Abstract:

Is reality best described in digital or analog terms? In proper context, we are asking: what type of math is best for that purpose? However, I argue that our universe is genuinely non-deterministic, as conventional notions of quantum mechanics imply. Since mathematics is by nature deterministic, reality is not fully describable by any true mathematical model. The best answer to the original question is then, "neither – reality transcends mathematics." It is argued that some popular attempts to avoid the quantum measurement problem, such as the decoherence interpretation, are flawed. The logical case for DI is marred by the circular argument at its core. More importantly: some experiments are described, which could falsify the DI. If successful, they would show that we can recover superpositions supposedly lost to decoherence. Hence, our finding definitive experimental outcomes instead of superposed results is not due to the effects of decoherence. Those definite, exclusionary results show a genuinely indeterminate character of the universe.

Our Non-Deterministic Reality Is Neither Digital Nor Analog: Experimental Tests Can Show That Decoherence Fails to Resolve the Measurement Problem

- Neil Bates

What is necessary "for the very existence of science," and what the characteristics of nature are, are not to be determined by pompous preconditions; they are determined always by the material with which we work, by nature herself. ... In fact it is necessary for the very existence of science that minds exist which do not allow that nature must satisfy some preconceived conditions, like that of our philosopher.

Richard Feynman, in *The Character of Physical Law* [1]

I. Introduction: We Start by Asking How to Ask ...

    Is our world digital or analog? Must it be either? ("Neither" must be an option, or we are unfairly forced into a "have you stopped smoking" type dilemma.) First we need to clarify our terms, since usage varies. In the current relevant sense, "digital" implies mathematical and equivalently physical discreteness (bit-like operations, grainy space-time etc.) as opposed to continuum processes, which are called "analog" in some contexts. However, to the extent continua are mathematically modeled (such as *via* differential equations, *e.g.* classical electromagnetism and pre-measurement Schrödinger evolution), they are still "deterministic" as affirmed below. So in advance of asking what *kind* of math best describes our reality, we first ask the pertinent question: is it even *fully representable* in "mathematical" terms? Here I argue in favor of the traditionalist view in quantum mechanics, that our universe expresses irreducible unpredictability at heart. This trans-mathematical character is revealed through logical challenges and proposed experimental tests for flaws in some increasingly popular interpretations like decoherence (DI) and many-worlds (MWI.) Refuting DI may seem a narrow focus, but its viability is at the nexus of this debate. These interpretations try to bypass the mysterious and fickle collapse of the wave function by maintaining a broader, ultimately deterministic evolution. Therefore, I humbly offer that reality is neither digital nor analog. Our world cannot be *fully* described by genuinely mathematical models of *any* kind. At some level it is truly inscrutable. It is not clear where to go from there. One thing is certain: attempts at understanding should not be fallacious, or driven by desperation to make the world conform to our prejudices or convenience. We must heed Richard Feynman's warning.

    MWI is often deployed as enabled by and giving meaning to DI, but we focus on DI proper. First, I explain how DI at its core is based on a fallacious circular argument and misleading semantics and

mathematical device, the density matrix. We then explore two empirical proposals that undermine the idea that decoherence reduces quantum reality to some sort of apparently classical world. These are doable experiments, and would refute the pretense that photon output from an interferometer subjected to decoherence was equivalent to a mixture. Instead, the output from this supposedly unrecoverable process should retain measurable superposed character in later recombination. It can well be argued under reasonable assumptions that results *would* be unfavorable to DI. Finally, I must insist that mathematical models are indeed deterministic, being based on logical necessity: objects like "random variables" are abstract entities, not generators of specific output. A mathematician wielding such variables must either treat them as generalizations and conceptual tools, or put in actual varying outputs "by hand" (or, ironically, by using a physical process like radioactive decay that is purported to be "genuinely random!")

