The Influence of Niels Bohr on Max Delbrück

Revisiting the Hopes Inspired by "Light and Life"

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ABSTRACT

The impact of Niels Bohr's 1932 "Light and Life" lecture on Max Delbrück's lifelong search for a form of "complementarity" in biology is well documented and much discussed, but the precise nature of that influence remains subject to misunderstanding. The standard reading, which sees Delbrück's transition from physics into biology as inspired by the hope that investigation of biological phenomena might lead to a breakthrough discovery of new laws of physics, is colored much more by Erwin Schrödinger's *What Is Life?* (1944) than is often acknowledged. Bohr's view was that teleological and mechanistic descriptions are mutually exclusive yet jointly necessary for an exhaustive understanding of life. Although Delbrück's approach was empirical and less self-consciously philosophical, he shared Bohr's hope that scientific investigation would vindicate the view that at least some aspects of life are not reducible to physico-chemical terms.

ON 15 AUGUST 1932 Niels Bohr delivered a lecture entitled "Light and Life" to the International Congress on Light Therapy in Copenhagen.[1] Bohr was at this time already a distinguished contributor to atomic physics, having been awarded the Nobel Prize in Physics in 1922 in recognition of his work on atomic structure. The address provided an occasion to reflect on the philosophical significance of developments in quantum theory

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[1] Niels Bohr, "Light and Life," *Nature,* 25 Mar. 1933, *133:*421–423 (Pt. 1), 1 Apr. 1933, *133:*457–459 (Pt. 2) (subsequent citations will be to this version of his essay unless otherwise indicated); the *Nature* translation is reprinted in *The Philosophical Writings of Niels Bohr,* Vol. 2: *Essays, 1932–1957, on Atomic Physics and Human Knowledge* (Woodbridge: Ox Bow, 1987), pp. 4–12. For the German version see Bohr, "Licht und Leben," *Naturwissenschaften,* 1933, *21:*245–250. The published English and German versions were translated with some "formal alterations" from the Danish version published in the report of the congress.

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for the life sciences. Bohr had formally introduced the concept of complementarity in a 1927 lecture at Como to deal with very specific problems arising in quantum mechanics, as a way of defining the conditions under which particular phenomena appear, but he remained interested in whether similar conceptual relations could also yield insights in other scientific domains.[2] In the "Light and Life" address Bohr suggested that a notion of complementarity might be needed to understand biological phenomena.

In attendance was Max Delbrück, a twenty-six-year-old postdoctoral student whom Bohr had mentored the previous summer.[3] Delbrück had gone to Copenhagen on a postdoctoral Rockefeller fellowship in February 1931 and remained there through the summer. During this initial stay, he developed close relationships with Bohr and George Gamow. Up to the time of his move to the United States in 1937, Delbrück returned to the Institute for Theoretical Physics on Blegdamsvej (now the Niels Bohr Institute), Bohr's base in Copenhagen, at least occasionally. By the summer of 1932 he had accepted a position at the Kaiser Wilhelm Institute for Chemistry in Berlin. He arrived for the conference by train on the morning of Bohr's opening "Light and Life" address. Delbrück would later describe the lecture as an event that changed the course of his life. The hope of finding a form of "complementarity" in the biological domain became something of a controlling thought that, for the remainder of Delbrück's life, served both to motivate his interest in biology and to shape the research projects he chose to pursue. Speaking retrospectively in "A Physicist Looks at Biology" (1949), Delbrück acknowledged: "Bohr's suggestion of a complementary situation in biology, analogous to that in physics, has been the *prime motive* for the interest in biology of at least one physicist and may possibly play a similar role for other physicists who come into the field of biology."[4] Repeatedly throughout his career Delbrück expressed an expectation that a rigorous and detailed examination of biological phenomena would confront us with a paradoxical situation similar in kind to that which confronted the physicists of the day in quantum physics.

[2] Two descriptions are *complementary* if and only if they require mutually exclusive experimental arrangements but are jointly necessary for our understanding of the phenomena. See Don Howard, "Who Invented the Copenhagen Interpretation? A Study in Mythology," in *PSA 2002,* Pt. 2: *Symposium Papers: Proceedings of the 2002 Biennial Meeting of the Philosophy of Science Association, Milwaukee, Wisconsin, November 7–9, 2002,* special issue of *Philosophy of Science,* Dec. 2004, *71:*669–682; Dugald Murdoch, *Niels Bohr's Philosophy of Physics* (Cambridge: Cambridge Univ. Press, 1987); and Mara Beller, "The Birth of Bohr's Complementarity: The Context and the Dialogues," *Studies in History and Philosophy of Science,* 1992, *23:*147–180.

[3] Carolyn Harding, interview with Max Delbrück, 14 July–11 Sept. 1978, Oral History Project, California Institute of Technology Archives, Pasadena, California (available online at http://oralhistories.library.caltech.edu/16/ or at http://resolver.caltech.edu/CaltechOH:OH_Delbruck_M). See also Ernst Peter Fisher and Carol Lipson, *Thinking about Science: Max Delbrück and the Origins of Molecular Biology* (New York: Norton, 1988) (hereafter cited as **Fisher and Lipson, *Thinking about Science***).

[4] Max Delbrück, "Light and Life III," *Carlsberg Research Communications,* 1976, *41*(6):299–309, on p. 299 (this lecture was given at the Centennial of the Carlsberg Laboratory, Copenhagen, 27 Sept. 1976); and Delbrück, "A Physicist Looks at Biology," *Transactions of the Connecticut Academy of Arts and Sciences,* 1949, *38:*173–190, rpt. in *Phage and the Origins of Molecular Biology,* ed. John Cairns, Gunther S. Stent, and James Watson (New York: Cold Spring Harbor Biological Laboratory of Quantitative Biology, 1966), pp. 9–22, on p. 22 (italics added) (subsequent citations will be to the reprinted version; it will be shown as **Delbrück, "Physicist Looks at Biology"**). Similarly, in 1963, pointing to a passage from "Light and Life," Delbrück said, "This is the passage which [Bohr] . . . worried about ever after very much, and the reason that I am standing here today, is that I am perhaps the only one of his associates of those days who took this passage so seriously that it determined his career, changing over into biology to find out whether indeed there was anything to this point of view": Delbrück, "Biophysics," in *Commemoration of the Fiftieth Anniversary of Niels Bohr's First Papers on Atomic Constitution, Held in Copenhagen on 8–15 July 1963* (Copenhagen: Institute for Theoretical Physics, Univ. Copenhagen, 1963), pp. 41–67, on p. 43. This talk was delivered at a session entitled "Cosmos and Life" held on 12 July 1963; the published version includes the discussion that followed.

But what was it, specifically, that Delbrück was hoping to find? Why was he so fervently devoted to the belief that such paradoxes would help us to solve the "riddle of life"? The traditional reading of the influence of Bohr on Delbrück, which has been standard in the historical literature for the last four decades, is mistaken. The standard reading, which sees Delbrück's transition from physics into biology as inspired by the hope that investigation of biological phenomena might lead to a breakthrough discovery of new laws of physics, is colored much more by Erwin Schrödinger's gloss on these issues in *What Is Life?* (1944) than is often acknowledged. Following Nils Roll-Hansen's location of Bohr within the Kantian teleo-mechanical tradition, I contend that Bohr, in keeping with the claims of Kantian philosophy, expected that purposive and functional aspects of biological phenomena could not be made intelligible on a strictly mechanistic approach.[5] Drawing an analogy with the complementarity relation that obtains in atomic physics, Bohr applied the language of complementarity to this dual-aspect approach to understanding organisms. Bohr envisioned an analysis employing teleological notions as basic concepts, irreducible to the language of physics and chemistry. He predicted that mutually exclusive teleological and mechanistic approaches would be jointly necessary for an exhaustive understanding of life. Although Delbrück took a more applied empirical and less self-consciously philosophical approach, he shared Bohr's hope that a detailed scientific investigation would vindicate the view that at least some aspects of life are not reducible to the terms of either classical mechanistic models or quantum physics. Delbrück did not, as James Watson, Gunther Stent, and others suggest, pursue a physical and chemical analysis of biological phenomena in anticipation that that investigation would lead to advances in physics such that functional features could be understood in entirely mechanistic terms. Rather, a central motivation for Delbrück's research was the hope of encountering essential limitations in the reductionist project that would require supplementation by a perspective employing purposive concepts.

My argument for this revisionist reading unfolds in four stages. First, I seek to establish that what I call the "other laws of physics" interpretation has been the standard reading of the influence of Bohr on Delbrück. Second, I argue that such a reading is particularly problematic when we contextualize Bohr's biological views within the Kantian tradition. Third, I present an alternative reading according to which Delbrück's turn toward the life sciences was motivated not primarily by a hope that it would lead to the discovery of "other laws of physics" but, rather, by the dream of providing an empirical justification for Bohr's conceptual framework of biological complementarity. Finally, I explain the reason why the "other laws of physics" interpretation has long persisted in the understanding of scientists and historians by showing it to have a plausible source in the influential writings of Erwin Schrödinger.

