trace elements. Using animal wastes is really sustainable, even if it means using more land per unit of output (Pollan, 2006).

UGW may be the best overall 21st century book on climatology for the educated general public. You should consider having a copy as an antidote to the prevailing unscientific dogma on this subject.

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The End of the Certain World: The Life and Science of Max Born by Nancy Thorndike Greenspan. Basic Books, 2005. 374 pp. $26.95 (hardcover). ISBN 0-7382-0693-8.

Here I shall review the book about Max Born by Nancy Greenspan. In addition, I shall give some deeper comment about probability in quantum mechanics. I have thought off and on for over 30 years about the matter. So have many other scientists. To be overly brief and blunt, I am currently of the opinion that Born's great contribution to quantum physics, that the evolving wave function $\psi(t)$ of a Schrodinger's equation $\psi(t) = H\psi(t)$ may be interpreted probabilistically, is more in the way of an artifice and not a reality. Schrodinger himself thought so. On the other hand, that $\|\psi(t)\|_2^2 = 1$ gives us the Born probability interpretation has been immensely useful, is irrevocably embedded into the quantum theory in countless books and treatises, and is accepted as equivalent to reality by many practicing and theoretical physicists ... so I don't want to be irreverent about it. I just think that we should understand the physical causative mechanisms that may underlie it better. So I will mention some of my thoughts and recent results of my own toward that end.

Generally, quantum theory still contains so many riddles and so much counter-intuitiveness, that I want to state the following right here at the outset: I refuse to be drawn into lots of future arguments with any readers of this review. I don't think my own understandings of quantum mechanics are in any way final. Einstein complained bitterly that he worked 200 times as hard on quantum mechanics as on relativity but still did not understand it.

Let us turn first to Greenspan's book. By chance I also had an old copy of The Born–Einstein Letters[1], from which Greenspan often quotes. She abbreviates that book as LTRS, and let us use that notation here. Let us also denote the book under review as MBG.

As Greenspan relates, she came to the idea of writing MBG from conversations with Born's daughter Irene, whose later married name became Newton-John. So the famous British-Australian singer-actress Olivia Newton-John (e.g., Grease, with John Travolta) is actually Max Born's granddaughter.

MBG carries a flavor as a book written by one rather close to the Born family. Indeed, there is much in the book also about Max Born's wife Hedwig. Apparently Born had to exercise great patience in dealing with his wife 'Hedi.' Of course, Born was also under great pressure from the Nazis in Germany, and eventually he moved to Great Britain, first to Cambridge in 1933, then to Edinburgh in 1936. Born refused to return to Germany in 1945 to take Arnold Sommerfeld's chair in Munich; also, he at that time refused an offer to return to Gottingen. However, Born retired to Germany in 1953, where he lived until his death in 1970.

Greenspan reports that on Born's gravestone "is carved $pq - qp = h/2\pi i$, ... first written down by Born in July 1925." Of course, this is nowadays usually called (one of the forms of) Heisenberg's uncertainty principle. A good account as to how this principal evolved is given in pages 120–128 of MBG. Heisenberg, Born, Bohr, Kramers, Slater, Pauli, and Jordan were all talking to each other frequently. One could, in retrospect, call it a team effort, out of which the old quantum mechanics was transformed into a new one, in which one thought in terms of oscillators and discontinuous jumps in energy levels: particles, rather than waves. Heisenberg wrote a first draft with multiplicative rules for transition amplitudes and gave it to Born. Born recast it into matrix language. He found (1925) that the Heisenberg matrix products difference was $pq - qp = h/2\pi i$. To quote MBG (p. 126), Born was proud that he was "the first person to write a physical law in terms of non-commuting symbols."

However, he could not prove that the nondiagonal terms were zero. Pascual Jordan completed that part. The three of them, Born, Heisenberg, and Jordan, then wrote the paper together, which appeared in Zeitschrift fur Physik in 1926.

Here I would like to interject three comments. First, many books imply that the uncertainty relation can be used to prove the stability of matter. That is not the case; see, e.g., the treatment in my book.[2] Second, the operator-theoretic mathematics, which is much more general than the quantum physics matrix mechanics for transition rules, is nicely exposited in the little Putnam book[3]. Third, although the operator-theoretic versions of the physical uncertainty principle are of course important (and there are three of those, usually called the Weyl, Schrodinger, or Heisenberg forms; see, e.g., our paper[4] or the Putnam book[3]), when many years ago I first encountered the uncertainty relation, I wanted a really simple proof of its mathematical essence. Here it is. Momentum p is the operator of differentiation: $pu(x) = du(x)/dx$. Position q is the operator of multiplication: $qu(x) = xu(x)$. All we need is differentiation by parts from calculus, usually written $d(uv) =$ udv + vdu. Then

$$\frac{d}{dx}(xu[x]) = x\frac{du}{dx} + u,$$

which is pqu = qpu + u, which is pq − qp = Identity operator.