II. TE1: What Is Claimed About Decoherence?
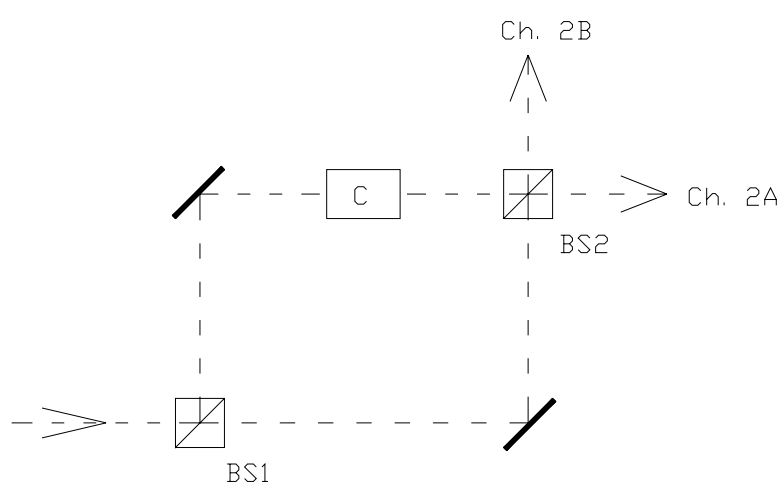
Thought Experiment #1

Figure #1

   To properly critique and refute DI, we first review how it supposedly works. Decoherence is said to explain why we don't see macroscopic superpositions, in a way not requiring a special "collapse" state-reduction process R. Without R, measuring devices should not be special either (other than convenience in being able to see and record results, however: "recording" is sometimes treated as providing extra constraints.) Discussions abound as DI gains in popularity. A classic informal model is explained [2] in *The QuantumChalleng*e (based on an example from H. Bernstein), as well as in a popular blog post [3] by Chad Orzel making essentially the same point (illustrative, its author is aware of the limitations of DI.) In both cases some factor varies from *instance to instance*, whereas in a given instance we expect decoherence to affect the distribution of the WF in space. We deal with that issue later.
   Orzel's example is at heart equivalent to Bernstein's, substituting relative phase of separated photon states for spin direction. We'll refer to his thought experiment as TE1. In **Figure #1** we see a traditional Mach-Zehnder interferometer M, but with an added "Confuser" C in the upper path. On demand, C can alter the phase of passing photon states to model the effects of environmental decoherence (such as from intervening media.) Keep in mind there are many ways to pattern phase changes, such as regular increments. For convenience, we use shorthand $s = \sqrt{2}/2 \approx 0.7$, show amplitudes and phases as coefficients, and say any reflection induces phase change $\pi/2$; multiplying complex phase by factor "i."

We fire sequential single photons horizontally into first beamsplitter BS_1, which induces an initial split (as imagined ...) into normalized lower state $s|1\rangle$ and upper state $si|2\rangle$. C is inactive (effectively absent) in our first series of shots. After reflections and recombinations, outputs from channels 2A (lower, and to correspond to BS index for later additions) and 2B from BS_2 are written respectively as

Channel 2A: $-0.5|1\rangle - 0.5|2\rangle$
Channel 2B: $0.5i|1\rangle - 0.5i|2\rangle$. $\hspace{6cm}$ (1)

Squared amplitudes, via either direct superposition or use of $A^2 + B^2 + 2AB\cos\theta$ give respective hit probabilities one and zero, so Ch. 2A corresponds to a bright "fringe" and 2B to a dark fringe. Recombination of states shows interference because their relative phases stayed constant among shots.

The next step is to enable C, which randomizes phases along upper path #2 by introducing a phase difference, of equal probability in the range zero through 2π. Hence each phase change $\cos\theta$ cancels a partner of change equaling $-\cos\theta$. Because all the terms in $2AB\cos\theta$ have been removed, we are left with the additive behavior of $A^2 + B^2$. The relevant wave function here is the entanglement with the measuring device, so we are not thinking of what happens to wave functions "in flight." Treatment in terms of a density matrix shows diagonalization, and the output statistics are equivalent to a mixture (sometimes one output and sometimes another: here in 0.5, 0.5 portions.) Many writers claim or imply, that production from an originally superposed WF of the same statistics as a mixture mean that we do have a mixture or "effective mixture," or at least there is an "appearance" of collapse (whatever that means) – without actually needing some inexplicable intrusion. Some go on to say, the original superpositions continue to evolve together, but are effectively separated into "different worlds" as in MWI. (Popular treatments of MWI rarely note, a two-way split can't effectively replicate unequal probabilities without contrived excuses such as "thickness" associated with statistical measure. Nor does hand-waving talk of "thinner slices" convincingly address problems with conservation of mass-energy.)

