Re: Virtual Particle confusion

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From: PD (*pdraper_at_yahoo.com*)
Date: 03/01/05

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Date: 1 Mar 2005 11:38:29 −0800
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Ranando King wrote:
> "PD" <pdraper@yahoo.com> wrote in message
> news:1109612128.931289.213860@f14g2000cwb.googlegroups.com...
> > Ranando King wrote:
> > > "PD" <pdraper@yahoo.com> wrote in message
> > > news:1109365094.518160.42090@l41g2000cwc.googlegroups.com...
> <snipped>
> > > > 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.
>
> In this case, it means that each particle in the closed environment
is
> equally statistically likely to have the same amount of kinetic
energy. That
> may not actually be the case, but on average, any sampling of the
> environment will tend to match any other sampling of the environment
in
> terms of average kinetic energy/particle.

And so you explain diffusion using this definition of entropy, how?

>
> > >
> > > > 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.
>
> Think of it this way. If someone made a stain of unknown type on your
> carpet, you'd be able to clean some fraction of it up. Depending on
your
> cleaner, you can even calculate a best case statistical percentage of
the
> stain you can expect to remove. That's the nature of Carnot's
"discovery".
> The only difference is that in his case, the stain is heat and the
carpet
> was water(a cold source). You can statiscally calculate how much of
the heat
> you'll be able to use in a best case scenario without knowing the
full
> nature of the heat.
>
> Now consider if you were working on converting heat to mechanical
motion.
> Heat is motion in seemingly random directions by a myriad of
particles. So
> in effect, it's already mechanical motion, just undirected. Suppose
you knew
> the nature and direction of each individual particle. Using that
knowledge,
> if you could isolate each individual particle in the environment, you
should
> be able to catch and use the energy from each individual particle
when it is
> finally headed along the vector most useful to you.

The "...you should be able to..." is *precisely* what Carnot said was theoretically impossible. Note that he didn't say, "This is how much we know how to convert." He demonstrated, by mathematical equivalent of theorem, that there was *no way* to convert more than the theoretical maximum.

Now, this may seem like intuitive nonsense to you, and that you can create a little thought experiment to get around it and say "it should be possible". However, I suggest that you digest the Carnot theorem completely, even though it is non−intuitive, before you casually say it's wrong or incomplete.

>
> > > _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 know they didn't know QM back then. That doesn't mean that I'm not_
free to
> _apply it now. What I'm saying is that the understandings provided by_
Carnot
> _& Gibbs showed us that at the present time, given our limited_
knowledge of
> _how things work, we're bound by certain limits on how efficiently we_
can
> _convert energy from one form to another without leaving behind some_
"waste
> _energy."_
>

> This may fall back to philosophy, but if we really have a proper
> understanding of even the most minute of details regarding QM and how
to
> manipulate them, then we should be able to devise devices that are
even more
> efficient than the limit proposed by the Carnot cycle.

No. That is precisely what you *cannot* do. The Carnot limit on efficiency is a theoretical maximum that is not bounded by the details of how things are working at a microscopic level. That's what you're not understanding. You need to study this proof more carefully.

>
> > >
> > > > 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."
>
> That's not quite what I'm saying. I'm saying that the "pattern that
> physicists use" is incomplete, and they're using statistics to patch
the
> holes. So there must be some hard science capable of explaining these
> statisically significant events and patterns. We just haven't
discovered it
> yet.

First of all, I dispute your claim that the integral nature of statistics in physics is evidence of a "patch" over incomplete understanding. That is an act of faith on your part, that nature simply MUST be completely deterministic at root level. That claim is not at all obvious and should not be accepted a priori. What I've been trying

to tell you is that nature at its most fundamental appears to be
statistical to the core, despite our intuition otherwise.

>
> > > 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.
>
> While a perfectly fine thing to do in the face of current knowledge,
> consider that trading the Lorentz Ether Relativity theory for General
&
> Special Relativity led to many mathematical artifacts,

Not clear they're "artifacts". That may simply be how complicated
nature is.

> clouds of infinite
> mass surrounding a core of negative infinite mass in just the right
> quantities to leave a net positive mass,

SR and GR and on the other hand Lorentz Ether Relativity theory have
nothing to do with this, and proving LERT correct would not address
your discomfort with renormalization in quantum mechanics at all.

> mulitiple quantum universes all
> coexisting in the exact same reference frame at the same time,
virtual
> particles existing for a period of time short enough not to violate
> Conservation of Energy yet carrying force exchange information,
etc...

Again, aether has nothing to do with this.

>
> If this is simpler than dealing with the possibility of aether...
well...

Note that adopting an aether model is *not* simpler than SR
mathematically or conceptually. It may seem that way to a casual
reader, but when you get into the implications of what LERT has to
invoke to make everything fit, it is just as complicated as SR.

