Re: Superposed observers (was No new Einstein)

Source: http://sci.tech−archive.net/Archive/sci.physics.research/2005−10/msg00037.html

- *From*: "I.Vecchi" <vecchi@xxxxxxxxxxxxx>
- *Date*: Sat, 1 Oct 2005 23:23:29 +0000 (UTC)

rof@xxxxxxxxxxxx wrote:
> "I.Vecchi" <vecchi@xxxxxxxxxxxxx> writes:
>
> >rof@xxxxxxxxxxxx ha scritto:
>
...
>
> >No difference here. Coin−tossing and Geiger counters do the same
> >quantum job.
>
> When a coin is tossed, the side on which it will land is mostly
> determined by the initial conditions.

The point is that the initial conditions are not known to the observer.
If they are known, then the process is no longer random. And any
chaotic process will quickly blow initial quantum scale
indeterminacies up to macroscopic scale ([2]).

Others may believe, unlike me, that physical instances of probability
come in two kinds, those with a quantum soul and those without.

...

>
> However, it isn't normalised. If I set alpha=1, for example,
> then my calculator gives the squared norm of the state vector
> to be (0.5*((1/sqrt(2))+1))^2+(0.5*((1/sqrt(2))−1))^2+(1/sqrt(2))^2=1.25
>
> Now the probability that the photon is absorbed should be 1/4, so
> you can solve the normalisation problem and the absorption
> problem by making the final state
> 0.5[(alpha/sqrt(2))−1)i|2> − (alpha/sqrt(2))+1)i|1>] + i|absorbed>/2
> instead.
>
> Is this what you intended?

Indeed, as you point out sqrt(2) should be replaced by 2 , but since
this affects only the fraction that is absorbed it is irrelevant to the

final result at the detector.

>
> >> Also, you say that the half photon which followed the lower
> >> path combines with the quarter photon which made it past
> >> the beam stopper on the other path. However, the quarter photon
> >> which made it past the beam stopper can only do so if
> >> the stopper is in the state |SA>, so its state is something
> >> like |SA>|photon state 1>, while the photon travelling along
> >> the other path doesn't care about the state of the stopper,
> >> so its state is something like (a|SA>+b|SB>)|photon state 2>/sqrt(2).
> >> So you can't just add |photon state 1> to |photon state 2>
> >> because they are multiplied by different beam stopper states.
>
> >This is indeed the crucial point.
> >When you say that the photon's states "are multiplied by different beam
> >stopper states" you are falling into a misunderstanding that mars large
> >chunks of current QM. Entanglement is an observer−dependent property of
> >the measurement process ([3], cf.[4]). Roughly speaking, prior to
> >measurement the photon is merely a ripple on the wave−function. When we
> >say that a particle is entangled we are saying that some measurement
> >outcomes entail certain constraints on other measurement outcomes.
> >However, this is a property of the measurement process, not of the
> >wave−function. As long as no information is extracted by the observer
> >(i.e. as long as evolution is unitary) entanglement is an empty concept
> >and all photons can be summed and subtracted, if you can track their
> >phases.
>
> Okay, so let me just check that you agree with the following
> points:
>
> 1. My calculation, which indicates that the probability that the
> detector will click 1/8 of the time (and not 3/8) is correct,
> according quantum mechanics as presented in standard quantum mechanics
> textbooks.
>
> Yes/No?

When I give an answer I choose the format.

I would be grateful if you could provide references to the textbooks
you mention.
Do they provide an explicit definition of entanglement? Do they discuss
whether it is a property of the wave−function or of the measurement
process? Based on my experience, entanglement is often introduced
handwavingly and the conceptual issues involved are ignored. Your usage
of the entanglement notation in this case leads to conclusions that
in my opinion are testably erroneous.

>
> 2. Your assertion is that quantum mechanics, as presented in
> textbooks, and the current treatment of entanglement in
> particular, is incorrect and needs to be modified according
> to your prescription.
>
> Yes/No?

Again, please provide references to textbooks containing an explicit definition of entaglement and a discussion thereof. Otherwise we are talking about nothing.

>
> 3. Your proposed modification of the notion of entanglement
> asserts that, as long as the observer doesn't receive any
> information from the system, "entanglement is an empty concept
> and all photons can be summed and subtracted, if you can track their
> phases." This has the following consequence for the two−slit
> interference experiment: If a detector is placed over one of the slits,
> so that it records which slit the particle passed through, *but*
> the observer doesn't extract information from the detector, then
> the particles will form an interference pattern. The standard
> answer − that no interference pattern will be formed because
> the particle's state is entangled with the detector, is wrong,
> according to you, because "entanglement is an empty concept".
>
> Yes/No?

