Not Even Wrong

Could a beautiful, logically complete formulation of physical law yield a unique solution that appears so lopsided and arbitrary? Though not impossible, perhaps it strains credulity.

2. Some of the values are fine-tuned to make complex structures and thus life possible:

It is logically possible that parameters determined uniquely by abstract theoretical principles just happen to exhibit all the apparent fine-tunings required to produce, by a lucky coincidence, a universe containing complex condensed structures. But that, I think, really strains credulity.

Personally I don't see the same degree of believability problems that Wilczek sees here. On the first point, it seems quite plausible to me that there are some crucial relevant ideas we have been missing, and that knowing them would allow calculation of standard model parameters, by a calculation whose results would have a complicated structure.

On the second, it's not at all clear to me how to think about this. Sure, the fact that our universe has highly non-generic features means that it is incompatible with generic values of the parameters, but there's no reason to expect the answer to a calculation of these parameters to be generic. I guess the argument is that there would then be two quite different ways of getting at some of these parameters: imposing the condition of existence of life, and a fundamental calculation; and if two different, independent calculations give the same result one expects them to be related. But the question is tricky: by imposing the condition of the existence of life in various forms, one is smuggling in different amounts of experimental observation. Once one does this, one has a reason for why the fundamental calculation has to come out the way it does: because it is has to reproduce experimental observations.

Wilczek avoids any mention of string theory, instead seeing inflationary cosmology and axion physics as legitmating the idea that standard model parameters are fixed by the dynamics of some scalar fields, or something similar. This dynamics may have lots of different solutions so:

We won't be able to calculate unique values of the parameters by solving the equations, for the very good reason that the solutions don't have unique values.

The fundamental issue with any such anthropic or environmental explanation is not that it isn't a consistent idea that could be true, but whether or not it can be tested and thus made a legitimate part of science. It's easy to produce all sorts of consistent models of a multiverse in which standard model parameters are determined by some kind of dynamics, but if one can't ever have experimental access to information about this dynamics other than the resulting observed value of the parameters, why should one believe such a theory? It is in principle

Of course, the very real possibility that we can't calculate everything in fundamental physics and cosmology doesn't mean that we won't be able to calculate anything *beyond what the standard models already achieve. It does mean, I think, that the explanatory power of the the equations of a "theory of everything" could be much less than those words portend. To paraphrase Albert Einstein, our theory of the world must be as calculable as possible, but no more.*

One can't argue with this: if a model make distinctive predictions, and these can be compared to the real world and potentially falsify the model, one can accumulate evidence for the model that could be convincing. Unfortunately I haven't seen any real examples of this so far. The kind of thing I would guess that Wilczek has in mind is his recent calculation with Tegmark and Aguirre that I discussed here. I remain confused about the degree to which their calculation provides any convincing evidence for the model they are discussing.

Unlike many theorists, Wilczek personally seems to be an admirably modest sort of person, and perhaps this has something to do with why the multiverse picture with its inherent thwarting of theorist's ambitions to be able to explain everything has some appeal for him. Over the years during which particle theory has been dominated by string theory, Wilczek has shown little interest in the subject, perhaps partly due to its immodest ambitions. But I see two sorts of dangers in the way his article ignores the string theory anthropic landscape scenario which is what is driving the interest of much of the theory community in these multiverse models. As his advisor David Gross likes to point out, accepting this scenario is a way of giving up on the perhaps immodest goal he believes theorists have traditionally pursued, and one shouldn't give up in this way unless one is really forced to. None of these models is anywhere convincing enough to force this kind of giving up.

The second danger is that what is happening now is worse than just giving up on a problem that is too hard. The string theory landscape anthropic scenario is being used to avoid acknowledging the failure of the string theory unification program, and this refusal to admit failure endangers the whole scientific enterprise in this area.

Update: It has been accurately pointed out to me that Wilczek does mention string theory briefly at one point in the article ("Superstring theory goes much further in the same direction"), and alludes to it at another place (when he talks about a "theory of everything").

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199 Responses to *Wilczek on the Anthropic Principle*

Bert Schroer *says:*
May 29, 2006 at 12:38 pm

JC
since AQFT incorporates QFT (I mean real time, in the Euclidean case you would need the subtle reflection positivity which is hard to verify and certainly does not hold for Chern-Simons actions) i. e. it implements the same principles (maybe in a conceptually more careful way), then against what do you want to compare it, what do you want to see replaced?
AQFT has not been elaborated for going beyond the speed limits of ordinary computations in QFT; it is not the computational speed you gain, it is the conceptual depth which you sometimes succeed to increase.
There is too much mystery around AQFT in this weblog, somehow I have the impression that you only tolerate sophisticated mathematics in string theory and you want to maintain QFT in a mathematical stone age and in case it is not you want to put it into another drawer.

Is it so difficult to understand that in an area which is so treacherous and paradigm-changing (see my remarks to Eugene Stefanovich) as compared to standard QM one must be a bit more careful?
AQFTists just have a higher awareness about these problems: I have never assisted a talk where speakers forget to explain the setting and the aim of a problem; whereas in string theory the comprehensible talks with a good balance between conceptual setting and technical tools and a clearcut separation of facts&fictions which I have assisted can be counted on the fingers of one hand (although here in Brazil there is somebody who succeeds to do just that).

Thomas Larsson *says:*
May 29, 2006 at 12:41 pm

"What would impress me would be something which could calculate the n-loop m-point function (n neq m, in general) in a few lines, without having to crank out zillions of feynman diagrams."
Then I suppose you are intrigued by the Witten/Svrcek/Cachazo/Spradlin/Volovich/… twistor-inspired reformulation of (super)Yang-Mills perturbation theory.

I'm somewhat confused by this example. My impression was that Witten's original twistor string, which would have led to massive simplifications (perhaps only for unphysical SUSY QCD, but anyway), was simply wrong (important lesson: conjectured dualities may be plain wrong). However, it was possible to save the idea by adding an extra interaction vertex. The modified twistor string is probably right, since it is just a canonical transformation of QCD, but now the question is whether this is useful anymore. A canonical transformation may be useful, e.g. if you reach action-angle coordinates, but this does not seem to be the case here. Otherwise, a canonical transformation usually lead to a formulation which is as hard, or harder, from what you started from. TANSTAAFL.

As an example how ignoring an interaction vertex leads to great simplification, consider QED. If you ignore the electron-electron-photon vertex, QED becomes a theory of free electrons and photons. This amounts to a major simplification, but it is also wrong.

Mentos *says:*
May 29, 2006 at 1:13 pm

Maybe you need to look at Cachazo and Svrcek's year-old review

http://arxiv.org/abs/hep-th/0504194

or (to pick one more recent work) Britto et al's paper on the 1-loop, 6-gluon amplitude in nonsupersymmetric QCD

http://arxiv.org/abs/hep-ph/0602178

JC *says:*
May 29, 2006 at 1:15 pm

Bert,

What I meant by "replace", was whether AQFT or another framework could completely replace the Feynman diagram method in generic graduate level textbooks on quantum field theory. At the present time, I don't see AQFT or any other framework being the dominant standard presentation in an introductory quantum field theory textbook. Despite the Feynman diagram calculations being really messy and tedious, I don't see it being pushed aside yet by any other framework.

