Re: Virtual Particle confusion

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From: PD (*pdraper_at_yahoo.com*)
Date: 02/28/05

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Date: 28 Feb 2005 09:35:28 −0800
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Ranando King wrote:
> "PD" <pdraper@yahoo.com> wrote in message
> news:1109365094.518160.42090@l41g2000cwc.googlegroups.com...
> > Ranando King wrote:
> > > "PD" <pdraper@yahoo.com> wrote in message
> > > news:1109354953.319263.10300@l41g2000cwc.googlegroups.com...
> <snipped>
> > Take it as you wish. Your terminology suggested that anybody with
any
> > common sense would see things your way, which I found to be
insulting.
>
> Certainly not what I intended. So I appologize for my wording. What I
> intended to say was that if you take someone new to the field, and
introduce
> them to the basics required to begin learning QM, then they will come
in
> with the same understanding. A machine with known states has no
random
> elements.
>
> > >
*> > > > Temperature of a body is not even *defined* without random*
kinetic
> > > > motion. If the motion were not random, temperature is not a
proper
> > > > description.
> > >
> > > How so? The word "random" is neither stated nor implied in the
> > definition of
> > > temperature. The motion of the particles of a substance whos
> > temperature is
> > > being measured is not random. If you know the postition,
orientation,
> > > velocity (speed & direction), mass, charge, and necessary outside
> > factors
> > > like boundary conditions on the container of all the particles at
any

> > > selected point, you can predict the motion of every individual
> > particle both
> > > forward and backward in time using a painfully long series of
> > simultaneous
> > > equations.
> >
> > Yes, but diffussion is NOT a reversible process, even if you can
> > account for each collision microscopically. Why not???
>
> See the 2nd law of thermodynamics.... Energy prefers a high entropy
state.

And in terms of the microscopic, reversible depiction of the substance,
please explain how to define the high−entropy state.

>
> > Moreover, you are wrong in saying there is no difference between
random
> > and non−random kinetic energy. The second is associated with, e.g.
> > macroscopic translational energy. The first is associated with
> > temperature. There is limited ability to extract the latter from
the
> > former −− that limit is what Carnot discovered and marks the line
in
> > the sand.
>
> Carnot "discovered" what people in the cleaning industry have known
for
> centuries. We have a limited ability to clean up a mess when we don't
know
> what's in the mess.

Actually that's not what he said. He said a specific fraction of the
energy is extractable, even theoretically. This is *not* based on
limited knowledge of the mess.

> To map this onto your previous statement, the mess is
> the quantum soup that actually comprises the closed environment we're
trying
> to extract energy from. Until we understand all possible interactions
and
> their results, we cannot possibly hope to extract 100% of the energy
> contained within that environment. Put another way, until we know
quantum
> mechanics well enough to begin dropping the use of statistics to
define
> behavior, we will never be able to use the immense pool of enery in
the
> universal background radiation to do anything useful.

And this reveals your misunderstanding of thermodynamics. Even theoretically, and based on arguments that have *nothing* to do with quantum mechanics (Carnot and Gibbs lived long before anyone knew anything about QM), not all energy can be extracted from the "immense pool" of thermal energy.

>
> > I don't want to push the analogy too far −− the randomness of
quantum
> > mechanics is different than the randomness of a thermal system.
> > However, you said that randomness is *only* a lack of information,
a
> > point that I disagree with. Randomness implies physics on its own.
>
> Funny how such a minor statement like "random = unknown" can have
such a
> large effect on things, isn't it? But again I posit, just because we
have
> not yet found a pattern that fits, does that imply that there isn't a
> pattern?

Of course not, but that's different than saying, "The pattern that physicists use makes no causal sense to me, and so there must be a different pattern underlying that one that does make causal sense to me."

> Same question in different words is: just because Einstein's
> equations work without considering aether, does that imply aether
doesn't
> exist?

