Bill Daniel's thoughts on Philip Ball's *Beyond Weird* pp. 1 to 195

While I found this book to be an interesting exploration of the various interpretations of QM, I think its subtitle, "Why everything you thought you knew about quantum mechanics is different," doesn't really apply to us. I didn't read anything particularly "different" in it and I expect most of us didn't either.

The various QM interpretations the author describes offer interesting opportunities for debate and I'm sorry I won't be able to participate in that debate, but unfortunately it also contains the usual collection of misconceptions and errors that always seem to crop up in books like this by science writers who are not trained in physics.

p. 30 mid: "… quantum mechanics is not really 'about' quanta… Quantization was just what alerted Einstein and his colleagues that something was up with classical physics."

> Ball supports this statement (p. 31 bot) by observing that Suskind doesn't mention quantization of Planck's harmonic oscillators until the last section of the last chapter of his *Theoretical Minimum* book. I certainly don't consider Suskind's book to be the canonical QM text, and many others cover quantized harmonic oscillators much earlier, but this this is really more about the need to lay theoretical and mathematical groundwork than about the unimportance of quantization.
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> Ball's statement also reflects a misunderstanding of the nature of "quantization." Quantization is not fundamentally about the "chunking of energy," as he says, but is the process of moving from classical field concepts to quantum mechanical operator fields. Any quantum field theory, in fact, must begin with a mechanism of quantization in order to be able to compute amplitudes. Quanta are fundamental to quantum mechanics.

p. 45 figure:

> There are two problems with this figure:
>
> 1) The graphs of Ψ_1 and Ψ_3 are incorrect. The wave functions are sinusoidal and do not flatten out at the interface with the walls.
> 2) In the "infinite square well" case illustrated, $E \propto n^2$, so the ladder rungs get more *widely* spaced with increasing energy, not closer as shown.

p. 56 footnote:

> Perhaps this is being picky, but neutron beta decay produces an *anti*-neutrino.

p. 91 bot: "There's something deeper at work, relating to our *gaining knowledge*."

> This is where QM interpretations far too often (in my opinion) veer off into vague notions of a requirement for consciousness to "collapse the wave function." Wheeler and others thought that conscious beings (perhaps limited to humans) had to gain that knowledge in order to effect collapse, while others emphasize the much more impersonal, information theoretic form of "knowledge."

p. 97 mid: "If we repeat the measurement, we'll keep getting C."

> Immediate repetition of the same measurement will indeed result in C, but the unitary evolution of the system that Ball has been describing will later restore it to states that are superpositions with non-zero amplitudes for A and B.

p. 130-143: ***When superpositions are expressed as linear sums of kets with complex coefficients, understanding the results of spin experiments is straightforward. It's only when translated into words that these experiments appear mysterious. For this reason, every popularization of QM that I have read fails to make spin understandable. Ball's book is no exception, but this section also contains many factual errors that make it even more confusing.***

p. 131 mid: "… each electron in an atom then has a unique 'barcode' of quantum numbers…"

> The *n*, *l*, and *m* quantum numbers do apply only to electrons *in atoms*, but spin orientation doesn't require electrons to be in an atomic bound state. The Stern-Gerlach experiment used silver atoms, but that was only an experimental convenience.

p. 134 mid: "an atom's total magnetic moment is a combination of those of the electrons and of the nucleus."

> The magnetic moment, μ, is inversely proportional to particle mass, so (neglecting the complicating factor that nucleons are composite) the electron μ_e is 3 orders of magnitude larger than the nuclear μ_n.

p. 135 figure:

> The magnetic field in a Stern-Gerlach device must be spatially inhomogeneous and is generated by having one magnet be ridge-shaped, not both flat as in the figure. The field must be inhomogeneous because the force on an uncharged magnetic dipole in a B field is given by $\boldsymbol{F} = \nabla (\boldsymbol{\mu} \bullet \boldsymbol{B})$. If $\boldsymbol{B}$ is spatially uniform, the gradient is zero – no force, no separation into spin up/spin down beams.

p. 136 top: "the electron has to rotate *twice* to get back to where it started."

