of the central nervous systems; embryology, ecology, the form and locomotion of animals and even psychology and sociology, dealing with the integration of aggregates of human beings. None of the subjects was developed in depth and followed through by Rashevsky. He approached each subject with one purpose to show that it can be treated mathematically. The rest was up to his students. This was consistent with his plan to systematize the entire field of biology. Convinced that this could be done within 25 years, and perhaps hoping to achieve it within his life time, he had to jump from one filed to another to make this happen. The subjects were approached superficially, as it is unreasonable that one person, not trained in biology, and learning it "on the go" could have the comprehension and appreciation of the complexity of the subjects he was experimenting with using his paper and pencil as instruments.

The fact that Rashevsky was a theorist was often held against him. Because he did not carry out experiments to verify or support his theories, he was accused of what his former student, Alvin Weinberg called-"vague scientific irresponsibility".[135] But "Rashevsky was strictly a theorist" and a proud one.[136]

[132]Correspondence with Householder. March 12, 1948, Box 7, Folder "Householder", NRP-SCRC.

[133]Weaver Interviews, February 22, 1951, RG1.1, Series 216D, Box 11, Folder 150 RAC.

[134]Letter from Rashevsky to Gustavson, 24 September, 1945, Box 137, Folder 6, HOP-SCRC.

[135]Ibid.

[136]Ibid. pg. 7.

Chapter 4
Breaking Through the Iron Curtain

The ability to sustain one's research hinges to a significant degree on the agenda and resources of the institute in which one conducts the research. After the establishment of the Committee, it would seem that at least institutionally, Rashevsky's endeavors were en route to success. First under the Section and later as the chairman of the Committee, Rashevsky prioritized rebuilding the staff which had diminished during and after WWII. The first student to join Rashevsky in 1946 was Anatol Rapoport, a Russian émigré and mathematician who had a PhD from the University of Chicago. Another member that joined the group 2 years later was statistician Hyman Garshin Landau. John Hearon, a biochemist and member of the staff of the National Cancer Institute in Bethesda, also joined Rashevsky in 1948 and received his PhD the following year. Alfonso Shimbel joined during the same time period and received his PhD in 1950. Scientists from outside the US were also showing interest in Rashevsky's program; the first graduate student to join in 1948 was George Karreman of Leyden, Netherlands, who receiving his PhD in 1951. That same year, Ernesto Trucco arrived from Switzerland and received his PhD in 1954.

The scientific pursuits of the group members spanned the gamut of biological sciences. Landahl continued to work on toxicity, focusing in particular on the dynamics of particulate material retained in different parts of the human respiratory system. When dealing with toxic substances, mathematical calculations were the only resource available in determining the possible effects of these in varying amounts. Landahl's work on the mathematical aspects of drug kinetics continued to interest him after the war for several years, resulting in a number of significant publications. Landahl was also interested in the dynamic of how malarial parasites are removed from blood streams and the central nervous system, with an emphasis on neuronic loops.

Hearon concentrated on the steady state kinetics of biological systems, which also formed part of his doctoral thesis. Shimbel contributed to the study of the central nervous system with an interest in the biophysics of learning. Karreman shared an interest in the peripheral and central nervous systems, studying in

M.M. Shmailov, *Intellectual Pursuits of Nicolas Rashevsky*, Science Networks. Historical Studies 55, DOI 10.1007/978-3-319-39922-5_4

particular the biophysics of excitation. Trucco continued Rashevsky's work on imitative behavior. George Schmidt worked on mathematical theory of capillary exchange as a function of tissue structure; he received his PhD in 1952. Clifford Patlak joined the group in the early 1950s and contributed to the study of the orientation of organisms; he received his PhD in 1953. Arthur Bierman received his PhD in 1954 for his work in chemical kinetics of spatial systems, concentrating on mitochondria and behavior of enzyme systems.

Although the group was prolific in its intellectual and research output, the backbone of Rashevsky's vision—namely, his institution—was in danger. Three factors contributed to the sense that his project was facing perilous times: (1) a change of administration within the division and the university; (2) the university's poor fiscal situation; and (3) the "Red Scare" an anti-communism movement which was directed at un-American activities and affected Rashevsky's committee as several of its members were believed to be pro-communism. Still, there was another underlying factor. Despite the growth in the number of students and associates and despite the rise in publications and contributions to Rashevsky's mathematical biology, there appeared to be no suitable place for Rashevsky's ideas within the division. At this stage, lacking association with the physical community and being largely out of step with the experimental biological community placed Rashevsky at a serious disadvantage.[1] What was Rashevsky's scientific agenda during this period of institutional struggle?

In Search of the Holy Grail: Discovering Form and Relations in Biology

During the period of 1948–1960, Rashevsky continued to research and publish in various areas. All of the articles that emerged planted seeds that were to flourish abundantly in the coming years.[2] Yet Rashevsky did not feel as if his mission and vision had been achieved: "While in some directions the method of further conquest is already indicated by merely following and extending present techniques, in some other directions the adoption in the future of new methods and techniques is strongly indicated".[3] Rashevsky came to the realization that "oversimplification of the problem must have a limit, beyond which the problem becomes completely distorted and unreal and a further simplification of the present" approximation

[1]A. Scott, *The Nonlinear Universe: Chaos, Emergence, Life* (Springer Verlag, 2007).

[2]N Rashevsky, "Physicomathematical Aspects of Biology", *Science* 132, no. 3440 (1960); Rashevsky, *Mathematical Biophysics Physico-Mathematical Foundations of Biology, Vol. 1 and 2*.

[3]N Rashevsky, "Outline of a New Mathematical Approach to General Biology: I", *Bulletin of Mathematical Biology* 5, no. 1 (1943).

method "would likely exceed that limit". He thus decided that "something else is to be done".[4]

Yet again Rashevsky felt the need to analogize the matter to physics, even though at this point he considered himself more of a biologist than a physicist.

> [A physicist] does not doubt that even the most complex mechanical or electromechanical phenomena are ultimately reducible to activities of individual atoms. Nevertheless, in studying for instance the complex electric circuit, a physicist does not fall back on the equations of the electron theory, but uses some general *formal* principles in his computations. It is quite true that at present almost all such formal principles can be shown directly to be deducible from atomic theory. But there was a time when this was not known, and yet those formal principles were used just as safely.[5]

He thus decided to search and propose formal principles that would advance the development of a theory of complex biological phenomena. The two principles Rashevsky formulated and believed to constitute a part of the permanent foundation of mathematical biology were the principle of organic form and that of relational biology. This need for innovation, the impetus for the creative act can be seen as his need for a continuous proof of his worth as a creator. In a sense it replenished his sense of self-importance and this relentless force, in turn, would distinguish his creativity from that of others.

The question that preceded the first principle was: "Why are organisms, both plants and animals, shaped as they are, and how can we describe and explain mathematically their shapes?"[6]

His logic was clear and simple. Mathematics provides the tools to represent the shape of any curve, its surface, or its volume. Thus if presented with a dog, in principle, equations of the surface which bind the shape of the dog can be found. However, such equations might be of little use because they would not only be tremendously complex but also apply only to the specific dog observed. Mathematics allows for the introduction of parameters that might permit simplification of the task and applicability of the equations to a larger number of observed specimens. This however, would require consideration of an infinite variation of the small details which are part of the shapes of dogs.

However, we are capable of distinguishing different types of animals, such as a dog from an elephant; and an elephant from a giraffe, and we do not analyze all the details between their shapes. According to Rashevsky, what we note, what strikes us, are the "over-all gross differences".[7] Looking at a silhouette, we might confuse the shape of the dog with that of a wolf or even a small deer, but we would not confuse them with a young bear or a baby elephant.

In Rashevsky's quest for an answer to the question what determines the shape of an animal or a plant, he viewed the organism as a machine designed by nature to

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[4] Ibid.

[5] Rashevsky, "Outline of a New Mathematical Approach to General Biology: I."

[6] N Rashevsky, *Mathematical Principles in Biology and Their Applications* (Thomas, 1961).

[7] Ibid.

perform specific functions. Thus he was "naturally led to expect that in the process of evolution only such designs will survive which are particularly well adapted to perform the prescribed functions under given conditions".[8] In forming his principle he further incorporated the production of energy to be used by the specific organism and named it the principle of optimal design:

> For a set of prescribed biological functions of prescribed intensities an organism has the optimal possible design with respect to economy of material used and energy expenditure needed for the performance of the prescribed functions. [9]

Realizing that perhaps the design of an organism is not necessarily "absolutely optimal", Rashevsky substituted the word "optimal" with "adequate". Thus he had his first fundamental principle in biology—the principle of adequate design.

Perhaps the most fundamental of Rashevsky's works at this time was a project that marks *the* shift in Rashevsky's intellectual trajectory. During the late 1940s and the 1950s Rashevsky's intellectual course was in no less of turmoil than his academic one and was about to undergo an extremely radical change. By the 1950s Rashevsky's own work was expanding, spreading to the problems of metabolism, brain functions, cardiovascular and cardiopulmonary functions as well as to a host of problems beyond physiology and even biology, including sociology, history and psychology. Although it was via a struggle, Rashevsky's work was finally receiving recognition by governmental agencies, foundations, and commercial bodies.[10] Nevertheless, Rashevsky was growing uneasy.[11]

Rashevsky was puzzled by what life is. In 1944, in Erwin Schrodinger's well-known book *What is Life*? he makes an important statement at the beginning of the chapter titled "Is life based on the laws of Physics?": "from what we have learnt about the structure of living matter, we must be prepared to find it working in a manner that cannot be reduced to the ordinary laws of physics".[12] This proclamation was echoed by Rashevsky many times in the 1950s, and in 1954 he changed direction in his research agenda and perhaps his view of what life is and why it is the way it is. He was finally recognizing and embracing the complexity of the life sciences.

What precisely stimulated Rashevsky's change of direction is unknown. Admittedly, Rashevsky was preoccupied with the question "Is the concept of an organism as a machine a useful one?"[13] In an article eventually published, using this question

[8]Ibid.

[9]Ibid.

[10]Rashevsky and his group received grants from the National Institute of Health, the National Science Foundation and the U.S. Air Force, including a training grant of over half million US dollars for training mathematical biologists as will be further discussed.

[11]Rosen, *Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life*. Pg. 111.

[12]E. Schrödinger, *What Is Life?, Rev. Ed* (Cambridge, England: University Press, 1967).

[13]Galley proof dated 13.11.1954 of an article by N. Rashevsky, "Is the Concept of an Organism as a Machine a Useful One?", *The Scientific Monthly* 80(1955).

as its title, in *Scientific Monthly* in 1955, he shared his feelings regarding the future of biology:

> . . .although in certain parts of biology, as, for example, some aspects of form, the mechanical analogies are likely to prove very fruitful, I do not think that the future of biology lies in too strong an emphasis of the analogies between organisms and machines. . .It. . .seems to me that if we are to map successfully complex organisms upon some complex electromechanical structures, we will have to be guided in our design of those structures by our biological knowledge, thus creating a logical circle. To whatever concept biological phenomena will be found to be isomorphic, I do think it will be different and more general that that of a machine. Biology is still awaiting its Einstein, who by a stroke of genius will map the complex organismic phenomena onto a known physicochemical, physicomathematical, or purely mathematical structure.[14]

By the mid-1950s Rashevsky came to realize that the reductionist treatment of physiology had led him to lose sight of the organisms themselves, so he proposed a new and grander scheme. Considering the development in observation techniques, multiplication of the experimental data, the richness and variety of the components, and the relationships that concur to form a biological reality, the reductionistic approach now appeared inadequate.[15] While reductionism had in effect allowed for the description of biological systems in terms of separate elements and functions, the same approach was now causing the biological systems to become detached from the complex environment in which they function and that determines their characteristics. It was becoming increasingly difficult to organize the accumulation of abundant—and at times, redundant—data into a coherent body of knowledge. In order to describe complex realities, scientists needed a methodology that would organize and unite the objects under study. It was, in fact, the environment of the systemic approach.[16]

Laying foundations for his new approach, in 1954 Rashevsky explained the deficit of his previous agenda:

> . . . a direct application of the physical principles, used in the mathematical models of biological phenomena, for the purpose of building a theory of life as an aggregate of individual cells is not likely to be fruitful. We must look for a principle which connects the different physical phenomena involved and expresses the biological unity of the organism and of the organic world as a whole.[17]

Rashevsky abandoned the mechanistic approach. He was in search of a "useful" theory that would not only be convenient but also allow for prediction. Rashevsky

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[14]Galley proof dated 13.11.1954, Ibid.

[15]A. Louie, "Categorical System Theory", *Bulletin of Mathematical Biology* 45, no. 6 (1983); AH Louie, "More Than Life Itself", (Ontos Verlag, Frankfurt, 2009); Louie, "Robert Rosen's Anticipatory Systems"; ———, "Essays on More Than Life Itself"; Rosen, *Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life*.

[16]Hammond, *The Science of Synthesis: Exploring the Social Implications of General Systems Theory*.

[17]N Rashevsky, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology", *Bulletin of Mathematical Biology,* 16, (1954), 317–348.

did not define "useful" in a classical utilitarian or practical sense. For Rashevsky, "the convenience or usefulness of a theory or theoretical concepts [was] measured by the savings it effects on our mental effort, which is needed to correlate the theory with experience."[18] It was this "economy of thought" that constituted for Rashevsky the measure of a theory's usefulness. Thus, whereas, according to Rashevsky, in physics it is possible to explain all physical phenomena from the viewpoint of the old concepts of absolute space, time, and motion by piling one hypothesis upon another, it still leads to theoretical mess which does not provide real insight into the complexity of the phenomena. On the other hand, following Einstein's ideas of relativity of space, time, and motion and the use of the concept of a four-dimensional space-time manifold, complex phenomena receive a simple, mathematically elegant explanation. Thus the relativistic concepts are convenient and useful as, according to Rashevsky, they allow for achieving "the economy of thought".[19]

Understanding the change in Rashevsky's attitude towards reductionism in biology lies primarily in understanding his struggle with the question, "What is Life?". For Rashevsky, answering that question primarily meant ascertaining the extent to which a living organism could be thought of as a machine. Naturally, the question cannot be answered with the matter-of-fact "it's a machine". Rashevsky's rejoinder to the question whether an organism can be described as a machine—or in his words, "is a particular, specified organism, or a clearly specified part thereof isomorphic to a given specified machine?"—was based on a "scientific" analysis of the matter. (Scientific meaning that the evidence used should withstand "the acid test of scientific reliability").[20] While within a "small, sharply circumscribed range" the answer to the question is "yes", isomorphism does not express itself when observing the organic world as a whole (no finite concatenation of mathematical equations describing separate functions/structures of an organism yields something that must be organism).[21] In fact, Rashevsky was now propagating "throw away the physics and keep the organization". He radically departed from the fold of mechanism.[22] He was now adopting the holistic approach.

The holistic approach emphasizes the organizing relations and highlights the concept of emergence, the idea that phenomena arising out of the functional interaction of component parts of an organism or system are more complex than the parts themselves and cannot be explained on the basis of the parts alone.[23] While debates on holistic, organismic thinking versus the reductionistic,

[18] Ibid.

[19] Rashevsky, "Is the Concept of an Organism as a Machine a Useful One?"

[20] Ibid.

[21] Rosen, *Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life.*, pg. 280.

[22] Louie, "More Than Life Itself".

[23] Hammond, *The Science of Synthesis: Exploring the Social Implications of General Systems Theory*. Pg. 32.

mechanistic thinking occupied biologists and philosophers of science as of the early decades of the twentieth century, the importance of organismic thinking resurfaced in the mid 1950s, with the revival of discussions of reductionism that accompanied the emergence of molecular biology.[24] Although Rashevsky did not use the terms "holism" and "organicism", he was clearly influenced by the discussions. During the late 1940s and the 1950s theoretical framework of general system theory in which holistic thinking was a dominant approach, was being developed by Ludwig von Bertalanffy. Bertalanffy and Rashevsky were well acquainted and Rashevsky was responsible for bringing Bertalanffy to the US as a Rockefeller Foundation fellow in 1937–1938 at the University of Chicago. Moreover, Rashevsky's former student and close family friend Anatol Rapoport for whom Rashevsky secured a position in 1952 at the newly established Ford Foundation Center for Advanced Study in the Behavioral Sciences (CASBS), was in 1954–1955 working closely with Bertalanffy at the CASBS. Interestingly he was also the founding member of the Society for General Systems Research which was established in 1955.

Bertalanffy and system biologists saw the general system theory as arising out of a growing tendency toward integration in both natural and social sciences. Presenting his views to the wider audience in 1948, the general system theory for Bertalanffy was a "new paradigm" that was being elaborated mathematically in terms of non linear equations and in terms of "verbal formulations", since he recognized that certain aspects of reality cannot be described using mathematical language. Organismic approach for Bertalanffy was to focus primarily on the principles and laws related to the organization of the living organisms. The goal of the general systems group in the 1950s was to overcome the fragmentation of knowledge and build bridges between the various ways of understanding the world. The new paradigm was to "cut across traditional disciplinary boundaries and provide models for integrating the physical, biological, psychological, and social sciences".[25] The intention was to survey through the organizational and functional similarities in systems at all levels and to search for a general principle uniting the systems.

While Rashevsky's new theoretical framework was very close to the new paradigm suggested by Bertalanffy, Rashevsky never credited him nor did he discuss his work in any of his publications on the subject of relational biology. An assumption that Rashevsky was not familiar with his approach does not seem to be reasonable. Rashevsky was in contact with Bertalanffy during at least two periods of time prior to 1954: 1937–1938 and 1946–1949. During the first period, as indicated above, with Rashevsky's assistance, Bertalanffy received a Rockefeller Fellowship to study mathematical biology with Rashevsky. The contact during the

[24] S. Sarkar, *The Biology and History of Molecular Biology: New Perspectives*, vol. 183 (Springer, 2001); Ruse, *The Philosophy of Biology*; Hull, *Philosophy of Biological Science*; Rosenberg, *The Structure of Biological Science*.

[25] Hammond, *The Science of Synthesis: Exploring the Social Implications of General Systems Theory*.

second period was of a more personal and collegial nature, Bertalanffy approached Rashevsky seeking his help in finding an academic position in the US.[26] It is more reasonable to assume that Rashevsky was influenced subconsciously, and viewed the new direction as a natural development of his scientific thought. Another explanation might be that Rashevsky viewed him as a philosopher. While Bertalanffy studied mathematical biology with Rashevsky and his group, Rashevsky considered his attempts in mathematical biology as a "complete flop".[27]

The person Rashevsky credited for the transformation of his approach was the English biologist Joseph Woodger, who was a proponent of the organismic conception in biology. Rashevsky credited Woodger's *Axiomatic Method in Biology*, published in 1937.[28] Woodger emphasized the logical foundations of biological concepts, especially genetics and the qualitative relations inherent in biological phenomena. For him the *axiomatic method* was aimed at "[providing] an exact and perfectly controllable *language* by means of which biological knowledge may be *ordered*".[29] With the assistance of Alfred Tarski and Rudolph Carnap, Woodger developed a framework for his new language based on *Principia Mathematica* by Alfred North Whitehead and Bertrand Russell published in 1910. Rashevsky believed that the difference between the approach of Woodger and his own was primarily in the subject-matter treated rather than in the basic methodological points of view. He repeatedly and proudly explained that "the reason for this difference was perhaps best formulated by Professor Alfred Tarski in a conversation with the author [Rashevsky]... [Tarski] remarked: "The difference between Woodger's approach and yours is due to the fact that Woodger is interested in the logical, while you are interested in the *biological* aspects of the problems." Rashevsky adopted this distinction and believed it "to be very true".[30]

In Rashevsky's *Topology and Life* he publicly rejected his hitherto-dominating view of the nature and purpose of mathematical biology. Rashevsky argued that "although in the last two decades mathematical biology has achieved considerable success in many directions, in others, which are of prime *importance to the*

[26]Correspondence in Folder "Bertalanffy", Box 2, NRP-SCRC; During the second period Bertalanffy contacted Rashevsky, indirectly, through Rashevsky's former students, seeking for help in leaving Austria. Bertalanffy and his family lost all their positions when their house was burnt down with all their possessions during the Vienna Offensive, launched by the Soviet 3rd Ukrainian Front in 1945 to capture Vienna. For over a year (1946–1947), pulling on his connections in universities around the US, Rashevsky corresponded with his friends and colleagues trying to find any position that would allow Bertalanffy to leave Vienna. Expressing concern and friendship Rashevsky kept updating Bertalanffy on the inquiries he placed for him at various universities and sent him packages with books as well as purchased CARE packages (aid distributed by the humanitarian organization) to be sent to Bertalanffy and his family.

[27]Correspondence with Alvin Weinberg in 1946-7, in Folder "Bertalanffy", Box 2, NRP-SCRC.

[28]J.H. Woodger and A. Tarski, *The Axiomatic Method in Biology* (The University Press, 1937), pg. vii.

[29]Ibid., pg. vii.

[30]Rashevsky, "Organismic Sets: Some Reflections on the Nature of Life and Society."

biologist, it has been lacking in any accomplishment." [31] He was moving into the domain of biology equipped this time not with theoretical physics and its mathematics but rather with mathematical logic:

> It is important to know that diffusion drag forces may produce cell division. It is important to know how pressure waves are reflected in blood vessels. It is important to have a mathematical theory of complicated neural networks. But nothing so far in those theories indicates that the proper functioning of arteries and veins is essential for the normal course of the intracellular processes; nor does anything in those theories indicate that a complex phenomenon in the central nervous system, by eventually resulting, for example, in the location of food, becomes very indirectly, yet intimately, tied up with some metabolic process of other cells of the organism. Nothing in those theories gives any inkling of a possible connection between a faulty response of a neural net, which leads to the accidental cutting of a finger, and the cell divisions, which thus result from a stimulation of the process of healing. And yet this integrated activity of the organism is probably the most essential manifestation of life.[32]

Rashevsky was realizing that the reductionist approach he had adopted thus far dealing with the individual functions of organisms and their possible interactions led to losing sight of the organisms themselves:

> A very serious shortcoming is this: All the theories ... deal with separate biological phenomena. There is no record of a successful mathematical theory which would treat the integrated activities of the organism as a whole.[33]

According to Rashevsky, the treatment thus far had no real connection to unveiling the mystery of life:

> So far as the theories mentioned above are concerned, we may just as well treat, in fact do treat, the effects of diffusion drag forces as a peculiar diffusion problem in a rather specialized physical system, and we do treat the problems of circulation as special hydrodynamical problems. *The fundamental manifestations of life ... drop out from all our theories in mathematical biology*.[34]

He was now engaged in an unrealistically ambitious search for a *principle* that would connect the different physical phenomena and express the unity of the organisms and the organic form as a whole in biological terms.

Responding to this criticism, he imagined that some may "object that this is also a matter of time". These would state that "...when the physicochemical dynamics of a cell are worked out, the dynamics of interaction of cells, and thus the dynamics of cellular aggregates, will become possible. This will eventually lead to the theory of the organism. ... Let us, however, appraise the problem realistically. ... What are the chances within a foreseeable number of generations to even approximately master the problem of an organism as an aggregate of cells, considering that this

[31] N Rashevsky, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology", *Bulletin of Mathematical Biology* 16, no. 4 (1954).

[32] N Rashevsky, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology", *Bulletin of Mathematical Biology* 16, no. 4 (1954). Emphasis added.

[33] Ibid. Emphasis added.

[34] Ibid. Emphasis added.

organism consists of some 10^{14} cells, hundreds of different tissues, and thousands of complex interrelated structures. *Pessimism is not a healthy thing in science, but neither is unrealistic optimism*".[35]

The 1954 paper was an element in a fundamental transformation that gradually took place in Rashevsky's outlook over the years from 1944 till the end of his life. The result of this shift was a fundamental alteration in both Rashevsky's intellectual identity and his research after three decades as a leader in defining the science of mathematical biology. Rashevsky now believed that the new "relational" approach would help him understand the organisms and life itself at the level of abstraction.[36]

This new principle—coined "relational biology"—would occupy most of Rashevsky's scientific work from that point onward. The outsider had realized that separate models of biological phenomena or organisms cannot be patched together to describe the entire organic world; he was in search of a mathematical theory, a fundamental biological principle that would unveil the organization and function of the phenomena or organisms. For Rashevsky, the new principle "emphasizes the unity of the organism as a whole and the unity of the organic world as a whole".[37]

But he was not abandoning in toto the optimistic vision he had held thus far of developing a mathematical theory of life starting at the cellular level. As he wrote in a letter dated 1969:

> When 35 years ago mathematical biology was still in an embryonic stage, my students and I were . . . universalists in that field. Now . . . a specialization has set of necessity. . .I consider that the field still must be developed as a whole and that its branches are closely interconnected at least methodologically. . . I feel that the time has come to introduce into biology <u>new methods of thinking</u>. Because of their novelty they may appear to be . . . crazy to biologists. . . But somebody has to make the first unorthodox step in a new direction in order to help a future Newton or Einstein of mathematical biology. . . my introduction of the concept of "Relational Forces". . . may appear crazy. But so did many new concepts in physics.[38]

While his students and followers were now specializing in mathematizing a specific niche in biology, Rashevsky still considered himself a universalist. He was paving the road, laying foundation for those to follow him, hoping eventually to realize his vision of a mathematical biology that would correspond in its grandness to mathematical physics. He was now is search of general principles and laws akin to the principles of theoretical physics.

Rashevsky was attempting to construct a mathematical framework in which "function, organization and behavior could be directly characterized and studied, apart from any structural basis".[39] As to the structure, the character of particular

[35]Ibid.

[36]DC Mikulecky, "Robert Rosen: The Well-Posed Question and Its Answer-Why Are Organisms Different from Machines?" *Systems research and behavioral science* 17, no. 5 (2000).

[37]Rashevsky, *Mathematical Principles in Biology and Their Applications*. Pg. 81.

[38]Rashevsky to Irving Gerring, February 26, 1969, Folder "NIGMS-RSS", NRP, SCRC.

[39]Ibid.

functional or organizational constraints could be expected to "place corresponding constraints on the structures which could manifest this kind of organization".[40]

In 1931, Bertrand Russell described 'scientific process' as comprising "three main stages; the first consists in observing the significant facts; the second in arriving at a hypothesis which, if it is true, would account for these facts; the third in deducing from this hypothesis further consequences which can be tested by observation."[41] Rashevsky was following these stages one by one in his new work.

He came to realize that mathematical biology had thus far emphasized almost exclusively the all-important *metric* aspects of biological phenomena. "Every physicist and mathematical biologist knows that a qualitative statement or prediction is usually of very limited value and frequently is meaningless" since for example, "[w]ithout finding a quantitative expression for the forces which may produce cell division and without comparing them quantitatively with forces that are necessary to divide a cell, no meaningful prediction can be made".[42] He continued: "[w]hen we *observe* the phenomena of biological integration we notice, however, not quantities, varying continuously or discontinuously, but certain rather *complex relations*".[43] Through the step of observation, Rashevsky realized that "the unity of the organism and the unity of all life is expressed by just that kind of relations."[44] Rashevsky argued that "in biological phenomena relational aspects are sometimes as important as the . . .metric aspect. . .Numerous biological phenomena occurring in different organisms differ from organism to organism", with the difference manifesting both quantitatively and in "their underlying mechanisms". Yet, he asserted "in all organisms those phenomena stand in the same relations to each other".[45] Searching for food, a mate, and the avoidance of danger are perhaps some of the basic functions common to all living organisms; this set of functions is a basis for life and a response to certain stimuli. Starting with one of the smallest organisms, a paramecium, Rashevsky explains: When stimulated, a paramecium performs some relatively simple movements that either bring it into contact with food or with another paramecium, with which it conjugates. These relatively simple movements are produced as responses to simple stimuli. After coming into contact with food, the paramecium ingests and then digests it, excreting indigestible waste. It reacts to light and avoids harmful stimuli. The relatively simple responses to simple stimuli result in movements that serve either the preservation of the individual or that of the species.

A bird flying after food or after its mate performs much more complex movements that are responses to even more complex sets of stimuli. It also performs very

[40]Rosen, *Fundamentals of Measurement and Representation of Natural Systems*. pg. xiv–xv.

[41]B. Russell, "The Scientific Outlook", (London: George Allen & Unwin Ltd, 1931).

[42]Rashevsky, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology."

[43]Ibid.

[44]Ibid.

[45]Final Report on Research, NIH GM 05181, NIH restricted Folder, NRP-SCRC.

complex movements with its larynx that result in the production of sounds. The latter have a definite survival value both for the individual and the species. A set of movements as a response to a set of stimuli is present, which result in either obtaining food, a mate, or avoiding harmful situations. When food is obtained, it is ingested and digested by a series of much more complex mechanisms than those in a paramecium. But the general pattern is the same.[46]

When a human being performs even more complex movements that facilitate either the survival of the individual or of the species, the pattern is the same:

> Considering the existing evidence that thought is but a covert speech, we notice that the thinking of a scientist or inventor is another very complex form of minute muscular movements, which much more indirectly than in the case of a bird or paramecium contribute to the survival of the individual or of the species. And the composition of a love sonnet by a poet and its writing down are again another highly complex set of covert and overt motions which may result in the finding of a mate and which correspond to the much simpler movements of a paramecium that produce the same result. The purely vegetative functions of ingestion and digestion of food, and other connected phenomena, are very much more complicated in the human being, but again follow the same general pattern as that in a protozoan.[47]

Even in plants, Rashevsky observed the same pattern: "Although it is customary to say that autotrophic plants manufacture their own food, it is perfectly logical to consider the solar energy and carbon dioxide as the primitive food, taken from outside. Phenomena of phototropism involve entirely different mechanisms from those found in the movements of a paramecium or higher animals, but these movements again occur in response to a stimulus and result in a better "contact" with food—the radiant energy of the sun." [48]

Rashevsky argued that these relations go even further, noting that many features of animal and in particular human societies show similar relations. Rashevsky was not asserting that what he was describing was unknown; quite the contrary. He believed it to be "so well-known as to be apt to be overlooked".[49] While overlooked, "this correspondence … is the essential feature of the organic world and that constitutes the unity of everything living". These statements were coming from someone who up to that point declared himself a "devout mechanist".