III. What is *Logically* Wrong With DI?

The most direct charge against DI is that it is based on a circular argument. In order to show that the post-Confuser statistics are similar to the statistics of a mixture, we have to assume that something can and does generate "statistics" of some kind from wave amplitudes in the first place! As Nick Herbert [4] aptly noted: "Whenever one looks closely at claims that randomization by itself collapsed the wave function, one always finds that collapse ... had to be put in 'by hand.' " The density matrix, an example of that fallacy, is misleading because it combines in itself the probability of there being a given WF, with the quantum probabilities derived from squared amplitudes of those same wave functions. Indeed, why not first construct a "UDM" that shows just the evolving unitary WF probabilities by themselves? Wouldn't that represent the real status before "collapse" added the other kind of statistics? Nor can a pattern of statistics requiring many measurements be the key to why I see only one result and not both in any one instance (which may be the only one in an actual experiment.) If there really is no special collapse or action by "measuring devices," shouldn't we assume that amplitudes just remain superposed but in a disorderly pattern? Consider a graphical simulation, with one state represented as red and the other as green: after decoherence, we imagine a messy and complex pattern with various shades of red, orange, yellow, green – but both colors are always part of the display. Why then wouldn't such a combination be part of our *observations* as well? Disorder shouldn't make superpositions "inaccessible" in *all* possible ways. Whatever "collapse" is, we need it as an additional factor to convert coherent superpositions into results that show interference, and messy superpositions into mixtures that don't.

Nor should it matter to my experience right now of an event, whether I can recover information such as the phase setting on C. Furthermore, why should phase changes that happen in the past or future, affect my current experience of exclusivity in measurement? Is there some "anticipation" of that? Note also,

there is no intrinsic probability for WFs themselves like there is for quantum statistics. What if we vary WFs for a short while in one way, and then another why for a time? How do we construct an average for the DM then? What DM is appropriate for WFs that change in a *regular way*, having the same average as a random collection but with radically different observational opportunities? Finally, the randomization process must be effectively perfect or we can find tell-tale signs of residual interference. Would natural processes (not always a heat bath) do that, or be more likely produce a weighted, Gaussian distribution?

We hear it said of incoherent states, "but they don't interfere anymore" – should that matter? Note that interference effects were used to originally establish *the model* of wave functions. We realize there is interference by finding "hits" and interpreting them, not the other way around. Once we have the model, we don't need to keep checking for interference to prove the WFs are still there. If we trust the model, we use it to describe unfolding events *even if* conditions for illustrating it are no longer ideal. We don't need the possibility of interference to have a measurement problem. A simple split by *one* BS, according to the model, sends two states in different directions toward detectors that may be light-years apart. One detector gets a hit, and now the other one cannot despite the gulf. Interference is not the key issue, and its absence doesn't explain why we don't see macroscopic superpositions.

Consider this strikingly overt "in-front-of-the-nose ah-ha" challenge to DI: if observing classical behavior is contingent upon decoherence confusing the wave functions, then why are we able to see evidence for quantum interference for comparison?! There should only be "classical statistics" since the quantum variety could never be "seen" – coherent quantum states and systems would stay superposed for us and never "appear to collapse." Some might argue, the *subsequent* decoherence of systems surrounding the detectors etc. allows us to see clicking. However, the measured statistics are indeed the evidence of coherent interference of the original WF in such cases presented as the "evidence for quantum interference" to start with. They were already separate clicks. DI comes across as a semantic parlor trick.