Years ago I use to think that string theory could one day completely replace the Feynman diagram framework of conventional "textbook" quantum field theory. (In hindsight this probably sounds very silly and naive).

Johan Richter *says:*

May 29, 2006 at 1:33 pm

Has anyone of you heard of casual pertubation theory? On Wikipedia it was described as a finite, mathematical rigorous formulation of QFT. Is it a crackpot theory?

Thomas Larsson *says:*
May 29, 2006 at 1:45 pm

My observation was based mainly on [hep-th/0605121](hep-th/0605121), where it was stated that a canonical change of field variables converts the Yang-Mills Lagrangian into an MHV-rules Lagrangian. Perhaps this canonical transformation does lead to major simplifications for SUSY theories (or perhaps not), but it remains to be seen whether this has any physical relevance.

There is no question that Witten's original twistor string from 2003 was wrong, since it gave the wrong results beyond tree level.

JC *says:*
May 29, 2006 at 1:48 pm

Mentos,

I was quite impressed by the work of Cachazo et. al. when they were able to reproduce some results found earlier by Bern, Dixon, Kosower, et. al, with less labor.

Bert,

For a long time I found AQFT somewhat mysterious, largely because I wasn't familiar with the mathematics involved. (I could imagine this to be the case for some theorists). It could just be laziness on my part, but for many years I more or less looked at advanced mathematics on a "need to know" basis. I didn't really put much effort into looking at unfamiliar math, if I didn't think it was directly applicable to string theory or physics in general.

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stevem *says:*
May 29, 2006 at 1:53 pm

Bert, thanks. The paper on the Hawking radiation is interesting. Yes, I see now that this is for the nonstationary case, which is the harder problem with the scalar field in the background of the collapsing star. Peter, off topic discussions like this should be encouraged as long as the discussion stays reasonable, interesting and civil with the prospect of learning something new or getting a different point of view.

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urs *says:*
May 29, 2006 at 2:01 pm

> *Urs I can perfectly understand that you are quite angry*

Hm, I didn'd say I am angry, did I? Maybe I am, maybe I am not. We didn't speak about that. (At least, given the context, there are clearly more likely candidates for anger, aren't there?)

I would be happy to further discuss facts about QFT, AQFT and the like. In particular, I would be very interested in a factual reply to my last comment .

(http://www.math.columbia.edu/~woit/wordpress/?p=396#comment-11299)

What I actually said was that it is not known how AQFT can deal with general boundary conditions.

On the other hand, in Atiyah-Segal-like formulations of QFT we do know how to deal with boundary conditions.

And its actually quite central to the understanding of QFT. The RCFT theorem says, among other things, that you can understand full RCFT from just knowing any one of its boundary conditions.

The reason is that the Frobenius algebra A which appears in that theorem (being the internalization of the Frobenius algebra known from topological 2D QFT, but now internalized, in the category-theoretic sense, into the representation category of some chiral vertex algebra) is nothing but the OPE algebra of open string states both whose ends have some given boundary condition.

It's pretty remarkable that all other boundary conditions are then obtained by looking at the modules for that particular algebra.

Behind this is a nice little piece of general abstract nonsense, due to a theorem by Viktor Ostrik.

This theorem says (->) that in every module category of our representation category of the chiral algebra, an internal algebra is given by the internal Hom of any one of its objects. These objects are nothing but RCFT boundary conditions, and the internal hom is something like the internal scalar product on these, when regarded as a issue in categorified linear algebra (->). It's interesting how this abstract nonsense translates into concrete CFT physics.

I am eager to learn, so if you like to teach, go ahead. (Maybe insert a paragraph line break here and there so that I can orient myself in the wealth of information given.) You may imagine that I am not a string theorist. Just a person interested in physics.

Bert Schroer *says:*

May 29, 2006 at 3:26 pm

Peter, I tried to recollect the chronology of my encounters with Sidney Coleman and I think you are right, this Berlin meeting must have been before the onset of that tragic incapacitation; in any case there was nothing visible in his talk (which different from his usual topics was on some fundamental problem in QT). I do remember however that he was behaving more supportive of string theory than the amount of politeness required if one is invited by a string theoretician. Memory is often not completely faithful and depends a bit on those aspects which one is thinking about in the present.
Somehow I have a much clearer memory of those times when I met Sidney in Rio at the invitation of Jorge Andre Swieca. One reason may be that he got me into a very funny situation. He was suffering from a very severe form of diabetis and had to keep his bood sugar always around a certain level and to achieve this he ate sometimes small amounts of bananas. Before his seminar talk he asked me if I could arrange 2 bananas for him, so I left the PUC compound and went to a bar on the opposite side of the street and asked for two bananas (the always used to have small amounts for bananas to make a mixed shake called vitamina). I will never forget that strange look, just the kind of look a Gringo who asks for two bananas in a bar in Brasil would receive.
The next day we went to the Tijuca forest because Sidney wanted to see a macumba. He looked interested at the somewhat strange ceremony but did not comment it. The day after we asked him what he thought. He took a deep breath and said: you know the best situation is to have no religion at all, but if the number cannot be zero, it should be infinite. He was of cause referring to the large almost continuous spectrum which is the result of religious syncretism between African, Indian and European ingredients. In fact some years ago there was a delegation of Nigerian academics who studied the preserved rites and African culture in the Bahian diaspora.
There is also a valley in the mountenous region of Espirito Santo were descendents of Pommeranians live. Their ancestors were bondslaves in the feudal system which came to an end when Napoleon went through Europe up to Moscow. These people were free but without land and so the imperial government of Brazil payed their crossing and gave them land. Since the old Pommerania does not any more exist (after worldwar II it became part of Poland) this is the only place in the world were one can study this unique form of old northern German. They invented German names for tropical fruits; for example the "frutta de conde" which you take apart with your hand in order to get to that marvelous tasting pulp which melts in your mouth, they call "Schmalzapfel", an ingeneously fitting term.
Returning to Sidney, I think that Robert Schrader attended an event which was in Sid's honor, so it probably was the same which you mentioned. He told me that he was in a not so good shape. If, as you seem to say, the situation improved somewhat, one would whish that it reaches a point where it becomes interesting and meaningful for his friends to converse with him. He has done a lot to maintain the clarity of content and presentation (including good terminology) to deserve a satisfying evening of life.

JC *says:*

May 29, 2006 at 6:41 pm

Johan Richter,

I haven't quite understood the point of "causal perturbation theory" in the Epstein-Glaser approach. Other than reproducing some results which were already known previously in conventional Feynman diagram calculatons, it seems to be an attempt at dealing with the formal details of the singular nature of fields which often gets "swept under the carpet" in textbook treatments of quantum field theory. (Somebody else can fill in the details).