Rashevsky was not claiming that an investigation of these observed relations should be theoretical. "[Q]uantities" is not the only "essential thing…with which mathematics deals".[50] He was moving into the domain of higher mathematics using

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[46]Rashevsky, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology"; ———, *Mathematical Principles in Biology and Their Applications*.

[47]Rashevsky, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology."

[48]Ibid.

[49]Ibid.

[50]Rashevsky, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology."

branches of topology, theory of sets, theory of groups, and in particular, the theory of relations.

While the 1954 paper presented arguments for the use of topology for representation of organisms, Rashevsky would soon move into domains of higher mathematics. In the examples of the paramecium, bird, a human being etc., to "some relatively simple property of a simpler organism there corresponds a more complex property of a higher organism". In the case of the "simple sensitive spot of a paramecium" there corresponds a "whole set of different sense organs in a higher animal". Each of these sense organs, such as an eye, for example, is in itself a "very complicated system", where the function of "vision in a higher animal consists of a large number of 'subfunctions', such as perception of color, shape, size etc." Yet all of these correspond relatively to the simple "mere sensitivity to light" in a unicellular organism. This assertion, he argued, stands true to all complicated properties of a higher organism, corresponding to simpler properties of a lower one.

Rashevsky developed the principle of "biotopological mapping", which he later renamed "biological epimorphism". Simply put, the principle states that "all the topological spaces or complexes which represent all organisms may be derived from some simplest topological space or complex. . ..by a universal pluriparametric transformation".[51] It was epimorphical mapping, known as "many to one mapping".[52] The simplest space or complex was given a name "primordial", and using this primordial as the starting point, the principle was defined as follows:

> . . .there exists one, or very few, primordial organisms, characterized by their graphs; the graphs of *all* other organisms are obtained from this primordial graph or graphs by a transformation, which contains one or more parameters. Different organisms correspond to the different values of those parameters.[53]

Rashevsky argued that within this principle, a theory of organisms can be developed by "studying different topologically interesting transformations and their properties and seeing which of them leads to the best agreement with observation".[54] Should the theory and observation fail in agreement, the theory should be revisited and changes made by additions to the biotopological maps.[55] The "primordial organism" was defined as an "organism which consists of [a] set of the most general and most inclusive properties". Rashevsky was not advocating that such "a primordial" be found in nature as "it may actually not exist".[56] This concept of "primordial" was important "not so much biologically but as an auxiliary mathematical concept" which he argued to be useful in the proof of the conclusions.

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[51]Rosen, *Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life*.

[52]Final Report on Research, NIH GM 05181, restricted Folder, NRP-SCRC.

[53]Rashevsky, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology."

[54]———, *Mathematical Principles in Biology and Their Applications*, 89.

[55]———, "Topology and Life: In Search of General Mathematical Principles in Biology and Sociology."

[56]———, *Mathematical Principles in Biology and Their Applications*, 89.

Yet the conclusions derived from the biotopological mapping were to provide "powerful stimulus for further experimental research" and did not clearly state in which organism one should expect to find a certain phenomenon.[57]

Congruent with Rashevsky's mode of research, he worked alone yet also engaged discussions with other scientists to verify his hypothesis and gather insights into the new mathematics with which he was engaged. His interlocutors in this period were Rudolph Carnap at the University of California, mathematician and logician Leon Henkin, Rashevsky's close collaborator Alfred Tarski at Berkeley, and the mathematician G.Y. Rainich at the University of Michigan. Within the Committee, the most valuable contributions to Rashevsky's theory were made by Robert Rosen, Hugo Martinez, and Ernesto Trucco. Writing his dissertation on the topic, Rosen took Rashevsky's theory further and studied the topic for the remainder of his scientific career.

Some of the verified conclusions deduced from the principle were those considered well-known facts, such as the conclusion that "in some organisms emotional disturbances affect the cardiovascular system", or the still-unverified conclusion that "there exist unicellular organisms which produce antibodies when stimulated by appropriate antigens".[58] Rashevsky considered that predictions in topological biology are in the "form of existential statements".[59] While he realized that if these statements were not verified, some might consider these as "hav[ing] no scientific value", Rashevsky considered such negative statements, of greater scientific value, as for example is the case with the "impossibility of a *perpetuum mobile*".[60]

Betting on a Dark Horse

In 1947 Harrison was promoted to the post of vice president of development, and Lowel Coggeshall was appointed dean of the division of biological sciences. Coggeshall's involvement would be instrumental to Rashevsky's vision during this period and he would play a major yet behind-the-scenes role during the final years of Rashevsky's career at Chicago. The administration deemed Coggeshall a brilliant scientist and a highly capable administrator. Coggeshall began his administrative career at the University of Chicago holding the position of Chairman of Medicine, acceding to the invitation presented by Hutchins; Coggeshall soon proved to be "a very effective and active leader".[61] His past experience in

[57]Ibid.

[58]N Rashevsky, "A Contribution to the Search of General Mathematical Principles in Biology", *Bulletin of Mathematical Biology* 20, no. 1 (1958); Rashevsky, *Mathematical Principles in Biology and Their Applications*. pg. 94.

[59]Final Report on Research, NIH GM 05181, restricted Folder, NRP-SCRC.

[60]Final Report on Research, NIH GM 05181, restricted Folder, NRP-SCRC.

[61]"Lowel Goggeshall" in E. Shils, *Remembering the University of Chicago: Teachers, Scientists, and Scholars* (University of Chicago Press, 1991).pg. 59–69.

resurrecting the medical sciences and immediate success as a leader persuaded Chancellor Hutchins that “he needed Coggeshall as dean of the entire division of the biological sciences”, a position Coggeshall would hold for nearly 16 years.[62] Coggeshall characterized by his colleagues as a “soft sell telephone diplomat” was determined to change the face of the division and lead it into the future.[63] His success as the dean of BSD was reflected in the doubling of the division’s endowment, improving the quality of the teaching staff, and boosting the research activities of the members of the biological division.[64]

Rashevsky’s adventuresome project was not cheap. He was caught in a perpetual race between income and expenditure for the Committee. Seizing opportunities as they appeared, investing in research projects outside biology (such as sociology, psychology, and even history), Rashevsky assumed the role of a salesman for his discipline. Having been raised in a family of successful businessmen, this trait seemed bred into his personality.

From the very beginning of his career at the University of Chicago, Rashevsky was scavenging for funds. From the Rockefeller Foundation through private foundations to governmental agencies, Rashevsky contacted them all; at times, he was considered by his associates more of an accomplished businessman than a scientist.[65]

Perhaps Rashevsky’s greatest fundraising attempt began in 1948. To sustain his research and that of his newly-patched-together group, Rashevsky needed financial support. Coggeshall had started to develop an aversion towards Rashevsky, resulting in the university’s declining his request for the appropriation of funds to expand his group—challenging him instead to obtain financial support independently. The first door Rashevsky knocked on was that of Rockefeller Foundation. He imagined that Weaver, who was familiar with his research, would be supportive, and so he turned to the Foundation’s Natural Sciences Division.

In Rashevsky’s interview with Weaver, he shared the complexity at Chicago and the difficult position he found himself in. On the one hand, the university was not about to provide him with any additional funds; on the other hand, the demand for mathematical biologists was increasing during the post war era. Despite Weaver’s “reservations concerning R[ashevsky]’s work”, he was sympathetic to the scientist’s needs; he indicated that as the “R[ashevsky]’s center is the most important and active center in the US and as they have already trained some excellent people he would be willing to put up $7,500 over a period of 3 years provided R[ashevsky] . . .[would be] able to raise a similar amount”.[66] This proposition set Rashevsky on a

[62]Ibid.

[63]Ibid.

[64]Ibid.

[65]Correspondence with Everett Kinsey (Howe laboratory of ophthalmology, Harvard University Medical School), Box 7, Folder “K”, NRP-SCRC.

[66]Weaver Interviews, December 20, 1948, RG 1.1, Series 216D, Box 11, Folder 149, RAC.

challenging course of fundraising. These kinds of constraints for appropriation of grants by the Rockefeller Foundation were not limited to Rashevsky's enterprise.

From its very beginning, the University of Chicago had depended upon communal endowments and financial support from foundations. As such, fundraising was a major part of responsibilities bestowed on the administration and the good reputation of the University in the eyes of the community was of importance. In 1941 Hutchins grew to understand that the only source of general support for the University during wartime would be the civic community. As emerges from the words of John D. Rockefeller Jr. (referring to himself in the third person), remarking to the trustees and the president: the Rockefeller Era was over. Now the University of the People, the institution should be in a position to raise necessary funds from the public:

> Though they [his father's and his own gifts] have been completed and it is not to be expected that further gifts from the same source will be forthcoming, this does not mean that the founder's son is any less interested in the University or its future than his father was for that is not the case. He rejoices in its present attainment and is eager for its increasing usefulness. It simply means he also feels that in one way alone can the University achieve the purposes for which it was created; that is, as the university not of a family, but of the people; wholly administered and supported by them; resting squarely on their shoulders; their responsibility alone; theirs to make as great as they will. . .[67]

It was now Rashevsky's responsibility to assume the job of a salesman and promote his committee to raise the funds necessary to sustain and enlarge his endeavor. After the interview with Weaver, Rashevsky contacted the University of Chicago administration and discussed the position and course of action with vice president Lynn A. Williams, Jr. and associate dean Morele Coultern. Rashevsky was determined to do anything and everything possible to rise to the challenge, thereby persuading the administration to submit a formal request to the foundation on his behalf. Rashevsky's Ukrainian stubbornness and rhetorical skills, traits of which he was always proud, ranked him in the eyes of Weaver as "an orator".[68] Rashevsky's persistence and his strong rhetoric were at play as will be discussed below. On March 31, 1949, the university issued a formal request and Weaver approved the conditional grant on April 13, 1949. The grant was appropriated for 3 years and slated to terminate on June 30, 1952.

Efforts to raise the matching sum from an outside source were put in motion by the vice president of development. Although matching funds was the initial goal, Rashevsky seized the opportunity to raise a larger sum of money, which allowed the

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[67]Cited in J.W. Boyer, *The Persistence to Keep Everlastingly at It: Fund-Raising and Philanthropy at Chicago in the Twentieth Century* (The College of the University of Chicago, 2004); Pg. 102 "Remarks by John D. Rockefeller, Jr. at the Citizens Dinner of the President and the Trustees of the University of Chicago", September 26, 1941, University Development Campaigns, Part 1: 1896–1941, Box 14, Folder 34. SCRC.

[68]Abraham, "Nicolas Rashevsky's Mathematical Biophysics"; R. Rosen, "Autobiographical Reminiscences", *International Journal of General Systems* 21(1992). Weaver, RG 1.1, Series 216D, Folder 147, RAC.

Committee to continue its activities comfortably. Initially setting the goal at $50,000 and later expanding it to more than $60,000 over the next 3 years, it was the Committee's largest fundraising activity.

The plan involved conducting interviews with interested donors to explain the importance of the field and raise money. Slated to do the interviews were Rashevsky and members of the administration. However, as Rashevsky would realize, the plan was not well executed. By June 1949, almost 6 months into the purported "campaign", Rashevsky wrote to Williams, outraged that the original plan was far from perfect: "upon my reporting to you at the end of December [1948] the results of my conversation with Dr. Weaver it was my understanding that your office would arrange for a large number of appointments with various individuals whom I should see either together with representative from your office or alone".[69] Yet, the administration was dragging its feet. Whereas the fundraising plan called for doing more than 20 interviews during the month of January, only five appointments took place—and none resulted in the desired effect. Although the administration had a list of 70 potential donors, not one was approached by the administration. Rashevsky worried that "at rate of 5 appointments in 6 months, the chance of obtaining any funds within reasonable time are nil". Not shy of expressing his opinion, Rashevsky wrote: "I realize that our problem may be a long and tedious one. As a scientist I am used to such problems. It is however, one thing to be patient in the face of very slow progress and another to see no progress at all".[70]

Yet, Rashevsky's fundraising concerns constituted but a small fraction of the University's overall race towards enlarging endowments from outside sources. Budgetary deficit required immediate action. Thus, alongside Rashevsky's challenges, the administration was dealing with its own pressing problem of obtaining enough funds to operate the University at large. Soon grasping the gravity of the situation, Rashevsky set his rhetorical skills in motion. He wrote to Williams, stating that in his conversation with Weaver, the latter implied his faith in the ability of "the University of Chicago to raise the necessary amount without any difficulty, if the University is really interested in doing so. . .I even begin to wonder whether we should not frankly tell them that we are not able to raise even such a small amount of money from private sources for the development of mathematical biology. . .one factor [for the apparent failure] may be. . .that you are contemplating approaching possible donors for much larger funds than are involved in our own campaign and that you do not wish to jeopardize those possible larger donations."[71] He continued, stating that while he recognized the importance of such a factor, "the scientific value of a project is in no way proportional to the financial need for it."[72] He explained that "the work of the Committee on Mathematical Biology consists

[69]Rashevsky to Williams, June 14, 1949, Box 3, Folder "fundraising", NRP-SCRC.

[70]Rashevsky to Williams, June 14, 1949, Box 3, Folder "fundraising", NRP-SCRC.

[71]Rashevsky to Williams, June 14, 1949, Box 3, Folder "fundraising", NRP-SCRC.

[72]Rashevsky to Williams, June 14, 1949, Box 3, Folder "fundraising", NRP-SCRC.

mostly of long-range projects. I have sometimes to make important decisions which are based on the estimation of the probabilities of certain events in the future." Rashevsky concluded by stating that "an estimate made by a person of your competence in that field will greatly minimize a possible error in my judgments".[73]

It seems that this communication did the trick. Brinton H. Stone was appointed as the point person in charge of the Committee's fundraising. Stone scheduled numerous appointments, designed a plan, and followed it through. By August 26, 1949, a matching grant was provided by the Lucius N. Littauer Foundations. Writing on behalf of the foundation, Harry Starr stated, "It gives me a great pleasure to advise you that upon my recommendation, our Board has approved a grant in total amount of $7,500 to the University of Chicago for the extension and development of studies and research in mathematical biology conducted by its committee on mathematical biology".[74]

With the goal set at between $50,000 and 60,000, the fundraising effort continued. The campaign entailed contacting foundations as well as private individuals. Those who did not donate justified their decision by stating that "preference is given to experimental efforts which appear to be promising to the Board [of the foundation] and grants are usually for a brief period of time in order to aid in demonstrating the usefulness of the project".[75]

Rashevsky's colleagues also intervened on his behalf. The correspondence shows that Pitirim Sorokin, Rashevsky's friend and associate at Harvard University, approached Eli Lilly in a letter dated February 14, 1949. Sorokin ranked Rashevsky as an "outstanding authority in, and the creator of, mathematical biology, overflowing into mathematical sociology", asserting that any assistance would be "help well invested and promising fruitful results".[76] This gesture reflects an interesting turn of events; approximately 1 year earlier, Sorokin had extended Rashevsky an invitation to take a position at Harvard, with Rashevsky declining because he was "well situated in Chicago".[77]

Sorokin evidently persuaded his close friend and his own benefactor Eli Lilly, who agreed to the meet with Rashevsky and his technical staff. At the meeting Rashevsky presented the outlines of Mathematical Biology; yet again he was told that the work lacked "practical applications" and that it was "highly theoretical and the practical results [which may be used by the Eli Lilly company] can only come in the relatively distant future."[78]

[73]Rashevsky to Williams, June 14, 1949, Box 3, Folder "fundraising", NRP-SCRC.

[74]Harry Starr from Lucius Littauer Foundation to Brinton Stone at Central Administration, August 26, 1949, Box 3, Folder "Fundraising", NRP-SCRC.

[75]New York foundation interview summary, February 10, 1949,, Box 3, Folder "Fundraising", NRP-SCRC.

[76]Sorokin to E. Lilly, February 14, 1949, Box 3, Folder "Fundraising", NRP-SCRC.

[77]Correspondence with P. Sorokin, January 29, 1948, translation from Russian by me. Box 8, Folder "Sorokin", NRP-SCRC.

[78]Eli Lilly to Rashevsky on February 24, 1949, Folder "Fundraising", NRP-SCRC, clearly as a follow-up to the meeting and before Rashevsky's letter following up on the meeting, dated February 23, 1949 reached him.

Rashevsky followed up the meeting with a letter to Eli Lilly, stating that he wanted to emphasize an important fact that he had failed to do in the meeting. He was now attempting to do so in the letter:

> . . .we, pure scientists, work for the sake of knowledge without regard to any applications. Practical applications usually come when least expected. The circumstance that by mathematical reasoning we can predict the course of some biological phenomena clearly shows that our research does add to fundamental knowledge for 'to know is to predict'. But knowledge is power, perhaps the only power that is greater than even the power of the atomic bomb. The increased knowledge of human nature and human behavior, which are the subjects of our study, may possibly not yield any dividends to any company, but in the long run they are sure to yield huge dividends to humanity. [79]

He concluded thus: "It is for the increase of this knowledge that we are asking the support of your endowment". [80] Nevertheless, the entreaty was met with no reply.

Other potential donors showed an interest in the program yet did not contribute any funds. The administration soon realized that asking a foundation to support Rashevsky's program might be perceived as "betting on a complete dark horse".[81] The administration decided to prepare promotional material, including newspaper clippings, charts sketching the connection between experiment and theory, as well as the prospectus that would demonstrate the success of Rashevsky's program. This array of materials apparently helped. The William Morris Foundation was the first to donate 1000 dollars. [82] In need of over 3,000 dollars to cover each assistant's annual salary, this sum was far from saving Rashevsky's enterprise.

"Mathematical Biology provides biological scientists with a usable theoretical basis for research in some of the many fields of investigation on which millions are now being spent for experimentation"; this was the first paragraph in the prospectus prepared by the University. The field of "[mathematical biology] is a difficult variety of research to finance," the exposition continued. Since "its subject matter and methods are far beyond the comprehension of all but thoroughly educated scientists. . .*.This is a gamble on brain power*. It is brain power that has consistently resulted in the greatest gains of science. We hope to find individuals and organizations with sufficient imagination, boldness and foresight to support this work."[83] The above statement illustrates that while Rashevsky was convinced of the success of his program the administration still viewed it as a "gamble". The effectiveness of Rashevsky's methods was still not clear to them nor was it convincing enough to allow them to make long term decisions, as will be discussed below.

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[79] Rashevsky to Eli Lilly on February 23, 1949, Box 3, Folder "Fundraising", NRP-SCRC, as a follow-up to the meeting.

[80] Rashevsky to Eli Lilly on February 23, 1949, Box 3, Folder "Fundraising", NRP-SCRC.

[81] Brinton Stone to Lee White at the William Morris Foundation, April 27, 1949, Box 3, Folder "Fundraising", NRP-SCRC.

[82] Lee White to Brinton Stone, June 22, 1949, Box 3, Folder "Fundraising", NRP-SCRC.

[83] Box 3, Folder "fundraising", NRP-SCRC, as well as in RG 1.1, Series 216D, Box 11, Folder 151, RAC.

While some potential donors and foundation directors were persuaded to make a donation, for others "immediate, tangible [research] results" remained the *sine qua non* for opening their wallets. As one of Rashevsky's friends tried to console him: "at the present there are so many money raising projects for thinks (sic) of urgent need that [possible contributors] do not feel able to divert anything even to something they approve of, such as your work, unless it is of immediate lifesaving character".[84] With Rashevsky interested in the purely scientific aspects of the problems rather than applicability of the theories developed in mathematical biology to experimental biology, fundraising proved tedious.

The administration assisted where they felt that they could. Each foundation was approached as if it was the "chosen one". Even Dean Coggeshall, who openly disliked Rashevsky, approached some of the foundations on behalf of the administration, perhaps pressured to do so by Hutchins who still had faith in Rashevsky. In a personal note to Dr. Roderick Heffron from the Commonwealth Fund, he wrote: "I believe [mathematical biology] belongs to that group for which there is no great popular enthusiasm-such as cancer, heart, etc, for support, but it is very important and can only appeal to foundations such as The Commonwealth Fund".[85]

Despite the well-intentioned sporadic attempts to act on behalf of Rashevsky, no one at the administrative level could evaluate the importance of Rashevsky's work. Hutchins felt uncomfortable raising money for a project whose value he could not surmise while pressed with more pressing budgetary problems.

On September 8, 1950, Hutchins paid Weaver a visit. Hutchins openly admitted that he had come to Weaver to get his opinion on Rashevsky. Apparently, Hutchins was unable to "…get, from his own people, any critical estimate as to how important or valuable was the work R[ashevsky] is doing".[86] Weavers' response was inconclusive:

> …if one went about the country and asked 20 well informed scientists who would have a presumptive interest in Rashevsky's work, … their report would be as follows. Probably five of them would say that they had no use for it whatsoever, and that they simply could not understand what R[ashevsky] is about. Of the other 15, all would agree that this is interesting and very possibly an important development. They would say that they thought that some adventure of this sort ought to be supported, and that the University of Chicago is probably a very good place to try such an adventure. About five of this 15 would be strongly confident that the adventure was going to be successful. About 5 of them would probably consider the adventure a good one, but would not rank it as really excellent nor would they be overly optimistic about the outcome. The final five would give a still lower rating, but would still say that they thought it ought to be supported and continued.[87]

Weaver did add that it "must be admitted that [Rashevsky and his group] have made some important progress". Weaver confessed that "the whole development is

[84]Ernest Zeisler, M.D. to Rashevsky June 21, 1949. Box 3, Folder "Fundraising", NRP-SCRC.

[85]Coggeshall to Roderick Heffron at the Commonwealth Fund, June 27, 1949, Box 3, Folder "fundraising", NRP-SCRC.

[86]Weaver Interviews, September 8, 1950, RG 1.1, Series 216D, Box 11, Folder 150, RAC.

[87]Weaver Interviews, September 8, 1950, RG 1.1, Series 216D, Box 11, Folder 150, RAC.

in much better shape, in point of fact, than [Weaver] would have forecasted".[88] While Weaver did not explicitly specify what exactly he viewed as the "important progress", with Rashevsky by this time having published three books, dozens of research papers, guiding an array of brilliant students, such a statement is not surprising. Weaver finally added that while he hopes that "the University of Chicago will not run out on an experiment which is, after all, going along pretty well", he does not believe that the experiment "either needs or deserves" a large inflation of staff or of support, demands with which Rashevsky had been "vigorously and strenuously" importuning Hutchins.[89]

Such importuning was reflected in conversations with Hutchins and memorandums replete with concerns and entreaties. Rashevsky believed that the 12 men on his staff were an insufficient number to realize his vision. For the group to grow, he needed funds. While the group had secured more than 14,000 dollars annually in grants, this sum—combined with the $35,000 from the regular university funds—was insufficient to support expansion. When the University announced that it was cutting all division budgets by 5 %, that depletion in funds of an already-meager Committee budget would translate into letting staff members go and inhibiting the progress of the program.[90]

Rashevsky repeatedly voiced his objections, summarizing them in an internal letter to Hutchins dated just 1 month before Hutchins resigned in December 1950. Recognizing that all departments were required to sustain budgetary cutbacks, Rashevsky underscored that there are "circumstances which make the position of mathematical biology different".[91] Rashevsky listed the circumstances, summarized below, in the following fashion:

1. The Committee is the only department in the world which engages in "organized research and . . .trains individuals";
2. Interest in the field is increasing "as manifested by the increase in the number of individuals who wish to join us";
3. Prospects of employment in other schools for the members of the committee are limited. Stating that whilst "a young man working in physiology. . ..is laid off here, [he] can find adequate position in many other schools", ". . .a man working in mathematical biology has no such possibility"

This latter point, argued Rashevsky, was on the cusp of a change, as he often fielded inquiries regarding available mathematical biologists to join other universities or even government agencies.

[88] Ibid.

[89] Ibid.

[90] Memorandum on the development of Mathematical Biology, to Hutchins November 17, 1950, Box 137, Folder 6, HOP-SCRC.

[91] Ibid.

By the end of 1949, the University of Chicago received a grant of 50 million dollars from the Ford Foundation to develop a program in behavioral sciences.[92] Cognizant of this financial influx, Rashevsky exploited the situation and pointed out in a letter to Hutchins that his group was involved in the appropriate area of study and could contribute to the social sciences. The administration subsequently contacted Sol Tax, an internationally renowned anthropologist of the Social Sciences division, to inquire as to whether Rashevsky's mathematical biology could make a "real contribution".[93] Feeling ill-equipped to answer the question, Tax turned to the economist Jacob Marschak, whom he believed to be "most qualified here [at Chicago] to answer your question". Marschak, who served then as the research director of the Cowles Commission for Research in Economics, was very familiar with the scientific exploration of Rashevsky and his group. The group members were periodically invited to lecture at the Commission, and were in close contact with the economists and social scientists, the latter thriving from an ongoing discussion with Rashevsky's group.[94]

Marschak approached Rashevsky earlier in 1949, indicating that he would like to design a plan in which Rashevsky and members of his group would address the Commission because it "may be fruitful for a larger proportion of economists to get acquainted with your work." [95] Fascinated with Rashevsky's work on the mathematical biology of Social Behavior, Marschak wrote to him that "your paper on mathematical models in social sciences [which he had presented a few months prior at Harvard] would be of great interest, as would, in fact, any other paper of your choice relevant to mathematical work in economics and social sciences".[96]

Marschak was indeed qualified to respond. His answer was an unequivocal "yes". He justified his answer by stating that "much can be achieved through cooperation of a group of "theoretical-deductive" workers with groups more familiar with specific empirical data".[97] The mode of cooperation according to Marschak would lead to predictions, an area that was not very well developed in the domain of social sciences. Theorist develops an "unambiguous...verifiable tentative model" employing tools that are not mastered by social scientists. The empirical workers will then criticize the "theorist's model explaining that this is not exactly what he

[92]Boyer, *The Persistence to Keep Everlastingly at It: Fund-Raising and Philanthropy at Chicago in the Twentieth Century.*

[93]Harrison (central admin.) to Sol Tax. December 8, 1950, Box 137, Folder 6, HOP-SCRC.

[94]M. Augier and J. March, *The Roots, Rituals, and Rhetorics of Change: North American Business Schools after the Second World War* (Stanford Business Books, 2011).pg. 66.

[95]Jacob Marschak to Rashevsky, June 25, 1949, Box 8, NRP-SCRC.

[96]Jacob Marschak to Rashevsky, June 25, 1949, Box 8, NRP-SCRC Rashevsky was invited to Harvard by his close friend, Pitirim Sorokin and Frederick Mosteller to participate in a Symposia and Mathematical Sociology in March 1949. Also invited to the symposia were John von Neumann and Norbert Wiener, each presenting their research on social sciences, followed by a panel discussion of the three. (Rashevsky to Mosteller March 2, 1949, Box 8, NRP-SCRC).

[97]Jacob Marschak to Sol Tax, January 23, 1950, Box 137, Folder 6, HOP-CRC.

had in mind".[98] This criticism would force the empirical scientist to "express himself clearer, and also to supply verifying material". The procedure would be repeated until "prediction can be achieved".[99]

Yet by the time Marschak responded, Hutchins had resigned, having accepted an invitation from the Ford Foundation to join their ranks and establish an institute for the study of social behavior. The era of interdisciplinary climate at the University of Chicago was coming to its end. Although Hutchins' enthusiasm for intellectual deviance was shared by a few, it was concurrently resisted by many. The period 1945–1960 presented difficulties for fundamental research to be brought on the map. In the institutional struggle at Chicago, the Hutchins gospel—which championed interdisciplinary scholarship—had ultimately succumbed to the disciplinary implications, resulting from an emphasis on the fundamental research.[100]

A New Reign in Chicago

Hutchins resigned from the position of University Chancellor on December 19, 1950. In terms of its fiscal situation, the University's budget had been in deficit for almost a decade. Donations were drying up, the university's corporate sponsors had been alienated, and the deterioration of the neighborhoods surrounding the campus made it difficult to attract faculty and students.[101] Stringent budget cuts were the only thing that could save the university.

Hutchins was succeeded by Lawrence A. Kimpton on April 13, 1951. The era of the Kimpton administration can be characterized as the period of the descent of Rashevsky's vision at the University of Chicago. Now the Committee was facing an ultimate question: 'to be or not to be'. Written correspondence indicates that while at the turn of a decade the administration at least seemed supportive of the enterprise attempting to raise funds for the Committee, by the middle of the decade the wind would drastically change its course.

Kimpton first joined the University in 1943 to work as the Chief Administrative Officer on the Metallurgical Project and was soon appointed Dean of Students. In 1947, he moved to Stanford, his alma mater, to assume the same position of dean of students there. Kimpton was persuaded by Hutchins, who had succumbed to the pressure of the Trustees, that Hutchins' administration must become more active on the fund-raising front. Hutchins thus offered Kimpton the newly created position of Vice-President for Development, which he accepted in 1949 and returned to

[98]Ibid.

[99]Ibid.

[100]Augier and March, *The Roots, Rituals, and Rhetorics of Change: North American Business Schools after the Second World War*.

[101]Boyer, *The Persistence to Keep Everlastingly at It: Fund-Raising and Philanthropy at Chicago in the Twentieth Century.*

Chicago in August 1950. Kimpton was a thoughtful and well-spoken person with suitable academic credentials (a PhD in philosophy from Cornell University).

Once in office, Kimpton took action immediately to try to restore financial order and plan a major campaign. The neighborhood was another concern. Housing stock in Hyde Park, most of which was built between the 1890s and 1920s, had not been maintained during the depression and war years. Older houses had been divided into smaller apartments, swelling the population and causing problems with sewers and traffic. Racial tensions increased as African-Americans moved from the old "Black Belt" areas into Englewood, Woodlawn, Kenwood, and Hyde Park. Crime was on the rise. Increasingly, faculty members chose to live in the suburbs, raising fears that the University would become a commuter campus.