IV. TE2: Recovering Coherent Interference From an Interferometer's Supposed Mixture Output
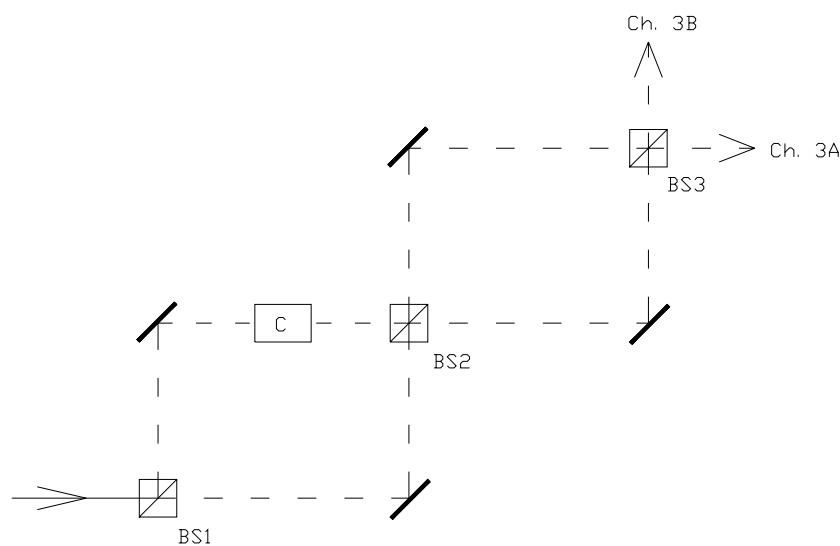
Thought Experiment #2

Figure #2

Arguments about decoherence are not as important as whether we can find an empirical difference between what DI claims (such as it is) versus CI (conventional or Copenhagen quantum interpretation.) If we can, perhaps surprising to many (as in the case of Bell inequalities), then these "interpretations" are more than that. Many DI proponents already refer to effects of decoherence (for example Ref. 5) as effective support for their viewpoint or parts of it, but these results can be subsumed within existing CI since we finally have to observe them. I pose here a practical way to *show* that the output of a suitable "confused MZI" is definitely not (or not equivalent to) a mixture of independent photons, but rather retains its full character as a superposition. Such results would directly contradict the DI characterization of such output. That superposed character is shown by recombining decohered MZI output into a *third* beamsplitter and predicting secondary output statistics from later interference. No independent examination of BS_2 output streams (comparing filtering etc. at each channel) could show this distinction. Although the interpretation of the outcome is perhaps debatable and has been so debated, in discussions with specialists none has denied (once cognizant) what the results should be. Of course, the experiment should still be done and I look forward to researchers reporting their results.

The setup of TE2 is shown in **Figure #2**, and is based on a blog post [6] by the author. First, we modify TE1 by specifying an asymmetrical BS_1, having unequal transmitted and reflected portions. For an ideal ABS we specify amplitudes transmitted a and reflected b, such that $a^2 + b^2 = 1$ (but idealization is not crucial.) Each shot elicits initial transmitted state $a|1\rangle$ and reflected state $bi|2\rangle$. After reflections and $|2\rangle$ having passed the (aptly named?) Confuser, the states now approach BS_2 (equal split) as $ai|1\rangle$ and $-\theta b|2\rangle$; where θ is the "random" phase shift introduced by C. After combining in BS_2, the output states are writable as

Channel 2A: $-s(a|1\rangle - b\theta|2\rangle)$
Channel 2B: $si(a|1\rangle -bi\theta|2\rangle)$.   (2)

Note we retain the original states in the representations even though they are not orthogonal basis states, and θ is a varying "unknown." This is not like a specific composition from say x and y polarization states. Note again, this superposition *could* just be combined into a single amplitude by the usual rules albeit using the unknown, but for clarity it isn't. The original breakdown is "hidden" in the superposition and does not affect average statistics because of the loss of the term $2ab \cos\theta$.

Again, since θ varies, the *average* resultant amplitude drops the term $2ab \cos\theta$ and we are left with $(a^2 + b^2)/2 = 0.5$ click rate for each channel as with symmetrical BS1. Interference is lost. DI again interprets this statistical result as "a 50/50 mixture or FAPP mixture comes out of or encounters detectors past BS_2, and there's no way to distinguish that from a "real superposition." This is said even though we can indeed calculate the amplitudes in the manner stated above. That should mean they "are" as conceived in principle by a wavefunction realist, known or unknown as the case may be; making one wonder what prods those amplitudes as such into doing more, or why it should turn out differently in the coherent case. Again, it's hard to imagine what effective difference knowing the phases and hence amplitudes would make for detector behavior, but our finding classical statistical behavior ("a mixture") is somehow supposed to be contingent on the untraceability of the phase changes.