[5] I shall focus primarily on the traditional interpretation as it appears in the following, which are representative of the relevant literature: Gunther S. Stent, "Introduction: Waiting for the Paradox," in *Phage and the Origins of Molecular Biology,* ed. Cairns *et al.,* pp. 3–8; Stent, "Light and Life: Niels Bohr's Legacy to Contemporary Biology," *Genome,* 1989, *31:*11–15; Stent, "Looking for Other Laws of Physics," *Journal of Contemporary History,* 1998, *33:*371–397; Lily E. Kay, "Conceptual Models and Analytical Tools: The Biology of the Physicist Max Delbrück," *Social Research,* 1984, pp. 641–673, rpt. in *Journal of the History of Biology,* 1985, *18:*207–246 (subsequent citations will be to this source); Kay, "The Secret of Life: Niels Bohr's Influence on the Biology Program of Max Delbrück," *Revista di Storia della Scienze,* 1985, *2:*487–510; Nils Roll-Hansen, "The Application of Complementarity to Biology: From Niels Bohr to Max Delbrück," *Historical Studies in the Physical and Biological Sciences,* 2000, *30:*417–442 (hereafter cited as **Roll-Hansen, "Application of Complementarity to Biology"**); and Fisher and Lipson, *Thinking about Science.*

I. THE TRADITIONAL READING OF BOHR'S INFLUENCE ON DELBRÜCK

Delbrück's first biological paper appeared in 1935, although it was not widely read until Schrödinger drew attention to it in *What Is Life?* Coauthored with the Russian *Drosophila* geneticist Nikolai Timoféeff-Ressovsky and the radiation experimentalist Karl Zimmer, it is an example of the kind of fruitful collaboration with experts from various specialties that marked Delbrück's career as a whole. The article, which came to be called the "Three-Man Paper," used radiation-induced mutations to speculate that genes were very small, relatively stable, physical structures, molecules whose configurations could be rearranged when targeted by X-rays.[6] A second Rockefeller fellowship brought Delbrück to the California Institute of Technology to work under T. H. Morgan in 1937. But rather than pursuing *Drosophila* genetics, Delbrück soon began to study bacteriophage with Emory Ellis. Between 1940 and 1947 he continued his research on bacteriophage replication while serving as an instructor in the physics department at Vanderbilt University. By the early 1940s he had begun collaborative work on phage with Salvador Luria and Alfred Hershey—research for which the three men shared the Nobel Prize in Physiology or Medicine in 1969. Their summer phage group at the Cold Spring Harbor Laboratory on Long Island attracted a number of scientists from diverse backgrounds (including Leo Szilard, James Watson, Renato Dulbecco, Seymour Benzer, Frank Stahl, and Gunther Stent) and helped to standardize experiments in the growing field of molecular genetics. In 1947 Delbrück returned to Caltech as a professor of biology, a post he retained until his retirement in 1977.

The fact that "Light and Life" played an integral part in Delbrück's decision to switch from theoretical physics to biology is well documented and much discussed. No one disputes that Delbrück was deeply intrigued by Bohr's suggestion that some sort of complementarity might have application to biological phenomena and that this played an integral part in his move into biology. My first claim is that there is a particular interpretation of the influence of Bohr on Delbrück, which has arguably become something of a standard reading, that sees Delbrück's transition from physics into biology as motivated by the hope that the investigation of biological phenomena might lead to a breakthrough discovery of new laws of physics. I will designate this the "other laws of physics" interpretation. The entry on Delbrück in the *Larousse Dictionary of Scientists* nicely illustrates the traditional interpretation: "Working with Niels Bohr at Copenhagen in 1932, he was influenced by Bohr's ideas about life and biology, and *came to believe that the study of biology might lead to new laws of physics.* These ideas are expressed in *What is Life,* written by the émigré physicist Schrödinger in Dublin in 1945 [*sic*], and widely read by physicists."[7]

The molecular biophysicist Gunther Stent, in particular, claimed that the central motive for Delbrück's transition into biology was the hope that a rigorous investigation of bio-

[6] Nikolai W. Timoféeff-Ressovsky, Karl G. Zimmer, and Max Delbrück, "Über die Natur der Genmutation und der Genstruktur," *Nachrichten von der Gessellschaft der Wissenschaften zu Göttingen: Mathematisch-Physische Klasse,* 1935, *6*(13):190–245. The English title of this paper is "The Nature of Genetic Mutations and the Structure of the Gene"; I have consulted the recent complete translation of this paper by Brandon Fogel and Phillip Sloan (personal communication), and subsequent references will be to their translation.

[7] Hazel Muir *et al.,* "Max Delbrück," in *Larousse Dictionary of Scientists* (Edinburgh: Larousse, 1994), p. 139 (emphasis added). Schrödinger's lectures were delivered at the Dublin Institute for Advanced Studies at Trinity College in February 1943 and first published as *What Is Life?* in 1944. On the role of "Light and Life" in Delbrück's decision to move into biology see Fisher and Lipson, *Thinking about Science;* Kay, "Conceptual Models and Analytical Tools" (cit. n. 5); Roll-Hansen, "Application of Complementarity to Biology"; Stent, "Introduction: Waiting for the Paradox" (cit. n. 5); and Stent, "Looking for Other Laws of Physics" (cit. n. 5).

logical phenomena might lead to the discovery of new laws of physics. In an essay entitled "Looking for Other Laws of Physics," Stent claimed that Bohr's "Light and Life" lecture "was the fountainhead of the beguiling idea that the study of living processes is likely to turn up 'other laws of physics.'" The idea that Delbrück was motivated by a quest to uncover new laws of physics is a central theme in that essay. Stent repeatedly asserts that Delbrück also held that a biological example of complementarity could lead to the discovery of "other laws" of physics. Recalling a 1949 summer session given by Delbrück in Pasadena, Stent writes, "Max gave a tutorial for the Phage Group summer conclave on 'complementarity,' a theme that, as we all knew, had once played a pivotal role in his expectation of running into other laws of physics."[8]

I highlight Stent's work because he is a key figure among the first generation of practicing scientists to tell the story of the rise of the phage side of molecular biology. But, as we shall see in Sections III and IV, Erwin Schrödinger and James Watson also understood the central issue concerning complementarity to be a hopeful expectation that careful biological research would uncover new laws of physics. Similarly, reflecting on various motives for entering the field of molecular biology in a 1971 interview with Horace Freeland Judson, Francis Crick said, "[Max Delbrück] went into it because he hoped that by looking at biological things you would find *new* laws of physics and chemistry."[9]

The "other laws of physics" interpretation pervades the work of even the most sensitive of those who have followed in the footsteps of these first historians of molecular biology. Lily Kay, for example, quite explicitly sees Delbrück's search for paradox as connected with a hope of uncovering new laws:

> Delbrück saw himself as a natural philosopher in pursuit of new physical laws, liberated from the traditional divisions that had prejudiced man's study of nature and seeking knowledge in its most integrated form. . . . Delbrück based his intellectual strategy for discovering new laws of nature on a search for a paradox analogous to the uncertainty principle of quantum mechanics, which set defined limits on our ability to measure details of atomic structure.

In another recent and deeply perceptive discussion of these issues, Nils Roll-Hansen also accepts the "other laws of physics" interpretation: "Delbrück also was a *reductionist* in the sense that he sought new physical concepts and principles to explain characteristic biological phenomena in reproduction, development, and sense perception. He *did not think of biology as an autonomous science* on par with physics but *saw it as a source of phenomena that demanded new fundamental advances in physics.*" Notice that Roll-Hansen sees Bohr and Delbrück as sharing expectations like in kind to that of Schrödinger: "Like Bohr and Delbrück, Schrödinger held that new physical 'laws' were needed to explain living systems." Comments by Ernst Peter Fisher and Carol Lipson reflect a similar view of Delbrück. Speaking of research conducted on the stability of genes, they note: "No paradox emerged from this study; no new epistemological position was required to understand genes; and no new laws of physics were uncovered."[10]

[8] Stent, "Looking for Other Laws of Physics," pp. 375, 387. See also Stent, "Introduction: Waiting for the Paradox."

[9] Horace Freeland Judson, *The Eighth Day of Creation: The Makers of the Revolution in Biology* (New York: Simon & Schuster, 1979), p. 613.

[10] Kay, "Conceptual Models and Analytical Tools" (cit. n. 5), pp. 209–210; Roll-Hansen, "Application of Complementarity to Biology," pp. 418 (emphasis added), 425; and Fisher and Lipson, *Thinking about Science,* p. 90.

There is, then, clear evidence of the central and important place that the "other laws of physics" interpretation of the influence of Bohr on Delbrück has played in the secondary literature on the history of molecular biology. In Sections II and III I shall argue that a careful reading of Bohr and Delbrück reveals the inadequacies of this way of characterizing either of their expectations.

II. BOHR'S FRAMEWORK FOR RELATING PHYSICS AND BIOLOGY: COMPLEMENTARITY OF MECHANISTIC AND TELEOLOGICAL DESCRIPTIONS

There is a body of first-rate historical literature locating Bohr's position in biology in the context of late nineteenth-century debates between vitalists and mechanists.[11] One recent line of interpretation that I find promising situates Bohr's views within a broadly Kantian tradition. A case for at least a secondhand debt to Kant's Third Critique has been made by Jan Faye and Nils Roll-Hansen.[12]

In the *Critique of Judgment* (1790) Kant offers an analysis of the relationship between teleology and mechanism as a framework for approaching the life sciences. He argues that mechanical causes are not sufficient for understanding the phenomena of life—even a blade of grass. Accounting for our experience of organisms as purposive requires the employment of teleological judgments as acts of the reflective judgment that makes them only "regulative" rather than explanatory principles. Purposive concepts supplement mechanical descriptions in a manner that Kant takes to be methodologically useful and, indeed, indispensable for a systematic and complete account of life.[13]