Kimpton understood right away the problems that Hutchins was leaving behind. Quick action was required, and Kimpton saw himself as the one who had to act to ensure the University's survival.

Kimpton's administration achieved many of its budgetary objectives. The University had run deficits nearly every year since the Depression had hit in the 1920s. However, 3 years of budget cuts and stringent review under Kimpton brought the budget into the black by 1954. As his budget cuts took a serious toll on faculty morale and as enrollment at the College continued to plummet, Kimpton assembled a key group of Trustees and senior staff to present them with another tough-but-pragmatic plan for resolving the University's future financial trouble. Kimpton's bold strategy for returning the University to budgetary solvency was based on a survey of faculty needs, unit-by-unit.[102]

Rashevsky's Committee was one of the units chosen to be evaluated and subject to revision. Although in early 1951 Rashevsky wrote to his former student Alston Householder that "now things look quite rosy", little did he know the struggles that lay ahead.[103] By 1952 Rashevsky was able to make ends meet through contributions from private individuals, ranging from $100 to $2500 and other funds such as the William Morris and Lucius N. Littauer Foundations (who each contributed an additional $2500). The Committee budget was stable yet did not allow for expansion. Any further assistance from the Rockefeller Foundation was out of the question. As Rashevsky was trying his luck with Weaver, the latter was stating in no uncertain terms that there would be no money coming in from the foundation:

> We helped to get this activity started, and we have contributed rather steadily through the developing years... you are now receiving substantial recognition and support from other quarters and I think that this is almost without question the natural moment for us to retire from the scene...our important function is to assist in the earlier and more adventuresome stages.[104]

Yet, Rashevsky was dissatisfied with merely making ends meet. He wanted to enlarge his group—and for that he needed money. Rashevsky pressured the

[102] Ibid., pg. 117.

[103] Rashevsky to Householder, February 1, 1951, Box 7, Folder "Householder", NRP-SCRC.

[104] Weaver to Rashevsky, November 17, 1951, RG 1.1, Series 216D, Box 11, Folder 150, RAC.

administration for support in raising funds, but the administration was in the midst of its own financial crisis with a pressing need to prioritize. As John Huck, an official in charge of fundraising, explained, "in your particular case, let me say that this office is anxious to do whatever it can to aid you in obtaining financing for your work" with an emphasis put on "aiding you".[105] This was due to "the primary responsibility" of the administration which was "to obtain financing for University projects (a library, a new hospital, nurses home…) and to obtain financing for the budget in general."[106] Financing for Rashevsky's project thus became a "secondary responsibility".[107]

Huck further explained in no uncertain terms what Rashevsky had already realized years ago: "if the office approaches a certain individual for a contribution to a particular research project which will expand the activity of the University, the University as a whole has lost that individual as a donor who might alleviate our budgetary situation, or as a donor who might help bring to reality a University project of a wide scope. There is, of course, nothing startling in this."[108] Reassuring Rashevsky, he added that the administration, while initially acting in the interest of the University as a whole in an attempt to solicit "unrestricted contributions", did point out "particular men, particular fields, particular projects", requiring smaller donations and make an attempt to interest those individuals. [109]

Huck highlighted that Rashevsky's "project" has been "the only…exception" to the general rule of obtaining funds primarily to cover the University budget. Uncertain as to why this was the case, he explained: "I believe it is because we as individuals have been captivated by the story which you have to tell, and because we feel there is a certain challenge in the idea of trying to sell it to other people".[110] Yet that approach was abandoned in favor of the university's priority: to get out of its debt.[111]

With the financial situation not improving and the university seeking ways to cut the budget, the logical next step was a review of departments and projects. Coggeshall, as the dean of the Division of Biological Sciences, began in 1953 to gather information on the Committee on Mathematical Biology, including its finances. The information was gleaned from the records in the President's office and included the memorandum Rashevsky wrote to Hutchins in November 1950. Kimpton noted on the memorandum that "it's rather interesting at this point"

[105]Weaver to Rashevsky, November 17, 1951, RG 1.1, Series 216D, Box 11, Folder 150, RAC.

[106]John Huck (Office of the Secretary at the University of Chicago) to Rashevsky, on January 24, 1952, Box 167, Folder 6, KOP-SCRC.

[107]Ibid.

[108]Ibid.

[109]Ibid.

[110]Ibid.

[111]Ibid.

sending it to Coggeshall.[112] Some of Rashevsky's statements in the memo 1950 would be used against him in a missive by Coggeshall a few years later.

Coincidently, at this very same time Rashevsky was approached by Leon Robidoux of the International Foundation for Cultural Advancement of Youth with questions as to what constituted "human success".[113] The first question related to the factors that can be attributed to success. Rashevsky responded thus:

> . . .the answer depends largely on what you call success and what you consider a measure of it. I personally feel that if I can speak of any success at all, this success can be measured only by the success of the development of mathematical biology, the science to which I devoted my life time. But then, to be quite candid I must say that the success of that development depended much less on me than others. This success is definitely due to the fact that I have been and am surrounded by highly talented associates and students who are loyal and devoted to science. If they had not developed mathematical biology to the point to which it has now reached, you would most likely have never heard of me. [114]

He continued with a rather false modesty:

> . . .whether there was anything in me which made this talented group gather here, or whether this was due simply to the fact that the idea underlying mathematical biology was ripe to be crystallized in the minds of the most talented young scientists, is something which I cannot answer objectively. I believe that the success or failure of an individual is not due to the qualities inherent in that individual alone, but to a large extent is due to the interactions of the individual with the society in which he lives.[115]

The second question was regarding the nature of failure and why people fail. Rashevsky's response was that it is related to the question of success and ". . . depends on what we consider failure." He explained it by an analogy to da Vinci:

> Undoubtedly a man like Leonardo da Vinci may be considered as having been in his days a failure, for not only did he not gain the recognition which he deserved, but he was frequently ridiculed by his contemporaries. Yet from an overall historical point of view nobody would of course consider him to be a failure. The reason for his failure at that time was due to the fact that he was too far ahead of his contemporary society. [116]

Was Rashevsky thinking of himself when he wrote this last analogy? After all, he was in the habit of taking things philosophically and placing himself at the ranks of great thinkers like Einstein, Newton and others. Such continuous self-aggrandizing fueled his belief of being unique and his need for excessive admiration. He did conclude the letter with these words: "such situations have also faced a number of other outstanding scientists and scholars", ostensibly referring to himself.[117]

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[112] Kimpton to Coggeshall, November 30, 1953, Box 167, Folder 6, KOP-SCRC.

[113] Rashevsky to Robidoux March 2, 1954, Box 1, Folder "R", NRP-SCRC).

[114] Ibid.

[115] Ibid.

[116] Ibid.

[117] Ibid.

Rashevsky did everything he could to publicize mathematical biology—whether it was by publishing articles in *Newsweek* magazine or in daily newspapers like *The Herald Tribune*, lecturing around the country, or even participating in an educational program on a commercial TV station as will be further discussed.[118]

The university perceived television to be "a great new instrument which a university may use in achieving its traditional objectives: (1) acquisition of knowledge; (2) the preservation of knowledge; and (3) the transmission of knowledge."[119] During the early 1950s, Chicago's WNBQ, owned by the National Broadcasting Company (NBC), ran a series of educational programs called *Live and Learn*. As was the case in other educational programs, the value of this program was that it was recognized as affording the viewer an opportunity "to acquire and develop, consciously or unconsciously, interests and knowledge in all manner of things".[120] In 1952 *Live and Learn* became interested in mathematical biology and approached Rashevsky and the Committee. The series was entitled "Mathematics and the Science of Life".[121] As one commentator argued, the subject chosen for the new series was ". . . so abstruse and erudite that the network has been able to obtain the services of only two professors to discuss it—instead of its normal complement of three".[122]

The two professors were Rashevsky and his associate professor, Anatol Rapoport; the former spoke on "Looking at biology through mathematics" whereas the latter discussed "Mathematics as language of Science".[123] The series was aired on two consecutive Sunday mornings in May 1952. Following the series of programs, Rashevsky received numerous letters of support, stating the show was the "most interesting and instructive" and had a good "pulling power".[124] Letters were also sent to the station. A commentator reviewing the WNBQ series pointed out that "the mail response to this series is particularly unique in NBC annals. The people who pen letters about it can spell, punctuate, and even write clear, lucid sentences, uncluttered by qualifying clauses, split infinitives, and dangling participles".[125] The spokesman of the network added that the people interested in the program were the "same kind of people who are interested in the Great Books program".[126]

The TV lectures were transcribed and sent to John Huck at the central administration to be used for promotion and publicity. Copies were sent to possible donors, including Warren Weaver, the Lucius Littauer Foundation, and Hutchins,

[118]Not dated newspaper and journal extracts as well as galley proofs are found in NRP-SCRC.

[119]Television and the University, page 203. n.d. NRP-SCRC.

[120]J. Waller, "Education Via Commercial Tv", *Adult Education Quarterly* 3, no. 4 (1953).

[121]Letter to WNBQ from a viewer in L. Wolters, "Television News and Views", *Chicago Tribune* (1952).

[122]Ibid.

[123]Transcripts of the two talks are found in Box 13, NRP-SCRC.

[124]Roger Faherty to Rashevsky, May 25, 1952, Box 3, Folder "miscellaneous", NRP-SCRC.

[125]Wolters, "Television News and Views."

[126]Ibid.

now as director of the Ford Foundation. The latter responded enthusiastically to Rashevsky: “I have to admit that you did it”.[127] Rashevsky used the opportunity to emphasize to Hutchins that mere words were not enough. In a desperate need of funds to sustain his school, he wrote rather bluntly, “the fact is that I still did not do it. The proof of the pudding is in the eating and we have had nothing to eat from the Ford Foundation”. He concluded by conceding that the fact that he was unable to receive any support from the Foundation “proves [his] inability as a salesman”.[128]

The members still engaged by the committee were hit hard. Members of the Committee were on the verge of being fired due to budget deficit or resigned, and taking any job they could get. Karreman left and Schmidt and Bierman, two research associates, were let go because of a lack of budget to continue their fellowships.

Yet budgetary problems were not the only challenge threatening the composition and size of Rashevsky’s project.[129] The anti-communist campaign of the Senator from Wisconsin, Joseph McCarthy, caused a scandal that shook the US scientific community. In 1950 the U.S. declared war on Communism in Korea and McCarthy’s “witch hunt” went into “high gear”.[130] The witch hunt filled people not only with fear but also with despair, dashing the hopes of those who had pined for a resumption of the promise of the New Deal. With the University suspected of harboring subversive individuals, “financial support for the University of Chicago…was drying up. To revive it, some assurance of loyalty to ‘American values’ had to be shown”.[131] The University of Chicago was reputed to have a “leftist” faculty, and thus became a target of Congressional investigation. To demonstrate that the university was duly “American”, Kimpton required all faculty members to sign a loyalty oath and swear that they were not and had never been Communists.[132] The U.S. senatorial Jerne Committee (one of the committees investigating “un-American” activities on campuses) was welcomed at the University of Chicago. With the “red scare” on the loose, McCarthyism was rushing through the halls of the Campus and Rashevsky’s small group was affected.

Rashevsky was not under personal investigation as he was considered “a very loyal American”.[133] But it was no secret that in his committee at least one person, Anatol Rapoport, had been an active and out-in-the-open communist prior to WWII. To protect Rapoport and uphold the ideals of liberty, the members of the

[127]Hutchins to Rashevsky, June 16, 1953, Box 3, Folder “miscellaneous”, NRP-SCRC.

[128]Rashevsky to Hutchins, June 19, 1953, Box 3, Folder “miscellaneous”, NRP-SCRC.

[129]Cull, “The Mathematical Biophysics of Nicolas Rashevsky”, Abraham, “Nicolas Rashevsky’s Mathematical Biophysics”, Rapoport, *Certainties and Doubts: A Philosophy of Life*; Rosen, *Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life*.

[130]Rapoport, *Certainties and Doubts: A Philosophy of Life*.pg. 106.

[131]McNeill, *Hutchins’ University: A Memoir of the University of Chicago, 1929-1950*.

[132]Cull, “The Mathematical Biophysics of Nicolas Rashevsky”; Writing from his personal experience.

[133]Weaver Interviews, November 7, 1949, RG 1.1, Series 216D, Box 11, Folder 149, RAC.

Committee on Mathematical Biology refused to sign the oath.[134] In a confidential letter of July 1953, Edward Levi, chairman of the Faculty committee on legislative investigations, approached Rashevsky to inquire about two of his staff members: H. Landau and I. Isenberg. Other members were also under investigation, including Alfonso Shimbell.[135] While Rashevsky reassured Levi that members of his staff were competent and honest scientists, he did indicate that Landau was "left off center", although how far he did not know.

Three of the members of the committee were called to testify.[136] As the procedure continued, those invited by the Committee were questioned regarding their engagement with espionage/communism; as soon as they invoked the Fifth Amendment to the constitution, they were excused from further testimony. Their contracts were never renewed and the whole matter was dealt with in a delicate fashion.[137] As it were, Rapoport was never called to testify. With the atmosphere at the University growing "oppressive" and the Committee on the verge of disappearing, Rapoport resigned.[138] The committee was on the verge of extinction; only Rashevsky and Landahl, both-protected by the tenure, were left to guard the post of Mathematical Biology at Chicago.

As noted earlier in this chapter, after Hutchins resigned, he was offered the position of Chairman at the Ford Foundation. Rashevsky stayed in touch with Hutchins and continued to press a button close to Hutchins' heart—the Social Sciences. Rashevsky sent him material on the subject, including Rapoport's memorandum on "The Contributions of Mathematical Biology to Behavioral Science" and a list of publications by his group on the subject. He concluded the letter with a quotation from a letter previously sent to Eli Lilly: "We pure scientists work for the sake of knowledge...for to 'know is to predict'...".[139] Hutchins transferred the case to Bernard Berelson, Executive Associate at the Ford Foundation responsible for building up the Behavioral Sciences Program. Berelson explained that because it would take time to get the Program in full operation, they could not help at that stage. In the end, the Ford Foundation rejected the application for a grant.[140]

However, Rapoport's work did intrigue the Foundation. In light of his work with Rashevsky on the mathematical biology of social problems, Rapoport was offered a position in 1952 by the newly established Ford Foundation Center for Advanced Study in the Behavioral Sciences (CASBS), which he accepted. There, Rapoport

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[134]Cull, "The Mathematical Biophysics of Nicolas Rashevsky."

[135]The two names are from a letter in the restricted "Levi" Folder, NRP-SCRC.

[136]Rapoport, *Certainties and Doubts: A Philosophy of Life*.

[137]Ibid. Scholarship on the effects of the "red scare" is vast. Perhaps the most comprehensive analysis on McCarthyism in the US and in Universities is by Ellen Schrecker. Note E.W. Schrecker, *No Ivory Tower: Mccarthyism and the Universities* (Oxford University Press, New York, 1986); E. Schrecker, *Many Are the Crimes: Mccarthyism in America* (Princeton University Press, 1998).

[138]Rapoport, *Certainties and Doubts: A Philosophy of Life*, page 106.

[139]Rashevsky to Hutchins, July 16, 1951, Box 3, Folder "fundraising", NRP-SCRC.

[140]Berelson to Rashevsky, December 28, 1951, NRP-SCRC.

worked with Ralph Gerard and Ludwig von Bertalanffy to develop the General System Theory, a development that, as will be discussed, would affect Rashevsky's own scientific agenda.[141]

The struggle for sustaining the Committee was not the only one on Rashevsky's agenda. The Bulletin was also in danger. University regulations precluded the use of the regular University of Chicago funds for subsidizing journals or for payment of publication expenses. While until 1951, the Dentan Printing Company manufactured the journal at relatively low rates which could be appropriated form the Committee budget, when the University of Chicago took over the printing, costs skyrocketed and amounted to more than $2000 a year. In light of the "complexities of the mathematical type" with which journal articles are loaded, the *BMB* could not be "self-supporting" despite the fact that the journal was subscribed "all over the world: in Japan, New Zealand, Australia, India, the Near East, Western Europe and even the U.S.S.R".[142]

To save the journal, which was the primary publishing outlet for mathematical biology, Rashevsky turned to the newly established National Science Foundation (NSF). Created in 1950, the NSF was based primarily on the vision of Vannevar Bush, a prominent physicist and director of the office of scientific research and development, advisor to President F.D. Roosevelt.[143] In his 1945 report *Science, the Endless Frontier*—which was produced in response to Roosevelt's request and served as the catalyst for the creation of NSF—Bush argued that an agency dedicated to basic research should be created. As Bush wrote in his report:

> Basic research is performed without thought of practical ends. It results in general knowledge and an understanding of nature and its laws. This general knowledge provides the means of answering a large number of important practical problems, though it may not give a complete specific answer to any one of them. The function of applied research is to provide such complete answers. The scientist doing basic research may not be at all interested in the practical applications of his work, yet the further progress of industrial development would eventually stagnate if basic scientific research were long neglected.[144]

The task and responsibility of directing the new venture was bestowed upon physicist Alan T. Waterman. Waterman, then chief scientist at the Office of Naval Research, entered the office in March 1951. One of the first actions he executed was

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[141]Rapoport, *Certainties and Doubts: A Philosophy of Life*; D. Hammond, *The Science of Synthesis: Exploring the Social Implications of General Systems Theory* (Univ Pr of Colorado, 2010); W. Pickren, "James Grier Miller (1916-2002)", (2003); D. Hammond and J. Wilby, "The Life and Work of James Grier Miller", *Systems research and behavioral science* 23, no. 3 (2006); J.G. Miller and J.L. Miller, "The Chicago Behavioral Science Committee", *The Science of Synthesis: Exploring the Social Implications of General Systems Theory* (2010).

[142]Rashesvky to Fernandus Payne, assistant director for biological and Medical Sciences, NSF, June 3, 1953, Box 7, NRP-SCRC.

[143]J.M. England, *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945-57* (National Science Foundation Washington, DC, 1982); D.J. Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945: A Political Interpretation of Science--the Endless Frontier", *Isis* 68, no. 1 (1977).

[144]V. Bush, *Science, the Endless Frontier: A Report to the President* (US Govt. print. off, 1945).

the establishment of a referee panel which was "to review the merit of research proposals".[145] From the outset, the foundation relied on 101 "individual consultants and program panelists". Nearly all of the panel members were active researchers based primarily at universities. Out of 101 members, five were from Chicago and as of 1953 Rashevsky was one of them. Through the referee system of consultants who acted as "mail reviewers", the foundation intended to "secure high quality in its sponsored research".[146] Waterman intended that the selection of research for a grant award would conform to "the best traditions of freedom of inquiry and integrity in research".[147] Proposals were turned down based solely on their merit or lack thereof; "no proposals were turned down for budgetary reasons".[148] Incidentally, Coggeshall was also nominated for the post of the Director of NSF, ranked #4 (whereas Waterman was ranked #7). However, he was not elected.

Rashevsky had a twofold relationship with the NSF. Beginning in 1953, he was one of its panel members, reviewing applications for grant awards in the fields bordering mathematics and biology; he was also dependent on NSF grants between the years 1953–1960. One grant Rashevsky received in 1953 for a 5-year period was for $10,000 to save his *BMB*.

In the grant application submitted on June 18, 1953, to Waterman, Rashevsky explained that "because of the rather unique nature of the papers in mathematical biology, no other journal or combination of journals can provide an adequate outlet for publications in this field".[149] This rationale is precisely what persuaded the Rockefeller Foundation to support the establishment of the *BMB* in 1939. Rashevsky asserted that the role of the *BMB* was of primary importance to mathematical biology "in as much as no research is completed until it is published, the operation of the *BMB* is an integral and essential part of the activities of the Committee on Mathematical Biology of the University of Chicago".[150] He also explained that ceasing the publication of the BMB "would more than cripple the work of the Committee" because the Committee had the responsibility "not only to carry[ing] out research in mathematical biology, but also... training research workers in this field for other institutions and to disseminate the results of research in this field throughout the world".[151] The NSF shared Rashevsky's view that "no research is complete until it is published" and awarded the grant; at least the BMB was safe for the next few years.[152] Nevertheless, the status of the Committee at Chicago remained in critical condition.

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[145]Ibid.

[146]England, *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945-57*. Page 187.

[147]Ibid. Page 187.

[148]Ibid. page 188.

[149]Rashevsky to Waterman, June 18, 1953, Box 7, NRP-SCRC.

[150]Ibid.

[151]Rashevsky to Waterman, June 18, 1953, Box 7, NRP-SCRC.

[152]Rashevsky to Robert Tumbleson (head of the office of the scientific information at the NSF), September 8, 1953, Box 7, NRP-SCRC.

Rashevsky found himself in a desperate spot. On the one hand, he had to fight tooth and nail to keep the operation running at Chicago. On the other hand, with six of his Committee members searching for new positions and only Landahl by his side, there was no one with whom to pursue the research. Rashevsky realized that it might be impossible to save the Committee. Desperate times called for desperate measures.

With money running out and no available funds to maintain his associates his group was slated for disassembly as of July 1, 1954. Three months before the scheduled disassembly, Rashevsky approached the Berkeley psychologist Egon Brunswik in search of advice. He presented the problem of securing an academic home for three of the members of his group, hoping that at least one of them would be accepted to continue his work at Berkeley. He shared his reason for turning to that university: "I learnt from a well informed source that the University of California at Berkley is now in the really exceptional position of being able to expand and is interested in adding new men to their faculty".[153] Rashevsky was thus wondering if there was a place for any of his associates or even perhaps the possibility of organizing a group similar to the Committee, at Berkeley. In the latter case he stated "either one of us of the senior members [Rashevsky and Landahl], perhaps myself, would be available for organizing and heading it".[154] Brunswik was not the only one approached at Berkeley. Following Brunswik's advice, Rashevsky sent a similar query to the mathematics department, addressing Alfred Tarski, C.B. Morrey and G.C. Evans as well as Benson Mates at the Department of Philosophy and Victor Lensen of the Department of Physics.

In response to the letter, Rashevsky was invited by Brunswik to deliver two lectures at Berkeley to present his approach and meet personally with the members of administration at Berkley. Rashevsky presented two lectures: "Cells, Organisms, and the Organic World" and "Mathematical Psychology, Sociology and History". While at Berkeley, Rashevsky had the chance to meet with the authoritative personnel and make his plea in person. Nevertheless, Berkley did not tender any offers and Rashevsky remained at Chicago to continue his struggle with a Committee reduced to "a skeleton staff".[155] Towards the end of 1954, the events took a toll on Rashevsky's health when he suffered cardiac arrest and had to undergo major surgery.[156]

Rashevsky was not alone in his time of need. His friends and colleagues came to the rescue. The turn of events at Chicago elicited letters of protest to the American Association for the Advancement of Science (AAAS, publisher of *Science* and *The Scientific Monthly*). Rashevsky was made aware of these letters by Dael Wolfle, director of the AAAS, who alluded to it casually in a letter regarding the publication

[153] Rashevsky to Brunswik, March 23, 1954, Box 6, Folder B, NRP-SCRC.

[154] Ibid.

[155] Letter to Dr. J.B. Calhoun at National Institute of Mental Health, January 23, 1957, Box 7, NRP-SCRC; Rapoport, *Certainties and Doubts: A Philosophy of Life*.

[156] Letter to Peter De Bruyn, January 18, 1955, Box 6, Folder B, NRP-SCRC.

of one of Rashevsky's articles in *Science*. Concerned with the situation and interested in the details, Wolfle wrote "I have had notes of protest over the curtailment of work in mathematical biophysics at the University of Chicago". Offering his services, Wolfle concluded the letter posing this question: "Is there anything appropriate for an outsider to do? I would like to do anything I can to help the excellent work of yourself and your staff to continue unhampered".[157] Rashevsky responded that while he knew nothing of such notes "[he] was not surprised and [was] pleased by such reaction of [his] fellow scientists".[158] Although he did not share any details of the events, he merely stated what was "a matter of public record": depleting budget and diminished staff. As to the measures that could be taken, Rashevsky suggested, "the only academic procedure would be to voice publically [sic] one's opinion about the merits of our work [at the committee]".[159]

The notes received by the AAAS were inspired by Rashevsky's collaborator and friend, Chicago's neuroanatomist Gerhardt von Bonin, who persuaded Warren McCulloch (then at MIT) to join him and several other prominent scientists (the physiologist George Bishop, psychologist Egon Brunswik, philosopher Rudolph Carnap, mathematician Kerl Menger, physician Russell Meyers, and Nobel laureate biochemist Albert Szent-Gyorgyi) in writing an official letter of protest to the University of Chicago.[160] Sent to the editor of *Science*, the letter was eventually published on April, 26, 1956, under the title "Committee on Mathematical Biology":

> We are disturbed by the drastic reductions that have been imposed on the Committee on Mathematical Biology, headed by N. Rashevsky at the University of Chicago. We wish to point out that the work of this department, the only one of its kind in the world, is of great interest and importance in our diverse fields of research, that is, biology, clinical medicine, mathematics, psychology, philosophy, and sociology. We feel that it would be a loss if that work were seriously reduced.[161]

Prior to publishing the letter, Wolfle contacted Dean Coggeshall who sent this official response to the notes received by the AAAS:

> The budget of the committee on mathematical biology at the University of Chicago has not been reduced drastically as indicated. There is no intention to alter the plan of the committee as agreed upon with the chairman over a year ago, unless it is to strengthen the unit. No signatory of the above letter has to my knowledge discussed this with any member of the University administration and certainly not with me.[162]

This alleged official stand of the administration seemed to differ greatly from its true intentions. The response was transferred to McCulloch with copies forwarded

[157]Wolfle to Rashevsky, October 24, 1955, Box 6, NRP-SCRC.

[158]Rashevsky to Wolfle, November 1, 1955, Box 6, NRP-SCRC.

[159]Ibid.

[160]Scott, *The Nonlinear Universe: Chaos, Emergence, Life*.pg. 90.

[161]W.S. McCulloch et al., "Committee on Mathematical Biology", *Science (New York, NY)* 123, no. 3200 (1956).

[162]Wolfle to Rashevsky, October 24, 1955, Box 6, NRP-SCRC.

to Rashevsky and Coggeshall, asking McCulloch as the representative of the signers to reconsider the letter and perhaps discuss the matter directly with the University. Although *Science* initially intended to publish the letter with the response, eventually it was published without any comment from the University.[163] The missive elicited a number of letters written to the Chancellor Kimpton by former students and associates of Rashevsky.

It was interesting to observe the Administration's reaction to the correspondence that ran in *Science*. In response to one of the letters received from an alumni, George Sacher—then with the Division of Biological and Medical research at the Argonne National Laboratory—Harrison, the vice president and Dean of Faculties, reassured Sacher that "the reductions made in Mathematical Biology were a matter of budgetary requirements and do not have other implications".[164] This formulation was the official stand directed to outsiders.

However, in a letter conveying a different tone, President Kimpton wrote to John Rockefeller III, "you will be amused by the attached", attaching an extract of the *Science* letter. Kimpton continued to elaborate:

> I believe I told you that we have been trying to reduce our level of operation in the Committee on Mathematical Biology, and we of course run into enormous trouble as we try to do it. You will recall that this is an activity established through the Rockefeller Foundation back in the Thirties that in spite of what these gentlemen say, never came off. Perhaps I should add that Mr. Rashevsky, Chairman of the Committee, has solicited this letter and these signatures. Life is difficult. [165]

Perhaps this letter was the Administration's way of softening the blow because of the impact the *Science* letter might have on the outside community in the midst of great fundraising efforts. Interestingly enough, as exhibited by the aforementioned exchange with Wolfle, the letter was not Rashevsky's work. The exchange in Rashevsky's papers indicated no efforts to solicit; only surprise and gratitude to the signees for their efforts. One such letter of gratitude was sent to Rudolph Carnap on May 3, 1956, in which Rashevsky expressed his thanks "for the moral support given to us". The exchange with Carnap in 1955 prior to the publication of the letter bears no indication that any sort of soliciting occurred; rather, it reflects discussion of symbolic logic, a subject that would become Rashevsky's new interest.[166]

Despite Coggeshall's "official response" to the AAAS and responses to the alumni, it was clear that the administration's position was to shut down Rashevsky's enterprise. In a memo dated January 14, 1956, Coggeshall, responding to a request by Kimpton, was in search of "expenditures growing out of foundation

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163 Wolfle to McCulloch, December 13, 195[5], Box 6, NRP-SCRC.

164 Harrison to George Sacher at the Division of Biological and Medical Research, Argonne National Laboratory, May 9, 1956, Box 167, KOH-SCRC.

165 Kimpton to John D. Rockefeller III, May 8, 1956, Box 167, KOH-SCRC.

166 Carnap was well familiar with Rashevsky's work and was a supporter of his approach. It is reasonable to assume that the letter was not solicited by Rashevsky.

appropriations designed to establish specific projects".[167] While there were "much more . . .examples. . .than [Coggeshall] was aware of", he made a point stating that the "most clear cut would be the committee on mathematical biology".[168]

Coggeshall's review of the Committee budget from 1936 revealed that the university had spent "hard money" with more than $250,000 out of its budget and additional funds amounting to approximately 180,000 from research funds (such as the Abbot endowment), totaling in almost $450,000.[169] He presented his opinion on the department: "I think it is significant that some of these ideas do materialize into outstanding accomplishments, but if we put it on the intellectual value of this enterprise in the interval since 1936 there has not been a committee or a department established in any other university, so as a discipline it has not had a very important impact in educational circles. To be fair, one would have to state that some good men have come out of the program. On the other hand, given a choice I think that we would not have put this amount of money into the program".[170] This memo apparently set the wheels in motion; a few days later, the University's Comptroller John Kirkpatrick was asked by Kimpton to review the Committee's finances, and he confirmed Coggeshall's figures. He also stated that since 1955 the department budget was not used for the program; "Abbot bears the departmental budget" in the case of the committee.[171]

Towards the Golden Years

Coggeshall was mistaken as to the "impact" of Rashevsky's enterprise. Between 1955 and 1968, a new director, James A. Shannon, presided over the National Institutes of Health (NIH) and its spectacular growth. His reign is often remembered as "the golden years" of NIH's expansion.[172] The "golden years" for Rashevsky's vision and for the discipline of Mathematical Biology were just around the corner. With Shannon assuming the position of director, NIH "ha[d] finally gotten around to becoming interested in mathematical biology",[173] to the extent that by 1957 they were planning to establish a unit to accommodate the interest.[174] NIH was interested in securing the services of Rashevsky's former student, John Z. Hearon who

[167]Coggeshall to Kimpton, January 14, 1956, box 167, KOH-SCRC.