Secondly, LERT's success with Michelson−Morley is not enough. It has to
also compete with SR's success in every area where it has been used
successfully, such as particle accelerator operation and particle
physics detector design and operation, its role in the Dirac equation
to describe fermion behavior, the QCD and QED parts of the Standard
Model, and so forth. When LERT is shown to be just as robust as SR in
supporting the successful predictions of all these applications, then
it would be considered a more serious contender.

Finally, your discomfort with QM has nothing to do with the aether. The
author of SR was just as uncomfortable with QM as you are, but very
confident in SR. Conversely, replacing SR with LERT would not address
any discomfort you have with QM's conceptual underpinnings.

>
> > > 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?
>
> Sorry about the wording there. I was getting 2 different ideas mixed
up my
> mind when typing that. I meant to refer the disconnect between
gravity and
> the Standard Model.

And so the point remains that QM and its expression in the Standard
Model (which does not include gravity) is highly successful in
explaining phenomena it is intended to address. There is no failure of
QM in 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.
>
> Actually, it's a logical extrapolation. Consider what the phrase
"average
> kinetic energy" actually means. The kinetic energy of any particle in
a
> closed environment is given by
>
> E=(mv^2)/2
>
> where m is the mass of the particle and v is the particle's velocity.
The
> temperature of the environment, being a measure of the average
kinetic
> energy of the environment, is given by adding all the E for the
individual
> particles and dividing by the total # of particles.

The previous statement is incorrect. I suggest you take a look at stimulated emission and ask yourself what a negative temperature in that context means.

> If the # of particles is
> 1, then the formula above applies.
>
> Thermodynamic equilibrium simply means that the system is neither

gaining or
> losing energy to the outside environment. So if that lonely particle
is
> maintaining the same E, then it is in thermodynamic equilibrium.

That is not the definition of thermodynamic equilibrium. If I have a
closed, insulated container that contains an ice cube in boiling water,
that system (though it transacts no energy to the environment) is not
in equilibrium. Your definition of equilibrium appears to be at odds
with that used by physicists and chemists.

>
> > >
> > > <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.*
>
> *Did you ever stop to ask why QM depends on statistics, or do you*
simply
> *accept it at face value? It's because of the HUP. When they attempt*
to
> *measure one property of something that small, they inevitably end up*
> *changing at least one of it's other properties.*

You are confusing the HUP with the "Copenhagen interpretation" of what's going on with the HUP. Of course I ask why QM is based on statistics and randomness. That's why I've studied the EPR paradox and Bell's theorem, and the experimental tests that show that there is something more fundamental to the HUP than the Copenhagen "clumsy thumbs" interpretation.

> *Using statistics, they can*
> *predict those changes to some degree. But since they do not know with*
> *impunity, they're not ever going to be completely certain until they*
come to
> *a better understanding of what's driving those statisitcal patterns.*

And you misunderstand or misrepresent the HUP.

>
>
> > >
> > > > >
> > > > > > *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.*
>
> *Wow... crossed up terminology. I never said that entropy "is a*
well−defined
> *quantity." I stated that entropy is *Not A Measure Of The Randomness*
Of A
> *System*. Entropy is a measure of the equality of energy state*
distribution
> *in a given environment. I'm simply saying that what's being measured*
by

> *entropy has nothing to do with randomness.*

Hmmmm, so you don't think entropy is quantifiable (as in, with a number
and units) at all? That's odd: most physicists and chemists do that
routinely, and make quantitative, verifiable predictions based on those
calculations.

Moreover, you don't exhibit a clear understanding of what entropy
really is. If you know how to calculate entropy, you will have the
better definition.

>
> > >
> > > > > *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".
>
> Skip it then. Every time I've tried to explain myself in a situation
like
> this, I inevitably wind up sounding sarcastic and/or demeaning. So
I'll just
> save myself the embarassment and you the emotional trauma. ;) If you
want
> me to try anyway, just ask and I'll do my best.

I thought I did ask. I trust you to not be demeaning or sarcastic.

>
> <snipped>
> > > > 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.
>
> I don't doubt that failure of "common sense" alone is insufficient
reason.
> However, let's take a look at Schrodinger and his quasi−dead cat.
> Fast−forward the experiment to where the halflife of the radioactive
element
> has already elapsed so that now the cat is both 100% alive and 100%
dead at
> the same time (bologna, but that's the experiment). Much like
subatomic
> particles which are supposed to be able to interfere with alternate
quantum
> versions of itself, the 100% alive state of the cat will almost
invariably
> interfere with the 100% dead state of the cat because cat's don't
like being
> trapped. So if Schrodinger's cat had survived, you could expect to
find the
> cat trying to escape the box...or at least meowing.