There are a number of reasons why it is plausible to contextualize Bohr within this broadly Kantian teleo-mechanical tradition. The case for at least an indirect Kantian influence on Bohr's way of conceiving the relationship between teleology and mechanism can be made by way of his intellectual upbringing. Niels's father, Christian Bohr, was a physiologist and, as Roll-Hansen puts it, "sharply opposed a purely mechanistic or reductionist biology that took the complete explanation of all biological phenomena in terms of physics and chemistry as an ultimate aim." The philosopher Harald Høffding was a close associate of the Bohr family—friend and colleague of Christian and philosophical mentor to Niels. Faye substantiates his argument for a clear link between Høffding's philosophical positions and the arguments of the *Critique of Judgment* and discusses Høffding's role as a philosophical mentor of Niels Bohr. Roll-Hansen points out that the physiologist J. S. Haldane, who "argued in the style of Kantian critical teleology, . . . was a close foreign colleague

[11] E.g., Timothy Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth-Century German Biology* (Chicago: Univ. Chicago Press, 1982) (Lenoir does not focus specifically on associations with Bohr, but his work brought attention to the notion of a nineteenth-century "teleo-mechanical" tradition in the early 1980s); Henry J. Folse, *The Philosophy of Niels Bohr: The Framework of Complementarity* (Amsterdam: North-Holland, 1985); Folse, "Complementarity and the Description of Nature in Biological Science," *Biology and Philosophy,* 1990, *5:*221–224; Jan Faye, "The Bohr–Høffding Relationship Reconsidered," *Stud. Hist. Phil. Sci.,* 1988, *19:*321–346; Faye, *Niels Bohr: His Heritage and Legacy: An Anti-Realist View of Quantum Mechanics* (Dordrecht: Kluwer, 1991); Faye, "Once More: Bohr–Høffding," *Danish Yearbook of Philosophy,* 1994, *29:*106–113; David Favrholdt, *Niels Bohr's Philosophical Background* (Copenhagen: Munksgaard, 1992); Faye and Folse, eds., *Niels Bohr and Contemporary Philosophy* (Dordrecht: Kluwer, 1994); Catherine Chevalley, "Niels Bohr's Words and the Atlantis of Kantianism," *ibid.,* pp. 33–55; and Roll-Hansen, "Application of Complementarity to Biology."

[12] Faye, *Niels Bohr;* and Roll-Hansen, "Application of Complementarity to Biology."

[13] Immanuel Kant, *The Critique of Judgment,* trans. Werner Pluhar (Indianapolis: Hackett, 1987). For detailed discussions of these issues see Peter McLaughlin, *Kant's Critique of Teleology in Biological Explanation: Antinomy and Teleology* (Studies in the History of Philosophy) (New York: Mellen, 1990); Daniel Kolb, "The Systematic Unity of Kant's Idea of Nature" (Ph.D. diss., Univ. Notre Dame, 1983); and Kolb, "Kant, Teleology, and Evolution," *Synthese,* 1992, *91*(1–2):9–28.

of Christian Bohr."[14] Niels himself explicitly noted that the debates between vitalists and mechanists of the late nineteenth and early twentieth century—on which Timothy Lenoir has argued that the Kantian "teleo-mechanical" tradition exerted an influential voice—were familiar topics of discussion in the Bohr household. At issue in these debates was the question of whether teleological explanations and concepts employed in biology could be reduced to or accounted for solely in terms of the concepts of physics and chemistry. From the time they were old enough to settle down and pay attention, Niels and his younger brother Harald (a future professor of mathematics) were permitted to attend meetings of the scientific discussion circle to which Christian and Høffding belonged, and such questions are seldom far from the surface in Niels's public talks. Citing an extensive passage from his father, Bohr recalled the intellectual milieu in which he was raised:

> At the same time that mechanistic ideas thus found even wider applications, so-called vitalistic or finalistic points of view, inspired by the wonderful power of regeneration and adaptation in organisms, were repeatedly expressed. Rather than returning to primitive ideas of a life force acting in the organisms, such views emphasized the insufficiency of physical approach [*sic*] in accounting for the characteristics of life. As a sober presentation of the situation as it stood in the beginning of this century, I should like to refer to the following statement by my father, the physiologist Christian Bohr, in the introduction to his paper "On Pathological Lung Expansion" which appeared in the anniversary publication of the Copenhagen University in 1910.
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>> . . . It is thus in the very nature of this task [the detailed empirical investigation of biological phenomena] to refer the word purpose to the maintenance of the organism and consider as purposive the regulation mechanisms which serve this maintenance. . . . The *a priori* assumption of the purposiveness of the organic process is, however, in itself quite natural as a heuristic principle and can, due to the extreme complication and difficult comprehension of the conditions in the organism, prove not only useful, but even indispensable for the formulation of the special problem for the investigation and the search of ways for its solution. But one thing is what may be conveniently used by the preliminary investigation, another what justifiably can be considered an actually achieved result. As regards the problem of purposiveness of a given function for the maintenance of the whole organism, such a result can, as stressed above, be secured only by a demonstration in detail of the ways in which the purpose is reached.
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> I have quoted these remarks which express the attitude in the circle in which I grew up and to whose discussions I listened in my youth, because they offer a suitable starting point for the investigation of the place of living organisms in the description of nature. As I shall try to show, modern development of atomic physics, at the same time as it has augmented our knowledge about atoms and their constitution of more elementary parts, has revealed the limitation in principle of the so-called mechanical conception of nature and thereby created a new background for the problem, most persistent to our subject, as to what we can understand by and demand of a scientific explanation.

Christian endorsed the characteristically Kantian *a priori* employment of teleological concepts, at least as a heuristic device, in the life sciences. His own work on gas exchange in respiration and on the fixation of oxygen by blood led him to think that such regulatory mechanisms defy explanation in strictly physical and chemical terms. In this passage, Niels clearly understood his father's work to exemplify a view that emphasizes the insufficiency

[14] Roll-Hansen, "Application of Complementarity to Biology," pp. 417, 420 (on Haldane). On Høffding's role see Faye, "Bohr–Høffding Relationship Reconsidered" (cit. n. 11); Faye, *Niels Bohr* (cit. n. 11); Faye, "Once More: Bohr–Høffding" (cit. n. 11); and Folse, *Philosophy of Niels Bohr* (cit. n. 11).

of purely physical accounts of life and, even in 1957, thought that such a methodology could still serve as the basis for a productive research program. He grew up in an intellectual climate in which biologists like his father were frequently challenged by reductionists for their employment of functional concepts, and he was interested in putting forward a view that made clear theoretical sense of the application of such concepts.[15]

I will offer a reading of Bohr's texts that can stand independently of any putative Kantian influences, but one that fits well with this way of contextualizing his views. I shall use the phrase "teleo-mechanical complementarity" as a label for what I take to be Bohr's view of the life sciences. My use of this designation calls attention to these Kantian resonances. Since Roll-Hansen and I are in agreement in situating Bohr within this general context, I will focus on explaining where my claim differs from his.

In addition to this Kantian reading of Bohr, we find Roll-Hansen endorsing the "other laws of physics" interpretation for both Bohr and Delbrück, a reading I challenge in both cases. For example, Roll-Hansen writes: "While Kant described a complementary relationship between mechanistic and teleological explanation, Bohr introduced complementarity already in mechanics. What he suggested was the *existence of additional, as yet unknown, principles of physics.* Or in other words, he sought a *further generalization of mechanics* in order to cover a larger part of biological experience."[16] Note here that Roll-Hansen assumes—in my view incorrectly—that the complementarity that Bohr took to obtain in biology would be a relation between two *mechanistic* descriptions. I will argue that this reading is mistaken, since it misses the "teleo-mechanical" outlook crucial to Bohr's view of the life sciences. Roll-Hansen's commitment to the "other laws of physics" interpretation of Bohr rests on a failure, in some places, to take the "teleo-mechanical" reading far enough.

Bohr began to develop his speculations about the relationship between teleology and mechanism publicly in "Light and Life." He ventured that revisions in the foundations of mechanics would force scientists to rethink our understanding of the notion of physical explanation and would create a "new background for the discussion of the problems of life in relation to physics." He argued that developments in atomic physics had revealed an "essential limitation of the mechanical description of natural phenomena."[17] Bohr's own work in atomic physics had legitimized modes of description that stand in a relation of complementarity. The empirical phenomena presented by light in various experimental contexts apparently require mutually exclusive descriptions, which are jointly necessary for our understanding of the evidence. Insistent that no aspects of the observable phenomena can be compromised, Bohr understood the need for complementary descriptions to have deep epistemic significance for our attempts to give a full account of our experience.

Bohr proceeded to draw a provocative analogy between light and life: Just as physicists must take the quantum of action as a basic fact, irreducible to classical physical concepts,

[15] Lenoir, *Strategy of Life* (cit. n. 11); and Niels Bohr, "Physical Science and the Problem of Life" (1957), rpt. in *Essays, 1932–1957, on Atomic Physics and Human Knowledge* (cit. n. 1), pp. 94–101, on pp. 95–96. On Niels's and Harald's attendance at meetings of the discussion circle see David Favrholdt, "Remarks on the Bohr–Høffding Relationship," *Stud. Hist. Phil. Sci.,* 1991, *22:*399–414, esp. p. 400.

[16] Roll-Hansen, "Application of Complementarity to Biology," p. 424 (emphasis added). In his own Kantian reading Roll-Hansen refers to Bohr's view as a "critical teleology" or "epistemological anti-reductionism." I prefer the label "teleo-mechanical complementarity" because it explicitly highlights Bohr's envisioned complementarity between teleological and mechanistic descriptions.

[17] Bohr, "Light and Life" (cit. n. 1), pp. 457a, 421a. See also Niels Bohr, "Light and Life Revisited," dedication for the Institute of Genetics (1962), in *The Philosophical Writings of Niels Bohr,* Vol. 3: *Essays, 1958–1962, on Atomic Physics and Human Knowledge* (Woodbridge: Ox Bow, 1963), pp. 23–29, on p. 24.

biologists must consider life as an elementary fact.[18] Convinced that teleological concepts cannot be excluded from biology without distorting or attenuating our understanding of life, Bohr was interested in whether the concept of complementarity can play an analogous role in elucidating the difficult issue of understanding how to reconcile a physical or mechanistic analysis of phenomena with the apparent purposive and self-regulative activity of organisms.