Yet we aren't done with these outputs. Instead of detectors, we'll use another set of mirrors to direct the outputs from BS_2 into a third, symmetrical beamsplitter BS_3. Note again that despite *raw* detector results equivalent to interception of "mixture" output, the outgoing superpositions are still (!) given by calculation. Hence we can find how they should combine at BS_3 for cascaded output into channels 3A and 3B. Following standard procedure and initially separating portions of a given state that are derived from different paths, we find for the recombined beams:

Channel 3A: $0.5 (a|1\rangle + b\theta|2\rangle - a|1\rangle + b\theta|2\rangle) = b\theta|2\rangle$
Channel 3B: $0.5i (- a|1\rangle + b\theta|2\rangle - a|1\rangle - b\theta|2\rangle) = - ai|1\rangle$.   (2)

We see that terms in $|1\rangle$ are canceled out from Ch. 3A and that terms in $|2\rangle$ are canceled out from Ch. 3B, it being irrelevant that θ is unknown and/or varies unpredictably. The combining of the beams from both BS$_2$ output channels reveals the otherwise hidden presence of the original division into amplitude portions of magnitudes *a* and *b*. The intensity output at 3A is b^2 and at 3B is a^2. This result shows that the output from Channels A2 and B2 was still a superposition and not a mixture, since 50/50 mixture output from BS$_2$ would of course exit Channels 3A and 3B with equal frequency. Therefore, the DI claim that "destruction of interference" leads to "mixture output" (however interpreted) is false.

The following is very important: it does no good to say: "But nothing is actually there at the BS$_2$ output. The results farther along are suspiciously based on counterfactuals. What happens if you put detectors there can't be used against DI when they aren't there. This is about observations etc, not what we imagine "really happens" etc.") First, in DI the WFs are indeed considered what really happens all the way through. It's game to ask proponents "so, what do you think really (or "in our world" at least) comes out of BS$_2$? " Perhaps more important, we could indeed put something there: glass plates anywhere in the course. We'd get the same results as known behavior in such experiments. Such materials effectively absorb and re-radiate, and we don't know why they should be different from "real measuring devices" (if indeed they are.) In CI they can be considered special to the extent we try to explain things at all, but in DI it's all the same in the flow. So "nothing" or "glass windows" are all grist for the mill of decoherence, once it already takes place. (Ask, why don't pieces of glass spoil interference in general.)

V. TE3: Recovering Coherent Interference From Spatially Decohered Double-Slit Output
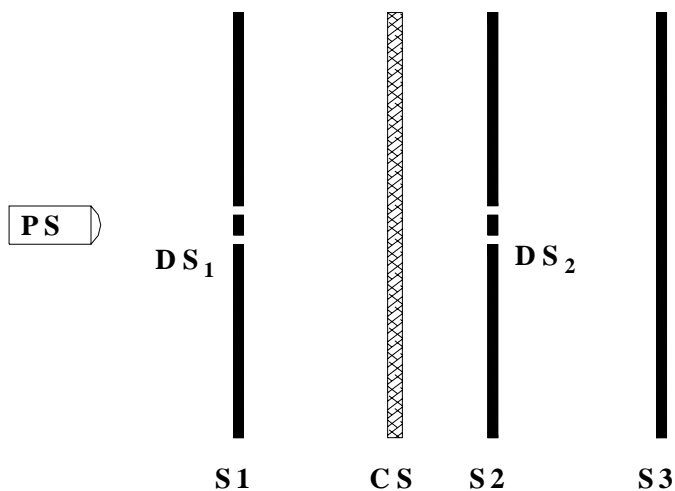
Thought Experiment #3

Figure #3

In **Figure #3** we see first at left, a coherent (highly so in temporal terms) photon source PS, projecting to a classic opaque wall with double slit DS$_1$. (Or imagine just "two tiny sources.") This is followed by a removable "confusion screen" CS, then we have another opaque screen #2 which is perforated by similar paired slits DS$_2$. The array ends with a plain screen #3 (or fine counter array) behind that. The CS is

basically a diffuser like finely rippled bathroom windows. Its job is to scatter light enough such that when put between a source or sources and another screen, the light reaching each point of the latter comes from numerous portions of the CS over a wide range. The CS in effect "decoheres" light passing through it.

