

Thanks, your explanation helps. In my usage a statevector defines and is defined by probability amplitudes for a complete set of variables, which may well be detector clicks.
(I don't like the word "complete" but the usage is established; I prefer "maximal" for the concept in question.)

And to get probability amplitudes rather than probabilities, one must, I assume, specify a complete (=maximal complementary) set of variables, or else you will compute and measure incoherent averages over the unmeasured variables.

So the difference between talking about statevectors and talking about probability amplitudes is like the difference between talking about functions and about their values: a matter of how one chooses to organize the discourse. The statevector theory and the probability amplitude theory do not seem to me to be significantly different theories. Perhaps this is exactly your point?

The advantage to discreteness is finiteness. Is that your motivation? That is half of my motivation for building with spins. The other half is that spins can have the observed symmetries.

Do you renounce these symmetries when you start from the discrete?

Do you expect them to result from smoothing, and expect the symmetries to go away under better measurements?

To make sure we are on the same channel: Would blocksystem describe your approach, or is blockworld an essential part of the idea? Canonical quantum theory doesn't talk much about the world. It says as much as it can about the system, and as little as it has to about the frame (experimenter, laboratory, etc.).

As I see it, ultimately, quantum physics is about 'explaining' click distributions in detectors, e.g., clicks per unit time at one detector compared to another detector (relative click rates), spatial distributions of clicks over a long period of time (interference patterns), clicks distributed through space to form 'trajectories' (particle physics), etc. Therefore, I don't want to conflate theoretical concepts (charge, mass, spin, etc.) with the clicks themselves since a click has no properties other than where it occurs (relationally) in spacetime. These other theoretical concepts are higher level abstractions used to characterize click *distributions*, i.e., *sets* of clicks. Our theory is an attempt at finding the amplitudes for the relative spacetime locations of *individual* clicks.

For example, if you have the amplitudes for click rates at two detectors, you normalize over those two detectors for your relative rates. Now suppose you add a third detector, which may or may not affect the click rates at the first two detectors, e.g., put into the arm of an interferometer or added along the screen of an interference pattern, respectively. Then, you simply recompute the amplitudes (where necessary) and renormalize. There are no concerns about "complete sets of variables" at this level. [See, for example, how we explain the interferometer in our *Foundations* paper.]

As to our motivation for a discrete approach, we were looking to build "relata from relations" so we had to consider building trans-temporal/dynamical entities from something more fundamental. [Again, the reason we're motivated to decompose clicks non-dynamically isn't QFT, where sets of clicks are easily explained by the existence of dynamical particles moving through the detector, but the EPR-Bell phenomena of QM.] Graph theory is well-suited for this non-dynamical decomposition. It's nice that this approach has no problem with the infinities

plaguing GR, for example, but that was not a motivating factor. While problems faced by QFT were not a motivating factor for us, recent work suggests our approach will clarify renormalization, i.e., provide an explanation for why renormalization works.

In our view, Z is a ‘mathematical machine’ used to produce a measure of the ‘symmetry’ contained in the invariant core of the discrete action ($\Sigma = \frac{1}{2} \bar{\bar{A}} + \bar{J}$), rather than a “path integral” through configuration (or real) space. [Aside – the SCC is a rule for constructing Σ , which is easily identified in the discrete approach but totally obscured in the continuous approach.] Thus, when we realized that our proposed $\bar{\bar{A}}$ was singular (has a zero eigenvalue), making Z undefined, we simply argued that Z should only be carried out in the (N-1)-dim subspace spanned by the rows of $\bar{\bar{A}}$, since it turns out that our relational definition of \bar{J} restricted it to this same (N-1)-dim subspace. This ‘renormalization’ is not *ad hoc* – why would you measure Σ outside the vector space in which it resides? Especially, given that the measure produces a worthless (undefined) result in the irrelevant dimension. There is no similar justification for restricting the integration in the path integral interpretation.

So, again, we want to produce the probability amplitude for the relative spatiotemporal locations of a particular (specific) set of discrete events (clicks) in some experimental configuration. We do that by constructing Σ such that $\bar{\bar{A}}\bar{v} \propto \bar{J}$ on our graph in accord with a specific set of outcomes in our particular experimental arrangement. We then “measure the symmetry” of Σ , i.e., we compute Z . We repeat this process for all competing outcomes of interest (possible experimental outcomes), then compute and normalize the probabilities. End of story.

What does this mean for particle physics? Essentially, QFT is a higher-level approximation needed when you’re dealing with thousands of clicks from which you can construct “trajectories.” Are there particles moving through the detector to cause these trajectories? Per QFT, yes, there are particles causing these click distributions and the game in QFT is to find the properties of these click-causing particles. Per RBW, no, there are no click-causing particles moving through the detector so the game in QFT is to find the ‘dynamical attributes’ which characterize the different types of trajectories. Yes, many of the same dynamical attributes we find at the level of particle physics are manifest at the level of classical physics (momentum, mass, energy, charge, etc.), so one can, in that simple sense, think of particle physics as fundamental to classical physics. However, when you explore the realm of ever more rarefied click distributions, e.g., EPR-Bell phenomena, you find evidence for the fact that clicks are not caused by particles. So, we’re claiming that particles (no matter how “macroscopic”) are the result of increasingly dense click distributions, rather than the conventional, converse claim that clicks are caused by particles. Our view is always tenable, whereas the conventional view fails in a wide variety of QM experiments.

While radical, our fundamental claim and proposed computational technique strike us as simple and straightforward. Whether RBW can be made to work remains to be seen, but at least we’re not starting from a contradiction to established physics. Quite the opposite, it looks like QM, QFT and GR will continue much as Newtonian mechanics continued after the advent of SR.

For example, the vacuum solutions of GR would still be considered relevant in their predictions of the trajectories for “small” objects (“small” meaning they don’t change the spacetime geometry). The caveat? Since there is no empty spacetime in RBW, the only vacuum trajectories of GR which can be realized are those on which it is possible to construct a divergence-free stress-energy tensor (ST tns). You don’t have to actually *construct* this “small,” additional ST tns to know what will happen, it just has to be *possible* to do so. When it’s possible to do so, you know the object in question will follow the trajectory given by the vacuum solution. When it’s not possible to do so, the problematic vacuum trajectory *does not exist*, contrary to its inclusion in the GR spacetime manifold.

We could continue with such heuristics, but they’re all moot until we finish the proposed formalism, so it’s back to work!

Thanks again for your input, David. It is GREATLY appreciated!

Mark