Let us be clear that Bohr was *not* claiming that the complementarity needed to understand biological phenomena is in any way a consequence of the complementarity in quantum mechanics. In a 1954 lecture, for example, he explicitly stated: "With these remarks it is in no way meant to imply that, in atomic physics, we possess a clue to the explanation of life." Bohr did, however, believe it necessary to rethink the notion of explanation in light of what he took to be an awareness of limitations in the mechanistic mode of description: "As I shall try to show, modern development of atomic physics . . . has revealed the limitation in principle of the so-called mechanical conception of nature and thereby created a new background for the problem . . . as to what we can understand by and demand of a scientific explanation."[19]

As Bohr emphasized again and again from 1932 onward, he held that in biology "mechanistic and vitalistic arguments are used in a typically complementary manner": "the analogy considered is the typical relation of complementarity. . . . [T]he concept of purpose, which is foreign to mechanical analysis, finds a certain field of application in problems where regard must be taken of the nature of life." Bohr held that "in actual biological research, a vitalistic approach is equally indispensable." He explicitly distanced his own more modest use of terms like "vitalistic" or "finalistic" from "the old idea of a mystic life force." He saw the need for "two scientific approaches which only together exhaust the possibilities of increasing our knowledge. In this sense, mechanistic and vitalistic viewpoints may be considered as complementary."[20] His thought is that the complex organization of biological systems prevents a complete quantum mechanical analysis, since organisms could not be kept alive under the conditions required by the relevant experimental arrangements. Bohr took physiology (which pursues causal explanation in physical and chemical terms) and psychology (which employs functional concepts as primitive) as examples of such contrasting points of view.[21]

Bohr was defending the legitimacy of a framework for relating physics and biology that takes teleological and mechanistic descriptions to be complementary in the precise sense that these modes of description are mutually exclusive and yet jointly necessary for a complete understanding of life: "Although science will of course strive for ever more detailed knowledge of the physical mechanism underlying the functions of organisms, a

[18] Bohr, "Light and Life," p. 458a; see also Bohr, "Light and Life Revisited," p. 26.

[19] Niels Bohr, "Seventh International Congress of Radiology" (1954), in *The Philosophical Writings of Niels Bohr,* Vol. 4: *Causality and Complementarity: Supplementary Papers,* ed. Jan Faye and Henry J. Folse (Woodbridge: Ox Bow, 1998), pp. 161–163, on p. 163; and Bohr, "Physical Science and the Problem of Life" (cit. n. 15), pp. 96–97.

[20] Niels Bohr, "On the Notions of Causality and Complementarity" (1948), in *Causality and Complementarity,* pp. 141–148, on p. 147; Bohr, "Light and Life" (cit. n. 1), p. 458b; and Bohr, "Medical Research and Natural Philosophy" (1951), in *Causality and Complementarity,* pp. 149–154, on p. 153. As Bohr sees it, traditional vitalism (in, say, its postulation of a life force or "occult qualities") tends to drift into obscurantism and mysticism, while the mechanical conception of nature excludes our experience of purposive behavior. Bohr seeks an epistemological resolution to the problem.

[21] Bohr, "Light and Life," p. 458a–b. See also Bohr, "Light and Life Revisited" (cit. n. 17), p. 26; and Bohr, "Medical Research and Natural Philosophy," p. 153.

description of life corresponding to the ideal of mechanism will only constitute one line of approach." Teleological approaches, which had so often been marginalized or associated with "the old idea of a mystic life force," can now be recognized as "equally indispensable" for biology.[22] Such approaches thus stand in a conceptual relation *analogous* to the notion of complementarity encountered in quantum mechanics. The clear need for such dual modes of description in atomic physics lends scientific respectability to the suggestion that biologists also quite properly employ complementary modes of description.

Notice that, on the teleo-mechanical reading, there is a clear sense in which Bohr took purposive concepts to be *irreducible* to mechanistic ones. He thought that purposive concepts could not be analyzed solely in terms of more basic concepts of physics and chemistry and must simply be assumed as a starting point for the description of organisms when seen as whole individuals.

> On this view, the existence of life must be considered as an elementary fact that cannot be explained, but must be taken as a starting point in biology, in a similar way as the quantum of action, which appears as an irrational element from the point of view of the classical mechanical physics, taken together with the existence of elementary particles, forms the foundation of atomic physics. The *asserted impossibility of a physical or chemical explanation of the function peculiar to life* would in this sense be analogous to the insufficiency of the mechanical analysis for the understanding of the stability of atoms.[23]

Bohr thought that, when we turn to the study of life, purely mechanistic modes of description face specific limitations. In his view, such an approach is unable to account, for instance, for particular aspects of an intact, functional cell or for phenomena such as consciousness.

Over the years Bohr wavered over whether there are, in fact, principled or merely practical reasons to think that mechanistic approaches have only a limited capacity to account for life. Commenting on the basis for this framework of biological complementarity in "The Connection between the Sciences" (1960), he concluded that "as long as the word 'life' is retained for practical or epistemological reasons, the dual approach in biology will surely persist."[24] Remaining somewhat ambiguous, Bohr still asserted that while no sharp distinction can be drawn between function and mechanism if we are to do justice to the life of the organism in its entirety, at least as a practical matter, and perhaps even for deep epistemic reasons, teleological modes of description remain indispensable in complementing the terminology of molecular biology.

[22] Bohr, "Light and Life," p. 458b.

[23] *Ibid.,* p. 458a (emphasis added).

[24] Niels Bohr, "The Connection between the Sciences" (1960), in *Essays, 1958–1962, on Atomic Physics and Human Knowledge* (cit. n. 17), pp. 17–22, on p. 21. One also finds this ambiguity between practical or principled epistemological reasons in Bohr, "Light and Life Revisited" (cit. n. 17), p. 26: "Surely, as long as for practical or epistemological reasons one speaks of life, such teleological terms will be used in complementing the terminology of molecular biology. This circumstance, however, does not in itself imply any limitation in the application to biology of the well-established principles of atomic physics." Delbrück seems to have focused on Bohr's suggestion in the original "Light and Life" lecture that one principled reason that there are limits to a mechanistic description of life might be the fact that "we should doubtless kill an animal if we tried to carry the investigation of its organs so far that we could describe the role played by single atoms in vital functions": Bohr, "Light and Life," p. 458a. Delbrück specifically returned to this passage in "Biophysics" (cit. n. 4), in "Light and Life III" (cit. n. 4), and in *Mind From Matter? An Essay on Evolutionary Epistemology* (Palo Alto, Calif.: Blackwell Scientific, 1986). He noted that, in "Light and Life Revisited," Bohr attempted to clarify (and to some extent qualify) his earlier view. Taking the opportunity to reexamine his original position in light of the growth of the field of molecular biology, Bohr pointed out in that 1962 talk that "the task of biology cannot be that of accounting for the fate of each of the innumerable atoms permanently or temporarily included in a living organism": Bohr, "Light and Life Revisited," p. 26.

We are now in a position to see why Roll-Hansen's claim that the complementarity that Bohr takes to obtain in biology will be a relation between two *mechanistic* descriptions is mistaken. On the reading of Bohr I have defended, he envisions a complementarity between teleological and mechanistic descriptions of life. This mistake is crucial to Roll-Hansen's claim that Bohr expects that "additional, as yet unknown, principles of physics" will be required for "a further generalization of mechanics" that can cover biological phenomena.[25]

An appreciation of Bohr's "teleo-mechanical" framework makes a difference here in a way that undermines the "other laws of physics" interpretation for the following reason. Any anticipated advances would have to come on either of the two sides of the complementary modes of description: mechanical or teleological. Now, for Bohr, physics and chemistry fall clearly on the mechanical side of the descriptions of life. But I see no reason to suppose that he expected the discovery of new laws on the mechanistic side.[26] On the contrary, it is because he expects that further inquiry will confront us with "a fundamental limit to the analysis of the phenomena of life in terms of physical concepts" that such descriptions will need to be supplemented with teleological ones.[27] Indeed, in some respects advances that were achieved on the reductionist side over the next thirty years represent a challenge to, or frustration of, Bohr's position. Bohr did not think that we can simply add more concepts to expand current physics and chemistry. His point was precisely that the required teleological concepts were *incompatible* with the *correct* physico-chemical description and that some biological phenomena are not intelligible in solely mechanical terms.

Did Bohr expect a discovery of new laws on the teleological side? There are, in fact, a few places—not many—where he expresses a need for the introduction of "biological laws" on the teleological side. But it would be highly misleading to call advances of that sort "other laws of *physics.*" On Bohr's view, physico-chemical explanations are precisely what laws of biology are *not.* He clearly thought that teleological or finalistic concepts transcend the domain of physics and chemistry: "Actually, we must recognize that the practical approach in biological research is characterized by the complementary way in which arguments, based on the full resources of physical and chemical science and *concepts* referring to the integrity of the organism *transcending the scope of these sciences* are employed."[28] At best, there may be room for a qualified claim that Bohr sees a need for "other laws of *biology.*" But even there, I don't think the prospects are all that promising. Here we do well to remember that Kantians characteristically employ purposive concepts as regulative (heuristic) principles rather than constitutive ones. We have seen that Bohr explicitly remains agnostic about whether teleological concepts are employed "for practical or epistemological reasons," and, as we shall see, in the "Three-Man Paper" Delbrück also speaks of a "heuristic scheme" in this connection.