Bert,

The main reason I first became interested in AQFT was that I always felt that there should be a way of doing quantum field theory without resorting to a classical Lagrangian in an intermediate stage. Ever since string theory has fallen into the anthropic abyss and particle phenomenology has more or less flatlined, I've been trying to understand other approaches like AQFT, LQG, etc … for which I previously looked at on and off for many years without much understanding. Since I don't write research papers anymore these days, I've been spending more time trying to understand AQFT and other older approaches such as in Bogolubov's two books.

Eugene Stefanovich *says:*

May 30, 2006 at 2:17 am

Bert:

B.S.: "I do not think that there are many people in this weblog who really know about the existence of a relativistic multiparticle theory (without the property of being "second quantize representable") which fulfills all the requirements one can formulate in terms of pure particle concepts (including the very nontrivial cluster factorization). But you probably agree with me that it is not what we consider as "fundamental (I do not mean the hegemonic string theory interpretation of this word)" since it lacks vacuum polarization (although you could think of manufacturing something which approximates this by adding channel couplings between particle states with different particle number). But I think that even you would not try to understand the Lambshift or the cosmological vacuum problem (involving the energy-momentum tensor) in such a framework; you would rather make this big paradigmatic shift into QFT, would't you?"

E.S.: To the contrary. There is no vacuum polarization in the "dressed particle" approach I was referring to. That's the beauty of it. The idea was first suggested in

O. W. Greenberg and S. S. Schweber, "Clothed particle operators in simple models of quantum field theory", Nuovo Cim. 8 (1958), 378.

In my opinion, this is the best paper about QFT written since Tomonaga-Schwinger-Feynman. The idea is to apply to the QFT Hamiltonian a unitary dressing transformation which kills all vacuum polarization terms and transforms them into particle-particle interactions. In this approach, the Lamb shifts and anomalous magnetic moments are not results of the vacuum polarization and virtual particle loops. They are consequences of small corrections to the particle-particle Coulomb potentials that arise from the dressing transformation. The particle-number-changing interactions that couple different channels result from the same transformation. They are not "manufactured". You can find the latests developments in this area and references in nucl-th/0102037 and in chapter 12 of
physics/0504062.

B.S.: "I looked at some of these other approaches you mentioned, but I have the intense impression (I don't have the time ti make the necessary lengthy calculations) that those fail precisely on this cluster issue. With other words I think that any relativistic particle theory has to look like C-P + more complicated channel couplings."

E.S.: I can't agree with you here. It is shown in vol. 1 of Weinberg's "The quantum theory of fields" that if interaction is written as a polynomial in particle creation and annihilation operators with coefficients that are smooth functions of momenta, then the theory is automatically cluster separable. Both Kita's and Shirokov's models belong to this class, so the cluster separability is not a problem for them.

However, Coester-Polyzou interactions are not written in terms of creation and annihilation operators (they are written as functions of relative momenta and positions of particles), so the cluster separability is a big issue there. It is achieved by a rather complicated combinatorial construction of the interaction potentials.
This is why I think that Coester-Polyzou type models do not have a bright future.

Eugene.

Thomas Larsson *says:*
May 30, 2006 at 3:17 am

Completely OT: I have just received the NEW book from amazon.co.uk.

Peter Woit *says:*
May 30, 2006 at 12:12 pm

Thomas,

Thanks for the news. Last I had heard British publication date was June 16th.
Interesting to know that it is being shipped. I still only have one copy myself…

Bert Schroer *says:*
May 30, 2006 at 12:36 pm

Urs
Our controversy is about the use of conceptual precision in the terminology of particle physics. Let me make a second attempt to overcome it, or at least to give my position sharper contures.
The reason why I think that terminology and semantics in physics are important in times of "everything goes" (somebody mentioned the tower of Babel the other day) is that, wanting or not, they carry some connotation about the physical content and when a novice enters an area by reading electronic articles there is not always a knowledgable person next to him who helps him to steer around the riffs and cliffs of misinterpretation.
Let us keep the discussion within the borders of the previous hot point: TQFT against QFT and without loss of generality we may look at what Atiyah called the Jones-Witten invariant.
Now there are two ways of connecting certain subfamilies of that gigantic edifice which Vaughn Jones called "the theory of subfactors" (no qualm about this beautiful and meaning-loaden terminology) which via his "Markov trace" formalism (a terminology with a beautiful subtle two-fold meaning as I mentioned on an earlier occasion) leads to that tracial (type II) "bone" algebra (without localization) which contains the data of knots, mapping class groups and all that. There are two QFT-inspired ways to get to this.

One way is to adaptate the DHR technique of 1970,71 (contained in Haag's book) of "thinning out" QFT (always localizable in my use of the word) to obtain tracial "bone" states on the group behind particle statistics. In the higher dimensional DHR context this finally leads from the observable net to a (modulo some conventions) unique field algebra net which contains all the charge transfer operators which communicate between the different superselected representations of the observable net. The adaptation to the richer statistics realization which the locality principle permits in low dimensional spacetime (which you find e.g. in FRS, Review of Mathematical Physics, Special Issue (1992) 113) then leads to the same tracial Markov state on the same algebra (which from the QFT point of view is a subalgebra of intertwiners). This is certainly what Jones would have done if at the time he looked at these structures with Wassermann our paper did not already exist.

On the other there is Witten's derivation (from the great magician of actions) which starts with a a functional integral involving a geometrically based Chern-Simon density. Witten extracts with his typical hindsight and artistic skill (imposing certain framing rules) the mapping class group invariants. This is fine and it finds my unrestricted admiration, but the derivation has little to do with QFT, its relation to QFT is metaphoric. Why? There are zillions of functional integrals which you can write as exponential of an action, but most of them do not lead to QFT (even if you succeed to make some sense out of them by a renormalization-massage which destroys the validity of the representation you started with). There is a very subtle filter (coming from the O-S work) called the "reflection positivity" (insuring together with a certain amount of