[168]Ibid.

[169]Coggeshall to Kimpton, January 14, 1956, box 167, KOH-SCRC; on the support from the Abbott endowment in the amount of $134,160 in the memorandum from John Kirkpatrick, Comptroller to Kimpton, on January 18, 1956, box 167, KOH-SCRC.

[170]Coggeshall to Kimpton, January 14, 1956, box 167, KOH-SCRC.

[171]John Kirkpatrick, Comptroller to Kimpton, on January 18, 1956, box 167, KOH-SCRC.

[172]L.P. Rowland, *Ninds at Fifty* (Demos Medical Pub, 2003).

[173]Joseph Rall to Rashevsky May 17, 1957, Box 6, Folder "H", NRP-SCRC.

[174]Ibid.

was currently serving on the Mathematics Panel at Oak Ridge National Laboratory. The director of the National Institute of Arthritis and Metabolic Diseases at the NIH, the endocrinologist Joseph Rall, contacted Rashevsky to solicit his opinion of Hearon in confidence. Rashevsky immediately responded stating “You cannot find a better man than Dr. John Z. Hearon. I highly recommend him both as a scientist and as a person”.[175] This endorsement was sufficient for Rall. Hearon was hired and in 1957 established a unit for mathematical biology under the Mathematical Research Branch of the National Institute of Arthritis, Metabolism and Digestive Diseases.[176]

As far back as 1953, NIH had recognized the need for training scientists versed in mathematics and biology. The first program established by the NIH was in “biometry” and it focused on the marriage of mathematics (specifically statistics) and biological sciences. With more than six programs initiated between 1953 and 1960 in “one phase of the program in quantitative biology”—the biostatistics program, 30 grants were made to various institutions, including the Committee on Mathematical Biology. But in most institutions, the primary goal of NIH—researchers well versed in mathematics and biology—was missed. Most of the training efforts were limited to biostatistical concepts rather than “nonprobabilistic applications of biology”. “These programs have covered only two portions of the entire spectrum [of the biometry program]”, admitted Emmarie Hemphill, the Executive secretary of the Advisory Committee on Epidemiology and Biometry. One portion was covered by statisticians who got “little experience in biology” and the other one was of the “biologist who gets a little experience in mathematics”.[177] People were needed who were knowledgeable between the two extremes. From 1958 onward, “in congressional language relating to our [NIH] appropriations”, a pronouncement appeared that “there should be more research concentration in mathematical biology”.[178]

Recognizing the “failure” and the existing need, an *Ad Hoc* Advisory panel was created at the NIH on February 11, 1960, to propose broader support for training in “mathematical biology”. The NIH position was that “the need to apply mathematics to biology is greater and more urgent than ever before. It is greater, because biology has not kept up with the physical sciences; it is more urgent, because the opportunities are begging to be taken advantage of”.[179]

[175] Rashevsky to Rall, May 22, 1957, Box 6, Folder “H”, NRP-SCRC.

[176] Hearon joined Rashevsky’s group first as a fellow from the NIH during 1947–1948, holding a PhD in biochemistry and later decided to join as a faculty member earning his PhD (second after Landahl) with his thesis on “Theory of Chemical Kinetics as Applied to Biological System”; Rashevsky to the Office of Press Relations, December 1, 1949, Box 8, Folder “H”, NRP-SCRC; Hearon’s dissertation work was partly published in the Bulletin: J.Z. Hearon, “The Steady State Kinetics of Some Biological Systems: I”, *Bulletin of Mathematical Biology* 11, no. 1 (1949).

[177] Lucas, *The Cullowhee Conference on Training in Biomathematics*, page 150.

[178] The term used was in fact biomathematics. Ibid.

[179] F.L. Stone, “Training program in Biomathematics” in Ibid. 368–373.

Yet there was a problem that needed to be addressed. Before research could be done, “much more training had to be done”. There was a shortage of manpower in “mathematical biology”, with the only organized body training mathematical biologists positioned at the University of Chicago. The NIH was convinced that the new field was “here to stay” and what had to be done is “to get busy and expand”.[180] Such congressional recognition and NIH’s support of the program would lead to a turn of events for Rashevsky’s dream and for the discipline as a whole.

No longer was Rashevsky dependent on fortuitous support from a collection of grants or appropriations from the university budget. He was now getting financial support from a governmental agency and money would no longer pose a problem to his Committee. Nevertheless, Rashevsky believed that “no matter how much help we shall have from the government”, universities should share the burden “50:50”. He was convinced that the university administration “cannot have something for nothing” and that they should be prepared to “pay for the expensive product, the mathematical biologist, which is still very rare on the market”.[181] Mathematical biology was finally emerging from its “isolation and incubation period”.[182] The NIH was encouraging and facilitating broad training on the interchange between mathematics and biology.[183] By the end of the 1960 Rashevsky’s committee would be the first to receive a grant totaling over $500,000 for a period of 5 years to train Mathematical Biologists. With mathematical biology analogous to a rocket and “green paper” (grant money) the solid fuel propellant, Rashevsky was fitting into this analogy as the first astronaut to carry biology into the future.[184]

While the NIH played a formative role in resurrecting mathematical biology at Chicago, it also had a hand in establishing mathematical biology as a recognized discipline. Between the years 1960–1964 the NIH would invest large amounts of funds in six different programs devoted to training and research in “Mathematical Biology”. The first two programs to follow Chicago’s program were developed by the mathematician and biostatistician Anthony Bartholomay at Harvard University and the biostatistician Henry L. Lucas at North Carolina State College. Other institutions followed Rashevsky’s pioneering lead, establishing units that offered training in Mathematical Biology. By the late 1960s, J. Jacques headed a unit at the University of Michigan in Ann Arbor, F. Graybill headed a unit at Colorado State University in Ft. Collins, W. Dixon headed a unit at University of California, and R. Evans headed a unit at the University of Minnesota. Groups were also active at

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[180]Ibid.

[181]Rashevsky commented during the discussion that followed F.L. Stone, “Training program in Biomathematics” in Ibid. pg. 374.

[182]A.F. Bartholomay, “Implementation of Training Programs in Biomathematics: Prepared Discussion”; in Ibid. page 367.

[183]E.C. Hemphill, “The Probable Impact of the Nih Training Grant Program on the Future Supply of Biostatisticians”, *American Journal of Public Health* 51, no. 12 (1961).

[184]Miller in Lucas, *The Cullowhee Conference on Training in Biomathematics*, page 351.

Stanford University, John Hopkins, and Emory. Yet until 1964 the Committee remained the only program in the United States to award degrees in mathematical biology.

The NIH's review and approval mechanism involved several steps; "before any grant is made by NIH, it is reviewed by scientists drawn from the scientific community and then is again reviewed by one of the nine Advisory Health Councils which are made up of nongovernmental experts in research, education, and public affairs. Only with the council's approval are awards made by the Surgeon General of the Public Health Service".[185] As early as 1957, Rashevsky built a relationship with Philip Sapir and John Calhoun from the branch of Research Grants and Fellowships at NIH to seek financial help for the purpose of training students in the "borderline fields".[186] One of such Rashevsky's letters to Sapir was transferred to Emmarie Hemphill, executive secretary of the Advisory Committee on Epidemiology and Biometry (established in 1953). The letter would get the ball rolling towards Rashevsky receiving the substantive training grant from the NIH. Hemphill responded enthusiastically to Rashevsky, "your needs [grant for training] would seem to fall in the area of interest of our new Advisory Committee on Epidemiology and Biometry".[187] That was because the Committee had a "primary objective... [of enhancing] both the quality and quantity of trained personnel available for research assignments and possessing special competency in epidemiology and/or biometry in all areas of the health and related sciences".[188] With Rashevsky's Committee providing the world's only organized body for training mathematical biologists, it was the perfect candidate—and Rashevsky would serve as the grant's director.

As noted earlier in the chapter, by the late 1950s the Committee comprised only Rashevsky and Landahl with one research associate in their ranks, Peter Greene. It was situated on the outskirts of the campus, occupying the top floor of a converted six flat that was "dark and damp".[189] In 1956 an eager new graduate student in mathematics named Robert Rosen arrived at the University of Chicago. Like many students before him, he did not know exactly what he wanted to do—yet he knew two things: he was interested in the theory of linear operators on Hilbert and Banach spaces (which was strongly represented at Chicago) and was fascinated with the program in Mathematical Biology, which he discovered happening upon Rashevsky's *magnum opus Mathematical Biophysics* while browsing at a book store. Although Rosen had been convinced that he was to settle in the Mathematics department, his curiosity about the Committee led him to its quarters. Initially, he was shocked when he first saw the Committee's digs. There it was: the field that captured his fascination which he had trailed for years through the BMB, dark,

[185]Ibid. Page 369–370.

[186]Letter to P. Sapir, April 11, 1957, restricted NIH Folder, NRP-SCRC.

[187]Hemphill to Rashevsky, June 23, 1957, Box 7, NIH Folder, NRP-SCRC.

[188]Hemphill to Rashevsky, June 23, 1957, Box 7, NIH Folder, NRP-SCRC.

[189]"Reminiscences of Nicolas Rashevsky", Robert Rosen, n.d.

damp, on the "fringes of the university community", with no person in sight except for the "pretty blond secretary".[190]

On the other hand, Rosen was exploring the option of the University of Chicago's mathematics department that with all its glory was "like an explosion of light".[191] Rosen reminisced years later: "The sheer intellectual ferment of the place was like nothing else in my experience, teeming with graduate students exceeding in quality that of most university faculty", filled with students such as Paul Cohen, John Thompson, Steve Schanuel, Hyman Bass (each of these men would become prominent mathematicians in their respective fields of study). Yet Rosen, who was captivated by the notion of Mathematical Biology, decided that he wanted to meet Rashevsky and set an appointment for the following day. Because the Committee was lacking in disciples, Rashevsky went to his campus office for a few hours three times a week, working the rest of the time at home. Rosen recalls being struck by Rashevsky's persona. Tall, bearded, wearing his customary vest and suspenders with a "booming, hearty voice" and a "characteristic Russian accent". Rashevsky was apparently intrigued by the fact that Rosen, a graduate student in mathematics, "had come of [his] own will to inquire into mathematical biology".[192] Rosen fell under Rashevsky's spell, hooked almost instantly. Rosen was particularly drawn to Rashevsky's new "baby"—*relational biology*, a topic that would intrigue Rosen for the rest of his scientific career and earn Rosen the moniker of "biology's Newton".[193]

While Rashevsky's enterprise had a great "impact" on the NIH, resulting in financial support and the establishment of similar groups in universities around the US, the enterprise at the University of Chicago was nearing its end.

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[190]Ibid; Rosen, "Autobiographical Reminiscences"; ———, "Autobiographical Reminiscences of Robert Rosen".

[191]Ibid.

[192]Ibid.

[193]"Reminiscences of Nicolas Rashevsky", Robert Rosen, n.d; Rosen and Agin, eds., *Foundations of Mathematical Biology*; R. Rosen, *Fundamentals of Measurement and Representation of Natural Systems* (North-Holland New York, 1978); Rosen, *Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life*; DC Mikulecky, "Robert Rosen (1934–1998): A Snapshot of Biology's Newton", *Computers and Chemistry* 25, no. 4 (2001); D.C. Mikulecky, "Robert Rosen, His Students and His Colleagues: A Glimpse into the Past and the Future as Well", *Chemistry & Biodiversity* 4, no. 10 (2007); AH Louie, "Robert Rosen's Anticipatory Systems", *Foresight* 12, no. 3 (2010).

Chapter 5
How Experiments End: The Drama at Chicago

> Ladies and Gentlemen: this is in a way my farewell address to you. . .As many of you already know, after serving the University of Chicago for thirty years I have resigned from the University now, only six months before my regular scheduled statutory retirement date. I don't believe that there is anybody here who is old enough to have served one institution for a period of thirty years. But some of you may, and then if you have served any institution for thirty years the way I have the University of Chicago, and if you find, yourself, that you must decide to resign, you possibly will remember me and understand how I feel today—because to understand the feeling, one must have lived through that feeling.[1]

This quote is taken from Rashevsky's address to his staff and students delivered on December 15, 1964. What leads a person to resign from a university after 30 years of service, 6 months before his statutory retirement? What are the administrative and institutional factors shaping that decision? How much weight does one's scientific recognition have on one's institutional standing? How do politics, sociology, psychology, economics and scholarship interweave and affect the parties involved? What role did Rashevsky's personality have in the events that lead to his resignation?

This chapter provides a detailed account of the factors and actors that led to Rashevsky's resignation. It centers on external, institutional and social settings rather than Rashevsky's scientific ideas. Discussion of these settings unfolds a detailed account of developments of "extrascientific" factors that dictated the future of Rashevsky's scientific ideas at the University of Chicago.[2] Examining the institutional settings and the political climate at the division of biological sciences, personal and institutional elements receive critical importance. The energetic debates between Rashevsky's proponents and the members of administration and leading figures at the division of biological sciences, underline the particular focal

[1] Address to the staff and students of the Committee on Mathematical Biology, 1964, Box 2, Folder "Manuscripts", NRP-SCRC.

[2] J. Maienschein, "History of Biology", *Osiris* 1(1985).

M.M. Shmailov, *Intellectual Pursuits of Nicolas Rashevsky*, Science Networks. Historical Studies 55, DOI 10.1007/978-3-319-39922-5_5

features of not only intellectual but political roots of the debate over the place mathematical biology should have at the University of Chicago. By providing a detailed chronology of the events I try to examine how the administration perceived Rashevsky's enterprise and the place it saw for his approach at the division.

For the field of mathematical biology and its founder, Rashevsky, the 1960s projected a fascinating-yet-conflicting set of images. On the one hand these were watershed years for mathematical biology. Other institutions followed Rashevsky's pioneering lead, establishing units that offered training in Mathematical Biology. A. Bartholomay ran a unit like that at Harvard, Henry Lucas headed a Unit at North Carolina State College in Raleigh, J. Jacques headed a unit at the University of Michigan in Ann Arbor, F. Graybill headed a unit at Colorado State University in Ft. Collins, W. Dixon headed a unit at University of California, and R. Evans headed a unit at the University of Minnesota. Groups were also active at Stanford University, John Hopkins, and Emory. Rashevsky's own group, with great financial support from NIH was now like the Phoenix, rising from his own ashes. On the other hand, those same years proved to be years of deterioration and eventual obliteration of the Committee of Mathematical Biology as Rashevsky had envisioned and built it.

In 1960, with the University of Chicago again on firm financial and academic footing, Kimpton announced his resignation, stating that he had accomplished what he had set out to do, and that it was time to move on: "My conviction is that the head of such a university as this one can do his best work for it within a reasonably short time. The University every so often requires a change in leaders who can apply fresh and sharply objective appraisals, and start anew, free of the associations, friendships, and scars of a common struggle."[3] Kimpton had no interest in running any other university; instead, he assumed an executive position with Standard Oil of Indiana, where he stayed until he retired in 1971 due to health concerns.

After the retrenchment of the Kimpton administration, Nobel laureate geneticist George Wells Beadle took over and presided over an impressive period of growth at the University of Chicago. The number of faculty members jumped from 860 to 1080, with full professors growing from 345 to 433; average salaries rose by 50 % and total campus expenditures doubled.[4] A 3-year development campaign reached its goal of $160 million. New buildings were constructed for high energy physics, astrophysics, the children's hospital, and the School of Social Service Administration; and new facilities were planned for geophysics and life sciences. In many ways the Joseph Regenstein Library, which was constructed in the middle of the old football field, symbolized the University's highest goals, serving to assist basic research in many disciplines and join their resources under one roof.

During this era Rashevsky, self-motivated and always inspiring others, achieved a new level of fame and even power. He was like a missionary, spreading his gospel

[3]Boyer, *The Persistence to Keep Everlastingly at It: Fund-Raising and Philanthropy at Chicago in the Twentieth Century.*

[4]Ibid.

of mathematical biology to whoever was willing to listen. In contrast to the downfall of the mid 1950s, the Committee was off to a striking start. Rashevsky's *curriculum vitae* in the 1960s, when he was already in his sixties, reads like a travelogue of a man many years younger. Jetting around from one international conference to another and accepting invitations to lecture at universities across the globe, Rashevsky crossed the American continent, as well as Russia and Europe. Graduate students, postodoctoral students, and visiting scholars flocked to the Committee at the University of Chicago. Rashevsky was never too busy to listen to others and share his ideas, and he worked diligently to enlist scholars to work on one of many of his projects.[5]

The resurrection of the Committee at the University of Chicago was buoyed by national support that followed from the establishment of the NSF and the increasing significance of the NIH. Mathematical biology was no longer solely an experiment in process at Chicago. It was now gaining recognition as a new discipline. With the turn of the decade (1960s) three international events on mathematical biology were held, financed by the NSF and the NIH. The first in the series was a 3-week course directed by Rashevsky at the invitation of the International School of Physics "Enrico Fermi" in Italy. The Summer School Program Course, entitled Physicomathematical Aspects of Biology, was held from July 11 through July 30, 1960, at Villa Monastero on Lake Como.

In July 1959, the president of the Italian Physics Society, G. Polvani, invited Rashevsky to undertake the organization and direction of the course. He was given "very wide latitude in selecting and inviting" the lecturers for the course and the freedom to invite participants from around the world.[6] Conference costs were covered in part by the Italian society and the travel expenses of the two American students, John Layman and Jan Polisser, were covered by the NSF. Some forty students—"highly trained" on the postdoctoral level—participated in the 3 week course. Reviewing each application, Rashevsky had to "give clearance to each individual student so far as his academic qualifications are concerned".[7] Nine lecturers spoke on seventeen topics, ranging from "physico mathematical foundations of reaction rate theory" by Bartholomay to "enzyme reactions" by E. Boeri. The lectures were divided equally between subjects with a focus on experimental biology and those concerned with the physico-mathematical treatment of biological phenomena. Representing the "theoretician" stance were Landahl and Bartholomay, whereas the side of the experimentalists was represented by Boeri, Polissar, Defares, Bouman, and Wise.[8]

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[5]"Reminiscences of Nicolas Rashevsky", Robert Rosen, n.d.

[6]Rashevsky to Waterman at the NSF, October 28, 1959, Box 1, Folder "NSF", NRP-SCRC.

[7]Rashevsky to Philip Hemily, Program director at the International Science Education Program at NSF, January 11, 1960, Box 1, Folder "NSF", NRP-SCRC; Rashevsky to Waterman at the NSF, October 28, 1959, Box 1, Folder "NSF", NRP-SCRC.

[8]Rashevsky, "Physicomathematical Aspects of Biology."

In Rashevsky's opening remarks he characterized the subject of the program as "a field which lies on the border-line of physics and biology".[9] He viewed the field as an "important innovation because frequently the most important milestones in the development of science have been characterized by the discovery of close relations between what appeared at first as unrelated fields".[10] In introducing the students to the role of physics in biology Rashevsky explained:

> The existence of physical aspects in biology has been apparent for a very long time. The very existence of biological phenomena can be perceived by us only through *physical* manifestation. . .Attempts at *explaining* these and similar phenomena in terms of known physical laws are almost as old as biology itself. . ...unfortunately either due to the complexity of some biological phenomena or due to insufficient knowledge of physics by some of the older biologists, explanation of some biological phenomena in terms of physics was not found in spite of assiduous efforts. This has led to a school of thought amongst the biologists that life is essentially a non-physical, or extra-physical phenomenon. . .However, with the development of physical techniques in biology as well as with the increased training of biologists in physics, the number of such pessimists has appreciably decreased. Moreover many investigators feel that it is possible to study fruitfully the physical mechanisms of separate biological phenomena without touching upon the dangerous question of whether the ultimate basic phenomena of life can be reduced to physics.[11]

He continued: "it is perfectly possible that in order to explain all the known biological phenomena as well as those still to be discovered, we shall have to generalize and extend contemporary physics, in particular quantum mechanics".[12]

Rashevsky's choice of lectures, he explained, was guided by the rationale of "present[ing] a general view of experimental and [mathematical biology], properly blended, as any mathematical and experimental science should be. . .to give a general survey of the field rather than a detailed cross-section of a specialized branch."[13] Another factor that influenced lecture choice and course structure was his conviction that "the successful development of any science is contingent upon a harmonious co-operation between experiment and theory".[14]

While all the other lecturers dealt with the practical applications of physico-mathematical methods to specific phenomena, Rashevsky's lecture concentrated on the general, fundamental principles, a subject that had been close to his heart since 1954. Although these principles did not elucidate any particular phenomena, they precede the construction of models and can be applied to specific situations. He was the last of the "universalists". After teaching the 3 week course in Varrena, Rashevsky was invited to stay along with Emilie, his wife, for 6 months as a

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[9]N. Rashevsky, *Physicomathematical Aspects of Biology*, vol. 16 (Academic Press Inc, 1962); Proceedings of the "International School of Physics 'Enrico Fermi'."

[10]Ibid.

[11]Ibid.

[12]Proceedings of the "International School of Physics 'Enrico Fermi'."

[13]Ibid.

[14]Ibid.

consultant on mathematical biology at the University of Genoa*'s Instituta di Fisica Teorica*.

The second event that occurred on an international scale organized by Rashevsky took place at the Barbizon Plaza in New York from May 8 to 10, 1961. Two years prior, in October 1959, Rashevsky had raised the notion of such a conference with Waterman, the Director of the NSF, and Rashevsky was encouraged to apply in February 1960. With the NSF providing the financial support and in collaboration with the New York Academy of Sciences, Rashevsky organized an international symposium on "mathematical theories of biological phenomena".[15]

Speakers were again chosen to represent both the experimentally oriented applications of physico-mathematical methods to biology as well as those of the more quantitatively oriented mathematical biologists. In addition, Rashevsky invited J.H. Woodger to discuss "Axiomatization in Biology", a subject related to Rashevsky's personal interest in relational biology. [16]

A press release issued by the University of Chicago's public relations office on April 20, 1961, stated that "scientists from the United States and Europe [would] participate in a three-day discussion on how to solve research problems in biology by means of mathematics".[17]

After the meeting, the press release on the summary of the conferences read:

> . . .physicists and astronomers have for centuries used mathematics to calculate things which cannot be directly observed or measured and to conclude about the existence of phenomena not yet observed. By means of mathematics the astronomers have calculated the size and the mass of the sun and the moon, and the distance from . . .the earth to the sun and to the moon. By mathematical reasoning Einstein inferred that energy and matter can be transformed into each other, and this mathematical discovery led forty years later to the birth of the atomic age. In biology this kind of research is relatively new. On a large scale it was studied less than forty years ago by the American Alfred Lotka. . ..who applied mathematical reasoning to the study of the struggle of existence amongst animals. Twenty seven years ago studies of large number of phenomena of life by mathematical reasoning was started at the University of Chicago, which for a while remained the world's only center for this research. Now scientists all over the world work on what is called mathematical biology.[18]

The conference was structured differently from what was standard at most scientific conferences. It was more akin to that of the Cold Spring Harbor Symposia that Rashevsky so admired. Every day, only two sessions were held that comprised

[15]Correspondence with Waterman, Box 1, Folder "NSF", NRP-SCRC; Proceedings were published in the Annals of the New York Academy of Sciences 96, no. 4 (1962); Correspondence with the associates at the New York academy of Sciences in Box 3, Folder "Symposium on Mathematical Theories of Biological Phenomena, 1961", NRP-SCRC.

[16]JH Woodger, "Biology and the Axiomatic Method", *Annals of the New York Academy of Sciences* 96, no. 4 (1962);Correspondence with Waterman, Box 1, Folder "NSF", NRP-SCRC.

[17]Press Release, Box 1, Folder "NSF", NRP-SCRC.

[18]Press Release, Box 3, "Symposium on Mathematical Theories of Biological Phenomena, 1961", NRP-SCRC.

the presentation of two papers each. Every lecture lasted for an hour and was followed by another hour or more of discussion. According to Rashevsky's report to the NSF, the symposia were very well attended, and attendance did not drop over the 3 days. Rashevsky reported proudly that "the discussions were so lively and interesting that those attending the sessions were usually late not only for luncheon, but even for the evening cocktails".[19]

Yet one of the most interesting meetings held on the subject was organized by H. Lucas at North Carolina State University in Raleigh (NSCU). NCSU was contemplating opening a program in mathematical biology. With the raising interest of the NIH and the large amounts of funds available in training grants, the NIH sponsored one of the largest meetings on the subject of training mathematical biologists. With more than 100 scientists in attendance, the meeting included representatives from 13 countries around the globe.[20] Recognized at the meeting as the founder of the "new discipline", Rashevsky beamed like a proud parent watching his child debut.

Pawns on a Chess Board

During the early 1960s the division of biological sciences at Chicago underwent a change, as did the central administration. The changing of the guard resulted in Dr. Stanley Bennett in the position of the dean of biological sciences, Edward Levi as Provost, and George Beadle as president of the University.

Beadle had been awarded the Nobel-Prize in Physiology/Medicine in 1958 jointly with Edward Lawrie Tatum for their discovery that genes control individual steps in metabolism. He was nominated for the position of university president by Lowell Coggeshall, chairman of the Division of Biological Sciences. After several rounds of visits to and by Beadle, by January 12, 1961 the appointment was made official. Beadle had the administrative experience of building a biology division at Caltech. He was the perfect candidate: he had the academic standing, a stellar scientific reputation, skill as a university administrator, success as a fundraiser, and a judicious approach to difficult issues. A highly respected scholar, Beadle was invited to participate in the development of science policies.[21] His astute views and leadership at Caltech earned him a good standing as a wise and prudent defender of science and scientists.

By the end of Beadle's first year as president, he had established the office of Provost to replace the existing office of Dean of Faculties. The Provost—Edward Levi—was to assume responsibility under the president for academic

[19] Correspondence with Waterman, Box 1, Folder "NSF", NRP-SCRC.

[20] Lucas, *The Cullowhee Conference on Training in Biomathematics*.

[21] Berg and Singer, *George Beadle, an Uncommon Farmer: The Emergence of Genetics in the 20th Century*. 221.

administration issues and authority over academic budgets. Responsible for the academic side of the university, Levi would have a major role in rejuvenating the faculty via control of the budget and influence on academic leadership. The provost and president formed a natural team, often referred to on campus as team Beadle-Levi. The two occupied adjacent offices and spoke all the time, perhaps resulting in a sparse written exchange between the two left behind in the archival records.[22] The first and primary goal of team Beadle-Levi was to recruit top scholars irrespective of their specialty.[23]

The Division of Biological Sciences was also undergoing a change at the turn of the decade. Coggeshall was promoted to a high rank position at the central administration and the new Dean-elect was Henry Stanley Bennett, a physician and highly regarded specialist in electron microscopy, who was appointed shortly before Beadle became President. Many of the biologists who were at the university in the 1960s thought that Bennett's 5-year reign as division dean was ineffective, even counterproductive. Biological sciences were languishing under his leadership.[24]

One of Bennett's first actions was to reorganize the division from the reigning departmental division which he considered to be "antiquated and illogical" into more malleable structure basically comprising "concept of independent professors".[25] In Bennett's vision, the new division structure should be like that of the Rockefeller Foundation. Professors and associate professors would be set as independent tenured fellows, and research fellows, instructors and assistant professors would report to these tenured fellows. Under the plan, the tenured member would report directly to the dean or his deputy.

According to Bennett, the advantage would be that "it would allow the appointment of persons without regard to formal affiliation and without the requirement that an appointment be initiated in a department".[26] In addition to this, the advantage would be that there would not be a body "which could block desirable appointments".[27] However, Bennett was skeptical as to the support he would get for this vision from faculty members, who regarded the formal affiliation to a department as a protection and as an "instrument through which faculty members can exert influence".[28] The Division of Biological Sciences was not the only place where concerns would be raised on new appointments. The Beadle-Levi efforts to bring in new members or replace current faculty were "thwarted by the almost unique prerogatives of the faculty in academic matters, particularly where new

[22]Ibid. Most of the communications that do exist between Beadle and Levi in the collection of the presidential papers are handwritten notes and comments on various correspondence exchange papers.

[23]Ibid.

[24]Ibid. 277.

[25]Bennett to Edward Levi, August 15, 1962, Box 58, Folder 7, BOP-SCRC.

[26]Ibid.

[27]Ibid.

