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**Physics in Perspective**

# Book Reviews

John Gribbin, *Erwin Schrödinger and the Quantum Revolution*. Hoboken, New Jersey: John Wiley & Sons, Inc., 2013, 321 pages. $27.95 (cloth).

Recent years have witnessed the publication of at least a couple of major biographies of participants in the quantum revolution (David Cassidy's *Beyond Uncertainty: Heisenberg, Quantum Physics, and the Bomb*, and Graham Farmelo's *The Strangest Man: The Hidden Life of Paul Dirac, Mystic of the Atom*); one collective biography (Sheilla Jones's *The Quantum Ten*); and one overview of the history of quantum physics from its inception to today (Jim Baggott's *The Quantum Story*). Whether or not they have read any or all of these books, readers of *Physics in Perspective* might consider paying homage to the centenary of the Bohr atom by reading Gribbin's short and accessible biography of Erwin Schrödinger, including but not limited to the role he played in the quantum revolution. Having read (and reviewed) all of the above books but never a full-scale biography of Schrödinger, I was pleased to find much that I didn't already know in the prolific John Gribbin's portrait of his subject. But first a word about Gribbin's broad overview of the history of physics, painted in very broad strokes, in which he frames this portrait.

Gribbin's compression of the history of science yields some insights that I find convincing but also others that appear forced or questionable. An example of the former appears in Chapter Two, "Physics before Schrödinger," in which Gribbin argues that although Galileo is usually credited with being the first to develop the scientific method, that claim might more properly be made about William Gilbert, who was twenty years Galileo's senior. In *De Magnete*, which we know Galileo read "from approving comments he made about Gilbert's book in a letter," Gilbert, in carefully laying out the "foundations of magnetic science," also describes the methodology of science in general: "In the discovery of hidden things and in the investigation of hidden causes, stronger reasons are obtained from sure experiments and demonstrated arguments than from probable conjectures and the opinions of philosophical speculators of the common sort." I was also struck by Gribbin's comparison of Schrödinger's "astonishing creative outburst" in 1926, in which he published the six papers that essentially completed wave mechanics, to Einstein's output in his *annus mirabilis* of 1905. Gribbin is careful to note here that Einstein's effort "arguably surpassed" Schrödinger's achievement simply because of the breadth of the subjects his five papers covered. An example of a claim that Gribbin makes that seems exaggerated, however, comes in Chapter Fourteen, "Schrödinger's Scientific Legacy," in which he asserts that "arguably the most significant development in science in the twentieth century" was "the resolution of the EPR 'Paradox' and experimental confirmation that quantum entanglement… is real." Surely genetic engineering and biotechnology, space exploration, and the development of the Internet are among many other developments of the last century that are contenders to that claim. In my opinion, by overstating the importance of the work of John Bell and Alain Aspect on EPR, Gribbin ends up undercutting their actual importance.

More distressing than a few exaggerated claims Gribbin makes in his history of physics overview, however, is an actual error that Gribbin makes in Chapter Four, "The First Quantum Revolution." That this error appears far too widely in historical summaries of science both on the Internet and in book popularizations is no excuse for Gribbin's not having been more careful. Gribbin asserts that Max Planck, in his famous 1900 treatment of blackbody radiation, proposed that light was quantized, which he did not; Planck claimed only that the energy of the oscillators in the walls of the cavity that gave rise to the blackbody radiation was quantized, not the electromagnetic radiation itself. Nonetheless, Gribbin states that in October 1900 Planck "realized that what he needed was [Ludwig] Boltzmann's statistical interpretation applied not just to the electromagnetic oscillators, but to energy—to the electromagnetic radiation itself." As Gribbin pushes the assertion, he claims that Planck went on to treat "the electromagnetic radiation as made up of little pieces of energy instead of a smooth wave," and that when "the equation he had already discovered empirically… fell out of the calculation before… the calculation had been completed," he was convinced "that what the equations were telling him was that the packages of electromagnetic radiation could only be emitted or absorbed in [discrete] amounts… but that the radiation itself was a classical wave." While Gribbin, thus, does conclude his discussion with a statement of Planck's belief that electromagnetic radiation is continuous, his treatment of the topic muddies the waters.

