Re: Superposed observers (was No new Einstein)

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- *From*: rof@xxxxxxxxxxxx
- *Date*: Tue, 27 Sep 2005 18:39:11 +0000 (UTC)

"I.Vecchi" <vecchi@xxxxxxxxxxxxx> writes:

>rof@xxxxxxxxxxxx ha scritto:

>>
>> So that you can get an idea of how this sounds from my point of view,
>> imagine that you toss a coin and look at the result, but don't
>> show me. The result might be tails (for example), and you would
>> know that but I wouldn't. If I were to say to you, "Relative
>> to the Italo who saw heads, the coin landed on heads, but
>> relative to the Italo who saw tails, the coin landed on tails,"
>> then you might think I was a little odd.

>Well, that's precisely the example I provide in my paper [1], where my
>model is proposed.
>I hope you appreciate the difference between "odd" and "provenly
>wrong".

Imagine if I said to you, "I'm going to pretend that a particular collection of supersensible undetectable entities exist, and nobody can prove me wrong because they're undetectable. I'm going to talk about them as though they're really there, and I invite other people to do the same."

This is what is done by people who espouse Bohmian mechanics or parallel worlds. "Oh, it all makes so much sense!" they proclaim. I could use the same procedure and claim that Zeus and Thor both exist and nobody could prove me wrong. I could claim that the existence of these gods makes so much sense and that quantum mechanics can only be fully understood from that standpoint. Thor is responsible for unitary evolution and Zeus takes care of wavepacket reduction, I might declare.

However, I have decided that this is not likely to be a fruitful approach to take to the question of understanding quantum mechanics. As you are very well aware, a hypothesis about the existence of undetectable entities isn't considered science even by those whose understanding of science is restricted to the kind of kindergarten philosophy of science prevailing today, with its profound understanding of concepts like falsifiability. Being impossible to disprove is seen as a disadvantage of a hypothesis rather than an advantage.

In any case, this kind of procedure, which does not concern itself with the prediction of results of experiments, but instead makes hypotheses about what "really exists" "in reality", as opposed to what is to be encountered in the course of experience, is exactly the kind of speculation about the noumenon that Kant proved to be fruitless.

Also, the strong form of the many−worlds interpretation, which asserts that the entire universe that we see, along with all the other parallel universes, is encoded in the Hamiltonian of the universe and the quantum state of the universe _is_ demonstrably wrong. Even the dimensionality of space can't be reconstructed from the Hamiltonian and state vector alone.

>> You might say that it does nobody any harm to talk this
>> way. I would say, though, that it hampers communication
>> and leads to confusion when purely hypothetical objects
>> are spoken of as if they genuinely exist in a non−hypothetical
>> way.
>>
>> You might also say that there's a difference between
>> the coin tossing case and the Schrodinger's cat case,
>> since interference between the two scenarios can in
>> theory be detected in the second case, and so we
>> have more of a reason to consider each of the two
>> scenarios to be "real". I would have to go back to
>> what I said before, then, which is that any
>> experimental arrangement which can detect interference
>> of observers and live/dead cats would also be able to
>> resurrect cats and alter memories, and would thus be such
>> an invasive procedure that one could not afterwards
>> say what had genuinely happened on the basis of
>> what the observer remembered or whether the cat was
>> found to be alive or dead.
>>

>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. You might say that there's a small quantum amplitude that it will land on the other side, because there's some non−zero amplitude that it will depart significantly from its classical path ("tunnelling"). You could say the same thing about almost anything. If I were inclined to adopt your practice

of pretending that every outcome really exists, then I could talk
about the Italo whose computer just exploded, or the Italo who just
grew to be seventy feet tall before glowing pink and then returning
to his normal size. The amplitudes for these events is exceedingly
small, but non−zero.

>> If you do actually believe that, then you wouldn't be able
>> to have a control experiment. For a control experiment, you
>> have to measure the same observable, but for a differently
>> prepared system.

>All I have to show is that the experimental procedure yields
>different measurement outcomes depending on whether the shutter is in a
>superposition or not, but I think in this discussion we have been
>dragging along some confusion about what we mean by initial states.
>Note that I am actually conducting two different experiments. In the
>control exeriments I do not use the Geiger counter. What I test/show
>is that if you use a Geiger counter the results are different than if
>you close the shutter by hand half of the times before the experiment.