[28]Ibid.

appointments were concerned".[29] With these prerogatives strongly guarded, "intrusions on that responsibility by the president's and the provost's offices were likely to be ignored if not resented".[30]

In light of the obstacles that the administration might face for changing the departmental framework, Bennett suggested a compromise. Since the university statutes provide a mechanism whereby the division chairman could be replaced at the end of the 3-year appointment, Bennett believed this could be a vantage point. It would allow the administration not to re-appoint chairmen who did not suit their agenda and instead recruit and appoint top scholars more appropriate for the job. He argued that it would be possible in this manner to create over a decade "a modern, lively, flexible biology division within the antiquated, illogical and obsolete departmental framework".[31]

That was the administration's position. In contradistinction to his predecessor, Bennett was fond of Rashevsky and the two bonded, becoming very close friends. Bennett was one of the handful of people whom Rashevsky referred to by his first name—'Stan'.[32] Perhaps this was due to Rashevsky's stance in the eyes of Bennett. Bennett had formulated a firm and clear opinion of what accounts for a successful department and its leader. He summarized it thus:

An active, successful department is characterized:

- By vigorous and productive research activities;
- By functioning intellectual contacts with scholars all over the world;
- By a stimulating teaching program which attracts students into the field;
- By wide-ranging interests which overlap into other disciplinary areas;
- By post-doctorals, graduate students, and visitors attracted by the scholars in the department;
- By staff members who participate in national advisory panels, in the affairs of their learned and professional societies, and in international scientific activities;
- By helpful contributions to the administrative and committee functions of its institution and to the strengths of other departments in the institution and in its field;
- By the sponsorship of frequent, stimulating seminars, conferences, and special lectures; and
- By a visible role in accelerating the assimilation of new ideas and techniques by its institution and its scholarly field.[33]

Rashevsky's Committee and his leadership fit this characterization. Bennett argued that "expansion of specialization and knowledge requires administrative

[29]Berg and Singer, *George Beadle, an Uncommon Farmer: The Emergence of Genetics in the 20th Century*. pg. 277.

[30]Ibid.

[31]Bennett to Edward Levi, August 15, 1962, Box 58, Folder 7, BOP-SCRC.

[32]R. Rosen, "Reminiscences of Nicolas Rashevsky", n.d.

[33]H.S. Bennett, "The Medical Faculty", *The Yale journal of biology and medicine* 39, no. 6 (1967).

accommodations. This is the framework within which the chairman operates. He must be an able scientist, creative in his own right, and versatile enough to perceive and to incorporate advances in other fields which enrich his own. He should be a missionary—that is, a man with a mission, committed to his field and to the betterment of mankind and his institution. The position demands a person of selflessness and generosity, willing to sacrifice part of his own scientific achievement in order to facilitate that of his colleagues."[34] Rashevsky was just that individual.

With Bennett assuming the position of Dean, he met with Rashevsky in February 1961, where he asked him to expand the Committee of Mathematical Biology before his planned retirement in 1965 so as "to undo the damage that was done to it in 1953–54".[35] Rashevsky agreed to do so under "two explicit conditions". One was a large budget and the second "that in the organization and planning of the expanded Committee on Mathematical Biology, [Rashevsky] should have completely [his] own way in everything". Apparently, Bennett did more than just not reject the conditions; in fact, he gave Rashevsky a "'solemn and sincere' pledge to back [him] in everything". The budget component was covered by the NIH training grant directed by Rashevsky and the faculty budget. However, Rashevsky would soon realize that the second condition was far from being approved. Still, Rashevsky did his part expanding the Committee from a group that "occupied a few rooms" in the dark and damp building to a group that would in less than 2 years occupy "a three-story building".[36]

In the manifest to Ed Levi in which Bennett disclosed his vision of the new Division of Biological Sciences, he raised the issue of the Committee of Mathematical biology. "The [committee of mathematical biology has] been functioning very much as a department...". He pointed out that its activities have been "really indistinguishable from those of the departments...[it has] its own budget...make their own nominations...give their own degrees".[37] He was proposing turning the Committee into a full-fledged department. Less than a week later, Ed Levi responded that the "memorandum ...is highly interesting" and that he had passed it on to Beadle. He also underscored to Bennett that any decision on restructuring and forming any departments would require Trustee Action. It is instructive to note that Coggeshall, who actively despised Rashevsky, was on the University's board of trustees. Beadle expressed interest in this memorandum and invited Bennett to discuss its content.

While the new Dean was responding positively towards the notion of the Committee becoming a Department, Rashevsky's opponents were highly active behind the scenes to stymie these efforts. With Harrison now in Administration, he shared his reservations with Levi on April 9, 1963: "I think it would be a mistake to

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[34] Ibid.

[35] Rashevsky to Bennett, 1963, Box 327, Folder 6, BOP-SCRC.

[36] R. Rosen, Reminiscences of Nicolas Rashevsky, n.d.

[37] Rashevsky to Bennett, 1963, Box 327, Folder 6, BOP-SCRC.

create a Department of Mathematical Biology".[38] Rashevsky was approaching 65, the age of mandatory retirement established by Hutchins at the very beginning of his presidency, his tenure with the University was coming to a close.[39] As far back as 2 years prior to Rashevsky's scheduled retirement (July 1, 1965), efforts were on the way towards finding a successor. This event of changing of the guard would be a dramatic turning point for the Committee, the Division and in fact for the University.

In preparation for Rashevsky's retirement, an *ad hoc* committee was assembled to include "distinguished professors knowledgeable in the field of mathematics" in a search of a successor.[40] To organize the committee, the president appointed Bennett in charge. Bennett approached Rashevsky and requested a list of nominees as early as in November 1962. The committee included Paul Harper, Paul Meir, S. Chandrasekhar, Saunders MacLane, Wright Adams, and Samuel Alison.[41] On January 22, 1963, Bennett wrote to the members of the Committee on Mathematical Biology soliciting suggestions for a new chairman. Robert Rosen replied, that the only person truly qualified by both ability and experience to take over the Committee Chairman in Rashevsky's stead is Professor H.D. Landahl. Rashevsky shared this opinion and strongly advocated for it, leading to a major drama at Chicago.[42]

Rashevsky was invited to give a general statement about the Committee on Mathematical Biology to the *ad hoc* committee on June 11, 1963; in particular, he was asked to share his thoughts for its future.[43] While no minutes of the meeting were recorded in the files, Rashevsky's appearance was followed by two memorandums dated June 12 and June 18, 1963. During the assembly Rashevsky provided an overview of what Mathematical Biology stood for and how he envisioned its future. He reflected on the history of the field, indicating that from the outset the "broadness of the field was essentially the basic idea of my whole program".[44] This wide prism was reflected in the works of the doctoral students who from the very beginning "worked almost always in different branches of mathematical biology", with the broadness varying from individual to individual.[45] The two individuals spotlighted were Landahl and Rapoport.

Towards the end of 1961, when discussions were made on resurrecting the committee, the name of the prominent statistician Samuel Karlin was raised as a

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[38]Harrison to Levi, April 9, 1963, Box 327, Folder 4, BOP-SCRC.

[39]Boyer, *The Persistence to Keep Everlastingly at It: Fund-Raising and Philanthropy at Chicago in the Twentieth Century* pg. 50.

[40]Bennett to Levi, May 16, 1963, Box 327, Folder 4, BOP-SCRC.

[41]Letter to trustees of the University of Chicago by Levi-confidential memorandum, Box 327, Folder 6, BOP-SCRC.

[42]Rashevsky to Bennett, June 18, 1963, Box 327, Folder 6, BOP-SCRC.

[43]Rashevsky to Bennett, Letter in follow up to Rashevsky's meeting with the ad hoc committee, June 12, 1963, Box 327, Folder 6, BOP-SCRC.

[44]Ibid.

[45]Ibid.

potential new member to join the committee. However, Rashevsky was given to understand by the administration that as long as he was a "lame duck" chairman, no permanent appointments could be made; on the other hand, the individuals he wished to bring in would not agree to less than a permanent appointment.

Rashevsky viewed the breadth of scope of his group members as the essential characteristic of the Committee. He shared with Bennett that interested students and research fellows conveyed to him that the Committee on Mathematical Biology "is the only place" that would satisfy them precisely because of its broad range of interests.

On the matter of organizing the work on mathematical biology, Rashevsky commented that *merging* the committee with other units would present administrative problems. Close cooperation between mathematical biology and fields in which experimental biologists and scientists in medical sciences were interested could be achieved by *joint appointments*. Such joint appointments were already in existence for some members of the Committee. For example, during that time Hugo Martinez was a member of the Computer Research section and the Committee. As for Samuel Karlin, Rashevsky suggested that he join the Committee alongside an affiliation with another department, such as the department of mathematics.

Rashevsky envisioned interdivisional appointments between the biological, social, computer, and physical sciences. This Rashevsky, indicated would be an arrangement "... that ... would lead to the greatest advantages for the division [of Biological Sciences] and for the University as a whole".[46] Alongside the emphasis that should be placed on the cooperation between different units and divisions, Rashevsky expressed that this collaboration should not be done "at the expense of purely theoretical work which for a while may have, or seem to have, no direct bearing on any experimental aspects".[47]

Rashevsky considered Landahl a man of unique abilities, perhaps as close as one could get to his own. Landahl could "discuss with <u>any</u> outsider the problems of that outsider, show familiarity with that problem, and suggest immediately useful ideas. Not only is he a born researcher, a fact which is already well-known, but he is also a born consultant and his advice is being sought very frequently from various quarters. He has contributed not only to practically every field of mathematical biology, but also to the fields of mathematical sociology, namely to the problems of imitative behavior and to the problem of rumor spread".[48] Rashevsky's appraisal of Landahl was shared by many colleagues within the Committee as well as those from the field at large.

However, the administration had different plans for the Committee. In their scheme to recruit top scholars, their mind was set on bringing in Samuel Karlin. Moreover, they chose to not consult with Rashevsky on this matter, which in his eyes was both absurd and an insult. After all, they were searching for a replacement

[46] Ibid.

[47] Ibid.

[48] Rashevsky to Bennett, June 12, 1963, Box 327, Folder 6, BOP-SCRC.

to chair the Committee that he had built from scratch and labored for over years. Rashevsky felt obliged to document his opinion on the subject for the files: “upon lengthy reflection, I came to the conclusion that it certainly could do no harm to make my opinion on this point to your [*ad hoc*] committee”.[49] As such, he went on record on June 18, 1963 that “professor Herbert D. Landahl is the only possible successor who would continue the work of the Committee on Mathematical Biology in the broad scope as I have outlined it, maintain its interests, and continue the rapid development of its research and training program, which has taken place since the beginning of your administration of the Division [1961]”.

While there were many capable and even brilliant men who had made noteworthy contributions to mathematical biology, Landahl was the most *versatile*, explained Rashevsky. Although the *ad hoc* Committee chose to not interview the junior, untenured staff members of the Committee on Mathematical Biology, Rashevsky pleaded with Bennett that they do so; these were the individuals who would have to live with the decision. On this point the Committee conceded.

On the matter of Karlin, Rashevsky made it clear that while he recommended Karlin to be appointed professor of applied mathematics in the Division of Biological Sciences, he stressed that this “does not imply that [Karlin] is even remotely qualified to be a leader in mathematical biology, a field in which he is only a beginner”.[50] He even indicated that appointing Karlin would be as ridiculous as choosing Erwin Schrödinger. He mentioned that such an appointment would even go against Bennett’s own interest, which was “to bring mathematical biology closer to the medical field”.[51] Rashevsky explained that Karlin was a mathematician who had no substantial training in biology, and “without substantial appreciation of the nature of biology could not be an appropriate person to take over the committee and lead it into its future”.[52]He asked “how should a man not trained in biology direct our vast training program in mathematical biology?”, while his own candidate did get this sort of education and has trained for more than 20 years PhD students in mathematical biology. In the words of Rashevsky, Landahl has “for years. . .shown his ability to talk the biologists’ language and to be helpful to experimentalists with theoretical and practical advice”.[53]Outraged that his enterprise was to be handed over to a person not of his choosing, Rashevsky threatened to resign. He believed that the new chairman should be Landahl or his equal, which he did not believe to exist. On a copy of this letter to Bennett that can be found in the presidential archives, Beadle commented to Ed Levi in handwriting that he was not keen on Landahl; another comment scrawled by an unidentified author read “That’s Rashevsky for you!” [54]

[49]Rashevsky to Bennett, Letter in follow up to Rashevsky’s meeting with the ad hoc committee, June 18, 1963, Box 327, Folder 6, BOP-SCRC.

[50]Rashevsky to Bennett, July 22, 1963, Box 327, Folder 6, BOP-SCRC.

[51]Rashevsky to Bennett, July 22, 1963, Box 327, Folder 6, BOP-SCRC.

[52]Rashevsky to Bennett, July 22, 1963, Box 327, Folder 6, BOP-SCRC.

[53]Rashevsky to Bennett, July 22, 1963, Box 327, Folder 6, BOP-SCRC.

[54]Ibid. handwritten markings on the letter, corresponding to Beadle’s handwriting in style.

The matter of the successor ballooned; it was widely discussed and drew attention on campus and throughout the field. Through various channels, Rashevsky and members of his staff received letters and notices form off campus associates that should he resign, the position of mathematical biology would be endangered. Some scholars, such as Giorgio Serge, who was slated to join the University of Chicago Committee on Rashevsky's invitation, notified that they would not assume the position. Others, such as Anthony Bartholomay, former graduate student and now a director of a program on mathematical biology at Harvard University, wrote letters to the administration. It seems that the members of the team making the decision were concerned about the opinion that the scientific community at large was forming of the University of Chicago. As E.A. Evans, Jr. of the Department of Biochemistry wrote to Bennett on October 16, 1963: "Doctor Rashevsky talked with me this morning about his concern with respect to the appointment of his successor, and he has, as you probably know, discussed this with a number of other senior members of the Division. It seems to me that the case of Mathematical Biology is quite unique inasmuch as Rashevsky is generally regarded as the prime mover in the field. In harmony with your comments in Washington ... it would seem to me most unfortunate for our reputation with the outside scientific community if there was the general impression that properly qualified people had not been consulted about the new appointment even if this was not the case."[55]

Rashevsky was drawing on all his people skills, talents, and connections to combat the administration's way of dealing with the situation. The fact that the *ad hoc* committee did not ask for his recommendation offended him on a personal level and was in flagrant violation of the university statutes and academic policy. Rashevsky wrote to Provost Levi, that shutting out Rashevsky in the search for a successor contradicted Section 2-b of the statutes which stated: "In making an appointment or reappointment of a chairman, the President shall notify the Department of the vacancy, request the department to make suggestions as to a Chairman, and give it opportunity to submit such suggestion".[56] Rashevsky was displeased with the fact that the *ad hoc* committee did not include a mathematical biologist in its ranks. Moreover, the Faculty of the Committee on Mathematical Biology unanimously declared Karlin to be unqualified and as such found the choice and procedure of the *ad hoc* Committee bewildering.

To calm the situation, Levi responded to Rashevsky that notification has been given to him in line with the statutes, even though Rashevsky disagreed with the form in which it was given. Levi added that the administration was yet to receive any recommendations for the Committee Chairmanship and that he had made "no recommendations to the President on this matter".[57] As a gesture of "good will" and in accordance with the formal statutes, Levi asked that Rashevsky view the letter as

[55]Evans to Bennett, October 16, 1963, Box 327, Folder 8, BOP-SCRC.

[56]Rashevsky to Levi, October 10, 1963, Box 327, Folder 6, BOP-SCRC.

[57]Levi to Rashevsky, October 14, 1963, Box 327, Folder 6, BOP-SCRC.

the "formal notification of the pending vacancy in the Chairmanship" and as a request extended to the Committee through Rashevsky to provide suggestions for the next chairman.[58]

Interestingly enough, on that very same day, Bennett sent Rashevsky an internal memo regarding Samuel Karlin's upcoming visit to the University of Chicago. In the memo, Bennett stated that he spoke with Professor Paul Meir. Meir apparently had had a conversation with Karlin in which Karlin "expressed himself as having doubts about his interest in a chair". Alongside with this, Karlin indicated that he is "very much interested in coming to the University of Chicago to work in mathematical biology".[59] Bennett was on some level asking for Rashevsky's blessing to appoint Karlin as a professor in the Committee rather than a chairman, something that he believed Rashevsky would agree to as he had indicated that Karlin was an able and qualified scientist.

The Administration needed Rashevsky to be on his best behavior. Most of the administration would be out of town on 2 out of the 3 days in which Karlin was slated to visit, thus Bennett continued to plead with Rashevsky to arrange opportunities for Karlin to "converse extensively and individually with members of the Committee on mathematical Biology", providing opportunities for informal conversation such as a luncheon. Bennett conveyed his hope that "we can put on a successful *courtship* and convey an atmosphere of cordiality and welcome, even though we are faced with the awkward circumstance that many of the very interested University people will be away during the first two days of his visit".[60]

He ended the letter with reference to Landahl, indicating that they had spoken about the sensitive situation and that Landahl should take into account any future relationships that might arise with Karlin. Concurrently, in yet another exchange of correspondence, the administration reached an agreement with Rashevsky that the Committee would be given 3-weeks to provide the *ad hoc* committee with their suggestions regarding a fitting successor to Rashevsky, with the "deadline" set for November 6, 1963.[61]

To meet the deadline, Rashevsky approached former students and others who had contributed to the field of mathematical biology. The letters were worded thus:

> In view of my impending retirement in 1965, a successor for me as Chairman of the committee on Mathematical biology must be appointed. The Provost of the University of Chicago. . .has asked the Committee on Mathematical biology. . .to make suggestions for a new Chairman. The opinion of the Committee on Mathematical Biology is unanimous in favor of Professor Herbert D. Landahl, as the only individual who is paramount to the successful continuation of our work.[62]

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[58]Ibid.

[59]Bennett to Rashevsky, October 14, 1963, Box 327, Folder 6, BOP-SCRC.

[60]Bennett to Rashevsky, October 14, 1963, Box 327, Folder 6, BOP-SCRC.

[61]Levi to Rashevsky, October 17, 1963, Box 327, Folder 6, BOP-SCRC.

[62]Rashevsky to Levi, November 4, 1963, Box 327, Folder 6, BOP-SCRC.

He continued the letter underscoring that because the Committee was a very small group, "in line with the democratic procedure", he requested the addressees to offer their suggestions on this critical matter.

Letters of response were received at the Committee from former students and associates. Among those who wrote in were John Hearon, Ernesto Trucco, E. Beccari, Director of the Institute of Pharmocology, University of Torino, Professor Torstan Teorell, and Alvin Weinberg. The letters were all forwarded to Provost Levi, who in turn shared these with Bennett and Beadle. On Beadle's copy of the letters, he remarked: "Sure looks to me as though he loaded the question very heavily".[63]

In the cover letter to Levi accompanying the letters of suggestions, Rashevsky reiterated again that Landahl was unanimously named as the appropriate person to assume the position of Chairmanship. The reasons were summarized again:

1. First and foremost he is the best person to provide the proper intellectual leadership. He is the best qualified person because of his many years of experience in the field of mathematical biology, his numerous scientific publications in that field, and the repeated demonstration of his ability to inspire young students and fellow scientists.
2. Professor Landahl's training includes not only that in physicomathematical sciences, but also a thorough training in biology. He speaks the language of the biologist and he has an excellent feeling for the problems of the experimental biologist. During the war, he did both excellent theoretical and experimental work at the University of Chicago Toxicity Laboratory. These qualities are of particular importance if this Committee is to preserve its emphasis of the broadest aspects of mathematical biology as distinct, for example, from the more limited point of view of biostatistics [apparently hinting at Karlin].
3. Professor Landahl has proved his administrative abilities, especially when he acted as Chairman during my more than 6 months' absence in Europe in 1960 [during Rashevsky's stay in Genoa, Italy]. The following example may be given of his administrative foresight. During that time he conceived and initiated program of training in mathematical biology to be sponsored by NIH. I myself at that time felt rather cool about the idea. This progress resulted, however, in an unequal development of the work of the Committee, and forms the largest item of the Committee's present annual budget of over a quarter million dollars.

Quite recently professor Landahl has secured another training grant [in the amount of $250,000] from NIH and started a training program in mathematics and computer techniques for bio-medical students.[64]

[63]Hand written note attached to letter addressed by Rashevsky to Levi, November 4, 1963, Box 327, Folder 6, BOP-SCRC; Collection of the letters from former students and others who felt the need to intervene and provide their opinion to the administration and the ad hoc committee are in Box 327, Folder 8, BOP-SCRC.

[64]Rashevsky to Levi, November 4, 1963, Box 327, Folder 6, BOP-SCRC.

In the letter from Weinberg addressed to Beadle (in which he stated that he felt "like an interloper to kibitz in University of Chicago affairs"), he gave this reasoning for Landahl as the successor: "My impression is that mathematical biology has been most successful where it has welded with experiment....It is for this reason that I was glad to write to Dr. Rashevsky on behalf of Dr. Landahl. Landahl had a taste for experiment when I knew him; I should think that he might be oriented toward experiment considerably more strongly than Rashevsky."[65] On his copy of the letter, Beadle noted on this passage: "I think this is the weakness of our group", referring to the lack of contact between the experimentalists and the current group of mathematical biologists.[66]

The next round of correspondence relates to the Committee on Mathematical Biology's opinion of Karlin following his visit. Karlin had a chance to meet with the group and their impressions were conveyed to Bennett in a missive from Rashevsky. In that letter Rashevsky indicated that while "no one can doubt or challenge the high qualifications of Dr. Karlin as an applied mathematician", the group unanimously believed that he was "not qualified as a mathematical biologist in the broadest sense of the word'.[67] Karlin gave an "unpleasant" impression that he is "interested only in the mathematical side of the problem which must be brought to him by a biologist in a clear-cut formulated manner".[68] Staff members dubbed him a "strategic problem solver". Karlin left an impression of arrogance with "an air of superiority", a quality that Rashevsky believed would hardly "make him a person that is easy to get along with." All and all, Rashevsky and his group summarized that they were "very much disappointed in him as a potential mathematical biologist".[69]

A few days after the letter was sent, Rashevsky paid Levi a visit, on which the latter reported confidentially to Beadle and Bennett. Rashevsky apparently opened his visit with a rather manipulative statement that "he had better resign" in light of the situation. Levi reassured Rashevsky that ever since his letter stating that the administration was acting contrary to the statute, the administration "made it clear that [they] were following the consultation pattern of the statute" and that "no offer of a chairmanship of the Committee was...outstanding to Mr. Karlin". This was apparently contrary to Rashevsky's own conversations with Karlin, in which the latter has indicated that he had been "offered the chairmanship of the Committee".[70]

The thorny situation was far from being resolved. While the administration was preoccupied with finding a new chairman for the committee, they were also

[65]Weinberg (at Oak Ridge National Laboratory) to Beadle, November 1, 1963, Box 327, Folder 8, BOP-SCRC.

[66]Hand written note on Ibid.

[67]Rashevsky to Bennett, November 26, 1963, Box 327, Folder 6, BOP-SCRC.

[68]Ibid.

[69]Ibid.

[70]Levi to Bennett and Beadle, November 29, 1963, Box 327, Folder 6, BOP-SCRC.

concurrently trying to reorganize parts of the division and wanted to use this opportunity to do so. In the eyes of the administration, the Committee under Rashevsky's chairmanship was becoming obsolete. They were looking for a fresh angle, a change. Although they had set their eye on Karlin, it was becoming clear that that choice would not pass as simply as they hoped.

Under Bennett's leadership, the administration was moving towards reorganizing the non-clinical groups at the division. Bennett proposed that the mathematically-inclined groups in the division, such as the Committee, the biophysics group and the computational sciences group, be reorganized to facilitate greater interaction between the experimentalists and the mathematicians.[71] Rashevsky's committee was not the only place in the Division that was applying mathematics to biology. Paul Meir was directing the Biological Sciences Computation Center and the biostatistics group; Lester Skaggs was directing the Analog Computer Center in the Department of Radiobiology; and several professors "with mathematical competence" were scattered throughout the division who were active and competent in utilizing mathematical tools and analyzing their research, including Dan Agyn in Physiology and Richard Lewontin in Zoology.

According to Bennett, no "single correlating body" presided over these groups; each had "its own separate functions, aims and financing", with the Committee representing the oldest one of the five.[72] However, Bennett stated that "many biologists regard the total work of Rashevsky's school as disappointing".[73] This experience was partially due to the fact that the "publications [were having] little impact on biology and that the models chosen for mathematical treatment. . .[were] usually of little relevance to biological activities".[74] However, Landahl stood out in contrast to this trend. It appeared that while "Landahl has been strongly influenced by Rashevsky. . .he [was] helpful in areas in which Rashevsky [did] not act".[75] Bennett regarded Landahl's willingness to collaborate with experimental biologists as an example of that difference.

The person to succeed Rashevsky and envisioned by Bennet, should be lenient enough to facilitate the integration of various groups within division and perhaps even head such a group. In contradistinction to Rashevsky's opinion of Landahl, Bennett felt strongly that there were two reasons why Landahl should not to be appointed Chairman: (1)"he does not show the depth of judgment and administrative skills which one would like to see in an academic leader"; (2) Landahl's thinking was "[not] sufficiently independent of Rashevsky". These qualities and especially the latter one were strikes against choosing him as the next leader for the Committee. With Rashevsky striving for independence for his committee from

[71] Bennett to Beadle and Levi, December 27, 1963, Box 327, Folder 6, BOP-SCRC.
[72] Ibid.
[73] Ibid.
[74] Bennett to Beadle and Levi, December 27, 1963, Box 327, Folder 6, BOP-SCRC.
[75] Ibid.

other departments within the division, it would be reasonable to assume that Landahl will be influenced by Rashevsky to refuse and fight any such integration.

Officially, Bennett set the new objectives for the future of applied mathematics at the division on December 27, 1963. In a memorandum to the provost and the president he listed objectives that the "new" program for applied mathematics at the Division of Biological Sciences ought to be able to achieve:

1. Sound and useful research contributions in theoretical biology based on mathematical approaches.
2. Foster the sound use of mathematics in interpreting and correlating experimental and observational data whenever appropriate in the teaching and research of the Division.
3. Increase the competence of Divisional students and staff in the use of mathematical tools, including computers, statistics, and mathematics in general.[76]

The decision to reorganize the activities of "applied mathematicians" at the Division was kept under wraps. Rashevsky assumed that he was advocating for Landahl to become Chairman of the Committee whereas the administration was seeking a much more substantive position namely, the head of a group for "theoretical biology" at the division of biological sciences.

The course of action proposed by Bennett was to "try to conduct ourselves so that the assets mentioned above are preserved, so that the interaction between our mathematically inclined staff members and the experimental biologists is enhanced, and so that the overall capabilities of the Division in theoretical biology are enhanced".[77]

This was the plan to be carried out following Rashevsky's retirement, as suggested by the dean, and can be summarized as follows:

1. To remove the Committee as an independent degree-granting entity and its finance from the Dean's Office and set it as a "Committee on Theoretical Biology" without a budget. The financing would come from training and research grants.
2. To appoint Landahl as a member of a department, e.g., Biophysics, and require all Committee members to be members of a department.
3. To reorganize the committee in such a way that membership is derived from a number of departments. It was foreseen by Bennett that the new Committee might accommodate Paul Meir, John Platt, Dan Agyn, and Richard Lewontin.
4. To broaden the committee's responsibilities to have supervisory and coordinating activity over the Divisional work in computers, teaching mathematics to biologists, and providing training and degree program to those interested in theoretical biology.

[76]Bennett to Beadle and Levi, December 27, 1963, Box 327, Folder 6, BOP-SCRC.

[77]Bennett to Beadle and Levi, December 27, 1963, Box 327, Folder 6, BOP-SCRC.

5. To appoint a chairperson for a 1-year period rather than the traditional 3–year period tenure.[78]

Harrison was consulted on this plan as the previous Dean of the Division. In a brief memo to Levi, Harrison wrote "'mathematical biology' grew from nothing into the present teaching and research program on the campus. It has since been initiated on several other campuses. When Taliaferro was Dean and when I was Dean the biologists said the program was good mathematics, poor biology, the mathematicians told us it was interesting biology but poor mathematics".[79] Harrison suggested seeking the advice of yet another previous dean, Coggeshall, as he believed "Bennett [had] a high opinion of Rashevsky" and perhaps, the "action of the Divisional Committee has been stimulated by Rashevsky".[80]

The question of the future chair of the Committee was gaining much attention from the wider academic community.[81] Perhaps to assuage Rashevsky's insecurity and calm thing down, Levi encouraged Bennett to appoint Rashevsky for another year, which, would "give. . .more flexibility than appointing Landahl for three years".[82] As such, Beadle offered Rashevsky Chairmanship on February 3, 1964, for another year beginning July 1, 1964.[83] Nevertheless, the situation that Rashevsky termed "disintegration" was essentially reaching a point of no return.[84] Hugo Martinez, a member of the Committee, resigned to take a position offered to him elsewhere. As he stated in his letter of resignation, "the recent events, culminating in your resignation, have made it abundantly clear that the atmosphere here is no longer conductive to long range efforts, and that there is little hope of it to be otherwise so long as the fate of the Committee remains in its present state of uncertainty".[85]

"Mustard Plaster"

After 6 months of battling the administration, Rashevsky was at a crossroads and unsure what to do. He wanted the Committee to continue its spirit and agenda—and he wanted this to be done with the successor of his choice, Landahl. Rashevsky grew to understand that he could not achieve both goals. He then called Bennett to tell him that "he [Rashevsky] had thought it over further, that he is not at all sure

[78]Ibid.

[79]Hand written note, Box 327, Folder 6, BOP-SCRC [emphasis in original].

[80]Ibid.

[81]Levi to Samuel Allison (in response to the latter's letter dated January 13, 1964), January 21, 1964, Box 327, Folder 6, BOP-SCRC.

[82]Levi to Bennett, January 22, 1964, Box 327, Folder 6, BOP-SCRC.

[83]Beadle to Rashevsky, February 3, 1964, Box 327, Folder 6, BOP-SCRC.

[84]Rashevsky to Bennett, January 31, 1964, Box 327, Folder 6, BOP-SCRC.