Before leaving behind Gribbin's historical survey for a closer look at his treatment of Schrödinger and colleagues, let me point to an interesting contrast I found embedded in his overview of the history of physics, though it is one he doesn't make specifically: between groundbreaking work of genius that is of little practical value, on the one hand, and, on the other hand, derivative popularizations that have widespread influence. In Chapter Seven, "The Second Quantum Revolution," we learn about the brilliant contribution of "the boy wonder, Paul Dirac" at the very end of 1926. While his transformation theory "proved (using some hairy mathematics) that all versions of quantum mechanics were contained within [it]," and while transformation theory "is the complete theory of quantum mechanics," it was of little use to "ordinary physicists in the 1920s (and few since, for that matter)," since "they didn't like the hairy mathematics." By contrast, in Chapter Twelve, "What Is Life?"—which is also the title of Schrödinger's 1944 book—we learn that the book in question is a popularization that contains little of value that was original to Schrödinger. This assessment, however, as Gribbin notes, "misses the point that you do not have to be original to be influential, and influential the book certainly was." One could argue that the main influence of the book was to intensify the brain drain from physics to the life sciences in the aftermath of World War II, when many physicists were appalled by the destruction wreaked on Japan by the two atomic bombs on which so many had worked so intently. But there is no denying that Francis Crick, part of that brain drain, who went on with James Watson to unlock the secret of the structure of DNA with their double-helix model, was first inspired to switch fields from physics to biology by reading Schrödinger's book. Though Crick later "came to see its limitations—like many physicists, [Schrödinger] knew nothing of chemistry," *What Is Life?* played a seminal role in Crick's intellectual development by suggesting that "great things were just around the corner." For his part, Watson said unequivocally in a 1984 lecture not only that "It was clear in those days that physicists were brighter than biologists" but also that "From the moment I read Schrödinger's *What Is Life?* I became polarised towards finding out the secret of the gene."

Turning now to some of the things Gribbin is likely to be able to teach even those readers fairly well versed in the history of the development of quantum physics, I was surprised to learn how much the achievements of their students and colleagues rankled the great contributors to the quantum revolution. (Of course, in the context of Crick and Watson, perhaps I should have been less surprised by the competitive and disparaging remarks the quantum scientists traded about one another. Watson and Crick, after all, personified the competition for scientific glory in their race to unlock the code of life, gleefully accepting unauthorized leaks from Linus Pauling's son of an erroneous model on which his father was working, and feeling no qualms about obtaining "some of [Rosalind] Franklin's key data" by hook or by crook, and using it as the basis for their double-helix model.)

The topic of professional resentment among the quantum physicists comes up first in Chapter Six, "Matrix Mechanics," as part of a discussion of the sometimes unjust awarding of Nobel Prizes, and then becomes a recurrent, if sporadic, theme through much of the book. Fairly early in the chapter we learn that while Heisenberg worked out quantum mechanics on his own in 1925, his mentor at the University of Göttingen, Max Born, whose training through his doctorate had been in mathematics, was the one to recognize Heisenberg's "tables of numbers as examples of a kind of mathematical entity known to mathematicians (but to very few physicists in 1925) as matrices." In fact, according to Gribbin, Born "was one of the few physicists of the time already familiar with matrices," having been introduced to them while still an undergraduate at Breslau, where his father was a professor of anatomy.

Fast-forward to the next decade, when the Nobel committee, bound by the "rule that a single Nobel Prize cannot be shared by more than three people," had to figure out how to recognize the contribution of "all the participants in the second quantum revolution." The awkward solution—to award the 1932 prize to Heisenberg alone—overlooked the contributions of Born and of his junior colleague Pascual Jordan, who together had furthered the development of quantum theory "using the language of matrices, establishing what became known as matrix mechanics." Complicating the matter even further, the Nobel committee also decided "to hold the 1932 physics prize over until 1933, and then to award the 1932 prize to Heisenberg, and the 1933 prize jointly to Schrödinger and Dirac, so that they could all be honoured together at the same ceremony."