analyticity the localization which is an inexorable property of QFT) which the C-S action does not pass. Ignoring this subtle property has led to what I call a "banalization" of Euclideaniztion (i.e that structure which is behind the "Wick rotation", if you want to learn more about this criticism look at some lectures of Rehren, hep-th/0411086). This statement about the nature of the C-S action is not a moral or even a mathematical judgement. Even without being QFT (but certainly being QFT-instigated), this looses nothing of its mathematical value.
The general problem behind this is the following. In the QFT setting (either a la Wightmann, or a la LSZ, or AQFT) if something appears as an elephant it really is an elephant. This is not the case at all with functional integrals unless you have gone through the whole O-S litanei.
This may be considered to be pedantic. But we are in the midst of a string-millenium clearance sale of particle physics and if we will not be careful about our Faraday-Maxwell heritage handed down by Dirac, Jordan, Pauli…a sellout of all those of our concepts which were important in past conquests will take place (superficially, just because a group of very influential people with significant past achievements, although this could not happen if the Zeitgeist would not allow them to do this). If we don't wake up and pay attention now, we will have to spend a very very long time on physically feable theories (with physical content smaller than any preassigned epsilon) or even on a totally failed project.
Some people may think that I am very courageous to say such things. Actually I am not, I just think that terminology in the exact sciences should be totally related to the content (examples: Tomita-Takesaki, Hilbert-Schmidt, or from physics Einstein-Hilbert or Haag-Ruelle etc). If we, like it is done in politics, have not only to accept our terminology subject to a hegemonic handdown and mining claims, and if even more so we are forced to accept the literal meaning of words (as in TQFT and as in Christian religions before the enlightenment), than I do not want to participate longer in such a lost course as far as particle physics is concerned. I am already doing part-time fishing in the Atlantic and enjoy it, why not do it full-time. It is as simple as that.
If Vaughn Jones would be around at this weblog, he would totally agree with me, we both have a very developed ability to distinguish metaphorical from intrinsic statements and we had it already at the beginning of the 90 when he invited me to Berkeley (I think that this was where this photo of mine in that collection which appeared the other day must have been taken). He would immediately tell you that category theory, Frobenius algebras + cutting and sewing makes very interesting mathematics (although I think he would not use those instruments) and was suggested from physics, but to construct (not just classify) chiral QFTs you have to use other methods. And he would consider that recent construction of the minimal model family by Kawahigashi et. al. (with an addition of a model which was still missing in the old classification) the finishing touch on a program which he started together with Anthony Wassermann.
Urs, in no way I wanted to downgrade the work which is presently done at the Math. department of Hamburg university in your group, this is honestly not my intention; I only ask you to have the same respect for terminology coming from particle physics as I have for that in mathematics. We both cannot change the existing TQFT terminology, but you should not press me to take that the QFT in that word literally. Even if there is a large community who would look down on me for not accepting that and remaining in their eyes a stone age QFTist, I can perfectly live with this since to have somebody like Vaughn Jones on my side is sufficient for me.
By the way I think that the problem which we are discussing here is very much related to that issue of what should go into hep-th and what should be posted in math-ph or mathematics. It is worthwhile to point out that the majority of AQFT papers are posted in math-ph despite the fact that they are significantly closer to QFT than most of the typical papers there (just have a look at the systematic work on renormalization with the separation from algebras and states which led to those marvelous achievements in curved spacetime QFT, culminating in the new local covariance principle, http://unith.desy.de/research/aqft/). This is because the AQFT authors are more conscious and they certainly do not post anything onto hep-th of the kind were the author has interesting math and desperately wants a connection to physics and hopes to find someone who is able to make that connection. Another reason why Distler allows purely mathematical articles (sometimes given the physics flavor by foregoing rigorous proofs because this may be bad for the physical intuition of those partners or groups which they want to address). But for a change I do not here want to criticize Distler (because he works under a hell of sociological pressure) as long as he does not overplay the saying: quod licet ceasar non licet bovi.

Bert Schroer *says:*

May 30, 2006 at 12:55 pm

Eugene
let us return to that interesting topic within a couple of days (se Peter quiser) since I have pressing other obligations.

Kea *says:*

May 30, 2006 at 5:00 pm

He would immediately tell you that category theory, Frobenius algebras [etc.] makes very interesting mathematics…but to construct (not just classify) chiral QFTs you have to use other methods.

I have asked Vaughn to comment. Let us hope that he does.

D R Lunsford *says:*
May 30, 2006 at 11:19 pm

Bert S said:

Alejandro,
Nobody is against geometry, but it has to come from the midst of raw local quantum physics…

This is a fundamental misconception. Everyone makes it, including string theorists. Insofar as I have a counterexample to this statement, it is wrong in fact, as well as simply in spirit.

-drl

Bert Schroer *says:*
May 31, 2006 at 9:05 am

Lunsford,
I honestly do not understand what you mean by that

Eugene,
just some preliminary remarks which should yet to be taken with a grain of salt. I do not understand your reading of Wally Greenberg's and Sam Schweber's old article (but since I do not have a copy anywhere near me I will be careful and preliminary).
If you really gave a correct account of the central point of the article, I would be still be reluctant. On the one hand mathematical physics in those days was on a lower level (although these authors belonged to the cream). For example those unitary dressings could lead out of the Hilbert space into inequivalent territory. Or perhaps the maintained a cutoff.
What I really do not understand is how could you dump such a complicated dynamical structure as vacuum polarization which depends on the region of localization (see my treatment of holography posted on that Goettingen server) into a modification of interaction (in standard approach (which was the only one known in those days) probably additional contribution to the interaction.
I think even in those days people had a bit more sophisticated vision of the QFT vacuum than that in that abominable mentioned level counting for the vacuum contribution to the cosmological constant; although my old colleagues (I am not significantly younger) certainly were still far removed from the level of understanding in the mentioned Holland-Wald paper.
Let me add that there is another area where such misleading views about the vacuum in locally covariant theories may have unfolded there treacherous ((wrongly simplifying) lure. This is the juxtaposition of a classical calculation based on differential geometry (Bekenstein's area law) with the Hawking thermal QFT setting. I really do not understand why quantum entropy should jump over that classical stick. Quantum localization and the ensuing autonomous thermal manifestation including quantum entropy are inexorably linked. And by the way, the area law for local quantum matter (which through AQFT holography becames associated with the causal horizon of the bulk in which it is localized) is totally universal whereas classically (taking the quantum entropy interpretation of the Bekenstein area law seriously for a moment) it is only valid for very special classical field theories including of course the Einstein-Hilbert theory.
Eugen, I think even among afficionados of string theory you would find little support for your vaccum viewpoint. And by the way, I have my serious doubts that that dubious sounding derivation of clustering from some momentum space analytic properties is due to Weinberg. What I was criticising before was Weinberg's logic:
P-invariance + clustering —-> (local) QFT. But this is something else.

[Alejandro Rivero] *says:*
May 31, 2006 at 9:47 am

Eugene,

I just glanced at the abstract of your papers during the coffee time, but I think to recall you were against the need of keeping relativistic invariance at quantum level, were you? I ask because most of the argumentations of string theoretists are that their quantisation of the string breaks reparametrisation on the world sheet and relativistic invariance in the target space, and that the only way to avoid it is to fix D=26 (or D=10). But in principle the non critical strings, at any D, should have Lorentz symmetry back in the classical limit. So it is really a problem?

Eugene Stefanovich *says:*
May 31, 2006 at 1:11 pm

Bert:

If you think that vacuum is a "boiling soup" of particles and antiparticles, then you need to explain the null result of the following simple experiment: place a photographic plate (or any other sensor) in an evacuated shielded chamber. Wait for a long time and then develop the plate. I think you agree that there will be no image on the plate. This means that all those virtual particles are not observable. Then what is the point to keep them in your theory?

…Or perhaps they maintained a cutoff.