[85]Hugo Martinez to Rashevsky, January 28, 1964, Box 327, Folder 6, BOP-SCRC.

that it would be best for him to resign at this time, that he was eager to cooperate in every way with the University in whatever it wanted to do, that he realized he had acted as a mustard plaster in the past, but he would not do so in the future".[86] The change in tone gave Bennett the impression that "[Rashevsky] has probably given up his insistence of Herb Landahl as his successor and that he wishes to help keep the enterprise going even if some changes are made".[87]

In February 1964 Rashevsky accepted the reappointment, stating that "[he] came to the conclusion that [his] responsibilities to this University, in particular to [his] staff and numerous students, are sufficiently compelling reasons".[88] Extending his appointment was Rashevsky's way of buying additional time to persuade the administration to keep the "enterprise" going and perhaps have input on the choice of the successor. On the other hand, the administration was hoping this 1-year respite would give them some quiet time to proceed with the plan of reorganizing the Division and in particular, place the Committee members within a new departmental framework in order to "couple their theoretical activities more closely with experimental biology".[89]

Two months after accepting the reappointment, Rashevsky started vociferously urging the administration to make a decision. He justified the pressure by noting that Committee members were uncertain as to their place thereby putting the entire group in jeopardy. This instability could in turn jeopardize the NIH training grant and other grants requested by the Committee, such as the Air Force grant. A new player was about to enter the scene who would play a "crucial" role—the former student of Rashevsky, George Karreman, now an associate Professor of Physiology at the University of Pennsylvania and later the first president of the Society of Mathematical Biology. Not shy about offering unsolicited advice, Karreman was characterized by some as "an energetic and provocative individual".[90] On April 17, 1964, Karreman wrote a letter to Rashevsky in which he expressed his disquiet of the Administration's actions with Bennett as its forerunner.[91]

On the other side, Bennett was not "greatly concerned" about the matter going public beyond the University, except in so far as "it [made] it difficult to recruit adequate leadership for mathematical biology and jeopardize[d] the retention of the valuable people . . .in the group".[92] He stuck to his position that it was "best for us to seek tactfully and persuasively to strengthen the ties of mathematical biology to the experimental departments by joint appointments and later on, by integration of the programs".[93]

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[86]Ibid.

[87]Ibid.

[88]Rashevsky to Beadle, February 24, 1964, Box 327, Folder 6, BOP-SCRC.

[89]Bennett to Levi, February 19, 1964, Box 327, Folder 6, BOP-SCRC.

[90]Lawrence Fogel to Rashevsky, June 2, 1961, Box 1, NSF Folder, NRP-SCRC.

[91]Karreman to Rashevsky, April 17, 1964, Box 327, Folder 8, BOP-SCRC.

[92]Bennett to Levi, February 19, 1964, Box 327, Folder 6, BOP-SCRC.

[93]Ibid.

By June of 1964, James Miller at the University of Michigan made an offer to Rashevsky to join the recently-established Mental Health Research Institute as a professor in Mathematical Biology. Miller was well familiar with Rashevsky from his time spent at the University of Chicago as the Chairman of the Psychology Department (1948–1955). He was in close contact with Rashevsky and members of the Committee and especially Anatol Rapoport, who joined Miller at the Mental Health Research Institute, considered by Miller as the "general systems institute".[94] This offer was both enticing and beguiling. Rashevsky would join a group of men he knew well, including Rapoport, his former student, and his colleague Ralph Gerard. With his retirement planned to be effective in less than a year, the position at the Mental Health Research institute would allow him to continue his enterprise in mathematical biology and especially relational biology. Rashevsky felt that "a very drastic and unusual step would have to be taken in order to shake the situation up".[95] Thus he finished the letter stating that unless a decision is made by mid-July, he will be forced to submit his resignation and accept the position at the University of Michigan.

On July 21, 1964, Rashevsky submitted his resignation, resigning as Chairman effective August 21, 1964 and as a professor at the University effective January 1, 1965.[96] This act of resignation started a chain of events, cluttered with letters of protest from the academic community outside the University of Chicago, with internal discussions and negotiations lasting for the next 6 months.

Upon receipt of the resignation letter, Levi turned to the only man in the administration to whom he thought Rashevsky might listen: Bennett. Levi encouraged Bennett to discuss the matter with Rashevsky in a friendly manner. However, Rashevsky felt betrayed and was now distrustful of Bennett.[97] After a brief discussion, there was a written exchange in which Bennett expressed his regret regarding the differences in their viewpoints as to what is "best for the future of mathematical biology".[98]

Beadle immediately followed with a brief letter in which he expressed his appreciation of Rashevsky and that he would reluctantly recommend to the Board of Trustees to accept the resignation.[99] The following day, Landahl was approached to accept the position of acting Chairman and Director of the NIH Training Program.

Rashevsky felt the need to summarize his version of the affairs which led to his resignation, which he did in a letter to Bennett dated August 7, 1964. According to

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[94]Hammond, *The Science of Synthesis: Exploring the Social Implications of General Systems Theory*. pg. 164.

[95]Address by N. Rashevsky to the Staff and Students of the Committee on Mathematical Biology, December 15, 1964, Box 2, NRP-SCRC.

[96]Bennett to Landahl, August 5, 1964, Box 327, Folder 6, BOP-SCRC.

[97]R. Rosen, Reminiscences of Nicolas Rashevsky, n.d.

[98]Bennett to Rashevsky, August 3, 1964, Box 327, Folder 6, BOP-SCRC.

[99]Beadle to Rashevsky, August 4, 1964, Box 327, Folder 6, BOP-SCRC.

Rashevsky, the "disagreement concerns matters of policy that are of paramount importance in academic life" which he felt require a "broader airing".[100]

On the matter of the *ad hoc* committee, Rashevsky expressed his disappointment that it did not comprise mathematical biologists or even biologists. The vagueness of Bennett's conduct, the lack of clarity regarding the "strengthening of 'applied mathematics' in the Division", the uncertainty as to persons with whom he consulted and received recommendations on possible successors, and the fact that "an open discussion" was not initiated with all interested parties, were noted in Rashevsky's letter. In Rashevsky's view, this turn of events did not constitute proper academic procedure. In particular, Rashevsky noted that the University of Chicago was considered once "The Athens of the Twentieth Century" and questioned if Bennett's "way of handling matters [was] the Athenian one" in lieu of the fact that "persons who spent their lives in creating mathematical biology were treated . . .as pawns on a chess board".[101]

In the penultimate paragraph of his letter, Rashevsky expressed his disappointment that Bennett did not try to convince him to stay and openly discuss the matter with him. He concluded the letter stating that Bennett's decision was dictatorial and that he, Rashevsky, disapproved of and would never approve of secrecy anywhere, "especially in academic matters". Rashevsky ended the letter with "*Sapienti sat*!" followed by "Good-bye".[102] Rashevsky copied on the letter his former students and associates as well as members of the administration and the faculty.

Growing uneasy with the fate of the Committee, Bennett sent a memo on August 14 to Levi asking the latter and Beadle to persuade Landahl to accept to position of acting Chairman. Should Landahl decline, Bennett sensed that "[the administration] may be driven to rather unattractive and disruptive alternatives", such as Paul Meier, which would not be "a happy solution".[103] Moreover, the University would lose the NIH training grant and other funds that the committee was directing. Politically and publicly, that turn of events could be detrimental for the University of Chicago's reputation in the eyes of the prominent scholars the administration might try to entice.

The administration was not alone in its concerns. The members of the Committee on mathematical biology were also troubled by the fate of the Committee and the signs reverberating as a result of the resignation sent to the outside community. On August 20, Landahl expressed in a letter to Bennett his concern that "this action [resignation] may be viewed as a lack of confidence by the administration in the committee and thus have a detrimental effect on the development of mathematical biology at other institutions".[104] He proposed to play a role in "correcting any misunderstandings which may have led to the present situation" and declined the

[100]Rashevsky to Bennett, August 7, 1964, Box 327, Folder 6, BOP-SCRC.

[101]Ibid.

[102]Ibid. in Latin "a word is enough to the wise."

[103]Bennett to Levi, August 14, 1964, Box 327, Folder 6, BOP-SCRC.

[104]Landahl to Bennett, August 20, 1964, Box 327, Folder 6, BOP-SCRC.

offer of Chairmanship "until all alternatives have been exhausted".[105] On that same day, several letters communicating a similar message were sent to Beadle and copied to Bennett and Levi signed by dozens of former students, associates and colleagues of Rashevsky. The letter was initiated by Karreman, who was vigorously involved in the matter acting openly against the administration and bringing this matter to the attention of former students, associates and anybody that he found appropriate.

In Karreman's letter to Beadle, he expresses his concern regarding the possibility of mathematical biology remaining a "separate discipline in the University of Chicago".[106] Karreman followed up his letter (on October 5, 1964) with a request to meet with Beadle along with Bartholomay (now the director of the Division of Mathematical Biology at Harvard) and W. H. Johnson (PhD '52 from the Committee, Chairman of Biology at Rensselaer Polytechnic Institute) to discuss their concerns. Both Johnson and Bartholomay voiced their opinions in separate letters articulating similar concerns. Johnson the "biologist" expressed his view on mathematical biology: "we in biology are often reluctant to recognize the importance of mathematical models in our fields, since these models have often been shown later to be crude approximations to the actual system in question. But models have helped us to formulate hypotheses which could be tested experimentally. Groups such as the Committee on Mathematical Biology have provided models at a level of mathematical and physical sophistication which the experimental biologist has not been able to achieve, owing to inadequate mathematical background."[107] Because the Committee was an interdisciplinary group, Johnson argued that it required "maximal freedom in determining its course of action".[108] And it was in this atmosphere of "intellectual freedom" that the Committee "ha[d] developed a unique approach to biological problems". With biology becoming increasingly quantitative, the need for those "devoted to construction of models for fundamental processes in biological systems" was obvious and those who merely attempt to apply mathematics to biology are insufficiently qualified to respond to the need.[109]

Beadle asked Bennett to join the meeting with Karreman, Johnson, and Bartholomay. Bennett pointed out that Karreman might not "like it" if he joins and regarding Bartholomay pointed out that he knew him and he "is not completely reliable nor is he specially able". Bennett apparently consulted with Coggeshall, who "had a lot of experience with Rashevsky" and in turn suggested adding him to the meeting. In preparation for the meeting, Beadle forwarded a handwritten note in which he summarized what should be their arguments:

> The significance of Math Biol [sic[. . .Our interest in it at Chicago,

[105] Ibid.

[106] Karreman to Beadle, August 20, 1964, Box 327, Folder 8, BOP-SCRC.

[107] Johnson to Beadle, October 6, 1964, Box 327, Folder 6, BOP-SCRC.

[108] Ibid.

[109] Ibid.

> Our responsibilities [are] to review this . . .and [get] outside opinions. . . .I do not believe we should say whose we did get (Delbruck, Weaver, Karlin and mathematicians etc.).
>
> In a delicate way make it clear cut that we cannot accept the view that a retiring Chair or Prof [sic] has any special right to name his successor.
>
> Further suggest that we would not get pulled into an argument about Rashevsky, and/or Landahl or anyone else, but that we listen sympathetically to their opinions.
>
> We can accomplish some good if we are careful what we say and how we say it and avoid getting baited into saying what we not [sic] want to say. [110]

Rashevsky's former student John Reiner also wrote a six-page letter to Beadle in which he pleaded with him to take a stand. He asked him to make an informed decision on the fate of the Committee rather than let it slip between his hands. Just as Harry Truman placed a sign on his desk that read "the buck stops here!", so too should Beadle by adopting the statement as his motto. Beadle responded in few sentences, concluding the letter that with the statement that the University appreciates "the significance and importance of mathematical biology and [has] no wish to abandon or curtail activities in this area".[111] Following Bennett's suggestion, Coggeshall—now a University vice president—shared his opinion of Rashevsky in a confidential letter to the President marked as "strictly for internal consumption, but mostly for the files".[112] It was apparently intended as a response to Rashevsky's letter dated August 7, 1964.

Coggeshall argued that the "statements of fact" in Rashevsky's letter were inaccurate. He first referred to the alleged "damage done in 1953–1954" to the Committee which was in his view misrepresented by Rashevsky. He explained in the letter that other departmental budgets for the basic sciences were also cut: e.g., Anatomy by $5000, Bacteriology by $2000, Biochemistry by $7000, whereas the Committee's budget was cut only by $1500. Coggeshall argued against the picture painted by Rashevsky that this budget cut was "a deliberate device to eliminate the Committee on Mathematical Biology". However, the picture emerging from the Kimpton administration papers, differs greatly from the one Coggeshall was trying to paint. As noted in Chap. 4, Kimpton wrote to John Rockefeller III in no uncertain terms: "I believe I told you that we have been trying to reduce our level of operation in the Committee on Mathematical Biology".[113]

Echoing Bennett's words, Coggeshall shared that in his consultations with mathematicians he was informed "that R. was a good biologist, but very limited in mathematics; and received the opposite reaction from the better-informed biologists".[114] His own opinion of Rashevsky was that he is "a very devious individual,

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[110]Handwritten note from Beadle to Levi and Bennett, dated October 10, 1964, Box 327, Folder 6, BOP-SCRC.

[111]Reiner to Beadle, August 31, 1964. Box 327, Folder 8, BOP-SCRC.

[112]Coggershall to Beadle in a letter dated October 12, 1964, Box 327, Folder 6, BOP-SCRC.

[113]Kimpton to John D. Rockefeller III, May 8, 1956, Box 167, KOH-SCRC.

[114]Coggeshall to Beadle in a letter dated October 12, 1964, Box 327, Folder 6, BOP-SCRC. The letter is marked by Coggeshall as "Strictly for internal consumption, but mostly for the files."

extremely selfish, and as far as a university is concerned, quite subversive".[115] In his opinion, Rashevsky had "twisted the facts to his own advantage", being completely uninformed when he claimed to discuss "matters of policy that of paramount importance to academic life" when he was, in fact "the principal violator".[116] This statement is compelling in light of the actions taken by Coggeshall and Kimpton in the mid 1950s with respect to the Committee. When Rashevsky accused Bennett of "'duplicity' and failure to give adequate support to Rashevsky's discipline", he may have leveled this statement at Coggeshall (in Coggeshall's experience of it) as he "admittedly felt very lukewarm about the unit".[117] Coggeshall's low opinion of Rashevsky becomes even more evident when he states that Rashevsky took pride in educating scientists including Weinberg and Householder, for Coggeshall, this claim did not constitute evidence. Rather, it was good fortune that "he did not deter two able men".[118]

In response, Beadle wrote to Coggeshall that Bennett and Levi share Coggeshall's views. Beadle relayed that some time ago he spoke to his close friend Warren Weaver, "because [Weaver] ha[d] been a supporter of Rashevsky and [Beadle] wanted [Weaver] to get our version before R. brainwashed him". Weaver shared his opinion and termed Rashevsky an "odd ball and a difficult guy". Beadle also shared in a post script that when in Caltech, they "reviewed the R-school to see if [they] wanted it there. The answer from Delbruck and others was a clear <u>no</u> ".[119]

The meeting with Karreman, Johnson and Bartholomay took place at 2.30 p.m. on October 23, 1964. Afterwards Beadle shared the minutes with Rashevsky, who was away in New York, via a letter he sent with Karreman. In the letter Beadle indicated that the meeting concerned a discussion regarding the future of mathematical biology "in general and specifically at the University of Chicago". Beadle mentioned that they "agreed on many things: the significance of Mathematical Biology, its proper place in science and the importance of the continuation of [Rashevsky's] school".[120] He continued to state that they "agreed that it would be in the best interests of all concerned, and especially of the field of Mathematical Biology, if [Rashevsky] could be persuaded to reconsider [his] resignation from the Faculty of the University of Chicago".[121] He added that they "all sincerely hope [he] will be willing to do this". He also suggested that "frank discussions concerning of [sic] all matters pertaining to Mathematical Biology by the Committee on Mathematical Biology and by the ad hoc faculty committee on the chairmanship of the Committee", offering his help and the assistance of Provost Levi.[122]

[115]Ibid.

[116]Ibid.

[117]Ibid.

[118]Ibid.

[119]Hand written note from Beadle to Cogg (Coggeshall) on October 13, 1964, Box 327, Folder 6, BOP-SCRC.

[120]Beadle to Rashevsky, October 24, 1964, Box 327, Folder 6, BOP-SCRC.

[121]Ibid.

[122]Ibid.

Rashevsky thanked Beadle for the reply, explaining that he could not respond in the affirmative as he "must give the matter very careful thought;. . .consult some of [his] trusted friends; and last but not least . . .I feel that I must have a long and perfectly frank discussion with you".[123] As chance would have it, on October 23—on the same day of the meeting with Karreman, Johnson and Bartholomay—Bennett reported to Levi and Beadle that George Kennedy, the Program administrator for the Advisory Committee on Epidemiology and Biometry of Research Training Grants at the NIH, requested a meeting with the President and Provost to discuss the future of Mathematical Biology at Chicago. In particular, they wanted to discuss the role Landahl would play as Program Director of the US Public Health Service training grant in Mathematical Biology. According to Bennett, "it was [his] impression that some of the members of the Review Committee concerned with the training grant [were] aware of Professor Rashevsky's unease about the future of mathematical biology at the University of Chicago" and that they wished "to ascertain [Beadle and Levi's] views . . .about the program".[124]

This request by the NIH was not surprising considering the close contact Rashevsky had with the Advisory Committee on Epidemiology and Biometry, also acting as a consultant to this Committee in evaluating training programs proposed by other institutions, such as the NCSU. Kennedy was joined by Tom Wayne of the University of Kentucky and Charles Flagle of Johns Hopkins. Bennett indicated that he did not "believe there are any realities about the University's posture towards mathematical biology" which made such a visit necessary. However, he suspected it "would be helpful" if the "site visitors could be reassured".[125]

Upon Rashevsky's return to Chicago on November 3, 1964 he wrote Beadle in a personal and confidential letter delivered by a messenger in which he indicated that he has "decided that the only condition under which [he] could withdraw [his] resignation in favor of retirement would be if [he] retire[s] as a Chairman of a *Department* of Mathematical Biology" [emphasis added].[126] This was to ensure that "[Rashevsky] was leaving something permanent behind".[127] Rashevsky agreed to a meeting with Beadle and Levi that took place at 10 a.m. on November 27, 1964. A copy of the letter in the President's archival papers was marked "*Ho Hum*", perhaps capturing the administration's stance on the entire matter.

The *ad hoc* Committee re-assembled on the morning of December 11, 1964 and presented a dilemma: there was no proper candidate for the position of the Chairman on one hand, yet they could not decide on organizational matters relating to the Committee (such as making the committee into a department) without a decision on the chairmanship. Moreover, in light of the establishment of a Divisional Planning Committee with a "charge to make recommendations as to academic organization,

[123] Rashevsky to Beadle, October 26, 1964, Box 327, Folder 6, BOP-SCRC.

[124] Bennett to Levi and Beadle, October 23, 1964, Box 327, Folder 6, BOP-SCRC.

[125] Ibid. Kennedy to Beadle November 23, 1964, Box 327, Folder 6, BOP-SCRC.

[126] Rashevsky to Beadle, November 3, 1964, Box 327, Folder 6, BOP-SCRC.

[127] Ibid.

program and space", it was "inappropriate to recommend establishment of a new department at [that] time".[128]

An unidentified individual present at the meeting shared with Beadle that when [he/she] joined the meeting "the group was quite hostile to appointing Landahl as chairman at that time. Each spoke on the subject. The one most blunt about Landahl's lack of qualifications was [Richard] Lewontin-who [was] not a regular member of the committee".[129] In view of the feelings of the ad hoc Committee members, a strong suggestion was made by one of the members not to appoint Landahl at that time, the unidentified member stated that he made "the argument for appointing Landahl on the point that if he were to be Acting Chairman and this was try-out period on administrative skill, it was not quite fair try-out, and also on the point that impression had been created that the question was not anything other than administrative skill".[130]

This point of "administrative skill" which Landahl was lacking elicited a strong reaction from the group, notably from Richard Lewontin, associate dean at the Division of Biological Sciences, who asserted that it was not "just a question of administrative skill-and…went much further than that".[131] Lewontin neither admired Rashevsky nor did he approve of his mathematical biology. As Lewontin would admit years later, "it was I who refused to appoint Rashevsky's hand-picked successor".[132] In the eyes of Lewontin, the Rashevsky school's approach was "to make simplified physical models that were supposed to capture the essence of a biological phenomenon and then describe models in mathematical terms".[133] According to Lewontin, the failure of Rashevsky's school was in not taking into account the conviction of biologists that real organisms were complex systems whose actual behavior would be lost in idealizations. As he would write several decades later "[t]he work of the school was regarded as irrelevant to biology and was effectively terminated in the late 1960s, leaving no lasting trace".[134]

Nevertheless, there was still a pressing need to fill for the position of a director for the Program for Training in Mathematical Biology, and Bennett offered Landahl the job of acting chairman "to work for the furtherance of this field by providing vigorous and far-sighted leadership". Landahl accepted for the time period of January 1, 1965 to June 30, 1965, Rashevsky's scheduled retirement. [135]

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[128]Bennett to Landahl, December 22, 1964, Box 327, Folder 6, BOP-SCRC.

[129]Typed note, addressed to Beadle, n.d., Box 327, Folder 6, BOP-SCRC.

[130]Ibid.

[131]Ibid.

[132]Richard Lewontin, "Science and Simplicity", *The New York Review of Books* (May 1, 2003).

[133]Ibid.

[134]Ibid.

[135]Beadle to Landahl January 15, 1964, Box 327, Folder 6, BOP-SCRC.

The End

For Rashevsky, the year 1964 was marked with special significance. Thirty years had passed since he came to Chicago as a Rockefeller Fellow; the year marked 35 years since the establishment of the Bulletin of Mathematical Biophysics; and it was also the year in which Rashevsky turned 65. To celebrate this event, his friends and colleagues organized a dinner in his honor on December 11, 1964. At this dinner Rashevsky was presented with a bound volume of letters written by colleagues and friends from around the world. Although the volume contained more than 100 letters, it was missing contributions from members of the administration—present or past, with the exception of Robert Hutchins, and perhaps most striking is the absence of Warren Weaver. A letter was written by the members of the Committee and was read to Rashevsky. It conveyed an unmistakable warmth and affection shared by his students and associates.

Peter Greene summarized Rashevsky's career in a poem he composed; these are its final verses:

> . . . he was more than an abstract innovator. . .
> He's so versatile, he can switch whenever he wishes. . .
> He's leaving us and to Michigan driving. . .
> Although some people think that this man must be nuts,
> That equations will never explain blood and guts,
> While others hold that to be pure, mathematics
> Must never desert ivory towers and attics;
> Although at first his following was lean
> (Does that man think that we're just a machine?),
> Nowadays he's scientifically living in clover—
> He's an official classic: reprinted by Dover.
> How far he's gone since he met the test
> Of a man with a gun aimed at his chest.
> But luckily the trigger he didn't pull. . . .
> . . . we have him, and so we hope
> It's written in his horoscope
> That always and forever may
> His e [excitation] exceed his i [inhibition]![136]

In a letter McCulloch sent to Rashevsky, he wrote: "You have put the scientific world in your debt by giving it a place to publish the mathematics necessary for biology. . .You have, with simple nobility, defended new ideas on their young hind legs. . .The solution of any problem is always less important than the proper challenge. We, who still remember these things, salute you as their beginner."[137]

By this time, Rashevsky had left for Michigan and begun a new chapter in his life where he concentrated primarily on research and further actions towards the

[136]HD Landahl, "A Letter Read to Professor Rashevsky at a Dinner in His Honor December 11, 1964", *Bulletin of Mathematical Biology* 27(1965).

[137]McCulloch to Rashevsky, December 16, 1964, Warren S. McCulloch Papers, APS. Cited in Abraham, "Nicolas Rashevsky's Mathematical Biophysics."

dissimilation of Mathematical Biology. By the end of 1965 plans were underway for the construction of a new building to accommodate the non-clinical sciences of the Division of Biological Sciences at the University of Chicago. Perhaps symbolically, the building on Drexel for mathematical biology was proposed for demolition to facilitate the new building for Biological Sciences.[138] The fate of Mathematical Biology at Chicago remained uncertain for several years, until the reorganization was complete. Ultimately a department for Theoretical Biology was established in 1972—and the Committee for Mathematical Biology was abolished.

With the way having been cleared by Rashevsky's departure, the Committee was about to undergo major changes as planned by the administration in general and Lewontin in particular. Bennett was not reappointed as Dean; instead, Jacobson assumed the position and Richard Lewontin filled the spot of Associate Dean. Yet the Rashevsky affair was far from over at the university, as the administration and trustees were soon to realize.

The sticky and uncertain situation in Chicago was of great concern to the scientific community. The *Rhode Island Medical Journal* ran an editorial article in its December issue entitled "Rashevsky Resigns at Chicago". The article spotlighted Rashevsky's resignation after 30 years at the university and 6 months prior to retirement, stating that "it should be of interest to all professional people". To the administration's dismay, the article stated that Rashevsky's resignation was due to "interference of administration in the areas which have been historically and traditionally the prerogative of department chairman and faculty members".[139] The article elaborated that a "lack of tact on the part of administration, lack of understanding, and lack of professional respect may well have been [the] center of the controversy". It stated that the crux of the Rashevsky affair was that the administration and trustees "speak of professors as their 'hired men'" Rashevsky was portrayed as not only a great scientist and philosopher but a man of loyalty and uncompromising and impeccable integrity, who acting on principle could not accept the dramatic and dishonorable action taken by the University. The University was accused of disregarding the petitions of "[m]athematical biologists throughout the world"; the article concluded with an exhortation that the University of Chicago must find a way to resolve"[the] problem with justice".[140]

Karreman sent a copy of the article to members of the board of trustees, Beadle, Coggeshall and Levi, with accompanying cover letters, writing one to Beadle and another to the rest. In the letter to Beadle, Karreman wrote that he "personally" agree[s] with the article', making reference to the meeting he had with Beadle months ago.[141] Beadle responded politely in writing, stating that he did not see how

[138]Plot Plan–Ellis-Drexel, November 9, 1965, memo to the university architect from vice president for administration, Box 58, Folder 6, BOP-SCRC.

[139]Editorial, "Rashevsky Resigns at Chicago", *Rhode Island Medical Journal* Vol. XLVIII, no. No. 12 (1965).

[140]Ibid.

[141]Karreman to Beadle, February 10, 1966, Box 327, Folder 6, BOP-SCRC.

they could do more than they were doing, "which [was] to keep Mathematical Biology as strong as possible". He was also pleased to report that with Richard (Dick) Lewontin in Zoology as the "new associate Dean for the non clinical departments in the Division, prospects look[ed] good".[142] Beadle considered Lewontin "competent in Mathematical Biology and [being] both sympathetic [to] and understanding of the general problem."[143] Again, Coggeshall's response was somewhat cynical, mocking and reflective of his opinion of Rashevsky. His sarcastic hand-written note to Beadle and Levi read thus:

> Old whiskers never die, they just spray away.
>
> What a remarkable editorial writer has the Rhode Island Med [sic] Journal-it could have been written by R.[ashevsky] himself so well does it portray his side of the story-but of course he would never stoop as low.[144]

Responses from the trustees varied. Some believed that the trustees should not get involved whereas others suggested that a member of administration of the University should respond. Eventually, after a discussion that took place on February 21, 1966, during a Budget Committee meeting, participants agreed that Walter Leen, Secretary of the Board of Trustees and the University's Legal Counsel acknowledge receipt of the letter to Karreman. In the laconic acknowledgement letter, Leen wrote "that the letter would receive the Trustees' careful attention".[145] Beadle issued an internal memo suggesting that Levi "prepare a brief statement for the board" describing the state of affairs in which Rashevsky "insisted he should have the right to name his successor".[146]

The confidential memorandum to the trustees' listed the factors that had led to the editorial and stated in brief that "because of the approaching retirement of Professor Rashevsky. . .Dean Bennett in the spring of 1963 appointed a faculty committee to advice the choice of a new chairman".[147] In a draft of the memorandum one of the "added difficulties" was the fact that members of the Committee on mathematical biology could not be invited because Landahl was the only other tenured member (aside from Rashevsky). However, this section was deleted from the final version sent to the trustees. One of the trustees, Howard Wood, requested that a response be drafted by Rod McKittrick as the latter has conveyed to Levi. On February 28, 1966, Levi responded that "he [was] somewhat surprised that Dr. Karreman who wishes to be helpful should think that his letter so broadcasted would be helpful" and ended the letter stating that "It all seems a little like C.P. Snow".[148]

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[142] Beadle to Karreman, February 17, 1966, Box 327, Folder 6, BOP-SCRC.

[143] Ibid.

[144] Handwritten note by Coggeshall, dated February 23, 1966, Box 327, Folder 6, BOP-SCRC.

[145] Walter Leen to Karreman, February 23, 1966, Box 327, Folder 6, BOP-SCRC.

[146] Handwritten note by Beadle, dated February 23, 1966, Box 327, Folder 6, BOP-SCRC.

[147] Confidential Memorandum "Comments on Professor George Karreman's letter to the Trustees regarding Professor Rashevsky", Ed Levi to Trustees, n.d. circa February 1966, Box 327, Folder 6, BOP-SCRC.

[148] Levi to McKittrick, February 28, 1966, Box 327, Folder 6, BOP-SCRC.

By October 1966 Lewontin announced that he had offered Jack Cowan (1933–) the Chairmanship of the Committee in mathematical biology; unsurprisingly, this move spurred a new wave of resistance and aggravation on the part of the members of the Committee.

Lewontin's choice of Jack Cowan raises a set of complicated questions. What was Lewontin looking for in a new Chairman for the Committee? While one of the primary arguments against Landahl was his lack of administrative skills, why did Lewontin choose a graduate student who had never held an administrative position to chair the Committee? If a lack of contact with the biological community was one of the accusations hurled against the current committee members, why was the position offered to an electrical engineer with a short track record in biology rather than a biologist or perhaps a theoretician with a record of collaboration with the experimentalists? Was Lewontin acting out of conviction that Cowan was most suited for the position or was he acting out of pressure to fill the position, evaluating his candidates based on one lecture? Could it be that ushering in a young and inexperienced 'administrator' would afford Lewontin more freedom to have his way at the Committee, changing its face and erasing all traces of Rashevsky? After all, that sort of move would definitely ensure that Rashevsky's school would be abolished as none of his followers would stay to work under Cowan.