Of course not, but the presence of the aether should have testable effects that distinguish it from an aetherless description. Many such models have indeed been proposed, tested, and shown to not bear on reality. You might ask whether an aether can exist *anyway* but be *wholly indiscernable* experimentally from an aetherless model. The answer there is Occam's razor: if there is no need for it, and there is no testable verification of it, then it should not be included in any description of nature.

> Or yet another way: just because we cannot connect QM with the
> standard model, does that mean that there's no solution?

I don't know where you got the idea that QM has a disconnect from the Standard Model. The Standard Model is *entirely based on* QM −− there is no disconnect. Perhaps you are thinking of gravity, which is *not* described by the Standard Model?

>
> > Temperature is a *statistical* property. If you think otherwise,
tell

> > me how to find the temperature of a single particle or a pair of
> > particles.
>
> If you really want the answer, just do this:
>
> T = (3868mv^2)/1.602E−20
>
> where m is the mass of the particle and v is the speed. The
statistics
> involved in taking temperature shortcut the need to know the mass and
speed
> of each individual particle and molecule. As I've stated before,
statistics
> are used to acquire results when all the info to perform hard
calculations
> is not available.

And this reveals your lack of understanding of thermodynamics. Temperature is not *defined* for a system that is not in equilibrium. One or two particles cannot be in equilibrium. You are taking a formula for the *average* kinetic energy of particles in a thermodynamic equilibrium and improperly extrapolating that to a system that is not, and cannot be, in thermodynamic equilibrium. This is a crucial point in thermodynamics.

>
> <snipped>
> > > The HUP merely states that we change the
> > > properties of those things we observe as we observe them, so it's
not
> > > presently possible to know all of the properties of something
we're
> > trying
> > > to observe.
> > >
> > > If you don't understand that this is admitting that we can't know
all
> > the
> > > information, then I feel I'm not the one to explain it to you.
It's
> > not
*> > > Heisenberg's *RANDOM* Principle. It's Heisenberg's *Uncertainty**
> > > Principle.... Uncertainty, as in not sure of. Just because you're
not
> > sure
> > > of a value doesn't mean that the value is actually random.
> >
> > Nuh−uh. The quantum correlations between distant electron spins
points
> > to the fact that there is more going on here than the Clumsy Thumbs
> > model of HUP.

>
> *Exactly, now just apply that same line of thinking to ALL of QM...*
anywhere
> *you find statistics instead of hard calculation.*

Nonsense. QM says that the hard calculation *must be done* using the
statistical nature of objects that small. An alternative that does not
do that has a testable difference from QM predictions; in tests, QM
wins.

>
> > >
> > > > *Moreover, entropy is a *measure* of the randomness of a*
> > > > *system. The 2nd law of thermodynamics is specifically a*
statement
> > *about*
> > > > *causal ordering and entropy.*
> > >
> > > *Entropy... Chaos−theory... the basic principle that all things*
seek
> > *to fall*
> > > *apart... the idea that things prefer to go from structured states*
> > *(high*
> > > *energy) to unstructured (low energy) is perfectly valid. It's*
just
> > *the old*
> > > *concept that "nature abhores a vacuum" repeated. It's far easier*
for
> > *the*
> > > *universe to maintain a state of constant, unilaterally equal*
> > *potential (high*
> > > *entropy) than it is to maintain clustered, high potential regions*
> > *with*
> > > *low−to−no potential areas inbetween (low entropy). That follows*
> > *common*
> > > *sense.*
> > >
> > > *The second law of thermodynamics simply says that you can't move*
> > *energy in a*
> > > *closed system from high entropy to low entropy.*
> >
> > *Define "high entropy" and "low entropy".*
>
> *High entropy: Highly consistant, unilateral energy state. Think messy*
> *bedroom where you're just as likely to find a shirt on the light*
fixture as
> *anywhere else in the room.*
>
> *Low entropy: Highly organized, inconsistent energy state. Think clean*
> *bedroom where there's a place for everything and everything is in its*
place.