Greenberg & Schweber paper didn't reach as far as to the loop integrals and renormalization problems. However, their approach can be used to systematically eliminate ultraviolet divergences from both the Hamiltonian and the S-matrix of QED in each order of the perturbation theory without cutoffs

E. V. Stefanovich, Quantum field
theory without infinities, Ann. Phys. (NY)
292 (2001), 139.

I have my serious doubts that that dubious sounding derivation of clustering from some momentum space analytic properties is due to Weinberg.

I am not sure if Weinberg is the original author of this derivation, but I don't have any problem with his proof in section 4.4. Do you?

I am sorry, most of your other comments about entropy, holography, etc. went over my head. I have no knowledge in these areas.

Alejandro:

…but I think to recall you were against the need of keeping relativistic invariance at quantum level, were you?"

Quite opposite. The relativistic invariance is the cornerstone of my approach. In my view, any sensible relativistic quantum theory should be formulated in terms of a unitary representation of the Poincare group in the Hilbert space.

No comments about strings.

Bert Schroer *says:*
May 31, 2006 at 2:33 pm

Dear Eugene,
you do not see the boiling soup on a plate, but whenever nature converts the Gedanken experiment of localizing quantum matter into reality, like in the case of black holes, you of course see the soup right on the event horizon and the resulting thermal Hawking radiation at large lighlike distances. Nobody can get this radiation back into virtuality, and if your theory can do, this it is not the right theory.
Whereas I believe that for a certain process you may encode the vacuum-polarization into a modification of interaction between particles, the claim that you can uniformely (i.e. for the whole theory and not only for a preselected process) dump

vacuum polarization elsewhere and still maintain the underlying principles of the theory (locality, positivity of the energy-momentum spectrum) is to me totally incredible. After all vacuum fluctuation on the causal boundaries of localized quantum matter is a direct consequence of those principles. Vacuum fluctuation was discovered by Heisenberg in connection with the quantum Noether theorem, when he tried to make sense of a "partial" charge, i.e. a charge attached to a finite bulk region. The above mentioned thermal aspect of localization (which is totally absent in your desired quantum mechanical description because Born localization does not cause such a thing) as a hallmark of QFT (not explainable in terms of the uncertainty principle!!) is of a more recent vintage.

Eugene Stefanovich *says:*
May 31, 2006 at 4:18 pm

Bert:

…whenever nature converts the Gedanken experiment of localizing quantum matter into reality, like in the case of black holes, you of course see the soup right on the event horizon and the resulting thermal Hawking radiation at large lighlike distances.

Nobody have seen the Hawking radiation in experiment, so I reserve the right to remain sceptical about this argument.

Whereas I believe that for a certain process you may encode the vacuum-polarization into a modification of interaction between particles, the claim that you can uniformely (i.e. for the whole theory and not only for a preselected process) dump vacuum polarization elsewhere and still maintain the underlying principles of the theory (locality, positivity of the energy-momentum spectrum) is to me totally incredible.

That's right, in the dressed particle approach, the vacuum polarization terms get absorbed into particle-particle interactions. These are direct action-at-a-distance interactions. So, you are right, the property of locality is lost. The question is how fundamental is this property? Even the usual Coulomb-gauge QED Hamiltonian contains a non-local direct interaction term. Nobody complains about it.

From the experimental standpoint, as far as I know, there is no direct evidence that electromagnetic interactions between charged particles are retarded. However, there are quite a few recent experiments (Chiao, Nimtz, Ranfagni,…) that demonstrate superluminal effects in the propagation of evanescent electromagnetic waves.

I am sure, you are going to say that action-at-a-distance contradicts relativity and causality. This is my favorite topic, and I can give you detailed counter-arguments, but I don't want to abuse the hospitality of our host Peter.
You can take a look at

E. V. Stefanovich, "Is Minkowski
space-time compatible with
quantum mechanics?" Found. Phys. 32 (2002), 673.

Bert Schroer *says:*
May 31, 2006 at 6:18 pm

Eugene,
I am saying what you say is all deja vue, but in a very bad sense. I kindly ask you to look up G.C. Hegerferfeldt, Phys. Rev. lett. 72 (1994) 596. It was claimed in this unfortunately published paper (total incompetence of the referee) that a more careful review of the famous Fermi atomic Gedankenexperiment (which Fermi used in order to argue that the velocity of light is not only the classical limiting velocity but this is maintained in QED as well) led to the possibility of a superluminal propagation. The paper had a grave conceptual flaw, (to say it in modern terms relevant to your problem) the author confused QM Born type of localization with localization carried by quantum fields (in field-coordinatization independent terms: modular localization). This was a big international splash, it went through all the international press (including new York Times), I saw it in Der Spiegel. The editor of Nature Maddox (I hope I remember correctly) had a big article under the headline: physicist from Goettingen proves feasability of time machines.
Two of the colleagues of the author had rapidly written a counterarticle:
http://br.arxiv.org/abs/hep-th/9403027

and I wrote a letter to the editor threatening to never review a paper again unless they undue this mess by publishing the correct version as well which they did (Phys.Rev.Lett. 73 (1994) 613).
Now I do not want to dismiss the Born probability interpretation as irrelevant; to the contrary it is absolutely necessary for scattering theory, there you need a localization with probability interpretation (coming with projection operators which the modular localization does not have) and lo and behold it is asymptotically Lorentz invariant (whereas the modular localization is throughout covariant but comes without a particle probability interpretation i.e. has no projectors). A modern account using this terminology you can find in
http://br.arxiv.org/abs/math-ph/0511042
This superluminal conceptual error is the evergreen of all errors, it is ineradicable and reappears almost vearly like Nessy in Scotland.

D R Lunsford *says:*
June 1, 2006 at 12:26 am

Bert -

A geometry is an invariance group, and has nothing at all to do with quanta directly. Quanta arise when one imposes a concept of measurement, which is outside the geometry. Therefore, it is simply wishful thinking, or a misunderstanding of geometry, to insist that geometry arise from quanta.

Consider that, in a theory in which space, time, and matter have a common origin, their mutual phase relations can lead to quanta.

-drl

Eugene Stefanovich *says:*
June 1, 2006 at 3:12 am

Bert,

I knew about Hegerfeldt controversy, but had no idea that it reached New York Times and der Spiegel. That's funny.

I am wondering why you oppose so strongly the Newton-Wigner concept of localization? Is it because the NW localization is observer-dependent? Indeed, if oberver at rest O prepares a particle in a localized state, then moving observer O' sees the wave function of the same state as being delocalized over entire space. However, I don't consider it as something totally unreasonable.

By the way, this observer-dependent localization seems to be helpful in solving the Hegerfeldt's paradox. Suppose that observer O prepares a particle localized at point A. Suppose that the wavepacket spreads superluminally so that detector at point B has a (small) chance to register the particle earlier than at t=R(A-B)/c.
Suppose further that observer O' moves with a high speed, so it (sometimes) sees that detector at B clicks earlier than the particle is released at point A. Definitely, this looks peculiar to O', but he has a good explanation: from his point of view the particle was not properly localized by O from the beginning. He thinks that the wave function of the particle was spread over the entire space (including point B) all the time. So, this doesn't look like a irreparable violation of causality. And this is rather far from building time machines. Isn't it?