> To be fair, Ball later characterizes this as a "meaningless statement," but I would be more inclined to call it a *misleading* statement. It is not the electron that must be rotated twice in physical space, but its mathematical representation as a spinor that must be rotated twice in Hilbert space. Objects that retain the same sign under a 2π Hilbert space rotation are called bosons; those that pick up a minus sign are fermions. No physical space rotation takes place.

p. 137 line 4: The ellipsis after the 2 is meaningless, as there are no known elementary bosons with spin > 2.

p. 138 top:

> First sentence is only true for spin ½ particles. Spin projections yield $2s + 1$ possibilities.

p. 146 mid: Ball correctly recognizes that the Uncertainty Principle is *not* due to the fact that "we can't help disturbing what it is we want to measure," as Heisenberg thought, but is a fundamental property of quantum systems. He amplifies this on p. 153-4. Ball does a good job of pointing out the error, but this common misconception often appears in other similar books.

p. 148 mid to bot: "… two properties of a quantum system, *p* and *q*… Δp times Δq can never be less than the value $h/2\pi$."

> If *p* and *q* are momentum and position, this statement uses ambiguous (though common) notation. Additionally, if stated with more precision, it's wrong by a factor of 1/2.
>
> The root of the problem is that the quantities Δp and Δq are not well defined. To be accurate, a statement of the position/momentum HUP should take *p* and *q* to be random variables and be written in terms of their standard deviations as: $\sigma_p\, \sigma_q \geq h/4\pi$. Ball's 2π rather than 4π can be a result of the ambiguity inherent in Δp and Δq, but it's certainly not a misprint, as he repeats it several more times.
>
> If, though, *p* and *q* are, as Ball says they are, any "two properties," then this statement is definitely wrong. The generalized version of the uncertainty principle, in which *p* and *q* are *any* two observables, is:
>
> $$\sigma_p^2\, \sigma_q^2 \;\geq\; \left(\frac{1}{2i}\,\langle[\hat{p},\hat{q}]\rangle\right)^2,$$
>
> where the square brackets indicate commutation and the angle brackets, expectation value. Using $\hat{q} = x$ and $\hat{p} \;= -\frac{ih}{2\pi}\frac{d}{dx}$, gives the above result. But for different operators, the product of their standard deviations may be different. In particular, for operators that share a complete set of common eigenfunctions (compatible operators), their commutator, and thus the RHS of the inequality, is zero.

p. 150 top: "energy and time… uncertainty relationship between them is subtly different from that between position and momentum"

> This is a correct and common observation, but rarely do popular QM books explain what that "subtle" difference is. They should, though, because it's simple, beautiful, and fundamental. (For an excellent description of all this, see section 44 of the Landau & Lifshitz book on Non-relativistic QM, but I'll summarize the ideas here.)
>
> The p-q HUP is stated in terms of uncertainties in two *different quantities* (*p* and *q*) measured *simultaneously* on the *same system*. It's a statement that quantities associated with two non-commuting operators acting on the same wavefunction can't both have definite values at the same time. The E-t uncertainty, though, can only be understood by imagining an *ensemble* of identically prepared quantum systems on which energy measurements are made. The times at which those measurements are made on *different systems* are separated by Δt. The ΔE refers to the *difference* between the results of energy measurements on the two different systems. Those individual energy measurements are *not uncertain*, but can be as precise as experimental error allows. Only in the $\Delta t \to \infty$ limit, though, will the measurements consistently return the *same value* for energy ($\Delta E = 0$). For finite Δt, the measured energy difference, ΔE, will fluctuate such

that $\Delta E \Delta t \geq h/4\pi$. (Note: I use Δ notation here, since writing σ_t would imply that *t* is a random variable.)

Now, that wasn't so bad, and it could have easily been said by Ball and other authors of similar books. I do want to take it a step further, though – a step that provides a more profound understanding at the cost of a bit of slightly deeper digging.