I'm looking for a more rigorous definition, not something that is based on a loose analogy. In particular, I'm looking for your definition that works in a microscopically deterministic model (any one).

I'm obviously calling your bluff here. You are asserting that statistical definitions are based on lack of information of the "true" deterministic model, and yet you say that entropy is a well−defined quantity for a true, deterministic model. I'm asking for a clear definition from you that is consistent with that.

>
> > > Treating entropy as a
> > > measure of randomness is a serious mistake... a mistake large
enough
> > to keep
> > > the obviously deterministic macroscopic universe from being
> > understood on a
> > > sub−atomic scale. Entropy is a measure of the lack of isolated
> > structure.
> > > Think of it this way. Suppose a universe as large as our own
existed
> > having
> > > no virtual particles, and no other energy outside of a single,
> > stationary
> > > electron. That universe would have very low (near 0) entropy
because
> > all of
> > > it's energy would be confined to a relatively very small space.
> > Suppose now
> > > that another universe existed that was just like the previous one
I
> > > described save for its size being infinitesimally larger than the
> > volume of
> > > the electron it contains. That universe would have an
exceptionally
> > high
> > > (near infinite) entropy because its energy would be spread out
fairly
> > > consistently across its entire volume.
> >
> > And that would be wrong. Your values of entropy in both cases are
> > wrong. Count allowable states.
>
> Ok. :)
> You got me on semantics with that one, but it's easy to fix the
example,
> replace the electron in the first universe with a black hole the size
of our
> solar system. Just for the sake of argument, this black hole isn't
radiating
> its energy away. Make the second universe to be infinitesimally

larger than
> our solar system but scatter all of the energy of the black hole
around that
> universe evenly as background radiation.

I'm sorry, I don't follow your example here. And I don't know what you mean by "semantics".

>
> > >
> > > Neither case showed any "random" properties. That's because
> > randomness
> > > doesn't belong in the description of entropy, or for that matter,
> > anything
> > > else. (Note the example given uses an assumption that an electron
is
> > *NOT* a
> > > point particle.)
> > >
> > > >
> > > > By the way, the great thermodynamicists of the 19th century
also
> > ran
> > > > into resistance from the determinists, who maintained that such
> > > > descriptions of randomness were abdicating the search for the
true,
> > > > underlying nature of things. In fact, it turned out that
entropy IS
> > the
> > > > true, underlying nature of things.
> > >
> > > > > Since
> > > > > quantum physics is merely the physics of states for
sub−atomic
> > > > things, then
> > > > > you're left to assume that all of these "random" sub−atomic
> > states
> > > > tend to
> > > > > add up to a stable atomic state. That doesn't work unless
each
> > > > "random"
> > > > > state tends toward particular values most of the time.
> > > >
> > > > No, and you don't know how quantum summation over all possible
> > > > histories works. I suggest you read a little book by Feynman
called
> > > > QED, that describes this process better −− no hidden
tendencies, no
> > > > underlying variables, no masked causes.
> > >
> > > If you truely understood the summation yourself, you would see

that
> > *it in*
> > > *fact is itself a method of hiding causality and looking only at*
the
> > > *outcomes.*
> >
> > *Not sure I know what you mean by this. Where is causality hidden?*
> > *The quantum interference is part of the causality!*
>
> *Quantum interference is part of the math, not the causality. There is*
no
> *reasonable, logical, or rational way to assume that all the quantum*
states
> *of each possible permutation of that particle interfere with each*
other to
> *form some coherent result without assuming that the particle can*
exist in
> *every possible state simultaneously.*

The statement, "There is no reasonable, logical, or rational way to
assume..." is precisely what I question. That is EXACTLY what QM
posits. To assert it is impossible is unjustified unless you can point
to
a) a theoretical self−inconsistency (the theory is not mathematically
or conceptually self−consistent);
b) a clear contradiction with experimental facts.
Violation of common sense is NOT sufficient grounds for discarding.
Many people thought that constant velocity without an applied
propulsive force was incompatible with common sense. Many people
thought that the Earth orbiting the sun was a violation of common
sense. Many people thought that observer dependence of simultaneity was
a violation of common sense.