Jack D. Cowan was born in 1933 in Leeds, England. He received his Bachelor in Science degree in Physics from Edinburgh University in 1955 and was awarded D.I.C. in electrical engineering in 1959 for a thesis on "Many–Valued Logics and Problem Solving Mechanisms". In 1960 he received his Master of Science degree from MIT for his work on "Analog-Digital Neural Nets". Cowan stayed at MIT as a staff member of the Research Laboratory of Electronics and then as a full-time research member of the Neurophysiology group headed by Warren McCulloch. In 1966, Cowan was offered a position of visiting scientist; moving back to England, he assumed a position at the Imperial College of Science and Technology where he worked on mathematical models of nervous activity and the problems of evolution, maintenance, and degradation of 'organization' in living systems. He earned his PhD, from the University of London in 1967 for his work on the mathematical modeling of nervous activity.

Cowan was chosen for the position of Chairman during the summer of 1966. Lewontin met Cowan at the 1966 IUSB conference on Theoretical Biology directed by Conrad Waddington (28 August–3 September). Conrad Hal Waddington was a pioneering theoretical biologist. His interest in theoretical biology first fostered in the Theoretical Biology Club whose members included Joseph Needham and his wife Dorothy, the biologist Joseph Woodger, physicist John Bernal, cell biologist E. Nevile Wilmer, and Peter B. Medawar.[149] Over the years, Waddington became increasingly active in promoting theoretical biology. This intense effort culminated in four annual symposia at the Villa Serbelloni on Lake Como in Italy, under the auspices of the Rockefeller Foundation. The symposia were eventually published as

[149] Waddington, "Towards a Theoretical Biology."

a set spanning four volumes by Edinburgh University Press entitled *Towards a Theoretical Biology* (1968, 1969, 1971, 1972). It contained what one commentator referred to as a “bizarre miscellany of articles” about whether there can be general theories in biology, and some specific speculations about development and evolution.[150]

With Lewontin considering the current Committee as “a waste of intellectual effort” and bearing no connection to the biological reality, he was set on restructuring the Committee and was given an open purse to do so. One of Lewontin’s primary concerns was to bring in young and ambitious people into the Committee. Invited to the IUSB meeting by Waddington, Lewontin incidentally heard a talk by Jack Cowan, where the latter “made a big impression” on him. Lewontin gathered that Cowan had his feet firmly grounded in what was going on in biology.[151] This one-time event was enough for Lewontin to extend the young graduate student a full professorship and the job of Committee Chairman. Lewontin invited Cowan for a walk in the countryside, accompanied by the distinguished population biologist Ernst Mayr, and laid the entire plan out for Cowan. That sort of an offer was hard to resist.[152] Cowan was to come in and change the face of the Committee, revive it and modify its direction. “They wanted it to be connected with biology and mathematics.”[153] He would have all the funds required to do so as well as the administration’s full support. Within weeks, Cowan was flown to Chicago with his wife, who was 7 months pregnant. At Chicago, Cowan was wooed by Beadle, Dean Jacobson, Levi, and Lewontin. As Cowan recalls it, he was talked into the job.[154]

Cowan’s name was by no means new to the Committee members or to Rashevsky. Cowan was a close friend of Peter Greene, who was Rashevsky’s student and later on a member of the Committee. Invited by Greene, Cowan lectured at a Friday meeting held by the Committee in the early 1960s. Moreover, Cowan, was invited to the Cullowhee meeting while working on his Master’s degree and lectured on “Information Theory in Biology”.[155] The decision to appoint Cowan, still a graduate student, to “replace” Rashevsky felt like a slap and an insult to the members of the Committee and in particular to Rashevsky and to Landahl. Robert Rosen, a Committee member, found out about the offer extended by Lewontin while visiting at the University of Buffalo from Peter Greene, who was also a good friend of his. Rosen vented his disapproval and

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[150]J.M.W. Slack, “Conrad Hal Waddington: The Last Renaissance Biologist?”, *Nature Reviews Genetics* 3, no. 11 (2002); Waddington, Zeeman, and Buneman, *Towards a Theoretical Biology*; Waddington, “Towards a Theoretical Biology”; ———, *Towards a Theoretical Biology. 1. Prolegomena: A Iubs Symposium*; Waddington, Sciences, and Biologiques, “Towards a Theoretical Biology.”

[151]Lewontin interview, March 8, 2011.

[152]Cowan interviews, 2010 and 2011.

[153]Cowan interview, 2010.

[154]Cowan interview, 2010.

[155]Lucas, *The Cullowhee Conference on Training in Biomathematics*. page 297–310.

anger regarding the absurd situation in a correspondence with Lewontin, sending a copy to Provost Levi.[156]

In Rosen's vernacular, the Committee was "in a desperate predicament…The whole world is aware of the situation of the Committee, and blames this situation on the deliberate enmity of the Administration over the past five years or more". Rosen lashed out, stating that "there have been no tenure appointments into the Committee since the middle 1940s" and that the "chairmanship question has been allowed to drift endlessly". These factors have led to a situation where "it is virtually impossible to recruit new staff" as "no one wants to come to Chicago under the circumstances" and in light of the fact that other "top" Universities provide for an alternative, these "are rushing to fill the need and demand for mathematical biologists".[157]Rosen was referring to Harvard, North Carolina State University, the University of Texas, and many others universities around the United States that were developing programs in mathematical biology with funding provided in the main by the NIH.

Rosen opined that in order to minimize the publicity damage to the Committee and administration, the University should issue an "immediate, public, meaningful gesture…which will demonstrate active support for the Committee and its work".[158] Such a gesture would be in the form of a "chairman of ***stature***". While Jack Cowan was a "fine young scientist" whom in the past the Committee "desperately tried…to sell to the Administration…without success", choosing him as chairman would not be considered as such a gesture. In the words of Rosen: While Cowan was "without doubt aggressive and ambitious", he was by no means considered a man of stature, and definitely "not the kind of man that is needed to show University support of Committee heavily tainted by the university neglect, if not active enmity".[159]

The right sort of individual "would have to be bargained with, would have to be wooed and promised things"—not someone whom "[Lewontin] met at random at some meeting and thought was impressive". Rosen accused Lewontin of taking "an easy way" and getting himself "off an unpleasant hook" by choosing Cowan.[160]

In the meantime, Lewontin "actively and continually heaped abuse on Herb Landahl…and made it impossible for him to function effectively".[161] According to Rosen, Lewontin continuously stated that "[Landahl] does not function effectively .... meaning that [the committee has] to hurry to replace him, and don't have the time for negotiations". The ultimatum intimated by Lewontin was "either Cowan or the dissolution of the Committee". Lewontin implied that not welcoming Cowan

[156]Rosen to Levi, October 25, 1966, Box 327, Folder 8, BOP-SCRC; Rosen to Lewontin, October 23, 1966, Box 327, Folder 8, BOP-SCRC.

[157]Ibid.

[158]Ibid.

[159]Ibid.

[160]Ibid.

[161]Ibid.

would be "unreasonable" on the part of the Committee; if that transpired they "deserve to be disbanded".[162] Rosen quite openly wrote to Lewontin: "If you insist on going ahead on the course you've indicated, then Mathematical Biology will indeed be dead at Chicago. Once again, the University will find itself 180° out of phase with reality, lagging rather than leading".[163] Perhaps this stance was what Lewontin desired, though he would have been loath to admit it. After all, Lewontin openly admitted that Rashevsky and his school were living in a world different from his, an opinion he formed without ever having met Rashevsky in person or reading any of his work.[164] Lewontin accused Rashevsky's school of failing to take into account "the conviction of biologists that real organisms were complex systems whose actual behavior would be lost in idealizations". [165] Hailing the work of the group as irrelevant to biology, Lewontin did everything in his power to "effectively" terminate it, leaving no lasting trace.[166]

In Lewontin's response to Rosen, he was equally aggressive and blunt, mincing no words. Lewontin viewed the Rosen letter as a curious series of "non sequitur". He accused Rosen of lacking "the analytical thinking" expected of him. Rosen's letter was for Lewontin contradictory as on one hand Rosen insists on appointing a Chairman as soon as possible and on the other requests to woo and search for a candidate, a process that takes time. Lewontin stated that the crux of his approach was to "rejuvenate that Committee and make it lively, attractive enterprise" whereas Rosen's tactic "would be to spend as long as necessary finding some fifty-year-old man whom you believe would bring universal luster to the committee".[167] Lewontin did not believe one could bring such a man to the University of Chicago. His tactic was different. The "men of stature" he approached were "not truly exciting" while with impressive records of "solid scholarship". "No! My Tactic is different" repeated Lewontin. It was "to appoint to the Committee as many young, active, aggressive and brilliant men as I will find and arrange that the one of them that seems to have the most aptitude for administration and dealing should become the Chairman".[168] Although Lewontin did not consider Cowan as "a paragon", he did believe that he had "sufficient vigor to pursue his fruitful research work and at the same time administer this small group and make it grow", as per the recommendations of Warren McCulloch and Jerome Lettvin.[169]

Rosen responded by clarifying that since he had disengaged with Chicago following his move to the University of Buffalo, "people have been a lot franker with [him] about their appraisal of the situation of the Committee" and emphasized

[162] Ibid.

[163] Ibid.

[164] Lewontin Interview, March 8, 2011.

[165] Lewontin to Rosen, November 3, 1966, Box 327, Folder 8, BOP-SCRC.

[166] Lewontin, "Science and Simplicity."

[167] Lewontin to Rosen, November 3, 1966, Box 327, Folder 8, BOP-SCRC.

[168] Lewontin to Rosen, November 3, 1966, Box 327, Folder 8, BOP-SCRC.

[169] Ibid.

the fact that "by 'people'" he meant "'*knowledgeable people*'".[170] To "drive [the] point home" as Rosen put it, he stated that the uncertainty regarding the situation of the Committee is such that "Warren McCulloch told [Rosen] that he would advise Cowan against accepting the Chairmanship, because he was certain that the University would reneg (sic) on any promises made to him".[171] Since McCulloch's judgment regarding Cowan's credentials was accepted, why should his judgment regarding the University be treated any differently, wondered Rosen.[172] In a letter to Beadle, Rosen wrote that "our chairmanship question touches on matters of urgent concern to the entire University community" and suggested he "should be directly informed of all its ramifications".[173]

Karreman also recorded his opinions on paper, writing to Levi that consideration of "Dr. J. Cowen (sic)" makes "the anxiety of the students of the committee who had heard rumors about the possibility of this appointment" understandable. He further indicated that Cowan is at the beginning of his career and wondered if "it will be possible to attract other staff to the Committee on Mathematical Biology if its Chairman has relatively little experience".[174] The note was forwarded to Lewontin by Levi. Lewontin responded bluntly "1)...this is not the first time that he has stuck his nose in the University's business without being asked; 2) that he is not telling the truth when he says the students are "anxious". He continued to state that "Nicolas Rashevsky is anxious. Not to mention Professor Landahl." Finally, as a side note, he added that " 3) his familiarity with the work of Dr. Cowan apparently does not extend to knowing how to spell his name".[175] Clearly, Lewontin did not intend to change his mind about Cowan. Lewontin's aggressiveness in this direction is somewhat foggy and unclear. When interviewed on this matter, he recalled being driven by an overwhelming feeling that a closer contact was needed with what was going on in Biology.[176]

Having accepted Lewontin's offer, Cowan soon received a cablegram from Beadle: "Your acceptance enthusiastically received. Red carpet on order".[177] Levi also sent a letter expressing his delight that Cowan was coming.[178] The members of the Committee were less enthusiastic. Following this action, the only member to remain, apart from graduate students, would be Cowan's friend, Peter Greene. Rosen resigned on April 24, 1967. Landahl followed suit, leaving for a sabbatical effective October 1, 1967, and never returned to Chicago. Cowan's

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170 Rosen to Lewontin, November 10, 1966, Box 327, Folder 8, BOP-SCRC.

171 Ibid.

172 Ibid.

173 Rosen to Beadle, November 21, 1966, Box 327, Folder 8, BOP-SCRC.

174 Karreman to Levi, February 1, 1967, Box 327, Folder 8, BOP-SCRC.

175 Lewontin to Levi, February 7, 1967, Box 327, Folder 8, BOP-SCRC.

176 Lewontin Interview, March 8, 2011.

177 Hand written note addresses to Cowan in London, by beadle dated April 10, 1967, Box 327, Folder 6, BOP-SCRC.

178 Levi to Cowan, April 19, 1967, Box 327, Folder 6, BOP-SCRC.

appointment was effective July 1, 1967. This date marks the official end of the "Mathematical Biology" experiment at Chicago. Within months the Committee would be transformed. As imagined by Lewontin, no trace of Rashevsky would remain. Even the Committee's name was to be changed into the Department of Theoretical Biology and Biophysics.

Whereas the administration made it easy for Lewontin to transform the Committee, the funding agencies were concerned with the changes. For example, the Sloan Foundation was slated to approve a 400,000 dollar grant to the Committee. The Foundation sent over Mark Farinholt to examine the situation at the Committee of mathematical biology. Drawing on his close connection with Warren Weaver, a trustee at the Sloan Foundation, Beadle wrote to him in January 1968: "I know you'll be asked your opinion, so I want you to know the atmosphere is one of enthusiasm. If there are any doubts in your mind, why not come have a look-see?"[179] He continued to state that the administration has "gone through a pretty traumatic experience with Rashevsky", who on retirement "insisted that he should name Herb Landahl as his successor". Beadle explained to Weaver that the resistance was on two counts "in principle and the qualifications of his candidate" who apparently was "no administrator whatsoever". While with Jack Cowan, Beadle asserted that the "Committee math biol(sic) is on its way with general support from medicine biology, math., biophysics". He reassured Weaver that the administration was "fully behind the new move and have pledged our best effort to find the support to move forward until vigor".[180]

The Sloan Foundation also sent over Warren McCulloch to Chicago to look at the Committee. Lewontin and Beadle spent time with McCulloch and as they reported to the Sloan Foundation "reassured all Warren's doubts about the future of Math Biol(sic)...so we ...hope he has given [Sloan Foundation] the green light to go all out", ending the letter "the prospects for the future of math boil(sic)really do look good".[181] These efforts did allay the concerns of the Sloan Foundation and the grant was approved by May, 1968 for the following 2 years.

Another funding agency in need of reassurance was the NIH; in particular, it was the National Institute of General Medical Sciences that provided the training grant to the Committee, which was now to be directed by Cowan. On Lewontin's request, Beadle wrote to Fred Stone (director at the National Institute of General Medical Sciences) at NIH that: "after a rather traumatic shakeup of Math Biology Com (sic), we have enormously improved the operation and made it a real part of the University. Lewontin is superb in this area...and so is Jack Cowan, the new Chairman. We've already stimulated wide interest in this area-...The Rashevsky school is gone and some of its members are pretty bitter about it. The change is of great and vast importance".[182] Should Stone have any doubts, he should visit or

[179]Beadle to Warren Weaver, at the Sloan Foundation, January 18, 1968, Box 327, Folder 6, BOP-SCRC.

[180]Ibid.

[181]Beadle to Mark Freinholt at Sloan Foundation on February 28, Box 327, Folder 6, BOP-SCRC.

[182]Beadle to Fred Stone at NIH, on April 11, 1968, Box 327, Folder 6, BOP-SCRC.

have a "session by some of [NIH] people with Warren Weaver" who was apparently persuaded of the great change. In this connection Beadle added that "Warren was once a backer of the Rashevsky school but now agrees this was not the most effective approach (understatement). He is 100 % behind our present set-up and convinced that the remnants of the old school are well gone" accentuating this last point by stating- "this is important."[183] Nonetheless, by April 1968, the NIH reported to Lewontin that the training grant that had supported the Committee since the early 1960s would be terminated by the end of the 1969 academic year.[184] If Cowan wanted the training grant, he had to submit a proposal in his own name that would fit the program's structure as he planned it.

Cowan changed the graduate students' curriculum. Calling the curriculum "trial and error", Cowan modified the courses offered to the students.[185] Thus it was not surprising that NIH requested a new proposal, because the grant had been extended based on the old one. A new chapter opened for the "Committee", but in no time at all, things changed. In 1970 Lewontin left the University of Chicago for Harvard; the Committee was turned into a department after merging with the biophysics section; and Cowan transferred to the mathematics department. Ultimately, no trace of the Committee was left at the University save the archival documents.

Trotsky of Mathematical Biology

After Rashevsky accepted the offer extended by James Miller, founder of the Mental Health Research Institute at the University of Michigan, he left for Michigan, never setting foot in the University of Chicago again. With the sole administrative responsibility confined to running and editing the Bulletin on Mathematical Biology, Rashevsky was free to pursue his scientific interests. His main pursuit was further development of the fundamental principles in biology, working in particular on relational biology as well on a new subject of personal interest; "mathematical biology of automobile driving". This final topic became part of Rashevsky's intellectual trajectory as of 1959.

Rashevsky's research on automobile driving attracted the interests of the research laboratories at General Motors (GM), in particular those of the physicist Robert Herman, an adviser to General Motors and head of its theoretical physics department.[186] Rashevsky maintained close personal and professional contact with

[183]Ibid.

[184]Trygve Tuve to Lewontin, April 22, 1968, Box 327, Folder 6, BOP-SCRC.

[185]Interview with Jack Cowan, March 2011, University of Chicago.

[186]Herman gained fame as a physicist by predicting in the 1940s that a microwave echo of the Big Bang would someday be found. (The echo was found in 1978 by Dr. Arno Penzias and Dr. Robert Wilson of Bell Laboratories. The two scientists shared the 1978 Nobel Prize in Physics for their discovery.)

Herman for several years, was invited to GM's research laboratories to lecture, interacted with its researchers and participated in symposia on the "Theory of Traffic Flow". Rashevsky visited the laboratories several times to collect data using laboratory simulators to verify his theories.[187]

In the main, Rashevsky examined aspects of the car-driver interaction in terms of the mathematical model of the central nervous system. What he found was that the safe speed of driving on a empty straight lane is inversely proportional to the reaction times of the driver for the stimuli that determine the steering wheel's "corrective movements". He used a neurobiophysical model for reaction times that was developed by H. D. Landahl in 1939 to outline a possible theoretical study of different "distracting" stimuli upon the reaction time of the driver and therefore on the safety of the driving.[188]

Rashevsky was interested in model driving as well as in "man-machine interaction".[189] The conclusion of his research was that the maximum safe speed for driving could be modeled by the following equation:

$$V_{\max} = \frac{w - 2\delta - c}{\theta t}$$

where Vmax is the maximum driving speed, w is the track width, δ is the distance of the boundary from the track edges, c accommodates the dimension of the car, θ is the angular error in driving, and t is the driver's reaction time. Although

[187]GM Folder, NRP-SCRC.

[188]HD Landahl, "A Contribution to the Mathematical Biophysics of Psychophysical Discrimination", *Psychometrika* 3, no. 2 (1938); ———, "A Contribution to the Mathematical Biophysics of Psychophysical Discrimination Ii", *Bulletin of Mathematical Biology* 1, no. 4 (1939); N. Rashevsky, "Mathematical Biophysics of Automobile Driving", *Bulletin of Mathematical Biology* 21, no. 4 (1959); ———, "Some Remarks on the Mathematical Aspects of Automobile Driving", *Bulletin of Mathematical Biology* 21, no. 3 (1959); ———, "Further Contributions to the Mathematical Biophysics of Automobile Driving", *Bulletin of Mathematical Biology* 22, no. 3 (1960); ———, "Contribution to the Mathematical Biophysics of Automobile Driving", *Bulletin of Mathematical Biology* 23, no. 1 (1961); ———, "Automobile Driving as Psychophysical Discrimination", *Bulletin of Mathematical Biology* 24, no. 3 (1962); ———, "Mathematical Biology of Learning to Drive an Automobile", *Bulletin of Mathematical Biology* 25, no. 1 (1963); ———, "Man-Machine Interaction in Automobile Driving", *Progress in Biocybernetics* 42(1964); ———, "Mathematical Biology of Automobile Driving: I. The Shape of the Tracking Curve on an Empty Straight Road", *Bulletin of Mathematical Biology* 26, no. 4 (1964); ———, "Two Remarks on the Mathematical Biology of Automobile Driving", *Bulletin of Mathematical Biology* 26, no. 1 (1964); ———, "A Note on the Mathematical Biology of Automobile Driving", *Bulletin of Mathematical Biology* 29, no. 1 (1967); ———, "Mathematical Biology of Automobile Driving", *Bulletin of Mathematical Biology* 29, no. 1 (1967); ———, "Mathematical Biology of Automobile Driving: Iii", *Bulletin of Mathematical Biology* 30, no. 1 (1968); ———, "Mathematical Biology of Automobile Driving Iv", *Bulletin of Mathematical Biology* 32, no. 1 (1970); ———, "Mathematical Biology of Automobile Driving: V", *Bulletin of Mathematical Biology* 32, no. 2 (1970).

[189]N. Rashevsky, "Man-machine interaction in automobile driving", *Traffic Safety*, 9:161–167, 1965.

Rashevsky's scientific project was initially entirely analytical, he performed empirical experiments at the General Motor's research laboratories to verify his theories.

He also proposed an algorithm for steering an automobile of designated width and length along a straight road of specified width, which resulted in issuing instantaneous steering corrections to the vehicle whenever it approached either lane edge within a specified margin. Although Rashevsky did not consider vehicle dynamics within this mathematical treatment, driver anticipation and driver delay properties were included as key parameters. Rashevsky observed:

> To sum up, we see that the combination of human parameters and of mechanical parameters enter into the process of driving in a manner which does not permit their clear-cut separation. The car and the driver form, in a sense, an individuum.[190]

In another work Rashevsky stated:

> The car and driver constitute a complex feedback system. The behavior of the car results in certain reactions by the driver. Inversely, the behavior of the driver affects the behavior of the car. This 'man-machine' system cannot, in many instances, be separated into the purely 'mechanical' and the purely 'human' components. The system must be treated as a whole.[191]

Rashevsky recognized the importance of treating the driver and vehicle as a combined system.[192] These research projects would attract considerable interest in the studies of "driver-vehicle interactions" and constitute perhaps some of his most cited publications by researchers beyond his close group of students and collaborators.[193]

Although Rashevsky considered himself more of a biologist than a physicist, he had been only very loosely associated with the biological community since his resignation.[194] The archival records lack any documentation of his contacts with the biological community during his stay in Michigan. His work published between the

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[190]Rashevsky, "Automobile Driving as Psychophysical Discrimination."

[191]Rashevsky, Neglected Factors in Highway Safety. University of Michigan Mental Health Research Institute, Grant GM-12032-01, 1966, not published.

[192]C.C. Macadam, "Understanding and Modeling the Human Driver", *Vehicle System Dynamics* 40, no. 1–3 (2003).

[193]The following list reflects just a handful of articles that discuss Rashevsky's work on the subject and its contribution to the field of study: S. Zhai, J. Accot, and R. Woltjer, "Human Action Laws in Electronic Virtual Worlds: An Empirical Study of Path Steering Performance in Vr", *Presence: Teleoperators & Virtual Environments* 13, no. 2 (2004); J. Accot and R. Woltjer, "Human Action Laws in Electronic Virtual Worlds: An Empirical Study of Path Steering Performance in Vr", (2004); E.R. Hoffmann, "Review of Models for Restricted-Path Movements", *International Journal of Industrial Ergonomics* 39, no. 4 (2009); N. Thibbotuwawa, R.S. Goonetilleke, and E.R. Hoffmann, "Constrained Path Tracking at Varying Angles in a Mouse Tracking Task", *Human Factors: The Journal of the Human Factors and Ergonomics Society* 54, no. 1 (2012); J. Accot and S. Zhai, "Three Different Approaches to the Law of Steering: A Historical Review"; Macadam, "Understanding and Modeling the Human Driver."

[194]Letter to James King, director of the project on the history of recent physics at the American Institute of Physics, May 29, 1963, Folder K, Box 1, NRP-SCRC.

years 1960–1971 lacks any reference to current biological points of interest. Institutionally and academically free to pursue his interests, he chose to concentrate on his search for fundamental principles in biology and specific problems which captured his interest, such as automobile driving; the quantitative aspects of schizophrenia; the effect of nervous stress on coronary thrombosis; mathematical bio-sociology of the relativity of injustice; leadership; and mass behavior, among others.

Despite his straying from current biological problems, his reputation as the founder of the field of Mathematical Biology and his previous works allowed him to gain financial support from the National Institute of Health, at least for a few years. Between the years 1965–1968, while affiliated with the Mental Health Research Institute at the University of Michigan, Rashevsky's work was primarily supported by NIH grants. He worked in isolation, without any research associates and permanent staff other than his secretary.

In the wider academic sector, beyond the confines of biology, Rashevsky's work gained him prestige. At the first Gordon Research Conference on Biomathematics, which was held in July 1965, Rashevsky was invited as a principal speaker by the conference organizer, neurologist and electrical engineer Lawrence Stark. In his invitation letter to Rashevsky, Stark courted Rashevsky:

> It gives me a great pleasure to invite you to be our principal speaker and to ask . . ..to review the development of Biomathematics at our Thursday afternoon session. The Thursday afternoon session is traditionally the one which is the most formal and has the most honored position, and you may be sure that this reflects my own feelings and those of my colleagues.[195]

Recognizing the importance of Rashevsky and his enterprise, he continued to state:

> As you are no doubt aware, the history of the development of biomathematics is practically your autobiography. . .[196]

Rashevsky gladly accepted the invitation to give the prestigious lecture, stipulating two conditions. One was that his spouse, Emily, be allowed to attend his lecture, as she always did, and the other pertained to the talk's title. He would under no circumstances agree to the use of the term *biomathematics* to describe his life's enterprise.

As he conveyed in the letter to Stark, he asked to substitute the words "Mathematical Biology" for the word "Biomathematics", which he deemed monstrous:

> I have always felt the word "biomathematics" is an etymological monstrosity. There are such disciplines as mathematical physics and mathematical chemistry. But there is no such thing as physicomathematics or chemicomathemaics. Mathematics is the same everywhere. The discipline under discussion is biology in which mathematics is applied. Hence the adjective "mathematical" biology.[197]

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[195]Lawrence Stark to Rashevsky, September 22, 1964, Box 10, Folder "Gordon Research Conference", NRP-SCRC.

[196]Ibid.

[197]Rashevsky to Stark, February 1965, Box 10, Folder "Gordon Research Conference", NRP-SCRC.

Despite the prevalence of the use of the term "biomathematics" to describe the field he spent almost four decades establishing, Rashevsky repeatedly voiced his objection to it. Rashevsky voiced the same rationale (and used the same terminology) when approached by Eugene A. Chofrey, director of the Research Documentation Section at the Division of Research Grants, NIH, to collect material on mathematical biology. Rashevsky insisted that Chofrey substitute the words "mathematical biology" everywhere for Biomathematics.

> I admit frankly that I consider the latter an etymological monstrosity. There has been for centuries such a thing as mathematical physics. We have now mathematical chemistry but there is now such[sic!] noun as physical mathematics, or chemical mathematics. The discipline being physics or chemistry, the adjective "mathematical" indicates that mathematical methods are being applied to that discipline. "Biomathematics" implies that it is some kind of special mathematics. The mathematics used in biology is the same mathematics as everywhere. Therefore, I very strongly feel that the designation "mathematical biology" patterned after mathematical physics, should be used, though it is somewhat longer.[198]

Despite Rashevsky's repeated objections, biomathematics would continue to be used extensively to describe the field of research. One example is in the establishment of the Biomathematics Training Program at the North Carolina State University under the directorship of biostatistician Henry Lucas.

The program was established following the Cullowhee conference in 1961. Sponsored by the National Institutes of Health, it granted Lucas the second training grant for training "biomathematicians" (the first had gone to Rashevsky, when at the University of Chicago). Rashevsky played a formative role in establishing the program, acting as a consultant for the NIH and as a member of the committee overseeing the program on behalf of the NIH. Along with Rashevsky, the NIH committee comprised a member of the NIH staff and two of Rashevsky's former students: Z. Hearon (then the director at the NIH), and A. Householder (at Oak Ridge National Laboratory).[199]

While Rashevsky deemed the term monstrous, others found it necessary to distinguish between Rashevsky's mathematical biology and biomathematics. One such example may be found in the brochure prepared by Lucas entitled *Biomathematics Training at North Carolina State University*.[200] Posing the question What is Biomathematics?, the answer according to Lucas was the "application of mathematics in biology", excluding no area of biology, observational, experimental, or theoretical, basic or applied; excluding no area of mathematics, pure or applied; and finally, excluding no level of sophistication in either field.

[198]Letter to Eugene A. Chofrey, Ph.D., Research documentation Section, Division of research grants, NIH. February 17, 1965, NIH Folder, NRP-SCRC.

[199]Memorandum from Lucas to the North Carolina State University Administration, 8.9.1965 UA135.001, Box 25, Folder 4, Biomathematics records, MBP-MBP-NCSU records.

[200]UA135.0001, Box 26, Folder 2, n.d. MBP-NCSU records.

While according to Lucas biomathematics is “thought of in widely varying ways depending on background and interests”, all the prevailing views are not mutually exclusive. Lucas viewed them as sharing two elements:

(1) They are relevant to biology (including human affairs).
(2) They involve mathematics.