Gribbin offers an intriguing explanation of why the committee acted in this way and how the humiliation affected Born for the rest of his life. He suggests that the committee ruled out Jordan's participation in the 1932 prize since he was among the German scientists to openly align themselves with the Nazi Party, but felt that the omission of Jordan necessarily involved the omission of Born also, "since it would be impossible to disentangle their joint work and award a prize to either of them alone." Although Heisenberg tried to soften the blow to Born by writing him a letter "expressing his bad feelings at receiving the prize on his own for 'work done in Göttingen in collaboration—you, Jordan and I,'" Born wrote Einstein two decades later that the decision to omit him from the 1932 award deeply hurt him at the time, "in spite of a kind letter from Heisenberg." After all, he told Einstein, even though Heisenberg "actually had no idea what a matrix was" until Born enlightened him, "It was he who reaped all the rewards of our work together, such as the Nobel Prize." Even though Born's second significant contribution to quantum physics—his probabilistic interpretation of quantum mechanics—was recognized by the Nobel committee in 1954, when he shared the physics award with Walter Bothe, he continued to believe that his original contributions to the field never received the credit due them. Born remained bitter that quantum physics in the public eye was associated mainly with Bohr and his institute in Copenhagen. In fact, the interpretation of quantum theory that includes the complementarity principle—that both wave and particle behavior must be considered to understand matter and radiation—and the uncertainty principle—that certain pairs of quantities, such as the position and velocity of a particle, cannot be measured with exactness at the same time—were yoked together under the term "Copenhagen Interpretation." Born complained to Einstein that the "Copenhagen school… today lends its name almost everywhere to the line of thinking I originated."

We later learn that Schrödinger himself was also prone to inveigh against the influence of Bohr and the Copenhagen Interpretation. In Chapter Thirteen, "Back to Vienna," we learn of a letter Schrödinger wrote near the end of his life (he died on January 4, 1961) to Irish mathematician and physicist John Synge, complaining about those of their colleagues who unthinkingly accepted the Copenhagen Interpretation: "With a very few exceptions (such as Einstein and Laue) all the rest of the theoretical physicists were unadulterated asses and I was the only sane person left." In particular he groused about the tendency of those colleagues to dismiss his reservations because of their conviction that "I am—naturally enough—in love with 'my' great success in life [and] therefore, so they say, I insist upon the view that 'all is waves,'" a belief these colleagues attribute to Schrödinger's "old age dotage [that] closes my eyes towards the marvelous discovery of 'complementarity.' So unable is the good average theoretical physicist to believe that any sound person could refuse to accept the Kopenhagen oracle [Niels Bohr]." Schrödinger draws the line, however, at imputing bad motives to Bohr: "If I were not thoroughly convinced that the man is honest and really believes in the relevance of his [idea] I should call it intellectually wicked."

Schrödinger also wrote a letter to Born, in which he (surprisingly, given Born's own grousing about Bohr) complains about "The impudence with which you assert time and again that the Copenhagen Interpretation is practically universally accepted, assert it without reservations, even before an audience of the laity—who are completely at your mercy—it's at the limit of the estimable…. Are you so convinced that the human race will succumb before long to your own folly?"

Immediately after quoting from Schrödinger's letter to Synge, Gribbin reveals his own hand, thus shedding light on why the theme of resentment of some of his colleagues toward Bohr is an undercurrent in this book: "As someone who was taught the gospel of the Copenhagen Interpretation just a few years later, and only much later still appreciated the nature of its folly, I find these words from Schrödinger in 1960 strike close to my heart!" In a section of Chapter Fourteen called "Quantum physics and reality," Gribbin takes the opportunity to explain his own dissatisfaction with the School of Bohr: "the Copenhagen Interpretation taken to its logical extreme [suggests that] the Universe only exists because we are here to observe it. But… what such a notion really indicates is the absurdity of the Copenhagen Interpretation." As the author of a short book-length biography of Bohr for teenagers, where he is my hero, I have to admit being surprised by the vehemence of the reactions to the Copenhagen Interpretation to which Gribbin has introduced me.