Not really. Roughly speaking , interaction with the detector will fudge the interference pattern anyways, but the "marks" it leaves are just ripples on the wave function as long as they are not interpreted by measurement.

>
> 4. Presuming that you agree with point 1 above, standard quantum
> mechanics with the usual notion of entanglement would predict
> that your experiment can not detect superpositions. Your modification
> to the notion of entanglement must be made in order to deduce that
> the experiment can detect superpositions.
>
> Yes/No?

Again, as far as entanglement is concerned , I am not sure what you mean by "Standard quantum mechanics",unless you provide references. As I said previously, entanglement is widely misunderstood. Misunderstandings breed mistakes.

>
> >> ... essentially it comes down to your claim in quant−ph/0007117
> >> that it is possible to distinguish between cases (a) and (b) above.

> >>
> >> As I said, I believe there's a calculational error in that paper and
> >> that in both cases the detector clicks 1/8 of the time, so it
> >> can't be used to detect superpositions.
>
> >I am confident that the calculations are correct.
>
> Confidence can be measured by wager.

> I think it's very unlikely
> that you would be able to get a paper published in, say,
> Foundations of Physics, in which you present this experiment
> and claim that it can detect macroscopic superpositions. How
> much would you be prepared to bet?

I bet only for fun and on outcomes that I regard as intrinsically
interesting.
I may bet on experimental results related to the model we are
discussing, which might be available soon (cf. my closing remarks in
[3])

>
> >In order to
> >facilitate verification however, I will provide a rationale for the
> >result.
> >The key point is very simple. The shutter on the upper photon induces a
> >random phase change, which destroys any interference pattern between
> >the upper and the lower photon. This is quite intutive and is actually
> >proven at the bottom of page 5 of [1].
>
> It's certainly not intuitive for me.

> I can't find a proof of it
> at the bottom of page 5. I just find assertions that it's true.
> I would go so far as to say that it's not true, and that you
> have made a mistake here. Can you present a proof of your assertion
> using equations?

It's the calculation of Pb(D) at the bottom of page 5, where the
photon's probability at the detector is obtained by averaging the
phases over [0, 2pi], where they are equidistributed,.

>
> >At the upper beam−splitter you
> >have an incoming upper photon with probability 1/4 and and incoming
> >lower photon with probability 1/2. Since they do not interfere, after
> >being halved passing through the beam splitter , they will yield a
> >1/4+1/8=3/8 probabilty at the detector. In the non−superposed shutters

> >situation the phases stay put and you get cancellation at the upper
> >beam−splitter, so the result is 1/8.
>
> This relies on the assertion that photon undergoes a random phase
> change when passing by the absorber.

Yes. More in general, on the assetion that interaction with the
absorber will destroy phase coherence with the other instances of the
photon.

>
> >Your calculation is based on an interpretation of entanglement that I
> >consider erroneous, although it may provide a handy rule of thumb in
> >trivial situations.
> >I have tackled the issue of entanglement in my post [2] (cf. [3],[4]).
> >Indeed the whole procedure I am proposing depends on the fact that
> >entanglement is an observer−dependent property of the measurement
> >process, not of the wave function, i.e. as long as evolution is
> >unitary, entanglement has no object.
> >In the setting we are discussing that implies that prior to measurement
> >all photons can be summed and subtracted, as long as their phases can
> >be tracked.
>
> I can't find anything in [2] or [3] which actually presents any
> concrete mathematical alternative to entanglement, while [4]
> seems to merely introduce a notion of "generalized entanglement",
> without actually claiming that any of the standard uses of
> entanglement give incorrect answers, which is what you are claiming.

Standard uses ... missing references ....common misconceptions ... .
I think that what I have written about entanglement is sufficient to
motivate my argument in the current setting.

...

>
> Probability distributions can be entangled, too. That is, not
> all probability distributions P(x,y) can be factorised into
> P1(x)P2(y). The analogy of unitarity for probability distributions
> is that the total probability should always add up to one, or
> we might say that the time−evolution operator for probability
> distributions should be "orthogonal". Would you say that
> introducing entanglement of probability distributions within
> an orthogonal process leads to testably erroneous results?