Nevertheless, Lucas did draw a distinction between biomathematics and mathematical biology:

> “Biomathematics” seems to imply an organization of subject matter and activities around mathematical topics with special attention given to biological applications, whereas “mathematical biology” seems to imply a structuring around biological topics with attention given to the appropriate mathematics.[201]

That sort of distinction was useful for developing and classifying courses or areas of work and in classifying people, in the eyes of Lucas. Thus, if one was dedicated to biology but focused primarily on associated mathematical problems, he or she should be classified as a biomathematician. Another individual who focused mainly on specific biological areas and limited his mathematical activity to those that are pertinent should be called a mathematical biologist.[202]

However, there was another reason for the distinction. With the title “Mathematical Biology” almost exclusively associated with Rashevsky’s vision and work, it was sullied by his own reputation as an outsider. Rashevsky’s mathematical biology did not employ statistics (other than for comparisons of theoretical studies with available experiments), nor did it employ computers. His work was concerned principally with the “development of physicomathematical theories of various phenomena”.[203]

Computers were not alien to Rashevsky, as he “followed the literature on application of computers”. However, when requested to evaluate a grant proposal by John Ward, director of the program of Metabolic Biology at the National Science Foundation, that included the application of computers to solution of problems in pharmacological chemistry, Rashevsky declined the request; he conceded that “[he had] never done any computer work…and [had] no direct experience with computers.”[204] The proposal was forwarded to Carol Newton, Assistant professor in the Department of Medicine at the University of Chicago who also held a joint appointment at Rashevsky’s committee in 1964. Many scientists and scientific agencies had met with similar responses from Rashevsky over the years when approaching him to discuss or evaluate studies employing the use of computers. Engaged in a study for the National Academy of Science and National Research Council on the use of electronic computers in biological research, Robert S. Ledley from the Committee on Electronic Computers approached Rashevsky back in 1958,

[201]Ibid.

[202]Ibid.

[203]Rashevsky to Robert Chien, at Searle, July 20, 1963, Folder Searle, NRP, SCRC.

[204]Letter to Ward, October 5, 1964, Box 1, NRP-SCRC.

inquiring about his use of computers as well as that of his staff as investigative or data-processing aids. Rashevsky admitted that he and his colleagues "[had] actually never used any electronic computers", although, "there are . . .a number of problems in mathematical biology which may lead to the need for using such computers."[205] Although Rashevsky and his associates did not use computers, as their research remained "of purely analytical character", there was an exception: the studies of Peter Greene, Rashevsky's former student and by 1963 a professor at the Committee who in the early 1960s was getting into "closer and closer association with computer methods". Though Greene's influence other members were also becoming interested in computers.[206]For Rashevsky, however, mathematical biology constituted the biological counterpart of mathematical physics. Its aim was to develop theories that would "bring an explanation of the intimate mechanisms of life".[207] While he believed the use of computers to be of tremendous value in mathematical biology just as it had been in theoretical physics, he nevertheless concentrated on the theoretical and mathematical ideas that originate in the brain of the researcher before they are fed into the computer.[208] He was interested in paper and pencil models rather than computer assisted calculations and simulations.

With biological sciences progressing faster than ever and revealing their complex nature, the computer and statistical methods were indispensable if one was to apply mathematical tools to investigate various biological phenomena. The younger generation entering the field was in need of a new moniker to describe their practice, one that differentiated (and even perhaps distanced) itself from Rashevsky's *mathematical biology*. Indeed, Rashevsky did the same in the 1940s when he changed Mathematical Biophysics into Mathematical Biology, in order to differentiate his field from biophysics and to broaden the area of research beyond the physico-mathematical interpretation of biological phenomena to include purely mathematical interpretation.

In the late 1960s Mathematical Biology, generally following the mechanistic approach, was, in a manner of speaking, in ill repute. Yet its poor reputation cannot

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[205]Correspondence with Ledley 1958, Box 1, Folder National Academy of Science, NRP-SCRC. In 1959, after touring around the US, Ledley published a widely-read article in *Science* outlining the steps he believed were necessary to bring together biologists and computers. For biologists who wanted to use computers, he prescribed a 'severe and formidable course of study' of the mathematical methods and techniques that formed the analytical basis for the statement of problems in computer programming languages. He also insisted that instead of relying on programming specialists, biologists must learn how to translate and delimit the data that they were gathering into information that the computer could process. J. November, "LINC: Biology's Revolutionary Little Computer", *Endeavour* 28, no. 3 (2004); ______, "*Digitizing Life: the Introduction of Computers to Biology and Medicine*", (Doctoral Dissertation, Princeton University, 2006) R.S. Ledley, "Digital Electronic Computers in Biomedical Science", *Science* 130, no. 3384 (1959).

[206]Correspondence with George Meneely, Director of American Medical Association March 27, 1963, Box 1, NRP-SCRC.

[207]Rashevsky, *Some Medical Aspects of Mathematical Biology*, pg. xiii–xiv.

[208]Ibid., pg. xiii–xiv.

be attributed solely to Rashevsky's outsiderness or his "failure" to move with the times and embrace computational or statistical tools. The atmosphere was that of an aftermath of the advances in molecular biology initiated in the fifties through the discovery of the significance of DNA. As the mathematical biologist Michael Conrad wrote in retrospect, "a generation of theoretical "speculation" was being sent to the graveyard in body bags."[209] Emphasis was placed on experimentation rather than theory. Nevertheless, various institutions were establishing programs in "biomathematics", placing emphasis not on the sophistication of approach but rather primarily on soundness in biology and mathematics. As Lucas wrote circa 1964 in the memorandum on "Biomathematics Training at North Carolina State University" under the section "Requirements to be a Biomathematician":

> . . .there are too many instances of mathematical modeling in the literature and in practical activities that demonstrate sophistication in mathematics and unsoundness in biology or vice versa. Such work can be misleading and even antagonizing to those who know only biology or mathematics, and [a] wasteful if not harmful impact can result.[210]

As such, the soundness of approach was stressed in the NCSU program. Abstraction and purely theoretical/mathematical thinking was no longer the order of the day.

Rashevsky's scientific achievements—including the first mathematical study of neural nets, early mathematical models of pattern formation, and mathematical analyses of cell fission—were overarched by a spirit of relationalism that was highly disconnected from molecules and from the material substratum of life more generally. Rashevsky's approach of abstraction of the "relational" aspects of biological organization at a time when the molecular and material aspects had come to the fore turned him into the Trotsky of mathematical biology.[211]

Last of the Mohicans

Approaching the age of 70, Rashevsky was gratified with his scientific achievements. He was finally reaching the pinnacle of the realization of his vision, planting the first seeds towards the formulation of fundamental principles in biology. By the late 1960s he was working on his last publication, one that he considered to be the "craziest" of them all: "Organismic Sets: Some Reflections on the Nature of Life and Society".[212] Published posthumously in 1972, this research project is considered by his followers and students as "the crown on his lifelong search for common

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[209]M Conrad, "Childhood, Boyhood, Youth", *Soc. Math. Biol. Newsletter* 9(1996).

[210]Memorandum from Lucas to the North Carolinat State University Administration, 8.9.1965 UA135.001, Box 25, Folder 4, Biomathematics records, MBP-NCSU.

[211]Conrad, "Childhood, Boyhood, Youth."

[212]N. Rashevsky, "Organismic Sets: Some Reflections on the Nature of Life and Society", *Holland, Michigan Math. Biology Inc* (1972). The book was published by J.M. Richards Laboratories, founder of which was Rashevsky's mentee, Ralph L. Sherman Sr. the book was edited by edited by Dr. George Karreman, Dr. A.F. Bartholomay, Dr. A. E. Ruark and Dr. H. D.Landahl.

relational aspects among the three basic sciences of physics, biology and sociology".[213]

For Rashevsky the purpose of this work was to "introduce new, iconoclastic ideas into biology, ideas which may at first appear absurd and utterly unpalatable… [the ideas] may be mere precursors of more profound changes in biological thought which will result in [a] real revolution[s] in biology".[214]

In *Organismic Sets* Rashevsky attempts to develop the qualitative theory of organisms and their functions akin to his theory of relational biology developed in 1954; even more than that, he attempts to show how the same kind of qualitative theory of function and process simultaneously underlie physics, biology and the social sciences.[215] In considering the plan underlying *Organismic Sets*, Robert Rosen was reminded of another posthumous work, Bach's *Die Kunst der Fuge*, in which the quadruple fugue which caps the work breaks off at the point where all the themes are about to be combined in one grand synthesis. Rashevsky's intellectual trajectory exhibits this same logic, and his posthumous work represents the point at which the various themes that occupied his work are "just beginning to be combined".[216] The new principles introduced in this book were nondeducibility and the elaboration of the previously developed concept of relational forces with the aim of shedding light on the "nature of life".[217]

Rashevsky's principle of nondeducibility is this:

> While all biological phenomena can be explained in terms of physics and never contradict any physical law, and while all sociological phenomena can be explained in terms of biology and never contradict biological laws, yet the existence of life cannot be *deduced* from physics, nor can the existence of societies be *deduced* from biology.[218]

This postulate led Rashevsky to conclude that a conceptual superstructure should exist to connect physics, biology, and sociology. From that kind of superstructure, physics, biology and sociology would follow "parallel branches", where none can be deduced from the other yet exhibit some similarities, and in mathematical terms are partially isomorphic. Rashevsky was introducing a conceptual superstructure in the form of a "theory of organismic sets".[219]

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[213] G. Karreman in Forward, page vii; Ibid; R. Rosen, "Organismic Sets: Some Reflections on the Nature of Life and Society", *Bulletin of Mathematical Biology* 37, no. 2 (1975).

[214] Rashevsky, "Organismic Sets: Some Reflections on the Nature of Life and Society", pg. 5.

[215] Rosen, "Organismic Sets: Some Reflections on the Nature of Life and Society."

[216] Ibid.

[217] Rashevsky, "Organismic Sets: Some Reflections on the Nature of Life and Society", pg. 17.

[218] Ibid. pg. 21.

[219] N. Rashevsky, "Organismic Sets and Biological Epimorphism", *Bulletin of Mathematical Biology* 29, no. 2 (1967); ———, "Organismic Sets: Outline of a General Theory of Biological and Social Organisms", *Bulletin of Mathematical Biology* 29, no. 1 (1967); ———, "Organismic Sets: Ii. Some General Considerations", *Bulletin of Mathematical Biology* 30, no. 1 (1968); I. Băianu and M. Marinescu, "Organismic Supercategories: I. Proposals for a General Unitary Theory of Systems", *Bulletin of Mathematical Biology* 30, no. 4 (1968); Rashevsky, "Organismic Sets: Some Reflections on the Nature of Life and Society."

An organismic set, the basic concept underlying the work, comprises this:

1. An abstract finite set S, whose elements may be regarded as individual molecules, genes, organisms or collections of organisms.
2. To each element $e_i \in S$ there is associated a set A_i, whose elements represent the potential activities of functions or behaviors which may be exhibited by the element e_i.
3. To each activity $a_{ij\in}$ A_i there is associated a set P_{ij} whose elements represent the product or result of the individual e_i engaging in activity a_{ij}.

The organismic set comprises a set of elements, engaging in a variety of behaviors of which they are capable, together with the products of the activities of the elements. Rashevsky shows how these organismic sets may be interpreted variously within the framework of physics, of biology and of social science. To introduce a dynamical principle into these organismic sets, Rashevsky generalizes the notion of force. Rashevsky introduces his dynamical principle indirectly, by postulating that, essentially, at any point in time the spectrum of activities, the total number of relations and the number of different kinds of relations during the total course of development of an organismic set, engaged in by the elements of S, will change in such a fashion that the number of relations induced by them on the organismic set will be maximized. This is Rashevsky's postulate of relational forces.[220]

Paradoxically, while Rashevsky was finally achieving the apex of his scientific trajectory, his institutional stance was at its nadir. Rashevsky's stay at the Mental Health Research Institute at the University of Michigan was nearing its end; he was to retire in December 1969 at the age of 70. He was in search of a new place to teach and pursue his research. As Rashevsky wrote in a letter dated 1969 to the director of the Division of Research Grants at the National Institutes of Health:

> . . .I simply refuse to believe the possibility that no institution in our country could find a relatively modest space for a man who is . . . acknowledged as the father of contemporary mathematical biology. . .[221]

Unable to renew his grants or receive new ones, Rashevsky was lacking funds to sustain his research in the institutional arena. While the NIH held Rashevsky in high regard, application for grants to support projects simply identified as "Research in Mathematical Biology" were not viewed favorably and were eventually refused. Although Rashevsky explained that he viewed himself as "the last of the Mohicans as a universalist" in the field of mathematical biology in which specialization has set in out of necessity, the NIH insisted that funding would be extended for specific projects alone rather than developing the field as a whole. [222]

[220] Rosen, "Organismic Sets: Some Reflections on the Nature of Life and Society"; Rashevsky, "Organismic Sets: Some Reflections on the Nature of Life and Society."

[221] Rashevsky to Irving Gerring, February 26, 1969, Folder "NIGMS-RSS", NRP, SCRC.

[222] Ibid.

A lack of funding and age were not the only reasons for leaving the Mental Health Research Institute. Rashevsky chose to move to Michigan based on promises made by Miller that Rashevsky claimed were never fulfilled.[223] Despite his frustration, Rashevsky found comfort in having his former student and close family friend, Anatol Rapoport, work at the same institution and live next door. However, when both Miller and Rapoport decided to leave Michigan in 1967, Rashevsky was now academically and socially isolated. He had no reason to stay at the University of Michigan.

Nicolas Rashevsky (*right*) and Anatol Rapoport in 1966 at the Mental Research Health Institute, Michigan. Courtesy of Mrs. Gwen Rapoport, used with permission

In search for a new position—a quest that despite extensive and valiant efforts would result in failure—Rashevsky wrote to individuals across various institutions around the United States. One was the physicist Herman R. Branson, who in the late 1960s became President of Central State University in Ohio. Branson was a well-known physicist with an interest in mathematical biology.[224] He was a longtime colleague of Rashevsky, having worked with him in the early 1950s as well as publishing in Rashevsky's Bulletin when associated with Howard University.

While Rashevsky began corresponding with Branson asking for his assistance to gain a position at Howard University, and a grant from the NIH to sustain his research, Rashevsky was also inquiring about the possibility of joining the Ohio

[223]Ibid.

[224]Branson played an important role in verifying the feasibility of Pauling and Corey's protein models in the late 1940s and early 1950s.

Central State University. Although Branson was keen on having Rashevsky at the Ohio Central State University, he had limited funds due to the small size of the University, and conditioned Rashevsky's joining upon receipt of an appropriate grant from the NIH. Branson lobbied for Rashevsky at Howard, at Central State University and with the powers that be at the NIH, including Director Fredrick Stone and chief of the Research and Development section Bernard Shachter. But while his efforts with the universities were fruitful, those with the NIH failed.

In December 1969 Rashevsky retired and spent the following 2 years at his summer home in Holland, Michigan. From there Rashevsky formed a non-profit organization called "*Mathematical Biology, Incorporated*", which was the precursor of "*The Society for Mathematical Biology*", with the purpose of "dissemination of information regarding Mathematical Biology". The funds for sustaining the organization came from the Bulletin of Mathematical Biology, which Rashevsky owned withdrawing it from the University of Chicago shortly after his resignation. His final scientific gathering was the international "*Symposium of Mathematical Biology*" held in Toledo, Ohio, sponsored by his new organization. The symposium was organized with the help of his former student, Dr. Anthony Bartholomay, who had become the Chairman of the first Department of Mathematical Medicine at Ohio State University.[225]

Rashevsky's health had been deteriorating ever since the drama at Chicago and the emotional turmoil of the late 1960s aggravated the condition even further. He suffered from coronary thrombosis and a series of heart attacks. On January 16, 1972, Professor Nicolas Rashevsky died at the age of 72.

[225] AF Bartholomay, G Karreman, and HD Landahl, "Obituary of Nicolas Rashevsky", *Bull. Math. Biophys* 34(1972).

Conclusions

Rashevsky's intellectual trajectory mimics that of a bouncing ball. It had its peaks and troughs, successes and failures, both in his quest towards scientific recognition and in his pursuit towards institutional recognition. Rashevsky—inspired by a vision, or what could be better characterized as a dream, of establishing *mathematical biology* similar in structure and aim to mathematical physics—never gave up, fighting indefatigably up to his very last day to transform his aspiration into reality. Never losing sight of the goal, he fought, manipulated and eventually risked his life's enterprise under the toughest of circumstances to establish a new discipline within biology. And yet, as Richard Lewontin once commented during my discussions with him: "Most modern-day biologists have never heard of Rashevsky. Why?"[1]

The answer to this question at least partially lies in the words of D'Arcy Thompson: "Dr. Rashevsky [had] a way of his own".[2] This study is the first intellectual biography of the consummate "outsider" in the world of biology—Nicolas Rashevsky. An examination of Rashevsky's intellectual profile and an account of his winding path illustrate his significance in the history of biology and his contribution to its "patchwork design".[3] Over the course of its development, biology as a science has come into being as a patchwork. It assumed its form and still does as "a consequence of myriad interactions between different traditions of knowledge, method, and philosophy with the larger quest for understanding of the natural world."[4] As this study shows, Rashevsky's mathematical biology undoubtedly forms part of biology's design.

[1]Correspondence with Richard Lewontin, February 18, 2011.

[2]D'Arcey. Thompson, "Review: Nicolas Rashevsky, Mathematical Biophysics. Physicomathematical Foundations of Biology, Dr. Rashevsky has a way of his own" *Nature* 142, 1938, 931–932.

[3]*Biology Outside the Box: Boundary Crossers and Innovation in the Life Sciences*. In Introduction, Oren Harman and Michael R. Dietrich, Eds.

[4]*Ibid*.

M.M. Shmailov, *Intellectual Pursuits of Nicolas Rashevsky*, Science Networks. Historical Studies 55, DOI 10.1007/978-3-319-39922-5

As such, this inquiry aimed at more than chronicling Rashevsky's scientific work. Lawrence Stark articulated it well in the invitation he posed to Rashevsky to attend the first Gordon Research Conference on Biomathematics in 1965 as its primary speaker: Rashevsky's biography is in fact the biography of the development of mathematical biology as a discipline in biology.[5]

As this study shows, the two aims are inevitably interwoven. The definition and conception of mathematical biology as a discipline within biology resulted largely from Rashevsky's identity as an "outsider" and his efforts to secure resources to institutionalize his enterprise and legitimize its work. Facing rejection from journals dominated by "insiders" and desiring a venue for publication, Rashevsky established the *Bulletin of Mathematical Biology* (BMB). Faced with the rejection of his methodology, perspectives, and general approach to studying the problems of life, he was unable to secure a comfortable position in the department of physiology and found himself out of place in the department of psychology. In the quest to institutionalize his enterprise, he fought for intellectual, academic, and financial independence which led to establishment of the *Section of Mathematical Biophysics*, a precursor to the more solid *Committee on Mathematical Biology* at the University of Chicago.

This study explores and illustrates how this interplay between personal, academic, institutional and broader political factors comes into play, affecting the intellectual genesis of a new discipline. The sociologist Andrew Abbott proposed that all existing professions practicing similar tasks can be conceived of in terms of a single system in which the professions compete over the definition of the problem at hand, finance and power.[6] Abbott suggests that within that system, the extent of jurisdiction of one profession depends on the extent of the jurisdictions of others that practice similar adjacent tasks. As such, the emergence of a profession can be studied not only by examining the characteristics of that profession, but also by observing the competition for resources with other professions[7] Envisioning Rashevsky and his students as the practitioners of a new intellectual enterprise as Abbot understands practitioners in an emerging profession, it is possible to explore the development of mathematical biology and assess how it was constructed.

As new professions emerge, they either erect or eliminate boundaries between themselves and others and at times do both at the same time in an effort to legitimize their endeavors.[8] In carving a niche for himself and his scientific agenda, Rashevsky set a goal of demarcating mathematical biology as a discipline separate

[5]Lawrence Stark to Rashevsky, September 22, 1964, Box 10, Folder "Gordon Research Conference", NRP-SCRC.

[6]Abbott, *The System of Professions: An Essay on the Division of Expert Labor*.

[7]In this connection it is interesting to note T. Gieryn, "Boundaries of Science." in S. Jasanoff, Gerald E. Markle, James C. Petersen, Trevor Pinch Eds., *Handbook of Science and Technology Studies* (1995): 393–443 pg. 409.

[8]Gieryn, "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists"; ———, "Boundaries of Science. S. Jasanoff, Gerald E. Markle, James C. Petersen, Trevor Pinch"; Fuller, "Disciplinary Boundaries and the

from biology and also separate from the sporadic attempts to mathematize biology. He did this consciously by attributing certain characteristics to his enterprise for example his "paper and pencil" approach to biology, "brain power" over experimentation, as well as his insistence on physical and academic isolation from the experimental biologists, so well exemplified by his refusal to allow his students to don white coats. The term "boundary-work" coined by the sociologist of science Thomas Gieryn has been used to designate that sort of demarcation.[9] As Gieryn has described it, boundary-work refers to the rhetorical "attribution of selected characteristics to the institution of science (i.e., to its practitioners, methods, stock of knowledge, values, and work organization) for purposes of constructing a social boundary that distinguishes some intellectual activities as 'non-science'".[10] Recent studies have illustrated the utility of the broad concept of "boundary–work" for describing contests within science, paying particular attention to the boundaries demarcating natural sciences from social sciences as well as the boundaries dividing competing social science disciplines.[11]The boundary-work takes place not only in the academic arena, but also in the institutional and public arenas. Thus it takes place before different audiences, working to secure each audience's support.[12]

Rashevsky was seeking to legitimize his new enterprise and institutionalize his new discipline. In so doing he needed the support, recognition, and *resources* of different *audiences*.[13] These resources—especially financial support and academic recognition—are crucial for the institutionalization of a new discipline.[14] Like all professionals, Rashevsky tried to obtain these resources by defining his work in ways acceptable to his audience. Thus, for example, recognizing the need to prove the applicability of mathematical biology to biology, he published *The Relation of Mathematical Biophysics to Experimental Biology* in 1938, the *Advances and Applications of Mathematical Biology* in 1940 and *Some Medical Aspects of Mathematical Biology* in 1964, thereby demonstrating to the audience in the academic arena why the new enterprise was necessary, legitimate, and significant.

Rhetoric of the Social Sciences"; Gaziano, "Ecological Metaphors as Scientific Boundary Work: Innovation and Authority in Interwar Sociology and Biology".

9Ibid; A. Abbott, "Things of Boundaries", *Social Research* 62, no. 4 (1995).

10Gieryn, "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists", pg. 782.

11Emanuel Gaziano, "Ecological Metaphors as Scientific Boundary Work: Innovation and Authority in Interwar Sociology and Biology", *American Journal of Sociology* 101, no. 4 (1996); Gieryn, *Cultural Boundaries of Science: Credibility on the Line*. S. Fuller, "Disciplinary Boundaries and the Rhetoric of the Social Sciences", *Poetics Today* 12, no. 2 (1991); S Fuller, "Talking Metaphysical Turkey About Epistemological Chicken, and the Poop on Pidgins", *The Disunity of Science: Boundaries, Contexts, and Power* (1996).

12Abbott, *The System of Professions: An Essay on the Division of Expert Labor*.

13Ibid.

14M.L. Small, "Department Conditions and the Emergence of New Disciplines: Two Cases in the Legitimation of African-American Studies", *Theory and Society* 28, no. 5 (1999)

Throughout his career and at times incidentally, Rashevsky engaged in boundary-work by concurrently erasing and erecting boundaries to differentiate himself and his enterprise from the existing entities competing for similar resources. By constantly defining the boundaries of mathematical biology and differentiating these from other attempts to introduce mathematical rationale to biology, such as those of Lotka, Thompson, Woodger, Haldane etc., Rashevsky fought not only for academic recognition of his research methodology but also paved the way towards institutional demarcation.

His battle to establish an institute for mathematical biology at the University of Chicago in the early 1940s, which resulted in the establishment of the Committee on Mathematical Biology, reflects his success in this arena. While the eventual demise of the Committee in the late 1960s can be perceived as a failure, the recognition of the new discipline by governmental agencies in the late 1950s and 1960s and the founding of the Society of Mathematical Biology, which remains active to this very day, illustrates his success in the arena beyond the academic setting of just one campus, namely, the University of Chicago.

Still, his aim was not to develop a profession. Rather, it was to establish a new intellectual identity for which institutionalization of the intellectual enterprise proved to be crucial. With a discipline to be created, the provision of tools, methodologies and intellectual orientations lay uppermost in Rashevsky's mind and forefront in his actions. However, he soon became aware of the need to develop not only the intellectual identity but the professional identity as well. Without these, his field of learning could never be secure, let alone accepted. Creating the necessary intellectual and organizational infrastructure for a discipline was a task that demanded a lifetime of faith and devotion. Yet this was a small price to pay for an expansive dream of systematic mathematical biology analogous to mathematical physics and the quest for general mathematical principles, fundamental laws that would apply to the entire realm of biology. Ironically, it was this yearning and the assertion of the theorists' independence that made Rashevsky's contributions to his own discipline incomplete.

By demarcating mathematical biology from biology, his mathematical biology became completely external to the general practice of biology. Rashevsky entered the academic arena, the domain of biology, as an "outsider". Equipped primarily with the techniques, methodologies and intellectual orientations prevalent in theoretical physics, he charged into biology head-on. Emphasizing the importance of acceptance by peers, Michele Lamont states that: "the legitimation of a theory depends on both the producer's definition of his work as important and the institutionalization of its importance by peers and the general intellectual public."[15] Rashevsky's work obtained this standing by engaging the following strategies.

From the outset, he defined his work and published the results of his research in journals such as *Zeitschrift fur Physic, Physics, Growth, Protoplasma,*

[15]M. Lamont, "How to Become a Dominant French Philosopher: The Case of Jacques Derrida", *American Journal of Sociology* (1987).

Psychometrica, and Journal of General Physiology. Thus he was reaching his peers and the "insiders". Yet, his work was often accused, even by the editors of these journals, as being so simplified for mathematical feasibility that it was biologically no longer interesting or conversely too "biological" which was mathematically uninteresting and at times intractable. Even when he published his ground breaking results of "two-factor theory" of nervous excitation and inhibition in *Protoplasma*, with the physiological community as his primary audience, his work was ignored and instead, a theory similar to his published three years later by A.V. Hill in *Proceedings of the Royal Society of London*, was presented in the "Recent Advances in Physiology" as Hill's theory. Rashevsky soon realized that his growing body of work was falling between the cracks as there was no suitable venue that could disseminate the research results. He was forced to move "from one journal to another as difficulties . . .developed."[16] To solve the problem, he needed financial support and backing from a public arena. To the rescue came Warren Weaver from the Rockefeller Foundation. It was Weaver's support, coupled with Rashevsky's determination to pursue his line of research that led to the birth of *BMB*. While the *BMB* provided a platform for publishing his research and allowed him to communicate it to those sharing a common interest in the growth of mathematical biology, it also clearly isolated Rashevsky and his research from the "insiders".

To propagate his views to audiences in the academic arena, Rashevsky organized various scientific meetings and international conferences where fellow scientists interested in his line of work could participate and be exposed to his line of thinking. He also participated in numerous conferences and scientific meetings on the border between mathematics and biology, such as the Cold Spring Harbor Symposia and the Gordon Research Conference. He was invited by universities and institutes in the US, Europe, and Russia as a guest lecturer, preaching like a missionary to anyone who would lend an ear.

In this task he also elicited participants from local departmental and university administrators, from whom he gained institutional support and financial resources. He engaged the wider academic arena by publishing in widely read journals such as *Nature*, *Science* and *Philosophy of Science*, articles that did not present any new results yet discussed the purpose of his endeavor and its importance. He published books summarizing his own work and that of his students, he organized seminars inviting leading scientists who shared the common interest in the growth of mathematical biology and would sympathetically recognize its importance. By following these strategies he gained academic recognition and intellectual legitimacy. Finally, he engaged a wider public arena from which he gained capital as well as political support-his relationships with Warren Weaver at the Rockefeller Foundation and Alan T. Waterman at the National Science Foundation are examples. Rashevsky was able to find a specific audience in each arena and persuade those individuals of the legitimacy of mathematical biology, in order to secure the necessary resources they could provide.

[16]Weaver Interviews, July 3, 1938, RG 1.1, Series 216D, Box 11, Folder 148, RAC.

This study also illustrates the role of academic politics when it comes to institutionalizing a new discipline. Rashevsky entered the Division of Biological Sciences at the University of Chicago when it was undergoing reorganization—and was then forced to resign under similar conditions. Although Rashevsky's employment and promotion path at the University of Chicago progressed from fellow (1934–1935) to Professor (1947–1964), he encountered several setbacks along the way that threatened his enterprise. Social and political factors coupled with institutional ones played a major role. But above all, it was the financial support that allowed him to prevail and continue his struggle towards establishing the new discipline. Initially via the patronage of the Rockefeller Foundation and later through fundraising in the public arena and grants from governmental agencies, Rashevsky was able to garner enough support for the university to keep him in its ranks and allow him to sustain his school. He was thus able to train graduate students and postdoctoral fellows from around the world, who applied to study with him even though the path towards a degree in mathematical biology was demanding.

Yet Rashevsky's success is ambiguous at best. Back in 1939, one commentator stated that "mathematical biology will never develop unless somebody starts the process. . ..fortunately it [was] started with the work of. . ..Rashevsky".[17] Despite his role as a pathfinder, Rashevsky's influence on the discipline he labored so faithfully to create has been obscured by his assertion of the theorist's independence as well as the independence of his discipline from the 'insiders'. Mathematical biology is now a firmly institutionalized field of learning in the United States and elsewhere. At first glance, it bears little trace of Rashevsky's influence, but when examined closely, mathematical biologists today use Rashevsky methodology of abstraction, approximation and isolation to study various biological phenomena. Rashevsky created and assembled the necessary building materials, and he was the first deliberate architect of mathematical biology as an independent and organized discipline. This study of the ways in which he succeeded and those in which he failed illuminates the subtle process of discipline-building and the complex career of a remarkable man.

[17]Pearl, "Review: Nicolas Rashevsky, Mathematical Biophysics. Physicomathematical Foundations of Biology."

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