> *That implies that the particle would*
> *have to have the energy of every possible state simultaneously. Not*
> *Possible. (I know, qm states that it is.)*

And why is it not possible? Note that energy is an observable quantity.
You have to be sure you understand how to calculate the energy (or for
that matter of any observable quantity) of a sum over all histories −−
it is NOT the algebraic sum of the energies of every possible state.
This may be what's standing in your way.

>
> > > *When it comes down to it, that summation is a method of decision*
> > > *making, not significanly different from the process most people*
use
> > *when*
> > > *making complex decisions. Think about the thoughts flying around*
in
> > *your*

> > > mind when your spouse/significant other is upset with you, when
you
> > don't
> > > know the reason. You tend to take the action that you think will
> > cause you
> > > the least grief. You make this decision without knowing why such
a
> > decision
> > > was necessary.
> >
> > That's a philosophical and unsupported generalization.
>
> Science is predicated on philosophy. Adopt a philosophy, model it,
test the
> model, adjust the philosophical model as appropriate and try again.
Tada!
> You're practicing science!

OK, then test this analogy that QM summation is essentially identical
to the process most people use when making complex decisions. Expand
your analogy so that becomes a model that points to a test of its
truth. I find the analogy hogwash so far, in its undeveloped state. You
state the analogy as truth. I see no truth in it at all. Do some
science to develop the analogy, or abandon it.

>
> > >
> > > > > If that's true, then
> > > > > there's got to be a fundamental reason behind that tendancy.
> > Quantum
> > > > physics
> > > > > merely states that there is a tendancy and shows how strong
that
> > > > tendancy
> > > > > is. However, it doesn't even come close to explaining why
that
> > > > tendancy
> > > > > exists. Therefore, we're missing information, information
that
> > would
> > > > likely
> > > > > lead to the elimination of the dependency on statistics in
> > quantum
> > > > physics.
> > > >
> > > > You are espousing what's called "hidden variable" model of
quantum
> > > > mechanics, which may make intuitive sense to you, but you're
far
> > from
> > > > the first to think of it. But intuitive good sense does not

make it
> > > > correct, let alone unavoidably correct. Hidden variable
theories
> > have
> > > > definite experimental predictions that are testable. Read the
> > > > literature on this subject. The experimental tests fail. Nature
> > does
> > > > NOT need hidden variables and in fact is inconsistent with that
> > > > picture, as counterintuitive as that may seem.
> > >
> > > I know I'm not the first to have this idea, and I won't be the
last.
> > Think
> > > of it this way. Quantum physics cannot currently be used to
describe
> > the
> > > macroscopic universe.
> >
> > And on what basis do you make THAT statement? Is a laser a
macroscopic
> > object or not?
>
> The laser is, the photons that comprise it aren't.

Careful. Photons are only one part of the laser. (A box full of photons cannot be made to lase.) Regardless, a laser is a macroscopic object whose properties are described (in fact, based on) quantum mechanics. You made a broad statement that QM cannot currently be used to describe the macroscopic universe. I gave you an example of one where it can. You now give me a few examples of cases where it cannot, aside from purely gravitational phenomena.

>
> > >Every attempt to do so to date has failed.
> >
> > What attempts are you referring to?

Unanswered.

> >
> > > What does
> > > that mean? It means there's something we're not yet
understanding,
> > right?
> >
> > Well, THAT's true, but it doesn't say that QM is full of hokum.
>
> QM isn't "full of hokum", it's full of statistical uncertainties.

No, it's full of statistical quantities, which produce extremely precise predictions which are not verified experimentally and not predicted by any deterministic model.