If you base your argument on an erroneous understanding of
entanglement, yes, definitely.

>
> >> Now, from Alice's point of view, if she measures along the x−axis,
> >> then, since she adopts the Copenhagen interpretation (which gives

> >> the same experimental predictions as anybody's favourite interpretation,
> >> and which she is entitled to use), she considers that Bob is receiving
> >> particles which are _either_ |up_x> or |down_x> and not a superposition
> >> of the two. Hence, if she measures along the x−axis, then she can
> >> say with certainty: "Bob's results, whatever they are, will be
> >> consistent with the results that he would get if he was receiving
> >> a beam of particles, each of which is in either |up_x> or |down_x>.
> >> That is, Bob's beam absorber is _either_ in the state |SA> or
> >> in the state |SB>."
> >>
> >> On the other hand, if Alice measures along the z−axis, then
> >> Bob (as far as Alice is concerned) is receiving particles
> >> with a well−defined value of z−spin. That is, Bob will receive
> >> particles whose states are either |up_x>+|down_x> or
> >> |up_x>−|down_x>. Consequently, Bob's absorber will end up
> >> in the states |SA>+|SB> or |SA>−|SB>.
> >>
> >> Notice the crucial point − Alice can *control* whether Bob's
> >> absorber is in a superposition or not.
>
> >This is meaningless.
> >In your setting Alice cannot control whether the system is in a
> >superposition or not in Bob's perspective.
>
> She doesn't have to control Bob's perspective. My point is
> that after she measures the state of her particle, she assigns
> a definite quantum state to Bob's particle.

When she measures the state of her particle she splits , in Bob's
perspective, into different instances measuring different states.
It should be clear that in an epistemic model there can be no such
thing as a "definite quantum state". Any state is relative to an
observer.

> When Bob shows her
> his results long after the experiment is over, his results
> will be consistent with the state that she assigned.

When she assigned, she split. Is that clear?
You can raise the same objections about Nick tossing coins and moving
the shutters accordingly.

> If she
> measures her particle along the z−axis, she will say "Bob's
> particle has a definite z−spin".
>
> When she meets Bob, and sees what he did with that particle,
> she will say "Oh yes, that particle − it had a definite

> spin along the z−axis. I know that because I measured the
> z−spin of its partner particle." Bob's results will be
> consistent with his particle having a definite z−spin,
> or else quantum mechanics fails. If quantum mechanics
> doesn't fail, then Bob's results *must* be consistent with the
> quantum state that Alice assigns to Bob's particle. This
> is because Alice is using quantum mechanics, and if quantum
> mechanics doesn't fail, then Alice's predictions must be
> correct.
>
> Similarly, Alice can measure the spin of her particle along
> the x−axis. Then she will say that Bob's particle has a
> definite value of spin along the x−axis − that is, that
> it is in a superposition of z−spin states. Bob's results
> will then be consistent with his particle having a definite
> value of spin along the x−axis.

Do you realise that when you use the word "can" you are referring to Alice's free will? And that such a process will put her in a superposition in Bob's perspective?

> If there is any possible experiment that Bob can do
> to detect whether his particle has a well−defined z−spin,
> rather than a well−defined x−spin, then Alice can signal
> to him faster than light. From Alice's point of view,
> Alice can control whether Bob's particle has a well−defined
> z−spin or a well−defined x−spin.

No, she can't. You neglect the fact that Bob's input in my experiment is determined all the instances of Alice, not just the one he will end up talking to.

>
> Your interferometry experiment, if it worked, would allow
> Bob to tell whether the incoming particle had a well−defined
> z−spin or a well−defined x−spin.
>

Your conclusion is erroneous for the same reasons that I explained in my previous post ([3]).

Cheers.

IV

[1] http://arxiv.org/abs/quant−ph/0007117
[2] "Newtonian Chaos + Heisenberg Uncertainty = macroscopic indeterminacy" by Barone, S.R., Kunhardt, E.E., Bentson, J., and Syljuasen, A., American Journal of

Physics, Vol 61, No. 5, May 1993.
[3]
http://groups.google.com/group/sci.physics.research/msg/966e6d487f7a3f3c?hl

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- ***Follow−Ups***:
    - ***Re: Superposed observers (was No new Einstein)***
        - *From:* rof

- Prev by Date: ***Re: Why do we need quantum fields in QED?***
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