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 a lone wolf than the applied variety. His work cannot be directed, 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

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[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. <u>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</u>. 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.

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[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,

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[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

[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.”

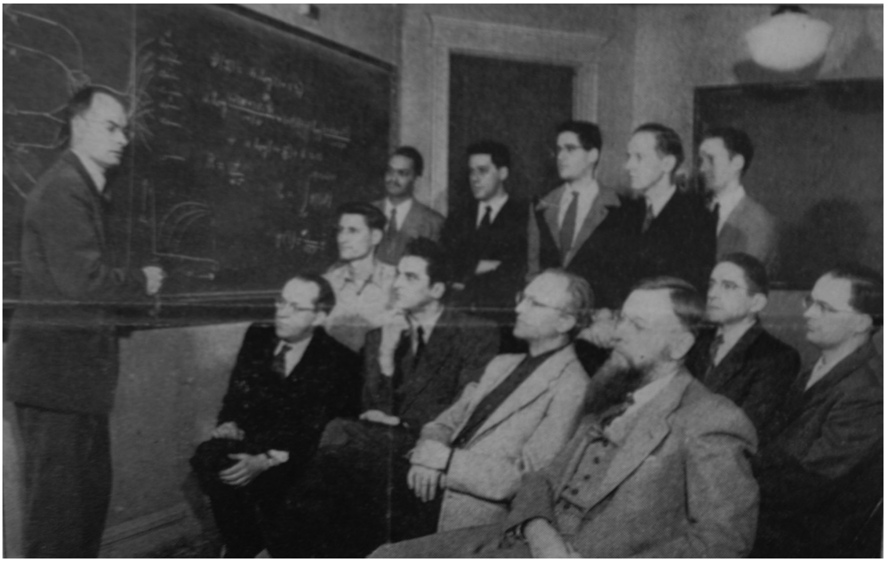
[82]A. Rapoport, *Certainties and Doubts: A Philosophy of Life* (Black Rose Books Ltd, 2000), pg. 89.

[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]

[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.

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