We first imagine what happens using a strong light source, removed CS, and classical interference. DS_1 projects an interference pattern to screen #2. The superposition of waves from each slit of DS_1 produces a new wave of definite phase and amplitude at each point of screen #2, in this case having a regular pattern of variation. The slits DS_2 are set to intercept reasonably bright portions of the pattern, but in any case each becomes (via Huygens' Principle, as from DS_1) a new source in turn. Hence each slit of DS_2 projects according to the net definite phase and amplitude it intercepted. So, we expect a new albeit fainter interference pattern on the final screen #3.

Now, what happens if we insert CS? The interference pattern on screen #2 is gone because of decoherence of radiation from DS_1. One's intuition may be: interference at screen #3 is dissolved as well, replaced by uniform light. But no. Remember that although CS affects light in a complicated way, that way is still deterministic and stable. Let's first follow the experiment in terms of CI. Each portion of CS directs a "ray" (narrow pencil but possibly diverged or narrowed) from a slit to one or more small regions dS on screen #2. This wave arriving at any dS shows a definite phase and amplitude based on various factors. Many of these destination rays will overlap. If we combine contributions starting from any one slit and directed by various portions of CS to a point p_1, the wave composition at p_1 can be written (in discrete approximation and combing phase and amplitude in the coefficients) as

$$a|1\rangle + b|2\rangle + c|3\rangle + ... \ = \ S|Sum\rangle \tag{4}$$

These contributions sum to a definite resultant $S|Sum\rangle$ of specific phase and amplitude. The other slit likewise projects a different composition and hence additional resultant to p_1, which we can represent with primed values. These two resultants in turn sum to a new resultant at p_1 of specific stable phase and amplitude relative to the original source. CS is a constant, and only a spatial mixer. The same process happens at other points p_i and in a disorderly way, because the superposition at each such point combines many irregularly diverse waves. It approximates a pseudorandom collection, so the previous orderly pattern of fringes is gone because amplitude squared tends to average out across the screen. *In the usual context* the light reaching #2 is irreversibly decohered. If we switch to photon shots and use a fine detector array, the pattern of click locations would no longer reveal self-interference of single photons. (Should that matter? We'll see.)

However, what illuminates screen #2 is still comprised of a definite pattern (this time, defined generally), however messy, of *unchanging* phases and amplitudes in superposition (requiring good temporal coherence from PS.) Simply put: there is a definite, constant relative phase and amplitude at each point of screen #2. So again, each slit of DS_2 projects according to the definite phase and amplitude of the resultant wave it intercepts. Surprisingly, each slit still serves as a new, reasonably "coherent" source. It might require very narrow slits to get usefully consistent wave sources - but all we need do is show a distinction, not pretty or rapid results. Hence, there should be an interference pattern at screen #3, where in CI the photon's journey (to the extent imaginable at all) at last ends in one small place. This final pattern would be even more attenuated than without CS, with even less contrast - but in principle and practice it should be there.

How would DI predict the results of this experiment? We should say, that CS causes decoherence of the simple superposition propagating from DS_1. Loss of interference in itself "turned the superposition into a mixture" of isolated photon "hits" here or there (via semantics?), as in the supposed explanation of why we get specific clicks in the DMZI. We might argue over how complete the loss of interference was, but that's an overall problem anyway'