Here is where you get into trouble. Your assessment of the interfering states is essentially correct. Your "calculation" of an observable behavior (meowing or clawing) is not correct. Start simple, with simple objects and simple observables, before you attempt to analyze an observable like this. By the way, the "expectation value" of an observable quantity has a very rigorous definition, with clear rules on how to find them.

>
> The really obsurd conclusion here is that if a cat meows in a
Schrodinger
> experiment and nothing is around to observe it, it might have been a
dead
> cat meowing!

Yes, that's quite right.

> *Not only is this a violation of common sense, it's a laughable*
> *absurdity.*

No, it is not. It is exactly what QM predicts.

> *The point here is that even though you either did not or cannot*
> *observe something's state for whatever reason, that does not imply*
that the
> *unobserved object occupies every possible state simultaneously.*

That's true. But what it does mean is that if you want to properly and
accurately predict the expectation value of an observable quantity,
then treating the system as though the unobserved object occupies every
possible state simultaneously will give you the correct answer, and
other approaches will not. Now comes the faith issue: If the
calculations work with all possible states occurring simultaneously, is
it right to say that they DO in fact occur simultaneously, or do we
insist that can't be right and there there MUST be another explanation?
You say the latter. I'm disputing that.

>
> *Halflives, even moreso than temperature, are a statistical property.*
Just
> *because a material has a half−life of 10 minutes doesn't mean that*
given 2
> *atoms of the material, in 10 minutes one of the atoms must have*
decayed. It
> *means that at least one of the atoms is likely to have decayed. But*
given
> *only 2 atoms, it's not significantly less probable that no or both*
atoms
> *will have decayed. That does not, however, place the atoms in some*
quantum
> *dual state until someone observes it. It mearly means that the atoms*
are in
> *some state that we do not presently know.*

No, the last statement is *one possible interpretation*. And it has
realizable consequences.