I am happiest writing biographies of people, whether scientists or writers or political figures, in whose lives and work I can find much to admire. Perhaps my greatest disappointment in Gribbin's biography is that I do not find Schrödinger a particularly admirable man. I am puzzled by one of the two epigraphs with which Gribbin introduces the book, an assessment of Schrödinger from Max Born's 1978 memoir, *My Life*: "His private life seemed strange to bourgeois people like ourselves. But all this does not matter. He was a most lovable person, independent, amusing, temperamental, kind and generous, and he had a most perfect and efficient brain." This characterization does not line up with my takeaway from Gribbin's treatment. Among the things about Schrödinger that I hadn't known before reading Gribbin's book are the extent of his womanizing and, even more disturbingly, his temporizing with the Nazi regime. With regard to the first, I wasn't bothered by the discovery that among the records he kept in his diaries were the names of all his lovers, "albeit in code"; I already knew that centuries earlier Robert Hooke also kept a diary in which he used the astrological symbol for the zodiacal constellation Pisces as his private code to designate a sexual encounter. But I was disturbed to learn of Schrödinger's attraction to young girls. It is one thing for a man to have affairs with adult women, particularly if his wife, involved in a long-term affair of her own, seems to welcome her spouse's dalliances, and if at the time, as Gribbin asserts, "liaisons between married members of the academic set and others who were not their spouses were regarded as normal, and nothing to make much fuss about." It is a different thing entirely for the man, however, to target girls for "grooming" until they reach the age of maturity, and then to pounce. Gribbin argues that "this aspect of Schrödinger's private life cannot be ignored, even in a scientific biography" because "when he was in love, by and large life was good and his scientific creativity benefited." He refers to a remark made to physicist-historian Abraham Pais by Hermann Weyl, the illustrious mathematical physicist as well as the long-term lover of Schrödinger's wife, Anny, that attributed Schrödinger's success in untangling wave mechanics at the relatively old age for a physicist of 38 to "a late erotic outburst in his life." One does not have to be a prude to resist justifying much of what is just plain sordid behavior on the grounds that it benefited science.

But previously knowing only that Schrödinger had left behind the hotbed of Nazism in Austria (his native land) and Germany (where he taught at the University of Berlin from 1927 until his resignation in 1933 because of his opposition to Nazism) to spend the World War II years in Ireland, a country that remained neutral during the war, I was completely shocked to learn the extent to which Schrödinger had curried favor with the Nazis. In Chapter Ten, "There, and Back Again," we read a letter published in both German and Austrian newspapers on March 30, 1938, two weeks after the *Anschluss*, the political union between Nazi Germany and Austria, in which

Schrödinger makes "willingly and joyfully" what he calls a public "repentant confession," apologizing for his having "misjudged up to the last the true will and the true destiny of my country… and I hope thereby to serve my homeland." Schrödinger followed up on this public kowtowing to the Nazis with two less public but no less reprehensible acts: he broke off, at least for the moment, with one of his long-time lovers because of her Jewish roots, asking her to burn the love letters he had sent her, lest they taint him if discovered, and also implied to a colleague from Oxford whom he bumped into while on vacation in the Tyrol that he "hoped for promotion to an important professorship in Vienna, made vacant by the dismissal of a Jewish incumbent." As it turned out, Schrödinger was fired in summer 1938 from his professorship at the University of Graz, which had been linked with an honorary but salaried professorship at the University of Vienna. Thanks to the interest of Irish prime minister Éamon de Valera in establishing Dublin as a center of theoretical physics, Schrödinger served as the first Professor in the School of Theoretical Physics at the Dublin Institute for Advanced Studies from its opening in October 1940 until 1956, when he returned to the University of Vienna to a professorial position specifically created for him.

While I do not admire everything about Gribbin's biography of Schrödinger, I am glad I read it, and I do recommend it to readers of *Physics in Perspective*. Although I regret the absence of a chronology of Schrödinger's life at the end of the book, I am particularly taken by Gribbin's *Postscript*, which fills the final six pages. It contains a surprise that I don't want to spoil for others who may pick up the book, but I will advise such readers to pay close attention to Gribbin's dedication; to his remark in Chapter Seven that he will eventually provide another reason why a biographer of Schrödinger shouldn't ignore his love affairs; and to his description in Chapter Eleven, "The Happiest Years of My Life," of the love affair that "is the hardest to justify on the grounds of 'true love.'"

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Phillip Schewe, *The Pioneering Odyssey of Freeman Dyson, Maverick Genius.* New York: Thomas Dunn Books, 2013, x + 339 pages. $27.99 (cloth).

Schewe has written a massive biography of Freeman Dyson, apparently without the collaboration of Dyson himself. Under those circumstances I don't know how he can be so sure of Dyson's state of mind, which he tells us about at many points in his life. But let us not engage in minor quibbles. This is a very fine book.

Dyson was born in 1923. The book begins in Winchester College, one of England's oldest and most distinguished prep schools, where he excelled at mathematics, and then went on to Cambridge University where he continued his mathematical education. During the Second World War Dyson was in England, convinced he was going to die in the conflict. As things turned out, he wound up during the war at Bomber Command Headquarters in the town of High Wycombe halfway between London and Oxford, as an air force theorist, where he concluded among other things that the British Lancaster bombers were having too few mid-air collisions. Flying in closer formation, their mid-airs went up, but their loses to German aircraft went down much more. He didn't die.