Both Bohr and Delbrück were interested in promoting our understanding of biological phenomena and no doubt would have welcomed revolutionary scientific contributions of

[25] Roll-Hansen, "Application of Complementarity to Biology," p. 424 (emphasis added).

[26] Delbrück, e.g., takes it for granted that "quantum mechanics is the *final word* as regards the behavior of atoms and we base this belief upon the analysis by Bohr and Heisenberg": Delbrück, "Physicist Looks at Biology," p. 19 (emphasis added).

[27] Niels Bohr, "Introductory Survey" (1929), in *The Philosophical Writings of Niels Bohr,* Vol. 1: *Atomic Theory and the Description of Nature* (Woodbridge: Ox Bow, 1987), pp. 1–24, on p. 22. The Ox Bow edition is a reprint of the Cambridge University Press volume originally published in 1934.

[28] Niels Bohr, "Physical Science and Man's Position" (1956), in *Causality and Complementarity* (cit. n. 19), pp. 170–179, on p. 176 (emphasis added). On the need for biological laws on the teleological side see Bohr, "Light and Light Revisited" (cit. n. 17), p. 26.

any sort. Much of what they said about the relation between physics and biology was also ambiguous and explicitly speculative in character, intended to raise questions and leave many possibilities open. My claim is not that there is nothing at all in what Bohr and Delbrück say that can plausibly be interpreted as raising hopes that new laws of some sort might be found by a close look at biological phenomena. Nor am I claiming that the "other laws of physics" interpretation is, say, logically inconsistent with Bohr's teleo-mechanical view of biology. What is at issue is whether attributing this expectation to him gives us insight into, or even aptly describes, the central features of his thought. My claim is that there is very little in what Bohr actually said in his repeated dealings with topics concerning the relation of physics and biology to suggest that the expectation that biological inquiry would lead to the discovery of "other laws of physics" played a central, or even significant, role in his thought.

Bohr's interest in biology, in my view, was never that of looking for "other laws of physics." Rather, what Bohr took himself to be providing was a "conceptual framework" that recognized the legitimacy of concepts *already* employed by biologists like his father and harmonized them with the mechanical descriptions rightly employed by the physico-chemical approach. Notice that Bohr took this attitude to be present already; it is the approach, as he put it, "always taken more or less intuitively by biologists." Bohr is instead suggesting that the use of complementary concepts in the description of nature—a practice "*by no means new* in other fields of science"—is a conceptually permissible strategy in the life sciences. As he put it in 1951, given that biologists do, in fact, make use of both vitalistic and mechanistic viewpoints, we face "the problem of finding a rational and consistent way to orient ourselves in this situation."[29] His concern was to harmonize these dual approaches. What is central to Bohr's position is not the anticipation of *new* breakthrough discoveries. Rather, it is the articulation of a *philosophical* "framework" intended to justify the *already* "familiar" employment of teleological concepts in the life sciences, which were a source of controversy because they seemed incompatible with a strictly physico-chemical approach.

III. DELBRÜCK'S RECEPTION OF BOHR: IRREDUCIBILITY AND BIOLOGICAL COMPLEMENTARITY

What did Delbrück find so invigorating about the "Light and Life" lecture? Even if Bohr did not anticipate that the study of biology would lead to "other laws of physics," is there any reason to think that Delbrück took Bohr's view to open the door to such discoveries?

There is some reason to think that Delbrück did not entirely understand Bohr's view. According to Fisher and Lipson, "The actual lecture, which differed from its published version, made an enormous impression on Max because in it Bohr went out on a limb to predict such complementarity [concerning the relation between life and atomic physics]." Fisher and Lipson also indicate that something in the talk prompted Delbrück to flip through Kant for further illumination. "Max was intrigued with the idea that complementarity could reveal great simplicity in a hopelessly complex situation. He was fascinated by this notion the moment Bohr presented it. As a first consequence, it motivated him to scrutinize the writings of Immanuel Kant on causality to see how the German philosopher

[29] Bohr, "Light and Life," in *Essays, 1932–1957, on Atomic Physics and Human Knowledge* (cit. n. 1), p. 4; Niels Bohr, "The Causality Problem in Physics" (1938), in *Causality and Complementarity,* pp. 94–121, on p. 108 (emphasis added); and Bohr, "Medical Research and Natural Philosophy" (cit. n. 20), p. 153.

could have overlooked this epistemological possibility; Max found this situation utterly removed from anything that Kant had conceived."[30] Apparently Delbrück was unable to find the connection with Kant.

In 1963, in a discussion following Delbrück's presentation on biological complementarity, Niels's son Aage Bohr, who had worked with Delbrück at Caltech for a short period in the late 1940s, attributes to Delbrück the expectation of discovering a new law of nature and distances his own understanding of biological complementarity from such an expectation:

> As I have understood, you have always taken the view that this complementarity really applies to the very concrete experimental facts, namely that if we really study in great detail the organisms and the simplest ones, the bacteria, we will come at a certain moment to a real paradox that will be similar to the stability paradox one had in quantum mechanics, and that *then there will be a fundamental new law in nature,* that will be discovered at that point. I can only say *personally I have never taken it in that way,* but more like this complementarity relation you mentioned between thermodynamics and classical mechanics. But of course in many ways it is very much deeper in biology, where one is faced with the situation that to describe an organism *one must use* many times *words that do not belong to physics and chemistry.*

While Delbrück's response does not clarify his own take on other laws of physics, later in the discussion he says: "in the next higher order we may run up against an observational complementarity, so that we have to introduce different notions, I mean *independent* notions." The question is then put to him directly: "New laws of nature?" Delbrück's response is: "I consider that it is possible and I am curious about it."[31] Whether this is a sign of substantive differences between Delbrück and Bohr or merely a breakdown in communication, Delbrück's acknowledged curiosity hardly indicates that the expectation of finding other laws of physics was central to his interest in Bohr or characteristic of his thinking about biological complementarity. Indeed, Delbrück's remark that "independent notions" might be required fits very well with Bohr's view that teleological concepts are required to understand life.

Delbrück also perceived a difference between his own views and the positions of Niels and Aage Bohr. Roll-Hansen observes that, in a 1954 letter to Niels, Delbrück expressed his desire "that you, or Aage and you, should write up the thoughts about complementarity in physics in greater detail." As late as 1957 Delbrück could write to Bohr concerning the complementarity argument, teleology, and mechanism and express bemusement: "At the same time I have a feeling that this is not what you have in mind and, therefore, that I do not properly understand your point of view."[32]

While Delbrück might have failed to appreciate some of the nuances and philosophical underpinnings of Bohr's own more Kantian position, it is clear that he understood Bohr's basic outlook. Very early on, when still closely under Bohr's influence, Delbrück and his coauthors held in the "Three-Man Paper" that the "physical-chemical character" of genetic

[30] Fisher and Lipson, *Thinking about Science,* pp. 79, 82. Fisher and Lipson are paraphrasing the Delbrück interview here: Harding interview with Delbrück (cit. n. 3), p. 40. Actually, remarks in the 1935 "Three-Man Paper" indicate that Delbrück may have been more aware of the Kantian view than this suggests.

[31] This discussion came after Delbrück delivered his paper "Biophysics" (cit. n. 4) at a conference held in Copenhagen in 1963 to commemorate Bohr's first papers on atomic constitution and was printed in the conference proceedings along with the paper; the quotations come from pp. 64, 66, and 67 (emphases added).

[32] Max Delbrück to Niels Bohr, 1 Dec. 1954, Bohr Archives, Bohr Institute, Copenhagen; and Delbrück to Bohr, 23 Dec. 1957, Delbrück Papers, Caltech Archive, Pasadena. Both letters are cited in Roll-Hansen, "Application of Complementarity to Biology," pp. 434, 437.

processes is puzzling "unless their coordination is viewed as arising on the basis of a *heuristic scheme* in which the life-process is *postulated.*" Delbrück also seems to have stuck to this understanding consistently in later writings. In 1949, for example, he wrote:

> It may turn out that certain features of the living cell, including perhaps even replication, stand in a *mutually exclusive relationship* to the strict application of quantum mechanics, and that a new conceptual language has to be developed to embrace this situation. The limitation in the applicability of present day physics may then prove to be, not the dead end of our search, but the open door to the admission of fresh views of the matter. Just as we find features of the atom, its stability, for instance, which are *not reducible* to mechanics, we may find features of the living cell which are *not reducible* to atomic physics but whose appearance stands in a *complementary relationship* to those of atomic physics.
>
> This idea, which is due to Bohr, puts the relation between physics and biology on a new footing. Instead of aiming from the molecular physics end at the whole of the phenomena exhibited by the living cell, we now expect to find natural limits to this approach, and thereby implicitly new virgin territories on which laws may hold which involve new concepts and which are only loosely related to those of physics, by virtue of the fact that they *apply to phenomena whose appearance is conditioned on not making observations of the type needed for a consistent interpretation in terms of atomic physics.*[33]

Delbrück appreciated the fact that Bohr envisioned a biology that employs both teleological and mechanistic concepts, and he shared Bohr's expectation that such descriptions are mutually exclusive and yet jointly necessary for understanding life.[34] Because teleological concepts stand in a complementary relation to physico-chemical ones, they are autonomous from and irreducible to physico-chemical terms.