How should DI specifically imagine the encounter between this presumptive, quasi-classical mixture and DS_2? We can agree that tiny "genuine" (but in DI "not special") detectors put at slits would click in typical exclusionary manner. But DI doesn't grant them special collapsing power. If decoherence is why we *would* get clicks at DS_2, then either slit (but not both at the same time) should act like a re-radiating detector hit. If the reader again finds that unconvincing because there's "nothing there" at a slit, then (as in TE2) we can use little glass windows or even *very* fine (finer than for CS) translucent material in each slit. Again, such materials effectively absorb and re-radiate, and of course do not foil interference experiments. So then, either each such altered slit should pass along averaged waves as described for CI, or instead in DI what *would have been* a true exclusionary impact because of decoherence. In DI, one or the other slit then becomes a new, exclusive source. Note that even if we imagine a post-decoherence yet undetected photon (hard to visualize in DI anyway) still extended enough (a "packet" with sufficient coherence volume) to encompass both slits, it still effectively comes from different directions out of CS – or there's no point in claiming ED makes a difference. In any case, in DI we expect no interference pattern at screen #3. By contrast, in CI each slit becomes one of a pair of *concurrently* radiating sources, because the special collapse doesn't happen there. Slits, even with transmitting inserts, just aren't "measuring devices": understand or like that or not. These two approaches offer conflicting predictions (to the extent that DI even provides a clear line of reasoning.) They aren't mere differing "interpretations" of the same facts. We expect outcomes to follow the CI practice of imagining a full, evolving superposition up to the moment a "genuine detector" can give a reading. Now we have another "Bell moment." TE3, like TE2, casts doubt on DI. The expected outcome would arguably falsify it.

VI. Can We Know What It All Means?

The failure of the decoherence interpretation (in this author's opinion, on so many levels) means that we must come face to face with the stark reality of "the quantum measurement paradox." The proper use of results from interference creates a *model* of a spreading and evolving wave. It doesn't matter to the *continuing* validity (and difficulties) of the model, whether interference happens that time or not; or whether orderliness is maintained. This wave grows to encompass potential detectors at great mutual separations. If one of them somehow, "finally" makes record of receipt of the particle represented by the wave, then the others can no longer do so no matter how far away they are. Furthermore, there seems no matter of fact in principle about that wave or the detectors, that decides which of them will respond. This behavior is not deterministic, and it just doesn't "make sense." Because it is not deterministic, there can be no true mathematical model showing what happens. Perhaps we live in a Kantian contextual world, perhaps it all involves awareness and knowledge, but we don't really understand this. I think it is better to face up to this situation honestly. We should not try to evade and elude it with dubious ideas because it is inconvenient or hard to accept. I have no answer to offer here, except that we should accept that there is no answer right now. I once had a dream, of being one of a band of horsemen of the Steppes chasing a white dodecahedron. (Yes, like the one [7] I later read about in *Shadows of the Mind* by Roger Penrose.) The elusive object would at first sit still on the grass. As we came near, it turned into a fabulous white bird and flew across the plain. The bird landed far away, turning back into that enigmatic shape, for us to fruitlessly chase again and again. I believe that dodecahedron is our reality. It is something we seek and may never catch.

References

1. Feynman, Richard; *The Character of Physical Law* (The MIT Press, Paperback Edition, Cambridge MA, 1967), pp. 147-148.

2. Greenstein, George; Zajonc, Arthur; *The Quantum Challenge: Modern Research on the Foundations of Quantum Mechanics* (Jones and Bartlett, Boston, 1997), pp. 169-171.

3. Orzel, Chad; "Many-Worlds and Decoherence: There Are No Other Universes" (http://scienceblogs.com/principles/2008/11/manyworlds_and_decoherence.php, posted 11/20/2008, verified last page update 2/16/2011.)

4. Herbert, Nick; *Quantum Reality: Beyond the New Physics* (Anchor Books paperback edition, New York, 1985), pp. 154-155.

5. Brune, M, *et al*; "Observing the Progressive Decoherence of the "Meter" in a Quantum Measurement," Phys. Rev. Lett. 77, 4887–4890 (1996.)

6. Bates, Neil; "Decoherence Interpretation Falsified?" (http://tyrannogenius.blogspot.com/2009/12/decoherence-interpretation-falsified.html, posted 12/21/2009, verified last page update 2/16/2011.)

7. Penrose, Roger; *Shadows of the Mind* (Oxford University Press, Oxford, 1994), p 240.