Instead, in 1947 he came to America and switched his allegiance to physics at Cornell University under the tutelage of Hans Bethe and then after a cross-country jaunt with Richard Feynman, he arrived at the Institute for Advanced Studies at Princeton under J. Robert Oppenheimer, where he would spend most of the remainder of his career.

At about this time he managed to reconcile the quantum-electrodynamics theories of Shin-Ichiro Tomonaga and Julian Schwinger with that of Feynman, an accomplishment that had Nobel Prize written all over it. More about that later. Hans Bethe in a letter of recommendation called Dyson the best English theoretical physicist since Paul Dirac. He went on to work in quantum physics, national defense and arms control, weapons and nuclear reactors, biology, astronomy, and pure mathematics. Dyson also won the Templeton Prize for his efforts to reconcile science and religion. He was and is truly a renaissance man.

Dyson is mildly contrarian as in his belief in the possibility of extrasensory perception, his disbelief in global warming, and, although not religious, his belief that there is something to the idea of a transcendent God. In the course of his life, he has spent a great deal of time involved in the Orion project and with Jason. The idea of Orion was to propel a space ship by means of a series of nuclear explosions. Perhaps fortunately, it has never been attempted. Jason is a summer think tank that takes place in La Jolla, a beach resort near San Diego that offers classified advice to the military. He continues in Jason to this day as a senior advisor.

Driving across the country together after the war, Dyson and Richard Feynman became good friends. My only encounter with Dyson came at a symposium in San Francisco in honor of Feynman shortly after his death. Both of us were speakers at the symposium. However, I was so keyed up for my own talk that I don't remember at all what Dyson said on that occasion.

Dyson never did win the Nobel Prize. The reason is that his most important accomplishment, reconciling Tomonaga-Schwinger with Feynman won the Prize for the other three, but once they were named there was no room for Dyson (Nobel Prizes can be shared a maximum of three ways). Many people have said that Dyson deserved a Prize of his own, but he never got one.

Nor did he ever manage to get a PhD. He was extolled as the best mathematician in England when he was in college, but the war directed him elsewhere and although he ended up with many honorary degrees, including doctorates, he never did get the real thing.

Schewe seems to love sprinkling various literary and cultural references in his book. He compares Dyson to the Beatles (they both had big hits early on) and later on the same page (p. 80) to Jack Kerouac. A few pages on he compares Dyson's first wife, Verena Huber Dyson, to Norwegian playwright Henryk Ibsen's Hedda Gabler. But we can forgive him his little hobbyhorses. Schewe has written a big, thorough biography of Freeman Dyson. For that he deserves our thanks.

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Lee Smolin, *Time Reborn: From the Crisis in Physics to the Future of the Universe*. New York: Houghton Mifflin Harcourt, 2013, xxi + 319 pages. $28.00 (cloth).

To maximize its readership, a popular book about physics should emulate a planetarium lecture. Imagine the scene: In the darkness of the auditorium the lecturer can't tell who's asleep and who's paying attention. She doesn't even know who's out there—the audience probably includes a sprinkling of children, wide-eyed and clueless, as well as an occasional professor of physics. The lecture should therefore strive for a sawtooth structure—starting at the most elementary level and rising slowly in sophistication to a sharp point, a rare tidbit that is way beyond the comprehension of the children, yet news to the expert. Then the level must drop quickly back to basics. An experienced planetarium lecturer adjusts the amplitude and frequency of this pattern in such a way that even the most diverse audience feels that it has received its money's worth.

Lee Smolin, of the Perimeter Institute for Theoretical Physics in Waterloo, Ontario, has mastered this rhythm. If his readers keep it in mind, they will patiently skim passages like definitions of a parabola and of acceleration on the way to such neological nuggets as *the principle of precedence* and, shudder, *quantum graphity*. But in spite of the leavening provided by its sawtooth modulation, and the total absence of formulas, the book is not an easy read for anyone.