Delbrück tells us, in more than one place, that he took the main themes of "Light and Life" to be *irreducibility* and *complementarity.* In a letter to Bohr on 30 November 1934 he attempted to articulate a concise statement of the position: "We do *not* state that the laws of atomic physics can explain the specific phenomena of life. On the contrary!" The position he took himself to share with Bohr was this: "Our claim: Those assumptions that are necessary for the existence of causality in biological phenomena may partly contradict the laws of physics and chemistry, because experiments with living organisms are certainly complementary to those that determine the physical and chemical events on the atomic level."[35]

In propounding the "other laws of physics" interpretation, Stent has made much of the idea that Delbrück turned to biological phenomena in hopes of finding a paradox. In a passage recounting advances in the recent history of atomic physics, Delbrück stated:

> The crucial point in this abbreviated account of an historical episode is the appearance of a conflict between separate areas of experience, which gradually sharpens into a paradox and must then be resolved by a radically new approach.

[33] Timoféeff-Ressovsky *et al.,* "Nature of Genetic Mutations and the Structure of the Gene" (cit. n. 6) (emphasis added); and Delbrück, "Physicist Looks at Biology" pp. 20–21 (emphasis added). Cf. Max Delbrück, "A Physicist's Renewed Look at Biology: Twenty Years Later," *Science,* 12 June 1970, *168:*1312–1315.

[34] This is also clear in the Harding interview with Delbrück (cit. n. 3), pp. 40–41: "Bohr then very vigorously asked the question whether this new dialectic wouldn't be important also in other aspects of science. He talked about that a lot, especially in relation to biology, in discussing the relation between life on the one hand and physics and chemistry on the other—whether there wasn't an experimental mutual exclusion, so that you could look at a living organism either as a living organism or as a jumble of molecules; you could do either, you could make observations that tell you where the molecules are, *or* you could make observations that tell you how the animal behaves, but there might well exist a mutually exclusive feature, analogous to the one found in atomic physics."

[35] Delbrück to Bohr, 30 Nov. 1935, quoted in Fisher and Lipson, *Thinking about Science,* p. 85.

> As is well known, the resolution of the paradoxes of atomic structure necessitated a revision of our ideals (or prejudices) regarding the description of nature.

Delbrück ended this 1949 talk with a ringing note about "a new intellectual approach to biology."[36] Should we, with the traditional reading, understand this talk of "paradoxes" that might demand a "radically new approach" or "revision" of fundamental scientific concepts as an indication that he came to believe that by finding paradoxical situations in biology he might uncover other laws of physics? This is unlikely, especially when we see how deeply Bohr's influence shaped his ongoing research interests.

Delbrück was not looking for "other laws of physics." He set out to vindicate "Bohr's subtle complementarity argument" empirically. As I read Delbrück's 1949 essay, "A Physicist Looks at Biology," his ambition was to bring a paradox in the biological domain into sharp focus so as to establish Bohr's suggestion that biology requires appeal to complementary *mechanical* and *teleological* descriptions. By the term "paradox," Delbrück intends an apparent "conflict between separate areas of experience." He hoped that by focusing on concrete research projects and by pursuing the mechanical analysis of biological phenomena to a sufficient degree of detail, the limitations of this approach would become clear. If Bohr was right that life is not reducible to physics, the rigorous pursuit of a *reductionist* research program, pushed to its limits, should bring to light precisely where those limits are. At least in 1949, Delbrück's attitude toward the reductionist program was this:

> It looks sane until the paradoxes crop up and come into sharper focus. In biology we are not yet at the point where we are presented with clear paradoxes and this will not happen until the analysis and behavior of living cells has been carried into far greater detail. This analysis should be done on the living cell's own terms and the theories should be formulated without fear of contradicting molecular physics.[37]

Delbrück would confront the scientific world with an empirical situation that was not analyzable in physico-chemical terms. In this way, he would legitimize a "new intellectual approach to biology" by showing that the situation could be resolved only by adopting the biological complementarity envisioned by Bohr. *That* was Delbrück's lifelong project.

Explaining his strategy in a letter to Bohr in 1954, Delbrück wrote:

> I talked about this system as something analogous to a gadget of physics and explained at length why it seemed more helpful to me to analyze this gadget in great detail, rather than the many other biological gadgets which have been the subject of conventional research for many years. What I failed to stress was my suspicion, you might say hope, that *when this analysis is carried sufficiently far,* it will run into a paradoxical situation analogous to that into which classical physics ran in its attempts to analyze atomic phenomena. *This, of course, has been my ulterior motive in biology from the very beginning.*

While Bohr seems more comfortable staying at the level of philosophical outlook, Delbrück adopted a reductionist methodology, hoping to find a paradox that would prove that some biological phenomena are *not* reducible to physico-chemical terms. As Delbrück put it, "the individual organism presents an indissoluble unit, barring us, at least at present, from a reduction to the terms of molecular physics. It may well turn out that the bar is not

[36] Delbrück, "Physicist Looks at Biology," pp. 18, 22.
[37] *Ibid.,* p. 22.

really an essential one, but a physicist is well prepared to find that it is essential." Although the reductionist program was successful to such an extent that Delbrück was forced to switch topics for investigation at various points during his career, he remained steadfast in this central ambition. In 1978 he still held that psychology and molecular biology "certainly are in a complementary relation which nobody can still formulate very well. They haven't been pursued to that bitter end where you *have* to make some kind of new dialectical approach."[38]

In trying to assess the impact of Bohr's thought on Delbrück, I thus find myself in disagreement with both parts of Roll-Hansen's claim that "[Delbrück] did not think of biology as an autonomous science on par with physics but saw it as a source of phenomena that demanded new fundamental advances in physics." There seems to be a straightforward sense in which the assertion that Delbrück "did not think of biology as an autonomous science on par with physics" is just false: he held that purposive concepts are irreducible to physico-chemical terms. (Although I am focusing the discussion on Roll-Hansen's claim, Lily Kay similarly concludes that Delbrück "was committed to biology as a branch of physics.") It is not entirely clear what Roll-Hansen intends by "autonomous" here. But surely one feature that would argue for a kind of autonomy of one discipline from another would be the fact that discipline X employs certain concepts and terms that are irreducible to (that is, not analyzable solely in terms of) the concepts and terms of discipline Y.[39] And yet, in precisely that sense, Delbrück thought that biology is autonomous of and irreducible to physics and chemistry. He thought that those latter two sciences would not be able to account for at least some aspects of biological phenomena. In Part 3 of the "Three-Man Paper" (a section penned by Delbrück), despite their speculation that genes are molecules, Delbrück and his coauthors take the involvement of genes in the biological processes of the cell as a whole as a reason to "assert that genetics is autonomous and that it should not be permitted to be blended with physical-chemical notions."[40]As we have seen, Delbrück understood the central point of the "Light and Life" lecture to be the idea that purposive concepts are *not reducible* to more basic physico-chemical terms. This is a view Delbrück shared deeply with Bohr; it expressed a hope that was, to be sure, severely tested by the success of the reductionist side of the project of molecular biology, but one that nevertheless survived and continued to inspire him at least through the time of Bohr's death. Precisely because Delbrück saw some biological phenomena as autonomous from and irreducible to physics and chemistry, the "other laws of physics" interpretation also fails—for the same reasons we saw in the case of Bohr.

The assertion that Delbrück saw biology "as a source of phenomena that demanded new fundamental advances in physics" is a concise statement of the widely held "other laws of physics" interpretation.[41] Consider several further difficulties with this view.

On the "other laws of physics" interpretation, I am unable to explain why Delbrück would assert, for example, either of the following statements. First, contrasting differences between the explanatory approach characteristic of physics and the sort of phenomena with

[38] Kay, "Conceptual Models and Analytical Tools" (cit. n. 5), p. 245 (citing letter to Bohr; emphasis added); Delbrück, "Physicist Looks at Biology," p. 17; and Harding interview with Delbrück (cit. n. 3) (the word "have" is underlined in the text of the interview).

[39] See Alexander Rosenberg, *The Structure of Biological Science* (Cambridge: Cambridge Univ. Press, 1985).

[40] Roll-Hansen, "Application of Complementarity to Biology," p. 418; Kay, "Conceptual Models and Analytical Tools," p. 246; and Timoféeff-Ressovsky *et al.,* "Nature of Genetic Mutations and the Structure of the Gene" (cit. n. 6).

[41] Roll-Hansen, "Application of Complementarity to Biology," p. 418.

which we are confronted in biology, Delbrück comments: "Such a situation from the outset *diminishes* the hope of understanding any one living thing by itself and *the hope of discovering universal laws, the pride and ambition of physicists.*" This remark is positively pessimistic about the prospect of physicists "discovering universal laws" by the study of biology! The rest of the essay seems to bear out this pessimism. Delbrück thought that when looking at biology the physicist must be extremely cautious in attributing laws to various phenomena: "when he thinks he has discovered a law of nature as pertaining to living matter, like Weber's law [which Delbrück thinks is very likely not a genuine law], he must beware lest he be fooled by natural selection simulating such a law." Second, Delbrück seemed to take it for granted that his biological investigations would not require either the revision or the enlargement of atomic physics: "We believe that quantum mechanics is the *final word* as regards the behavior of atoms and we base this belief upon the analysis by Bohr and Heisenberg."[42]

IV. *WHAT IS LIFE?* AS A PLAUSIBLE SOURCE FOR THE "OTHER LAWS OF PHYSICS" MISREADING OF DELBRÜCK

Although I am challenging the traditional reading of Delbrück as centrally motivated by a hope that finding an example of complementarity in biology would lead to the discovery of "other laws of physics," it is a reading that must be taken seriously, particularly because Watson and Stent, both of whom worked with Delbrück personally, understood him in just this way. If we reject the "other laws of physics" interpretation of both Bohr and Delbrück, can we at least explain how close associates of Delbrück could have misunderstood him on this point? My view is that the "other laws of physics" interpretation of Delbrück's aspirations is a misreading that has its source in Erwin Schrödinger's *What Is Life?* Schrödinger *did* expect the study of biological phenomena to lead to the discovery of "other laws of physics," and his failure—in this influential book—to appreciate crucial differences between his approach and that of Delbrück was in large part responsible for the resulting confusion among those who have told the story of the rise of the phage side of molecular biology.

