Who's Afraid of Max Delbrück?

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Labels say nothing about nature, but they do say something about the people studying it. In practice, whether one calls oneself a physicist, biologist, or something else is thought to have real sociological consequences—for getting academic jobs, getting funded, and getting published, among other things. These questions of academic identity can become particularly conspicuous for those researchers that work on the boundary of multiple disciplines, who may for professional reasons feel the need to assert that they are a member of one tribe over another.

As Martin notes in an account of the recent rise of condensed-matter physics, its success "is a story of categories and why they matter." Identifying condensed-matter physics as a distinct subdiscipline "made a statement about the type of activity physics was supposed to be", and "drew a line between who was a physicist and who wasn't". Even the rebranding of "solid-state physics" to "condensed-matter physics" in the 1960s and 1970s was an effort to emphasize the intellectual value and conceptual unity of the field as much as it was an attempt to accurately describe it.[1] Nowadays, with some flavor of condensed-matter physics being the dominant activity of most working physicists, there is no longer any real dispute regarding whether it counts as legitimate physics.

The story may be repeating again on the interface of physics and biology. In 2020, the National Academies explicitly included "Biological Physics/The Physics of Living Systems" for the first time in its decadal survey of US research,[2] a sign that it is increasingly thought of as truly a part of physics, rather than just physics applied to an outside subject. With the confluence of recent breakthroughs in experimental techniques (e.g. CRISPR), the high-throughput collection of multi-omic data, and the developing sophistication of algorithms for 'big data'-style analyses, the field is poised for explosive growth in the twenty-first century.

But there remains significant confusion over the nature of the field, which is in part reflected by the diversity of names in use. Here are a few:

> biological physics, physics of living systems, biophysics, physical biology,
> living state physics, living matter physics

The oldest of these terms is "biophysics", which was proposed by Karl Pearson in his 1892 book *The Grammar of Science*. On the need to understand "the genesis of the living from the lifeless", he writes:[3]

"A branch of science is therefore needed dealing with the application of the laws of inorganic phenomena, or Physics, to the development of organic forms. This branch of science which endeavors to show that the facts of *Biology* . . . constitute particular cases of general physical laws has been termed *Ætiology.* It would perhaps be better to call it *Bio-physics.* This science does not appear to have advanced very far at present, but it not improbably has an important future."

By 1920, the challenge of creating such a field seems to have been met, with Alexander Forbes outlining his view of biophysics in a *Science* article and reporting the development of a biophysics course at Harvard.[4] The next term to appear may be "physical biology", which seems to have been introduced by Walter Porstmann in a 1915 article:[5]

"It will be the function of this new branch of science to investigate biological phenomena as regards their physical aspects, just as Physical Chemistry has treated the physical aspects of chemical phenomena. Because this field has not yet been systematically explored . . . the individual data of *Physical Biology* appear, as yet, as more or less disconnected facts . . . As results gathered in this disconnected fashion accumulate, the need of their unification into a harmonious whole, into a distinct discipline of science, becomes more and more acutely felt."

In 1925, the first textbook on physical biology—Alfred Lotka's (of Lotka-Volterra fame) *Elements of Physical Biology*—appeared. In it, he more than once distinguishes the project of physical biology from that of biophysics, citing Forbes' article and describing biophysics as studying "the physics of individual life processes" (e.g. nerve conduction), and physical biology as the application of physical principles to biology in a broader sense (and seemed to have in mind modeling evolutionary dynamics as an example). "Physical biology would, in this terminology, include biophysics as a subordinate province", noted Lotka.[6]

Being particular about labels was by no means exclusive to Lotka. Decades later, after Nicolas Rashevsky spent a lifetime advancing what he called a "mathematical biophysics" analogous to the more familiar mathematical physics, Rashevsky was invited to give a prestigious lecture at the first Gordon Research Conference on "biomathematics", which was to be held in 1965. Recoiling at the term, Rashevsky wrote in a letter to the conference organizer that it was an "etymological monstrosity" that gave the wrong sense of what the field was about.[7]

In the late 1960s and early 1970s, many physicists studying biological problems faced "skepticism when presenting . . . at the Biophysical Society Meetings"; these mounting frustrations led directly to the 1973 establishment of the American Physical Society's Division of Biological Physics.[8] One speculates that "biological physics" may have been chosen to distinguish the organization from the culture of the Biophysical Society and "biophysics", which (then and now) has different connotations.

All of this is just to say that labels seem to have mattered historically to many of the workers on the interface of physics and biology. Even now, the connotations of terms like "biological physics" and "biophysics" differ for a variety of historical and sociological reasons. The recently popularity of the "physics of living systems" label within the physics community may be analogous to the "condensed-matter" rebranding of "solid-state physics", its use intending to situate the discipline as an activity within physics rather than outside or adjacent to it.

For the remainder of this essay, let us put aside the business of what this field might be called. Regardless of its name, to what ethos should it aspire? In search of a spirit of biological physics, it may be appropriate to reflect on the diverging experiences of two physicists that turned to biology: Max Delbrück and Nicolas Rashevsky.