The response by Buchholz and Yngvason does not look entirely convincing to me. In order to decide unambiguously whether the excitation spreads slower or faster than light one needs to perform a calculation of the time-dependent wavefunction, which is still missing. This leads to my other question which worries me a lot.

From what I can see in textbooks, relativistic QFT is concerned only with calculations of the S-matrix (scattering cross-sections and energies of bound states are ultimately related to the S-matrix elements). However, I wasn't able to find any RQFT calculation of the time evolution in an interacting system from first principles.
I think I understand why such calculations are missing. Please tell me whether I am right or not.

In my opinion, the problem is that in realistic RQFT theories (e.g., in QED) there is no well-defined Hamiltonian. Without a good Hamiltonian one cannot form the time evolution operator and study the time dependence of wave functions and observables. The Hamiltonian of QED is deficient for two reasons. First, it must contain infinite counterterms (if we want to get the S-matrix right). Second (and most importantly), it contains those vacuum polarization terms that transform the vacuum and one-particle states into infinite linear combinations of multiparticle states, which is totally unphysical.

My diagnosis is this: the particles whose creation and annihilation operators are used to write down the QED Hamiltonian are actually fictitious "bare" particles that are never observed. If we want to study the time evolution we should be concerned about "physical" or "dressed" particles that are some linear combinations of bare particle states. This seems to suggest that time evolution calculations (and, by the way, the solution of the Fermi problem) require transition to the "dressed particle" representation where the vacuum polarization effects are not present. Do you agree, or I am totally wrong?

I am not sure what is the role of modular localization in all this. I am still studying your "string localization" paper, but I have more questions than this weblog can handle.

Thank you.
Eugene.

Bert Schroer *says:*
June 1, 2006 at 3:46 am

to Lunford,
now that I understand your statement, I can say that I disagree with it. The autonomous modular localization theory (see the last reference in my previous blog contribution) does just this: it extracts geometrical and group theoretical data from the abstract domain of definition of certain unbounde operators in the modular setting. A very poignant account can also be found in
http://br.arxiv.org/abs/hep-th/0502014
One small consolation: I also had to go back to school (already many years ago) and to undue my picture of QFT (not so different from yours) and relearn a lot of things, so I encourage you to make a yet additional investment into QFT and new math. This new setting also leads to a completely autonomous picture of local quantum physical reality (i.e. models of QFT) in terms of relative position of a finite number of copies of (Leibniz) monades as I pointed out in my comment to Christine's last contribution.
By the way, string theory can never ever lead to such a setting, because it is a physically-instigated powerful production machine for mathematical conjectures (and pseudo-physics from geometrical imaginations about particle physics, i.e. Lundfors's direction) and as such it is very very good.

Kea *says:*
June 1, 2006 at 5:02 pm

In discussion with Urs, Bert Schroer said:

He would immediately tell you that category theory, Frobenius algebras [etc.] makes very interesting mathematics, but to construct (not just classify) chiral QFTs you have to use other methods.

Vaughn Jones has expressed reluctance to participate in a public discussion of this kind. He has, however, informed me that he completely supports Bert's statement above and that he also confirms the pivotal role of Anthony Wassermann in the first construction of concrete chiral models (the local nets associated with loop groups).

Bert Schroer *says:*

June 1, 2006 at 9:09 pm

Eugene,
I am not at all opposing the use of the (only asymptotically covariant) Newton-Wigner (= Born probability density) localization in the context where it is valid, I am opposing only the context in which you have been using it.
Without N-W localization and the associated probabilties and projectors you would not be able to derive scattering theory from the QFT priniciples (Haag's book, and in more details Arakis little book) and hence there would be no particle physics.
One should not emphasize so much its negative side (not covariant=observer dependent for finite distances) but concentrate on its asymptotic covariance and observer independence=invariant S-matrix.
It is very very unfortunate that even philosophers of science have fallen into this pifall see D. Malament, In defense of a dogma, why there cannot be a relativistic quantum mechanics of (localizable) particles, in R. K. Clifton (Ed.) perspectives of quantum reality, Dortrecht Kluwer, 1996 . The title of this article is total humbug but its main No-Go theorem is not only correct, it is something of the finest. The author shows that the N-W non covariance syndrome is part of a more general theorem (which bears his name) which establishes the impossibility of projectors with certain properties; it in turn is a special case of the by now well-understood property that any notion of localization that requires the set of states localized in a spacetime region O to be orthogonal to the states localized in the causal complement O' is incompatible with translational covariance and positivity of the energy. Unfortunately no word in Malament that this conclusion is evaded in asymptotia which makes particle physics more than an entertainment on a high intellectual level for philosophers of science. Working with the right concepts of modular localization (which does not lead to localization probabilities but rather to expectations which cannot be resolved in terms of probabilities). Before you ask any question about such things you should study the articles (I cannot be your weblog nanny). If you look into
http://br.arxiv.org/abs/math-ph/0511042 (in print in CMP)
You will realize that the main problem is not computation or mathematics rather these problems are conceptually unusual and very demanding. As often in life, the biggest step is to overcome one owns prejudices. What makes me sometimes angry are those string aficionados who have lost the intellectual modesty one needs for understanding local quantum physics, but I never had the impression that you, Eugene, are part of that group.
By the way, localization is absolutely crucial for the autonomous physical interpretation of the theory, it is very very basic. It is the place where string theory manages to create the greatest tohu-wabohu under the sun.
Your other questions I will comment on in a separate blog.
Sorry for the delay, there was some work on my server which took more than half the day.

JC *says:*
June 1, 2006 at 10:40 pm

Bert,

I think I'm starting to understand where you are coming from with respect to AQFT. For many years I more or less looked at AQFT as if it was just purely a formal math problem. After reading your posts here for awhile and some of your papers, I'm getting the impression that the conceptual issues behind AQFT are just as important as the math content. (ie. No math for the sake of math).

When I first studied quantum field theory, I largely glossed over a lot of the conceptual issues. In those days I was more interested in grinding out Feynman diagrams.

Bert Schroer *says:*
June 1, 2006 at 11:52 pm

JC
even such a phantastic vizualization of perturbation theory as Feynman's gift to QFT could become oppressive and counterproductive after half of a century if its iconization is driven to a point where it becomes synonimous with the vast an still largely unexplored territorry of local quantum physics.

Eugene Stefanovich *says:*
June 2, 2006 at 2:31 am

Bert,

thank you very much for your detailed explanations. Now I start to understand that your earlier comment that the only thing common to both QM and QFT is the Planck constant, may be not as big exaggeration as I originally thought. I can't swallow this so easily.
Need to think.