Perhaps the most compelling reason to reject the "other laws of physics" interpretation of Delbrück's aspirations comes from contrasting his outlook with the one expressed in Schrödinger's *What Is Life?* As is well known, Schrödinger's *What Is Life?* lectures became an influential focal point for discussion about the potential significance of quantum physics for biology. In these lectures this Nobel laureate and founder of wave mechanics drew attention to the target theory of the gene and to the structural model of the gene put forward in the "Three-Man Paper" of Timoféeff-Ressovsky, Zimmer, and Delbrück.[43] He ventured that the 1926–1927 Heitler-London quantum theory of the chemical bond could resolve questions about the stability of genetic material, which seemed so puzzling from

[42] Delbrück, "Physicist Looks at Biology," pp. 10 (emphasis added), 16, 19 (emphasis added).

[43] Erwin Schrödinger, *What Is Life? The Physical Aspect of the Living Cell* (1944), was reprinted with *Mind and Matter* (1958) and "Autobiographical Sketches" (Cambridge: Cambridge Univ. Press, 1992). For discussion of the impact of *What Is Life?* and its influence in leading a number of trained physicists to transition into biology see esp. Robert Olby, *The Path to the Double Helix* (London: Macmillan, 1974); Olby, "Schrödinger's Problem: *What Is Life?*" *J. Hist. Biol.,* 1971, *4:*119–148; and Edward J. Yoxen, "Where Does Schroedinger's "What Is Life?" Belong in the History of Molecular Biology?" *History of Science,* 1979, *17:*17–52. Schrödinger and Paul Dirac were co-recipients of the 1933 Nobel Prize in Physics "for the discovery of new productive forms of atomic theory." The "Three-Man Paper" is Timoféeff-Ressovsky *et al.,* "The Nature of Genetic Mutations and the Structure of the Gene" (cit. n. 6).

the perspective of classical statistical physics. Schrödinger, who famously disputed Bohr's interpretation of complementarity even in physics, envisioned no such need for complementarity in biology.[44] But he did expect that new physical laws, which he called "order-from-order principles," would be required to explain the mechanism by which the order of the aperiodic crystal is translated to the dynamic processes of the cell.[45]

> We must therefore not be discouraged by the *difficulty of interpreting life by the ordinary laws of physics.* For that is just what is to be expected from the knowledge we have gained from the structure of living matter. We must be *prepared to find a new type of physical law* prevailing in it. Or are we to term it a non-physical, not to say a super-physical, law?
>
> THE NEW PRINCIPLE IS NOT ALIEN TO PHYSICS
>
> No. I do not think that. *For the new principle that is involved is a genuinely physical one: it is, in my opinion, nothing else than the principle of quantum theory over again.*

So there *is* a clear sense in which Schrödinger expects the discovery of "other laws of physics" as an outcome of biological inquiry. What excited Schrödinger was the idea that the physics and chemistry of his day would have to be supplemented by new higher-level laws, wholly compatible with laws at the lower level of description, that are encountered only by examining physical systems above a certain level of complexity. Indeed, the central question of the book is, "How can the events in space and time which take place within the spatial boundary of a living organism be accounted for by physics and chemistry?" Schrödinger takes it that "the obvious inability of present-day physics and chemistry to account for such events is *no reason at all* for doubting that they can be accounted for by those sciences." At best, this represents an *enlargement* or expansion of an essentially mechanistic physics. Schrödinger's metaphors in Chapter 7 remain entirely on the level of mechanism. In the section entitled "New Laws to Be Expected in the Organism" he explains:

> What I wish to make clear in this last chapter is, in short, that from all 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. And that not on the ground that there is any "new force" or what not, directing the behavior of the single atoms within the living organism, but because the construction is different from anything we have yet tested in the physical laboratory. To put it crudely, an engineer, familiar with heat engines only, will, after inspecting the construction of an electric motor, be prepared to find it working along principles which he does not yet understand. He finds the copper familiar to him in the kettles used here in the form of long, long wires wound in coils; the iron familiar to him in levers and bars and steam cylinders is here filling the interior of those coils of copper wire. He will be convinced that it is the same copper and the same iron, subject to the same laws of Nature, and he is right in that. The difference in construction is enough to prepare him for an entirely different way of functioning.[46]

Notice that Schrödinger assumes—in my view incorrectly—that Delbrück shares a similar expectation:

[44] Here, I am in agreement with Robert Olby and Lenny Moss that Schrödinger did *not* expect that these new laws would require appeal to a form of complementarity in the life sciences. See Olby, *Path to the Double Helix;* and Lenny Moss, *What Genes Can't Do* (Cambridge, Mass.: MIT Press, 2003), p. 55.

[45] See Schrödinger, *What Is Life?* (cit. n. 43), pp. 80–85. Moss, *What Genes Can't Do,* pp. 53–62, provides a lucid discussion of Schrödinger. As he puts it, "Schrödinger forsees the finding of new higher-level laws or principles that explain the ability of living systems to parlay high levels of order between the chemically stable but metabolically inert aperiodic crystal and the growing and metabolizing, but entropically vulnerable, apparatus of the cell and organism" (p. 60).

[46] Schrödinger, *What Is Life?* pp. 80–81 (emphasis added; the capital letters appear in the original text and are used to mark a new section heading), 3, 4 (emphasis added), 76.

> But, strangely enough, there is just one general conclusion to be obtained from [Delbrück's molecular model], and that, I confess, was my only motive for writing this book.
>
> From Delbrück's general picture of the hereditary substance it emerges that living matter, while not eluding the "laws of physics" as established up to date, is *likely to involve "other laws of physics" hitherto unknown,* which, however, once they have been revealed, *will form just as integral a part of this science as the former.*[47]

The evidence for Delbrück's alleged agreement on this point is quite tenuous. Citing this passage from Schrödinger, Stent claims that one can *infer* that Delbrück shared this view: "Schrödinger states an important credo which, as can be *inferred* from the article 'A Physicist Looks at Biology,' had been embraced also by Max Delbrück. . . . Thus it was the romantic idea that 'other laws of physics' might be discovered by studying the gene that really fascinated the physicists." Yet in the "Three-Man Paper," Delbrück and his coauthors had emphasized the importance of *functional* aspects of the cell as primitive "starting points" for understanding some aspects of biological phenomena, a suggestion that Bohr would no doubt have been sympathetic to: "Changes to individual parts (gene mutation) would influence the overall functioning of the cell in a specific way and, thus, would affect the individual development processes as well. Therefore, we do not need the cell to dissolve into genes, and the 'starting points' of the developmental sequences are not ascribed to individual genes, but rather to cell functions or even to intercellular processes (which after all are controlled by the genome)."[48]

Delbrück published a review of *What Is Life?* in 1945. He read Schrödinger as raising the question of "*whether* physics and chemistry will be able to give a complete account [of the biological sciences]" and understood him to offer a "prophecy" that a future physics will succeed. For Delbrück these are open empirical questions. He understood Schrödinger's position in direct contrast to Bohr's:

> Perhaps our present knowledge of cellular processes is insufficient to make such a discussion convincing. If that be the case, then any statement about the physical nature of cellular organization would appear premature.
>
> Schrödinger delimits his stand further by stating that, in his opinion, "and contrary to the opinion upheld in some quarters, *quantum indeterminacy* plays no biologically relevant role" (in the cell). The opinions here referred to are presumably those of Bohr and those of Jordan. This is not the place to take up the challenge for these authors, but it is hoped that they will themselves continue the discussion.

Although he did not defend Bohr in the review, Delbrück clearly thought that Schrödinger's conclusions were premature. Schrödinger's belief that life can be understood in purely physical terms remained an article of faith: "After this profession of faith in the physical nature of the workings of the cell, Schrödinger attempts a further characterization of its mechanism, as one which produces order from order." However suggestive Schrödinger's ideas may be, Delbrück did not think Schrödinger had really succeeded in advancing our understanding of cellular mechanisms, and, with Bohr, he feels that it is too quick to assume that life will turn out to be intelligible in merely physical terms. "Physicists and biologists who are not familiar with Bohr's subtle complementarity argument will be inclined to take the physical nature of cellular processes for granted at the outset, and may be dissatisfied

[47] *Ibid.,* p. 68 (emphasis added).

[48] Stent, "Introduction: Waiting for the Paradox" (cit. n. 5), p. 4 (emphasis added); and Timoféeff-Ressovsky *et al.,* "Nature of Genetic Mutations and the Structure of the Gene" (cit. n. 6).

because Schrödinger does not advance our understanding of cellular mechanisms in any specific respect. The reviewer believes, however, that Schrödinger's discussion of the types of laws of nature might exert a clarifying influence in biological thinking."[49]

Delbrück correctly understood that, on Schrödinger's view, an expanded physics will eventually provide an exhaustive understanding of life. But he also saw a notable difference between Schrödinger's view and Bohr's more complicated take on the relation between physics and biology. For Delbrück, the central question of whether advances on the purely mechanistic side would be sufficient for understanding all aspects of life remained an open question.

If Delbrück did not turn to biology expecting to discover "other laws of physics," how could Stent, Watson, and other close associates have misunderstood him on this point? Although Fisher and Lipson claim that "Max's friends will note that he hardly spoke to them about complementarity," Watson, at least, recalls Delbrück's mentioning the search for complementarity even on the occasion of their first meeting:

> Then, as on many subsequent occasions, Delbrück talked about Bohr and his belief that a complementarity principle, perhaps like that needed for understanding quantum mechanics, would be the key to the real understanding of biology. Luria's views were less firm, but there was no doubt that on most days he too felt that the gene would not be simple and that high powered brains, like Delbrück's or that of the even more legendary Szilard, might be needed to *formulate the new laws of physics (chemistry?)* upon which the self-replication of genes was based.