Returning to our review of MBG, Born also is remembered for several other contributions to quantum physics. One of them is the Born-Oppenheimer approximation, useful in quantum scattering theory, which in essence determines the shape of a molecule. However, I want to focus on Born's probabilistic interpretation of the Schrodinger equation wave function. As is well known, the Schrodinger-de Broglie wave mechanics is essentially equivalent to the Heisenberg-Born matrix mechanics. The history of Schrodinger's derivation of his partial differential equations of atomic systems is interesting in itself and will not be discussed here. But by 1926 there were the two rival camps, Gottingen and Zurich. Pauli stepped in from the latter and with Schrodinger showed the (unitary) equivalence of these two formulations of quantum dynamics. Born's reaction is treated in pages 138–140 of MBG. He concluded that one could not know the state of an electron after colliding with an atom; one could know only "how probable is the specific outcome of the collision"; Born continued: "I myself am inclined to renounce determinism in the world of atoms." If $\psi(t)$ is an evolving wave function solution of the Schodinger partial differential equation, then the absolute value $|\psi(x,t)(\mathrm{x,t})|^2$ is the probability of finding the scattered particle at x. One then renormalizes these probabilities to sum to 1 by setting the L^2 integral over the whole configuration space to 1: $\|\psi(t)\|_2^2 = 1$.

I note that one sees in this ansatz (or is it, more correctly, a rationalization?) the influence of Bohr's original views earlier, that one must always take the statistical view of quantum mechanics, viz., as discussed in MBG (p. 122). Then Von Neumann entered the picture and wrote his foundations of quantum mechanics[5], which forever put quantum mechanics into a Hilbert Space, and probabilistically so.

So we are still stuck, 80 years later, with wave-particle duality, and as a consequence, Einstein's questioning, from MBG (p. 140), that "I, at any rate, am convinced that He does not play dice." Heisenberg, on the other side, by interpreting his uncertainty principle as meaning that more precision in measuring momentum meant less precision in measuring position, came down on the side of the statistical interpretation. This included Pauli's interpretation that the observer had become part of the problem, an issue that is still with us.

Let us now go to the Born-Einstein book LTRS for some corroboration, which I would like to add here for the convenience of the reader. In LTRS (pp. 83–88) one finds Born's letter to Einstein, dated July 15, 1925, and then Born's later commentary on it. The latter is a bit vague. Born states that "Later on ... the waves represent the spread of probability in the presence of particles. But this is not the place to pursue these matters in detail. Nor ...." On page 87 Born mentions that he already knew that Heisenberg's manuscript's arguments could be interpreted as the matrix equation $pq - qp = h/2\pi i$. On p. 91 of LTRS you will find Einstein's famous statement, which I quoted above, about God not playing dice. This is found in a letter to Born on December 4, 1926.

Another interesting clarification of Born's view of his statistical interpretation of quantum mechanics may be found on p. 186 of LTRS. Born tells Einstein that $\psi(t)$ does not really describe a state of a single system. Rather, "For what is really meant is, of course, that you take all individuals of 67 and count the percentage who live for a certain length of time [to say his life expectation is 4.3 more years, the example Born was using]. This has always been my own concept of how to interpret $|\psi|^2$. Instead you [Einstein] propose a system of a large number of identical individuals—a statistical total. It seems to me that the difference is not essential, but merely a matter of language."

I remark that what is going on here is, mathematically, an ergodic hypothesis, that time averages equal space averages. Most physicists today take an ensemble view of the statistical interpretation of quantum mechanics, although recently there are theories and experiments (welcher weg) about single trajectories.

Einstein replies (we are now in 1950, in Einstein's later years) and as usual sticks with his view that quantum mechanics through the wave function $\psi(t)$ is incomplete and is a theory that he hopes will [I skip back to quote directly from LTRS p. 173] "be replaced at some later date by a more complete and direct one." The Heisenberg-Born philosophy is that one must just live with it, and not delve further. Einstein wants deeper knowledge. Schrodinger (LTRS, p. 202) stubbornly believes there are no particles, no jumps, only waves. Born (LTRS, p. 229) takes full credit for the statistical interpretation of the Schrodinger equation wave function $\psi(t)$, which had just brought him the 1954 Nobel Prize. This was 22 years after Heisenberg had received the Nobel Prize principally for the uncertainty principle, which, as I noted above, was substantially a team effort.