Yes, and I was saying that if you had an observable which you could
measure which could distinguish between a shutter in a superposition
and a shutter which was prepared by hand, then measuring that
observable would put the shutter into a superposition (even if it
wasn't in one to begin with), at least some non−zero fraction of
the time. You are insisting that your experimental set−up can detect
a superposition of shutters without having the property that I describe,
so it all comes down to the question of whether your experiment
can genuinely detect superpositions or not. I argue below that
it can not.

>> You prepare the beam stopper in the state (a|SA>+b|SB>)/sqrt(2),
>> where |a^2|=1, and then claim that in this case the probability
>> of the detector clicking is 3/8, and from this conclude that
>> the experiment can detect the difference between the superposition
>> (a|SA>+b|SB>)/sqrt(2) and the statistical mixture of 50% |SA> and
>> 50% |SB>.
>>
>> The crucial step on which your calculation yielding 3/8 relies,
>> however, appears to me to be incorrect. You say: "In this situation
>> the photon crossing the superposed absorber undergoes a random phase
>> change induced by the coupling between the photon's and the absorber's
>> states." After this, I'm not sure what you do. Your
>> expression for the final state of the photon doesn't appear
>> to be normalised, and I think the analysis needs to be repeated
>> taking into account the fact that the state of the photon is
>> entangled with the state of the beam stopper.

>I am confident that the calculations are correct. As for entanglement,
>see below.

>>
>> As an example of something which has gone wrong, according to
>> your expression for the final state of the photon, the probability
>> that it has been absorbed by the beam stopper is 50%,

>I cannot make sense of your point here. Please elaborate. There is
>obviously the second beam−splitter that has to be taken into account.

On page 5, you write the final state of the photon as:

0.5[(alpha/sqrt(2))−1)i|2> − (alpha/sqrt(2))+1)i|1>] + i|absorbed>/sqrt(2)

where alpha = cos(theta) + i sin(theta) for some unknown theta.

If this were a normalised state, I would infer that the probability
that the state of the photon is |absorbed> is (1/sqrt(2))^2=1/2.

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?

>> 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?

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?

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?

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?

>> ... 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?

>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?

>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.

>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.

>> Here's another reason why it's impossible to detect superpositions
>> that way. Your claim is that you can distinguish between a series
>> of states (a_n|SA>+b_n|SB>)/sqrt(2) where the a_n and b_n are
>> random complex numbers of modulus 1, and a series of states
>> each of which is |SA> or |SB>, with 50% probability of each.
>>
>> The reason that I have subscript n on a and b is because, as you
>> say, the relative phases are uncontrollable, and hence must
>> be different each time you prepare the system and make one of
>> your weak measurements.
>>
>> Now, I claim that if you can do the above, namely distinguish between
>> the cases (a) and (b) which you described above through any series
>> of measurements, then you can signal faster than light.
>>
>> Proof: Let the superposition (a_n|SA>+b_n|SB>)/sqrt(2) arise
>> in the following way. Alice and Bob, situated far away from one another,
>> receive spin−half particles in the singlet state
>> (|up>|down>−|down>|up>)/sqrt(2). Alice can measure the spin of
>> her particles along the z−axis or along the x−axis.
>>
>> Bob allows his incoming particle to interact with the beam absorber
>> so that if the x−spin of the particle is up, the beam absorber does
>> not block the beam, while if the x−spin is down, the absorber does
>> block the beam.
>>
>> That is, Bob allows the incoming particle to interact with the
>> absorber with an interaction which implements the following:
>>
>> |up_x>|SA> −−> |up_x>|SA>
>>
>> |down_x>|SA> −−> |up_x>|SB>
>>
>> (It is an exercise in quantum mechanics to show that the above can
>> be implemented with a unitary time−evolution operator.)

>As I said, in this context I regard this notation as meaningless. Since
>entanglement is a property of the measurement process, introducing it
>within a unitary process leads to results that, if I am right, are
>testably erroneous.

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?

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

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.

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.

Best,
R.

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- ***References***:
    - ***Re: Superposed observers (was No new Einstein)***
        - *From:* I.Vecchi
    - ***Re: Superposed observers (was No new Einstein)***
        - *From:* I.Vecchi

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