>
> > > *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.
>
> It's not possible because the sum of the energies of a number of
states has
> to sum up to the energy of the state of the particle as it is when
the
> particle is observed.
That is incorrect. You are not calculating the expectation value of the
energy at all correctly.
> It doesn't matter that it's not an algebraic sum.
It most certainly does.
> The
> rules of math reveal an inconsistency here. If the letters a to e
represent
> the 5 possible well known energy states of a particular unobserved in
its
> multi-state, f is the summation function, and a is the observed state
of
> that particle, then:
>
>   a = f(a, b, c, d, e)
>
> If a, b, c, d, and e, are all known to be different state values then
if the
> experiment is repeated exactly so that nothing has changed in the
input to
> the experiment but the output is now b, then:
>
>   b = f(a, b, c, d, e)
>
> Since the values of the individual states are well known and known to
be
> different, then:
>
>   a != b
>
> Therefore
>
>   f(a,b,c,d,e) != f(a,b,c,d,e)
>
> and that's quite impossible if a,b,c,d, and e are known values. The
only way
> this is even remotely possible is if a,b,c,d, and e are functions
> themselves. If indeed they are functions, then something is going on
> underneath the statistics that must be understood.
This is hogwash.
The expectation value of the energy of a system is
<E> = Sum(i,j) [<state-i|H|state-j>] / Sum(i,j)[<state-i|state-j>]
where H is the energy operator (the Hamiltonian) acting on the states.
You'll note there are interference terms in the sum not accounted for
in your summation.
>
> > >
> > > > >
> > > > > > > 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.
>
> Sorry, but eliminating gravitational phenomena, fairly well
eliminates much,
> if not most of the macroscopic universe.
I beg to differ. Gravitation is only one of the four interactions, and
the weakest by far. Essentially all of chemistry, biology, condensed
matter physics, and electrical engineering is based on one interaction
-- the electromagnetic interaction (QED).
> Try this one. Use QM to explain
> *why* the variant mass of an object traveling at near light speeds
*DOES
> NOT* accompany an increase in gravitational potential.
I don't need QM to explain that. SR does it on its own. Gravitational
mass is based on invariant mass, not on the poorly named "relativistic
mass".
> It's a
> non-gravitational phenomena, just as you requested. If you want me to
> exclude any question regarding gravity in its entirety then try this
one.
> Explain *why* I can neither find nor create magnetic monopoles.
Because they don't exist? I'm not being glib. There are many "why"
questions that physics does not (and never will) address, though we may
learn the answers to *some* of them as our understanding deepens.
Again, an edge to our understanding does NOT mean that what we
currently DO understand must be wrong and will be contradicted in a
deeper theory.
>
> Here's a microscopic question, too. How is it that the point electron
is not
> a black hole if it's mass is all contained within a single point in
space,
> aka a singularity? Compound this with the notion of renormalization
where
> the electron core itself has an infinite negative mass (a white hole)
> encased in a cloud of virtual particles of positive infinite mass (a
black
> hole).
Ah, but that IS a gravitational question, for which I confess we do not
have a good QM understanding.
>
> > >
> > > > >Every attempt to do so to date has failed.
> > > >
> > > > What attempts are you referring to?
> >
> > Unanswered.
>
> Unanswered because it was answered before it was asked by the
statements
> that preceeded it.
>
> > > >
> > > > > 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.
>
> You forgot the words "to date" at the end of that statement.
Naturally. However, that's a far cry from asserting that there MUST be
such a theory, because it is OBVIOUSLY apparent that QM is incomplete
BECAUSE it is statistical. Such an assertion is a statement of faith,
not something based on solid ground.
>
> > >
> > > > > 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.
>
> At the same rate, before the invention of the telescope, saying the
earth
> was the center of the universe also explained lots of things quite
well. I
> know from years of experience programming that a bad solution can
still
> handle 99.9% of all problems thrown at it. But eventually, because
the
> solution is a bad one, there will be an irrecoverable error that
cannot be
> made to fit the desired results. In the case of QM, that error is the
> inability
You forgot to include "to date" after this phrase. It's only an error
if you can prove an inherent inability. There is no demonstrated
incompatibility.
> to include gravity. In such cases, the core of the program has to
```

> be examined and rewritten. In this case, that would mean altering the
basic
> premise of QM and changing the model accordingly.
>
> > > To date, no
> > > unified theory that covers all known science is available.
> >
> > And that makes the pieces that are known a failure?
>
> The fact that we can explain it all to a high degree of precision,
but only
> in pieces that do not fit with each other, reveals that those pieces
fail to
> accurately describe the universe as a whole. Some of the pieces may
be
> correct. In fact, there may only be 1 errant piece, but until we know
for
> sure, all of it is suspect.
Nah. I simply don't like this "It's all crap and fundamentally flawed
until we know the FINAL answer." What if we NEVER find the final
answer? What if there IS no final answer?
>
> > Does the fact that Darwinian evolutionary theory doesn't predict
> > radioactive decay make evolutionary theory a failure?
>
> No. Different subject. Excuse my obtuse terminology above. I meant to
say
> "all known physics".
Same argument applies. Just because QED doesn't model gravity is no
indictment of QED. Different subject.
> As for evolution, the fact that the statistics predict
> that it would have taken significantly longer than the age of the
universe
> to evolve humans makes that theory a failure.
>
> > > Gravity is a hard
> > > target to hit.
> >
> > Indeed, no argument. What's your point?
>
> One misplaced piece in a jigsaw puzzle fouls the entire puzzle.
I disagree, though it does inspire to look for the missing piece. A
puzzle of a scene in Holland may have missing pieces, but you can
still see the windmill as well as you are ever going to see it, even
after the missing piece on the other side is included.
> It doesn't
> matter how good the piece looks there or how well it fits, if that's
not the
> place where the piece belongs, the puzzle cannot be properly
completed.
>
> > >
> > > > >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.
>
> Gravity is a major factor in explaining the macroscopic universe. If
you
> don't account for the effects of gravity, all calculations on real
objects
> will be off.
Every theory -- I mean every theory -- is an "effective theory". That
is, it applies to a certain domain and with some assumptions. There is
no evidence whatsoever that there is a theory that makes no assumptions
and has no unanswered "why" questions, nor that there will ever be.
Indeed, refer to Godel.
> The amount of that error may be "negligable" in the opinion of
> many, but it's still an error that can be avoided by proper
accounting.
>
> > >
> > > <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.
> >
>
> If we go with the assumption that QED and QCD aren't completely
wrong, then
> there's no doubt that we'd agree on certain things. I'm still having
> difficulty with renormalization. Is this supposed to represent a real
> process? Or is it just a matter of trying to fix a mathematical
> inconsistency?
Probably neither/both. It was imposed by a mathematical surprise, but
gee, so was spacetime and relativity of simultaneity. And there is a
physical interpretation to what the mathematical process of
renormalization means. And it's all tightly coupled with what the
meaning of an observable quantity is. The problem comes in from
sometimes insisting that there should be too much correlation between

the mathematical process and the physical process.
Consider Fermat's principle of least time in optics, which accounts for refraction correctly. Is there an explanation of *why* the path of least time is taken by a light ray? No, only that it does. Do we *have* to assume, even for classical rays, that a ray has to pass through all paths and choose the one that takes the least time? No, certainly not. Is there a QED interpretation of light (not rays, not waves -- just light) taking all possible paths and the *measured* one being the one that happens to take the least amount of time? Yes.
PD
>
> R