>
> > > *String theory was invented to try and find that something.*
M−Theory
> > *was*
> > > *invented to try and find that something. QED, QCD, and probably*
other
> > > *theories were invented to try and find that something. All have*
> > *failed to*
> > > *date.*
> >
> > *QED and QCD have failed? How so?*
>
> *They were both part of the attempt to produce a Unified Theory.*

That is incorrect. Neither were produced as an attempt to produce a Unified Theory. QED is a quantum mechanical (strictly speaking, quantum field) description of electromagnetism, pure and simple. Nothing more is claimed of it. QCD is a quantum mechanical (quantum field) description of the strong nuclear force, pure and simple. Nothing more is claimed of it. To date, QED and QCD are both extremely well−tested models.

Since then, there have been some suggestions that an underlying theory that is also quantum mechanical can absorb QED, QCD, weak and gravitational interactions should be discoverable. It hasn't been found yet. That doesn't mean that QCD or QED or quantum mechanics are failures −− far from it.

> *To date, no*
> *unified theory that covers all known science is available.*

And that makes the pieces that are known a failure?

Does the fact that Darwinian evolutionary theory doesn't predict radioactive decay make evolutionary theory a failure?

> *Gravity is a hard*
> *target to hit.*

Indeed, no argument. What's your point?

>
> > >*Does that mean that nature does not need the physics describing*
> > > *sub−atomic stuff to be consistent with the physics for*
macrosocpic
> > *stuff?*
> > > *That's the kind of argument you just fed me.*
> >
> > *Nonsense. The macroscopic limit of every well−defined QM theory*

> > *reproduces what we see classically.*
>
> *So we've successfully explained gravity with QM? Last I checked,*
quantum
> *gravity was inconsistent.*

Inconsistent? No. If that were demonstrated, then there wouldn't be so many people attempting to make a quantum theory of gravity.

Has it been found? No, certainly not.

Does failing to account for gravity make QM a failure at describing anything in the macroscopic universe? Certainly not. Does QM presently account for everything in the macroscopic universe? No, but this is not grounds for dismissing QM.

>
> *<snipped>*
> > > > *Moreover, your prescription still does not work. Take two*
particles
> > *A*
> > > > *and B which have central mass values of, say 140 MeV and 142*
MeV,
> > > > *respectively, but which are otherwise completely distinct. The*
> > > > *invariant mass of a virtual state is measured to be 139.5 MeV.*
> > > > *According to your prescription, this would *always* be assigned*
to
> > > > *particle A. However, it is quite likely that the particle will*
> > *instead*
> > > > *be identified as particle B, based on conserved quantum*
numbers.
> > *This*
> > > > *is routinely seen in experiment, but is inconsistent with your*
> > *scheme.*
> > >
> > > *Unlike those of you that believe "random" has a place in physics,*
> > *that just*
> > > *leads me to believe that there's another term to be considered*
that
> > *is a*
> > > *factor of the original contributing masses. My first guess would*
be
> > *to*
> > > *examine the charges involved to see if there is a pattern.*
There's
> > *likely*
> > > *something in the properties of the original masses involved that*
> > *creates an*
> > > *additional limiting factor. It might be that particle A has no*
charge
> > *and*

> > > particle B does. So when the interaction occurs, if there is a
net
> > charge
> > > left behind with the 139.5 MeV, only the 142 MeV charged particle
is
> > a
> > > viable option.
> > >
> > > The bottom line: Just because a pattern hasn't been found doesn't
> > imply that
> > > there isn't one.
> > >
> > > R.
> >
> > So you are saying that charge and lepton number and QCD color and
> > baryon number contribute to mass? How so? How does your model work
to
> > put that together?
> >
>
> Not so much that they contribute to mass but rather constrain the
list of
> available virtual particles that a given invariant mass can become.

Ah, then we agree after all! And that's precisely what current theory
says.

PD

>
> R.