If, as Watson's comment suggests, Delbrück often discussed his ideas on complementarity quite freely and openly—even on a first meeting—then we would have reason to place more confidence in the recollections of personal friends regarding Delbrück's own views on this issue. Yet, as Stent notes, even after the tutorial on complementarity there was a fair amount of confusion as to what precisely Delbrück had in mind: "Max gave a tutorial for the Phage Group summer conclave on 'complementarity,' a theme that, as we all knew, had once played a pivotal role in his expectation of running into other laws of physics. Although we were aware that complementarity had something to do with quantum mechanics, few of us (and certainly not I) understood just what exactly Max had in mind when he used that term."[50] This self-professed confusion about how to understand Delbrück's interest in complementarity gives us less reason for confidence in Stent's reading of Delbrück as motivated primarily by a search for other laws.

Stent's tentativeness here in interpreting Delbrück on the very issue of complementarity is made more problematic by the fact that it is surprisingly difficult to find direct textual support for the "other laws" reading of complementarity in Delbrück's own writings. Note well: In Stent's paraphrases of Delbrück's lectures in "Looking for Other Laws of Physics" and "Introduction: Waiting for Paradox," claims about "other laws of physics" are, in every instance but one, Stent's own gloss rather than direct quotations from Delbrück. In the single exception, Stent offers a *reconstruction* of a casual personal conversation he once had with Delbrück about Delbrück's shifting research interests in 1949 (a conversation that apparently took place forty-nine years before the publication of Stent's article):

[49] Max Delbrück, "What Is Life? What Is Truth? *Quarterly Review of Biology,* 1945, *20:*370–372, on pp. 370a, 371b, 371a, 372b.

[50] Fisher and Lipson, *Thinking about Science,* p. 84; James D. Watson, "Growing Up in the Phage Group," in *Phage and the Origins of Molecular Biology,* ed. Cairns *et al.* (cit. n. 4), pp. 239–245, on p. 240 (emphasis added); and Stent, "Looking for Other Laws of Physics" (cit. n. 5), p. 387.

> At one of our laboratory lunches towards the end of 1949, Max let us know indirectly that he was beginning to lose interest in genetics. This was before the first great breakthroughs in molecular biology—the identification of the phage DNA as the genetic material and the discovery of the DNA double helix—had been made.
>
> "It seems to me that phage research is now in good hands," Max announced. "All the fine work that's being done these days is bound to lead to an understanding of biological self-replication before long."
>
> I was astounded. "You mean, Max, that we'll find the solution to the self-replication problem without meeting up with any paradoxes along the way?"
>
> "Yes, I've begun to think so."
>
> "What about those other laws of physics?"
>
> "After phage research has solved the puzzle of self replication," Max answered, "there'd still remain an even harder problem posed by living creatures. I mean the brain, for which reasonable mechanisms can't even be imagined. I bet that some other laws of physics are needed to explain the function of this most mysterious ensemble of atoms in the universe, to explain how mind arose from matter."[51]

Clearly, Stent recollects Delbrück's primary motive for the pursuit of paradox to be the hope of finding other laws of physics. If we could be confident that this reconstructed conversation is not tinted by Stent's own take on the issues, it would count as one piece of evidence in support of his claims about Delbrück.

But it is not difficult to establish that these early proponents of this mistaken view were all influenced by *What Is Life?* As Fisher and Lipson observe: "The influence of Schrödinger's book is discussed in detail in the literature (Olby, 1974; Yoxen, 1979). It is known that four outstanding biologists who read it were attracted to the mysteries of biology as a result: Seymour Benzer, Francis Crick, Gunther Stent, and James Watson. The reasons they offered for the attraction vary from personal to philosophical ones, as they are described in the 1966 Delbrück Festschrift."[52] There is, for example, evidence that the young Stent became captivated by the idea that the study of biology might lead to the discovery of "other laws of physics" before meeting Delbrück, on the basis of his reading of Schrödinger's text:

> As a mere PhD candidate of 22, I was too green to be suffering from anything as blasé as the professional malaise of my elder colleagues. *Yet I was so captivated by the idea that by studying genes I might turn up "other laws of physics"* that I resolved to join the search after completing my doctoral work. Delbrück, the young German physicist, had probably been drafted into the Wehrmacht and been killed during the war. But perhaps there were people in the USA working along these lines. Fortunately, I erred in my conjecture. As it turned out, young Delbrück (now in his forties) was not only still alive, but had just been appointed Professor of Biophysics at Caltech.

Stent also frankly acknowledges that *prior* to applying to work with Delbrück in Pasadena "I had a hidden agenda, namely looking for other laws of physics."[53]

[51] Stent, "Looking for Other Laws of Physics," pp. 390–391. Stent makes a similar point, but provides less context, in "Introduction: Waiting for the Paradox" (cit. n. 5), p. 7.

[52] Fisher and Lipson, *Thinking about Science,* p. 164. Maurice Wilkins also highlights the importance of *What Is Life?* in influencing his transition from physics to biology in his 1962 Nobel acceptance address: Maurice F. Wilkins, "The Molecular Configuration of Nucleic Acids," Nobel Lecture, 11 Dec. 1962, in *Nobel Lectures: Physiology or Medicine, 1942–1962* (Amsterdam: Elsevier, 1964). Francis Crick, Salvador Luria, and Seymour Benzer also acknowledge the excitement stirred by *What Is Life?* as a part of their intellectual development. See Olby, *Path to the Double Helix* (cit. n. 43), Ch. 15.

[53] Stent, "Looking for Other Laws of Physics" (cit. n. 5), p. 372 (emphasis added); see also p. 378, where he

James Watson's view of Delbrück's project was also directly influenced by his reading of Schrödinger. As Stent recalls,"Like me, Jim had been fascinated by Schrödinger's *What is Life?*" Speaking similarly of Watson's initial interest in molecular biology, William Hayes notes that "James Watson, on the other hand, admits that his main incentive was the 'legendary figure' of Max evoked by Schrödinger's book."[54]

V. CONCLUSION

I have challenged the traditional story of the rise of the phage side of the history of molecular biology in six specific ways. First, I have shown that the Bohr-Delbrück view differs importantly from that of Schrödinger. Second, I have indicated that the relation between Bohr and Delbrück is itself more complicated than has often been appreciated. Third, I have explained that Schrödinger misunderstood the Bohr-Delbrück view, taking it to be much closer to his own "other laws of physics" outlook than is actually the case. Fourth, I have demonstrated that subsequent commentators who have told the "story" of the phage side of the history of molecular biology—Stent, Watson, and the historians who followed in their footsteps—allowed Schrödinger's *What Is Life?* to define their reading of Bohr and incorrectly read Schrödinger's views back into Bohr and Delbrück. Fifth, I have argued that Bohr and Delbrück shared an antireductionist outlook which served as an important motivation for the research pursued in this episode of the development of molecular biology. Perhaps in large part because Bohr and Delbrück were unable to communicate the framework to their scientific colleagues effectively, their ideas had little direct influence on other researchers. Indeed, in later years Delbrück himself came to think that the reductionist research program had been more successful than he had anticipated, but he continued to look for a paradoxical situation in which such an approach would need to be supplemented with a teleological perspective in the study of consciousness.

Finally, and most boldly, I have called into question the idea that either Bohr or Delbrück is best understood along the lines of the "other laws of physics" interpretation. We should not see Delbrück as an aspiring physicist who, hoping to make the next big contribution to physics, turned to biology as a repository for heretofore insufficiently scrutinized phenomena. The "teleo-mechanical" reading of these two scientists more aptly characterizes their approach. Whereas Schrödinger did expect a need for new order-from-order principles, Bohr saw himself as providing a philosophical framework for reconciling incompatible features of our experience of living systems. Obviously Bohr and Delbrück would have welcomed any extension of physical concepts and principles that might result from biological research. But, as it is commonly formulated in the literature, the "other laws of physics" interpretation obscures key differences between Schrödinger's view and the perspective adopted by Bohr and Delbrück and misrepresents the core motivations for Delbrück's migration from physics into biology. There may be another way of anticipating other laws of physics, distinct from the approach adopted by Schrödinger. Such a possi-

again notes that his own reason for studying phage was that he was "hoping that it would lead me to the discovery of other laws of physics." Stent clearly (and, as I have argued, incorrectly) took Bohr to be the "original source for the idea of 'other laws of physics.'" Noting that *What Is Life?* drew attention to the "Three-Man Paper," he writes: "This paper remained virtually unknown until Schrödinger drew attention to it a decade later in his *What is Life?* (in which he made no reference to Bohr as the original source of the idea of 'other laws of physics')" (*ibid.,* p. 375).

[54] *Ibid.,* p. 380; and William Hayes, "Max Ludwig Henning Delbrück, September 4, 1906–March 10, 1981," in *Biographical Memoirs* (London: Royal Society, 1982), pp. 66–117, on p. 88.

bility is worth revisiting and might help further to explain colleagues who remembered Delbrück in that way even if there was a tendency to conflate such a view with Schrödinger's. In my alternative reading, the crucial issue for understanding the influence of Bohr on Delbrück was their shared hope that a detailed scientific investigation will vindicate the view that at least some aspects of biological phenomena are *not reducible* to physico-chemical terms. By pursuing a reductionist research program to its limits, Delbrück hoped to find a paradox that would decisively unveil the inherent inability of a mechanistic program to account for all aspects of life and force the scientific community to recognize a legitimate and complementary role for teleological concepts in biology.