One could go on with these retrospectral historical arguments forever. To move on, let me go back to the book under review, MBG, and to be specific, to Greenspan's statement and quote (MBF, p. 139, bottom) that "Born anticipated that some physicists would 'assume that there are other parameters, not given in the [statistical] theory, that determine the individual event'—that is, that something else lay deeper that would reestablish cause and effect." These other parameters have come to be called hidden variables. I remark that to my understanding, Born himself never advocated looking for such parameters. Although for many years after the turbulent quantum years of 1920s and 1930s the Heisenberg-Born-Bohr view that one could not go deeper held sway, in more recent years the belief in possible hidden variables has gained some support.[6]

As I stated at the beginning of this review, I now want to give some of my own thoughts and results on these matters. As I write this now, I realize that there is no way I can provide all the intermediate background for these thoughts and results: there would just be 50 years of all of the developments in quantum theory to get us nearer to the present. So with apologies, I will just jump to these thoughts and results. The reader may imagine this review to be a short account of a long conversation in which has been recorded only the very beginning and the very end!

Briefly, I want to address some very specific issues concerning (1) the statistical interpretation of quantum mechanics and (2) the question of incompleteness of quantum mechanics. I will call these two issues, respectively, (1) the Zeno Problem and (2) the Bell Problem. For efficient references the reader may consult, respectively, the recent Solvay Congress proceedings[7] and the recent book.[8]

The so-called Zeno's paradox[9] goes back to Von Neumann's original formulation of quantum mechanics in Hilbert Space. It has become quite important in quantum computing's need to control decoherence.[7] Von Neumann saw it as a way to steer one state into another. It started as an attempt to model 'continuous viewing' of a quantum state evolution, $\psi(t) = U(t)\psi_0$. It is commonly described as the quantum version of "a watched pot never boils."

I was actually (in 1974, with B. Misra, here in Boulder, Colorado) one of the first to consider the Zeno Problem. We modelled a continuous quantum observation by the operator limit

$$s - \lim_{n \to \infty} (Ee^{itH/n}E)^n$$

and found, for the interesting case physically (in my opinion), where the projection E does not commute with the Hamiltonian, that we had mathematical difficulties with this limit. Then Misra with Sudarshan published their famous paper[9] in 1977. Probably I should have been coauthor. But I forgave my friend Misra, as he was between positions and the paper certainly helped him secure a position in Bruxelles. And his work with Sudarshan was completely independent of me. If you carefully read their paper you will see that the operator limits, such as the one above, which Misra and I were considering arc simply assumed to exist. So the mathematical problems remain. Recently I have recorded my own thoughts on the Zeno Problem in a number of papers.[10-13]

In particular, I found the following fundamental improvement in the Born probabilistic interpretation of quantum mechanics. Given a quantum evolution $\psi(t) = U(t)\psi_0$, which exponentiates a Schrodinger equation initial value problem in which ψ_0 is the initial wave packet which you have prepared, the mathematical theory requires that ψ_0 be in the domain $D(H)$ of the governing Hamiltonian if you want evolution equivalence to the Schrodinger partial differential equation. Then my theory says that $U(t)$ must take $D(H)$ one-to-one onto itself, at every second (hour, eon) of the Schrodinger quantum dynamics. The other states ψ_0 in the Hilbert Space which are not well enough prepared to properly evolve, i.e., which are not in $D(H)$, will also map one-to-one onto the complement of $D(H)$, but that is less important. The main point is that if we accept the Schrodinger equation and the Born interpretation, then the evolution $U(t)$ cannot lose even one probability distribution $\psi(t)$ from $D(H)$, not even for one instant. Should it lose even one possible prepared 'probability' state, uniformity is lost and irreversibility has occurred.

As to the Bell Problem, in recent years I have found intriguing connections between the Bell inequalities of the hidden variable theory and my independent (since 1967) theory of noncommutative trigonometry. This has been reported in a number of papers.[14-17] The details are technical, but I may summarize roughly by saying that all of the Bell inequalities may be placed within my more general noncommutative trigonometry. As the latter is essentially a new vector geometry in Hilbert Space, these developments mean that much of the mysticism and physical argumentation about nonlocality, realism, and the like, which surrounds the Bell Problem, may be more precisely seen as mathematical probabilistic-geometric issues. In particular, questions about what quantum probabilities and correlations mean and imply physically may be seen geomerically in terms of my new spin Bell equalities.

My new theory does not solve the hidden variable problem. But it does show, in my opinion, that the Bell inequalities also do not solve the hidden variable problem. The hidden variables problem lies deeper, and to understand it one must bring in, in a comprehensive way, all the Zeno quantum measurement issues, all the Bell geometrical issues, and, in particular, fundamental questions about the Von Neumann Projection Postulate in his quantum measurement theory. Of course other mathematical physicists have wondered about the Projection Postulate, or, if you will, the 'wave function collapse.' There are many, many papers that have been written about this, many of which I certainly have not seen. And this is not the place to try to give a representative view of wave function collapse and the various attempts at explaining it. Perhaps I will eventually finish my study of it, underway some years now. However, let me for the reader's advantage mention here a few treatments.