Bert Schroer *says:*
June 2, 2006 at 5:02 am

Eugene,
I sometimes use a bit provocative formulation, but I never manipulate facts. In the present case the reason is of course that you are not the only one who has (had?) this quantum mechanical view of the vauum; to the contrary you are in the illustrous company of Weinberg (see earlier remarks) and all those illustrous prople who use the classical (differential geometric) Bekenstein law for quantum mechanical degrees of freedom counting in order to extract an energy/entropy formula. Without having looked at the details, the violation of the local covariance principle (leading to a localization-dependent vacuum polarization) makes such counting arguments very very suspicious to me (and I am not the only one, see http://br.arxiv.org/abs/gr-qc/0405082). This new principle to which Hollands and Wald have prepared the groundwork and which received the final conceptual and mathematical touch from the AQFT university of Hamburg group, cannot be violated without running into absurdities.

JC *says:*
June 2, 2006 at 2:14 pm

Bert,

The only topics resembling "formal" aspects of QFT which were covered when I first took QFT courses, was stuff like Kallen-Lehman representation, LSZ reduction, and the CPT theorem. The rest of the course was largely covering the details of cranking out Feynman diagrams and doing renormalization.

These days some of my former colleagues start a QFT course by teaching the path integral from the very start, and only giving minimal lip service to "old style" canonical quantization. It's as if they're treating the path integral as if it was a "god given" object of some sort.

Eugene Stefanovich *says:*
June 2, 2006 at 2:32 pm

Bert,

let me get it straight… If I understand you correctly, in your view of QFT there is no Born's probability interpretation, there are no projection operators, and the Hermitian operator of position is absent as well. So, it is fair to say that QFT does not respect the basic postulates of quantum mechanics. Is it right?

Does it mean that the very first sentence in Weinberg's vol. 1 is wrong?

First, some good news: quantum field theory is based on the same quantum mechanics that was invented by Schroedinger, Heisenberg, Pauli, Born and others in 1925-26, and has been used ever since in atomic, molecular, nuclear, and condensed matter physics.

Does it mean that QFT is not (as I naively thought) an attempt of unification of quantum mechanics and the principle of relativity? From your remarks and references it appears that QFT makes its own rules which are quite different from the laws of quantum mechanics as we know them. Is this an accurate description of your position?

Bert Schroer *says:*
June 2, 2006 at 4:15 pm

Eugene,
not quite, you do have localization probability even in QFT and you do also have associated projectors, bot they are neither covariant nor local even though the setting in which they occur is covariant and local (but those noncovariant ones are very good for scattering theory). There is however also a covariant and local concept of localization. If you apply the polynomial algebra P(O generated by smeared field (where the smearing functions range over all smooth functions which have their support in a spacetime region O).to the vacuum i.e. P(O)|0> you get a dense linear subspace of the full Hilbert space (e.g. the Fock space). The totally unusual (from a QM viewpoint) fact is that there is very deep physical information in this inclusion of this dense subspace within the full space. With a change of O the position of there dense subspaces change their position. With the help of the definition of the Tomita operator S (look into the paper) you can write this dense space as K+iK where K is the +1 eigenspace of S. K is real because S is anti-linear! The unusual aspect is the reality of K (which of cause in general have no bounded projectors), nowhere in QM you need to highlight real subspaces. In the case of O being the wedge region W, the polar decomposition of this S (see paper) leads to two operators with a known geometric and physical significance (see paper) which is related to to the K-spaces. As Mund has shown (see previous reference) one can also avoid the introduction of K's and work directly with the S(W)'s and their intersection. S is also intrinsically related to the wedge-localized subalgebra A (W). By forming algebraic intersections you can get to compactly localized subalgebras. There are of course projectors inside these subalgebras, which behave completely covariantly but none of them has an associated probability interpretation i.e. they are completely consistent with Malaments No-Go theorem.
I think with these remarks I have led you .beyond the first hurdle; try to go the rest of the way yourself (and let me know when you get stuck for more than two days).
This theory is very very deep. For the first time it supplies the entrance into QFT "without classical crutches" which Jordan pleaded for in his 1929 Kharkov talk. But everything is very much at the beginning, there is a vast territory to understand and the expected light at the end of the tunnel would be a genuine intrinsic approach in which objects cannot be characterized any more by their Lagrangian names. The first glimpses of this new way of looking at QFT is supplied by chiral and factorizing theories. If you clinge to Lagrangians, forget it.
Sorry, nobody has planned the great abyss between the standard way and this new perspective, but as a theoretical physicist you have to follow the intrinsic logic of things and not any fashions of the day.

Arun *says:*
June 2, 2006 at 5:02 pm

How does the Hollands-Wald paper tie in with Wilsonian renormalization group ideas, if at all? Presumably the "holistic aspects of quantum field theory" do not show up in effective field theories with a finite cut-off?

Bert Schroer *says:*
June 2, 2006 at 5:35 pm

A very tough question, Arun, since the Wilson RG is in momentum space whereas for the formulation of the local covariance the space of localization space is essential. On top of this, there is the problem that the Wilson RG is formulated in the Euclidean setting where the integration over certain over subvariables (the thinning-out process) corresponds to abelian conditional expectations. On the other hand its real time analog has to face to deal with nonabelian conditional expectations. There is a related very deep unsolved problem which is to study the relation between boundary problems in the Euclidean setting and the superselected charge sectors in real time local observables. From studies of 2-dim. conformal QFT one knows that there is a relation (the Cardy's Euclidean way and the Longo-Rehren real time setting).
Once in the middle of the 90ies, I suggested this problem to Schomerus (he had the necessary conceptional and mathematica recourses), but that was at a time when he had to build his career. Of course usually after people have made it, and could afford to really deep problems they are usually already very much attached to those problems which brought them there.
From Urs formulation I have the impression that this has something to do with that turned down research project of the AQFT Hamburg group, but since I lost the connection with that group, I am not sure.

Eugene Stefanovich *says:*
June 2, 2006 at 6:00 pm

Now the difference between our approaches is clear to me. You insist on manifest covariance (= exact, interaction-independent tensor transformations of observables, = Minkowski space-time) and allow bending of the rules of quantum mechanics.
I adhere to strict QM and relativistic invariance (= Poincare group properties) and I am willing to sacrifice the manifest covariance. This is in
http://www.arxiv.org/physics/0504062

Bert Schroer *says:*
June 2, 2006 at 6:40 pm

Not quite, Eugene, not with the N-W localization, in that case I would only insist in asymptotic covariance. If this would not be possible there would be no invariant S-matrix. But in the case of finite distances where I have to use the covariant localization (achieved by modular theory) I am not forced to use any covariant tensor formalism. The only thing I have to pay attention to is that when I have operators which I suspect to be localized in a spacetime region O, the application of the Lorentz transformation should lead to operators which are localized in the L-transformed region. And this is precisely where the Born localization fails: if something is N-W (quantum mechanically) localized in e.g. a sphere at a given time, then its L-transform is noncompactly spread all over the place although this spread is numerically small at large distances. But there is no uniform controll of this!!
But all of these "superluminal" effect are irrelevant in the asymptotic calculation of the S-matrix, they do not leave any on-shell effect. But do not use N-W (QM) for any other purpose than scattering theory.