In the above, we assumed that the systems were "identically prepared." This restriction was only for simplicity and I want now to consider the decay of a quantum particle. Decay takes place with a "lifetime" I'll call τ. In this case, τ plays the role of Δt, while ΔE refers to the difference in energy of the system before and after decay. This means that the energy level before decay can only be determined to an accuracy given by the energy-time UP (assuming that E after decay can be measured accurately) and results in an energy level width of the order of $\Gamma \cong h/4\pi\tau$. This uncertainty becomes important in measuring the energy (mass) of strongly-decaying particles with lifetimes of ~10^{-24} second or so.

If you'll permit me, there is another interesting point that is a little deeper still, that relates to this *q*-*p* vs. *E*-*t* UP distinction. The quantities *q*, *p* and *E* are all dynamical variables – and hence have associated Hermitian operators. Time, though, is just a parameter – a label – not an operator conjugate to the Hamiltonian (energy operator) the way *q* and *p* are conjugate operators. This might seem like picking nits, but the operator/parameter distinction becomes fundamentally important in QFT.

To make the non-relativistic Schrödinger equation consistent with special relativistic melding of time and space, we must put time and space on equal footing – either both operators or both parameters. Time can be promoted to an operator by using the relativistic proper time (identically experienced by all systems), τ, as a label and the (system dependent) coordinate time, *T*, as an operator. It turns out that relativistic QM can be developed this way, but it's a mess. A much better approach is to demote position to parameter status.

To do this, we assign an operator $\varphi(\mathbf{x})$ to each location in space, creating a quantum field parameterized by position. If we allow these φ operators to also be time dependent (the "Heisenberg picture," for those familiar with the lingo), we write $\varphi(\mathbf{x}, t)$ and can then construct a Lorentz covariant Schrödinger equation. It was this that Klein and Gordon solved for what was later realized to be the spinless case and Dirac, brilliantly (of course, this *was* Dirac after all), solved for the all-important spin-1/2 case.

(Side note: Non-relativistic QM can be written as a quantum field theory in this way, too, but it's not very useful since it requires the number of particles to remain fixed. Any relativistic quantum field theory must allow particle number to change, since if you happen to have $2mc^2$ of energy lying around, experiments show that pair creation spontaneously happens.)

pp. 155-156:

I am unfamiliar with the work of Ozawa that Ball mentions and would be interested in what others know about it. This paper describes Ozawa's error-disturbance uncertainty relation in general terms. A much more detailed development can be found here.

pp. 164 to 177: **While Ball does an adequate job of describing Bell's theorem, a much better and (fairly) non-mathematical description is Bell's own in his "Bertlmann's socks" article in *Speakable and Unspeakable in Quantum Mechanics* (don't know why it's so absurdly expensive).**

p. 164 first line: "if we prepare a particle…"

> This statement is only true for spin-1/2 particles.

p. 171 mid: "… how strongly correlated two particles can be…"

> Perhaps this is just unclearly stated, but the spins of particles (or polarizations of photons) of an entangled pair are either "correlated" or "anti-correlated." Those are the only choices. The intermediate degrees of correlation seen in Bell's theorem are not between a single pair of particles (photons), but are the average of correlation measurements on an *ensemble* of identically prepared entangled pairs. This distinction may seem picky, but it's vital to understanding Bell's result.

p. 173 last paragraph: This is the crux of Bell's theorem.

p. 174 bot: "…their sum has to lie within the range +2 to -2…"

> The way this is written, it could sound like Ball is referring to the sum of particle spin correlations. That could be +2, 0, or -2. Actually, what he has in mind is the so called CHSH inequality, *S*. (He hints at this in the footnote.) Hidden variable theories require $|S| \leq 2$, while QM allows $|S|$ to be as large as $2\sqrt{2}$.

p. 192 bot: "Hidden variables, remember, are *local*…"

> Not necessarily. Bell tests have ruled out local hidden variable theories, but leave open the possibility of *non-local* hidden variable theories, such as the Bohm approach. In the Bohm theory, the hidden variables are non-local because they are represented by the interaction between the particle and the pilot wave, which is spread through space.