The physicist Ballentine in his books and papers has addressed the Projection Postulate, e.g., see the cited paper[18] and citations therein. I should mention that prior to Gustafson and Misra,[19] Misra and I tried to develop a model which (quoting from Ballentine[18]) "discards branches of the state vector that are not relevant to the task at hand," a view of collapse that Ballentine advances in his conclusion. But our mathematics showed something quite different. Our so-called decaying states without regeneration did not exist for unstable quantum systems. Instead, they represented regular stationary stochastic processes.

Then there are those who picture quantum probabilities as Brownian motion 'corrections' to the usual Schrodinger dynamics. See, for example, the recent book by Adler[20] and the citations therein. A similar approach is advocated by Percival.[21] One then tries to argue for the Born quantum probability interpretation as an emergent phenomenon. While the mathematics is nice, i.e., becomes a challenging chance to bring the Ito stochastic calculus into the physical model, I cannot buy it as foundational physics. To quote Adler (p. 169): "when ... the operator coefficients of the noise terms are mutually commutative and commute with H_{eff}, the state vector reduces on the eigenstate basis that diagonalizes the operator coefficients of the noise terms, with reduction probabilities given by the Born rule ...." I have, I suppose, always had a sort of contrarian reaction whenever I see operators blanketly assumed to commute. I have elsewhere[22] set out my objections to what I perceive as hidden assumptions of detailed balance.

To avoid getting deeper into these issues here, let me now just refer the reader to the book by Penrose[23], which in my opinion nicely discusses many aspects of the quantum measurement problem and, moreover, keeps those discussions grounded within the larger context of other key matters in modern physics. In particular, one finds a nice classification of the measurement paradox explanations (a) 'Copenhagen,' (b) many worlds, (c) environmental decoherence, (d) consistent histories, (e) pilot wave, and (f) new theory with objective R. As I am, and always have been, Penrose is also uncomfortable with the density matrix (Heisenberg picture) ansatz.

I need to close this review, so let me rather abruptly say, and with paraphrase, that philosophically, my thoughts on these matters would lead me to currently vote for Einstein's view, that we should seek a deeper understanding, physically, e.g., of the real nature of an electron and a photon in a field, even their own, rather than to settle for the Born-Heisenberg view that to seek such is hopeless.

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Science Sold Out: Does HIV Really Cause AIDS? by Rebecca Culshaw. Berkeley, CA: North Atlantic Books, 2007. 96 + xii pp. $14.95 (paper). ISBN 1556436424.

That HIV does not cause AIDS could hardly be stated more cogently, compactly, readably, and even comprehensively than in this book. There are also insights here not sufficiently emphasized in other such contrarian or "dissident" works: the sheer lack of logic in much of what the mainstream says about HIV/AIDS; and that this illustrates a pervasive decline in intellectual standards in science as a whole.

Culshaw is assistant professor of mathematics at the University of Texas at Tyler. Her graduate work, on modeling immunological aspects of HIV theory, led her to recognize deficiencies in HIV/AIDS theory. The book's emphasis on logic may well owe something to the discipline of mathematics: in much of empirical science, paradoxes and anomalies are tolerated so long as something "works"; but being illogical in mathematics is fatal. Perhaps, too, the earlier role of graduate student allowed Culshaw to take a fresh look not easily taken by people immersed in the subject for a couple of decades; though one may hope that HIV/AIDS theory will not (continue to?) exemplify Planck's dictum,' that the old fogies need to pass away before a mistaken mainstream consensus is corrected.

The Introduction begins with a conundrum pointed out by Duesberg. Official estimates have had about 1 million Americans HIV-positive, steadily, year after year, since 1985. That is incompatible with the chronology of AIDS cases and AIDS deaths: official figures show for AIDS cases a linear increase, and for deaths a rapid increase, peaking in the early 1990s and then decreasing again. Those HIV data are also incompatible, quantitatively as well as qualitatively, with the official estimate of about 10 years from infection to AIDS. Further, the symptoms and epidemiology of AIDS are entirely different in the First and Third Worlds, which is not the case with any other disease, be it sexually transmitted or not. Moreover,

> AIDS has become so mired in emotion, hysteria, and politics that it is no longer primarily a health issue .... [P]ronouncements by powerful government officials and ill-informed celebrities are taken as gospel, and no one even remembers when, a few years later, these pronouncements turn out to be false. (p. 4)

Chapter 1 recounts briefly how Culshaw came to realize that the HIV/AIDS model is wrong. Chapter 2 points out that there is no agreement on, or evidence for, any mechanism by which HIV lulls the cells that are depleted in AIDS.