The title effectively telegraphs the content. A new understanding of the concept of time, Smolin insists, is a prerequisite for the solution of what he perceives as the current *crisis in physics.*

This, in turn, includes the unsolved problem of bringing physical cosmology under the umbrella of an overarching fundamental physical theory—a problem he calls *the cosmological challenge.* It is an ambitious project. Of course, not everyone agrees that physics or cosmology are in crisis. In fact, recent successes, ranging from the triumphant detection of the Higgs particle to the astonishing discovery that the expansion of the universe is accelerating, suggest that physical science is thriving. The *crisis* in the title is confined to the subdiscipline of fundamental theoretical physics, a field closer to philosophy than to experimentation. I mention this distinction not to diminish, but to clarify Smolin's claim. Attention must be paid to the foundations of the house of physics, to be sure, but they're not the whole story.

The principal message of the book is that TIME IS REAL. I wasn't able to discover what Smolin means by this phrase, except in terms of a double negative: Time is not an illusion. Like so many fruitful ideas in physics, that way of putting it harkens back to Einstein, who proclaimed its opposite: "the distinction between past, present and future is only a stubbornly insistent illusion." (p. 88) Smolin admits that he too used to believe in the essential unreality of time. Furthermore, he reports that many of his "nonscientific friends" feel that the passage of time is deceptive, (p. xi) a claim that I find hard to swallow. On the contrary, I think that most people believe that time, impossible as it is to define, is part of our experience of the real world. (My humanist wife, who has her feet firmly planted in the here and now, is of that opinion.) But be that as it may, Smolin defends the proposition that time is real with the zeal of a convert.

In order to defend it, he must first prove that it is under siege. The first part of his book is a persuasive compilation of nine arguments for the "expulsion of time from the physicists' conception of nature." (p. 93) (Note the shift from nonscientific friends to physicists.) The principal culprit, which Smolin calls the Newtonian paradigm, is the stuff of high school physics. The description of a system is divided into a set of initial conditions and a law of motion. From these the history of the system can be computed mathematically and represented by a trajectory, or graph, or worldline. Neither the initial conditions, which obtain at an instant, nor the law, which is timeless, nor the graph, which is a static mathematical object like a parabola, are subject to change. Motion has been frozen. Time has been eliminated.

Special relativity complicates the picture by subjecting the worldlines to the relativity of simultaneity, and general relativity bends them, but the end result is Einstein's cold, immutable block universe. There is no room for the universal human experience of the unique NOW, a failure of physics that Einstein lamented with resignation. (p. 91)

Smolin blames the elimination of time for three related fundamental problems: the persistent difficulty of finding a satisfactory interpretation of quantum mechanics, the failure of the fifty-year search for a unique quantum theory of gravity, and the challenge of formulating a coherent explanation of the properties of the universe, because we know neither its initial conditions nor its governing laws. The second part of his book—devoted to re-introducing time into physics as an essential aspect of reality—is admittedly much more tentative and speculative than the first. Quantum theory, relativity, and precision cosmology have been spectacularly successful, so any theory that ventures beyond them must be careful to preserve their achievements. Smolin doesn't claim to have resolved the perceived crisis, attempting only to point the way out by describing recent concrete proposals by himself and others.

His clearest and simplest example concerns cosmology. The crucial hypothesis is the unconventional assumption that the fundamental laws themselves are not timeless, but evolve in real time. Many of the great physicists of the twentieth century have toyed with this idea, but Smolin traces it even further back. He quotes the American pragmatist philosopher Charles Sanders Peirce (usually pronounced *purse*), who wrote in 1891: "the only possible way of accounting for the laws of nature and for the uniformity in general is to suppose them the result of evolution." (pp. xxv and 119) The explanation for the way nature is arranged would thereby reside in its historical development, rather than in timeless, general laws. (Could this be a hint that physics, the characteristic science of the twentieth century, will yield its preeminent position to biology in the twenty-first, and for biology envy to replace physics envy?) Of course this suggestion demands that a mechanism for the evolution of physical laws must be identified, and Smolin obliges with his own theory of *cosmological natural selection*. (It is amusing to note that in order to make it work, he has to introduce "small random changes" at unpredictable times. For similar reasons Lucretius needed his mysterious *swerve*, Darwin his then-unexplained *mutations*, and certain modern interpreters of quantum theory the very rare and unexplained *random collapses of wavefunctions*.)

A much more radical speculation is the *principle of precedence*, which Smolin proposed last year as a possible resolution of the quandaries of quantum mechanics. It too has its precursor in the work of Peirce. Stated crudely, it posits that repeated measurements tend to yield the same outcome—not because they are governed by a timeless law, but because nature, like people, forms habits. In this way the future will resemble the past, without being strictly determined. Furthermore, unprecedented measurements will not be ruled by any prior law. Smolin has high hopes for this notion. "I'm willing to bet that the principle of precedence will generate new ideas for experiments," (p. 152) he writes, and "it is a hypothesis capable of inspiring new ideas and driving a robust research program." (p. 153) We shall see.