The case of Max Delbrück

Max Delbrück was born in Berlin in 1906, and matured amidst the scarcity and social upheaval associated with the first World War. Although he began his university studies interested in astronomy, exciting developments in quantum mechanics and the lamentable state of German astronomy led him to pursue theoretical physics instead.[9]

Delbrück completed his PhD thesis on a quantum mechanical treatment of the lithium molecule in 1930, and continued to work on topics related to quantum mechanics until around 1937. He was accepted by the 'insiders' of physics, and wrote papers with luminaries like Lise Meitner and George Gamow. The extent to which he was intimately familiar with the movers and shakers of the day may be illustrated by the time he jokingly served as a butler at a costume party thrown by Schrödinger.[10]

But Delbrück's primary research interests were not to remain in physics. Captivated by a lecture given by Bohr in 1932 entitled "Light and Life", Delbrück took very seriously Bohr's suggestion that there may be some manifestation of his complementarity principle in biology. In particular, an organism can be viewed "either as a living organism or as a jumble of molecules", just as an electron can be viewed either as a particle or a wave; perhaps there is some "mutually exclusive feature" of each description analogous to position and momentum in quantum mechanics.[9]

While working as a theoretical physicist for Lise Meitner, Delbrück moonlighted as a biologist, and in 1935 published his first biology-related paper. In the 1940s, following a Rockefeller Foundation-supported mutation research fellowship at Caltech, and with some continued support from the foundation, Delbrück's career in biology blossomed.[10] Over the course of the decade, he published a landmark paper with Salvador Luria establishing that Darwinian selection applies to bacteria; helped build the Phage Group, an intellectual school and forebear of much of modern molecular biology; and began the legendary phage course at Cold Spring Harbor Laboratory, intending to "spread the new gospel"[11] of bacteriophage work. For his community building and contributions to genetics, he received a third of the 1969 Nobel Prize in Physiology or Medicine.

During the war, he was an instructor of physics at Vanderbilt University, but had a rudimentary research lab in the biology department. While the arrangement was strange for the time, Delbrück's sterling physics credentials and the quality of his research output yielded no complaints. In 1947, he left Vanderbilt to become a Professor of Biology at Caltech, where he remained for the rest of his career.[10]

The case of Nicolas Rashevsky

Nicolas Rashevsky was born in 1899 in Chernigov, a small village in Ukraine. He was well-educated—in literature and languages, for example—and learned voraciously. Already by the age of 19, he had a PhD in theoretical physics from the University of Kiev. Afterwards, Rashevsky was rapidly uprooted by the Russian Revolution, and by 1925 had moved to the United States to work as a theoretical physicist at Westinghouse Electric Company.

Rashevsky did most of his work in theoretical physics in the 1920s, publishing on a variety of fields: quantum mechanics, relativity, photomagnetism, thermionic emission, and colloidal dynamics, to name a few. But a chance encounter at a "social occasion" with a biologist, who described the reason for cell division as a mystery, piqued his interest in biological problems. Soon enough, Rashevsky—reflecting on the absence of a biology analogue to the then-well-established field of mathematical physics—in 1926 began dreaming of a similar 'mathematization' of biology.[7,12]

In the 1930s, Rashevsky concentrated his efforts fully on this project, and with Rockefeller Foundation support found a position at the University of Chicago. He published on a variety of problems, including nerve conduction, cell growth, and development. His first book-length examination of "mathematical biophysics" as he envisioned it, *Mathematical Biophysics: Physicomathematical Foundations of Biology*, was released in 1938.[13]

At the same time, Rashevsky ran into mounting obstacles. Facing a chilly reception in mainstream biology journals, in 1939 he started the Bulletin of Mathematical Biophysics, which housed the majority of his articles for the remainder of his career. Over the years, he fought to maintain funding for the intellectual school he had founded at the University of Chicago (the Committee on Mathematical Biology), for mathematical biology to receive official recognition from the National Institutes of Health, and to maintain the independence of his group from meddlesome university administrators. In many of these battles, he was thwarted, or only partially successful. By the time of his death, he left a mixed scientific legacy.[7]

In some ways, the lives of Delbrück and Rashevsky were strikingly parallel. Both grew up against a backdrop of war, both were trained as theoretical physicists and switched to thinking primarily about biological problems in the 1930s, both received significant financial support from the Rockefeller Foundation, and both were community builders and prolific researchers. But Delbrück met mainstream acceptance, while Rashevsky encountered obstacle after obstacle. Why?

Perhaps Delbrück was luckier, or had better connections. But one can readily identify two marked differences in scientific philosophy that may have impacted their trajectories. Firstly, Delbrück thought of himself more as a natural philosopher than a physicist or biologist,[14] whereas Rashevsky adhered strictly to the ethos of theoretical physics. Rashevsky was interested in the 'mathematization' of biology not just for its practical utility, but for its own sake.[15] Crudely, one might say that Delbrück pursued questions, while Rashevsky pursued an identity. Secondly, Delbrück was more willing to meet biologists halfway by doing experiments and assiduously checking his hypotheses against the experimental results of others.

There is something to be said for Rashevsky's iron will, and his sturdy commitment to his scientific vision in spite of all opposition. He was a trailblazer in the establishment of mathematical biology, and his efforts directly led to the founding of the Society for Mathematical Biology.[7] But his dogmatic approach may have been alienating to many biologists.

A play by American playwright Edward Albee famously had the provocative title *Who's Afraid of Virginia Woolf?*. Virginia Woolf was an English novelist whose plays often featured themes of people in British high society living inauthentic lives under many masks. The title of the play, in one interpretation, means: 'Who's afraid to live without illusions?'[16]

Max Delbrück never seemed to worry about whether he was called a physicist or a biologist. In fact, he seemed to delight in using one label or the other as it suited him. He saw himself more akin to a natural philosopher, someone who pursues the ultimate truths about the natural world regardless of what tools or paradigms their answers require. As we continue to pursue biological physics, it may be prudent to emulate that style, and courageously pursue interesting questions whether or not they are properly considered physics at the time—to act as if we live in a world without disciplinary boundaries. Who's afraid of Max Delbrück?

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