Eugene Stefanovich *says:*
June 2, 2006 at 7:03 pm

Bert,

The only thing I have to pay attention to is that when I have operators which I suspect to be localized in a spacetime region O, the application of the Lorentz transformation should lead to operators which are localized in the L-transformed region.

That's exactly what I meant by "manifest covariance"

And this is precisely where the Born localization fails: if something is N-W (quantum mechanically) localized in e.g. a sphere at a given time, then its L-transform is noncompactly spread all over the place although this spread is numerically small at large distances.

I see that you are trying to avoid the Hegerfeldt's paradox at all cost. Even at the cost of abandoning the laws of quantum mechanics. As I wrote earlier, I don't consider this "superluminal spreading" to be that dangerous. If you take into account the probabilistic nature of measurement, this curious effect becomes rather harmless, and it doesn't contradict causality. There is no way one can build a time machine or kill his grandfather by using this effect.

Bert Schroer *says:*
June 2, 2006 at 7:24 pm

Eugene
It would be harmless if you would have a uniform control, but you havn't. Your view gose against Fermi's conclusion from his two-atomic Gedankenexperiment. His way of arguing may be mathematically doubtful (people at that time compensated the more feeble mathematics by stronger guts-feelings) but nobody wants to resurrect Hegerfeldt's paper, not even the author himself. The numerical argument is not enough, you need uniformity otherwise you mav be able to think about a sequence of modified Fermi-like Gedankenexperiments where the effect becomes so large that even you would not want to see the resulting acausality in your life (a time-machine sequence). Lack of uniformity does not permit you to use the word "small" inasmuch as one cannot say that a women is a little bit pregnant. And by the way, all the reported experimental verifications of superluminality turned out to be poltergeist effects

Eugene Stefanovich *says:*
June 2, 2006 at 7:44 pm

Bert,

I didn't mean that the effect is harmless because it is small. In my opinion, it would be harmless even if it were big. I agree, it is strange that different observers disagree on whether the electron is localized or not. But I still don't see how one can build a time machine out of this, even assuming that this effect is amplified to the macroscopic scale. If you have a concrete proposal how to build a causality-violating machine using the "superluminal spreading", then I am ready to abandon quantum mechanics and the Newton-Wigner position operator and join the AQFT research.

Bert Schroer *says:*
June 2, 2006 at 8:33 pm

Eugene,
I frankly do not understand why you have this psychosomatic attachment to an anti-Fermi (pro superluminal) formalism against all common sense (it does not only violate experience, but you also forego any chance to understand the quantum version of the Einstein local covariance principle) and you do get nothing in return. I tried to convince you the quantum mechanical particle interpretation is asymptotically valid in QFT, but you insist to use it outside its domain of its validity. I have reached the end of my possibilities; I cannot stop travellers who go into such a strange direction. In any case I hope at least that I convinced you that you do not have to do this, there is nothing in particle physics which forces you

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Eugene Stefanovich *says:*
June 3, 2006 at 1:57 am

Bert,

thank you for trying to convince me. It was great that Peter was distracted by discussions of his NEW book (congratulations, Peter!) and allowed us to have this totally off-topic exchange. I think it is perfectly OK that we travel in different directions. As long as we stay honest to our core beliefs and respectful to each other we have a chance to find the truth somewhere down the road.

Regarding some of your comments.
1. I don't know a single experiment which directly measures the speed of propagation of interparticle interactions (not to be confused with the speed of propagation of light). So, it is too early to tell that action-at-a-distance *violates experience* . Superluminal effects were seen by experimenters in Koln, Florence, Berkeley, and a number of other physical laboratories around the world, not just in poltergeist movies.

2. I agree that my approach is not compatible with general relativity. There are hundreds of theorist who try to *understand the quantum version of the Einstein local covariance principle* . Did they move an inch closer to this understanding in the last few decades? I just don't think that this road leads to quantum gravity.

3. You asked what I get in return for my stubborn adherence to particle-based quantum mechanics and the principle of relativity (= Poincare group). Most importantly, I have a finite Hamiltonian of QED that can be used not only for usual S-matrix calculations, but also for the time evolution and bound state problems. All integrals are convergent, and there is no need for renormalization or cutoffs. This Hamiltonian incorporates all radiative corrections: the Lamb shifts and what have you… The interaction is written explicitly in the 2nd perturbation order. For higher order terms, there is a well-defined algorithm how to get them and rigorous theorems proving that what I am saying is actually true.

Chris Oakley *says:*
June 3, 2006 at 4:01 am

Just to chime in here: it is not clear to me what "causality" actually means at the quantum level. If we take

$\partial 2 A\mu(x)= j\mu(x)$

in classical electodynamics we may solve to obtain an "advanced" and "retarded" solution. The "retarded" solution expresses the principle of causality in that the amplitude of the four-current may not be known to observers until the time it takes a light ray to reach them has elapsed. This I can understand.
In QFT a field has a positive-energy part, which creates particles, and a negative-energy part which annihilates them.
Thus
$\varphi(x)|o>$
represents a particle at position **x** and time xo, but then so does the convolution
$\int d4x' C(x-x') \varphi(x')|o>$
where
$C(x) = \int d4p\ eip.x\ \theta(po)$
which is a non-local smearing that filters out the positive-energy part, so although it seems clear to me that fields need to commute or anticommute for spacelike intervals, if only because of the spin-statistics connection, the connection with causality, or even the definition of causality at the microscopic level remains – to me – unclear.

Bert Schroer *says:*
June 3, 2006 at 4:26 am

Eugene,
the local covariance principle is a principle in the setting of QFT in curved spacetime (black holes, Hawking radiation etc.) and not in a still illusive QG.
When I said that you are in the illustrous company of Weinberg I meant this in a metaphorical sence and I certainly did not want you to take this literally and feel encouraged by this. His extreme use of a quantum mechanical argument in the calculation of the vacuum polarization contribution to the cosmological constant is a blooper in is otherwise impeccable ouvre. He has never ever used such a picture for the calculation of Lambshift in his book (which really is an evergreen), whereas your quantum mechanical extremism would force you to do a new computation, of which none of us can imagine of how it would look like.
I am sad, but o.k. as long as you never tell anybody that you talked to me, go in God's name.

Bert Schroer *says:*
June 3, 2006 at 4:31 am

Oh no Chris, not again.

Chris Oakley *says:*
June 3, 2006 at 7:42 pm

Sorry if I'm boring you, Bert, but I've seen spacelike commutativity/anticommutativity called the "causality" statement on more than one occasion. No doubt AQFT folks would never be so sloppy, in which case, good for them … and why does this ignore <sup> and <sub>? Maybe I should try [tex] … $\int d^4x' C(x-x')\phi(x')|0\rangle$

Not Even Wrong
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