Two thirds of the way through the book I was pulled up short. The chapter entitled The Emergence of Space begins with the refrain "I believe that time is real," but continues "I think it likely that space will turn out to be an illusion.…" In that case the underlying reality of the world would include, besides the passage of time, a quantum-mechanical structure of causal relationships, from which space would emerge the way thermodynamics emerges from the mechanics of individual atoms. The aforementioned *quantum graphity* is one such model. This is heady stuff, but with neither biology, nor everyday intuition, nor experimental evidence, nor technical expertise to encourage me, I found it impossible to develop a taste for it.

But the overall thrust of the book has a lot of common appeal. In his summing up Smolin advocates moving beyond the Newtonian paradigm, and even beyond the strict reliance on mathematics, which has served us so well since Galileo. "Logic and mathematics capture aspects of nature, but never the whole of nature." (p. 248) One of those ineffable aspects is, as Einstein recognized, that in the real world it is always some particular moment. For that reason nature cannot be fully described by a single logical or mathematical system—a theory of everything. But if mathematics won't do it, what will? Smolin answers: "Mathematics is a great tool, but the ultimate governing language of science is language." (p. 247) The poet Muriel Rukeyser put it more memorably: "The universe is made of stories, not of atoms." The best example of this worldview is provided by evolution which, though shored up in detail by mathematical arguments, ultimately amounts to a grandiose epic.

A new philosophy of physics, according to Smolin, should "replace the false hope of transcendence to a timeless, absolute perfection with a genuinely hopeful view of an ever expanding realm for human agency, within a cosmos with an open future." (p. 257) When the problems of our polluted, overpopulated, brutal world threaten to throw doubt on the beneficial value of science and technology, Smolin's holistic, humanistic, and profoundly optimistic vision of the future provides a welcome antidote. Even as we labor in the orchard of science, picking fruit from trees planted by previous generations, it is good to look up from time to time, to survey the landscape, to contemplate the big picture, and to wonder what comes next. Some of Smolin's ideas are deep, some are over the top, and all are debatable. In short, he makes you think. What greater compliment is there for a writer, a teacher, or a planetarium lecturer?

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Kostas Gavroglu and Ana Simões, *Neither Physics nor Chemistry: A History of Quantum Chemistry*. Cambridge, Mass.: The MIT Press, 2012, xiv + 351 pages. $40.00 (cloth).

The atomic theory of matter carries with it the idea of the *chemical bond*, a concept that plays a central role in quantifying the stability and geometry of molecules. The history of our perception of the chemical bond is essentially a history of the language of chemistry. Heitler and London showed (1927) that a quantum-mechanical description of the electrons in the $H_2$ molecule could lead to an understanding of the bonding between two hydrogen atoms. Hückel showed (1930) that a quantum-mechanical image of the electrons in aromatic molecules could give an understanding of the difference in reactivity of different sites in these molecules. These observations were the first steps in the creation of a new discipline, *quantum chemistry*, an intertwining of physics (the quantum-mechanical description of electrons in molecules) and chemistry (the formation of chemical bonds). *Neither Physics nor Chemistry* (*NPNC*) gives two historians' perspective of how "quantum chemistry" emerged as a subdiscipline.

At the beginning of the book's introduction, C.A. Coulson is quoted as stating that "one of the primary tasks of the chemists during the initial stage in the development of quantum chemistry was to escape from the thought forms of the physicists." It is against this image that the authors title their book and tell their history. While the chronology and attribution of the jargon of quantum chemistry are carefully documented, there is no acknowledgement that the developers of quantum chemistry (ill-defined as this discipline remains throughout the book) were, like other scientists in other areas, motivated by the solution of specific problems. Quantum chemistry did not emerge by escaping the thought forms of physicists. It emerged from a struggle to develop a quantum-mechanical understanding of chemical questions. As the authors seem more interested in the sociology of the emergence of quantum chemistry as a discipline than they are in these chemical questions, *NPNC* offers no sense of the animating spirit of modern quantum chemistry. It is not clear what the unique aspects of the physics-chemistry weave are that underlie quantum chemistry.

Bohr, in noting that the quantum-mechanical description of reality was at odds with typical human experience, always emphasized that quantum mechanics was a human construction. It was constructed, with constant correction by experimental facts and mental images molded by these facts. *NPNC* repeats the often half-quoted remark by Dirac that, while all of chemistry could, in principle, be understood with the new quantum mechanics, the "exact application of these laws leads to equations much too complicated to be soluble. It therefore becomes desirable that approximate practical methods of applying quantum mechanics be developed...." The success of quantum chemistry has been the result of the development of "approximate practical methods" constructed in the spirit of Bohr's perspective. This was foreseen by two early giants of quantum theory. Both Heisenberg and Wigner doubted that quantum mechanics would be able to deduce chemical results without prior chemical knowledge. The vast majority of science is the art of wise approximation. Observation of the world generates experimental knowledge that inspires approximation schemes. Unwise approximation schemes are abandoned, wise ones are doubled back upon to justify on more rigorous grounds. This is how physical sciences have typically evolved. After reading *NPNC*, one will not know if quantum chemistry has anything unique to offer on this recurring theme. While *NPN*C characterizes at great length the conflict between those early workers who stuck close to exactness and those who were willing to use chemical insight to make empirical approximations that, at the time, were not defensible *ab initio*, it fails to appreciate the significance of the chemical insight that shaped the formation of quantum chemistry.

Instructors in chemistry commonly introduce topics in a "historical context." Not because the history is particularly pertinent, but because the chemist's worldview is not a literal picture of reality. Telling stories about a new view is often a useful way to help internalize the new perspective. To this end, it is the vividness of the story rather than the historical accuracy that is important. But the professional historians' recounting of the development of quantum chemistry in *NPNC* does not actually clarify the new mental images of quantum chemistry. (The authors claim that the mathematical symbols of quantum mechanics present an obstacle to understanding quantum mechanics and, to avoid this embarrassment, they describe the ideas introduced in quantum chemistry in a simplified English. Neither the diagrams nor the mathematical symbols that quantum chemists use to communicate with each other appear in *NPNC*.) While many topics *are* discussed, only a reader familiar with practical calculations will be able to identify what the various discussions are actually about.

*NCNP* does note the importance of the use of digital computers in quantum chemistry, both leading to the implementation of ideas and to the formation of new ideas. But, the role of semiempirical quantum-mechanical software is not noted. In replacing quantum-mechanical computational details with empirical data, these programs speak to the chemist's image of chemical bonding while, at the same time, providing data that are closely connected to the laboratory experience (s)he is trying to emulate. The contribution of density-functional methods (DFT) to the production of results of "chemical accuracy" is similarly neglected.

The value of an approach is the set of questions that yield to it. *NPNC* leaves the reader with little insight into either the historical successes of approaches (for example, *Valence Bond* and *Molecular Orbital* models) or the role these and other models (for example, DFT) play today. These absences give a text that has little educational value for students of quantum chemistry.

Today, even high school chemistry texts dance around "bonding orbitals." That this revision of the instruction of chemistry reflects Pauling's lucid connection between *orbitals* and G.N. Lewis's preexisting explanation of bonding in terms of electron pairs is described quite well in *NPNC*. But the emphasis on picturing orbital wave functions is really a reflection of the development of the Woodward-Hoffmann rules that explain *reaction paths*. This is a vital force in modern quantum chemistry and is not described in *NPNC*.

Pauling introduced the term *resonance*—Hückel would have preferred the label *mesomerism*. *NPNC* describes the historical controversy associated with this term—does it refer to an observable physical property of a system? The controversy epitomizes the difference between a physicist's and a chemist's view of quantum chemistry. The controversy was and remains a real historical rift between the physicist's and chemist's view of quantum chemistry. But the authors of *NPNC* focus on the minutia of the conflicts between the first group of scientists to grapple with the quantum interpretation of chemical bonding. In the presentation offered in *NCNP* distinctions between substance and style are occasionally blurred. For example, the reader will not be able to distinguish Pauling's scientific successes from his personality.

Any approach to quantum chemistry can only be evaluated by examining its ability to rationalize reality and provide predictive power. The main problem with *NPNC* is that no measures of these attributes are provided.

However, in addition to its 22 pages of notes and a 47-page bibliography (true treasure troves for chemists and physicists interested in the early history of quantum chemistry), *NPNC* has an additional considerable attribute. It will stimulate among others, as it has among us, a discussion of the question, "Were Heisenberg and Wigner right?"

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