Re: the basis of relativity

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- *From*: Baugh <baconbaugh@xxxxxxxxxxx>
- *Date*: Thu, 02 Jun 2005 00:46:45 −0400

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Bilge wrote:

      Baugh: >Bilge wrote:
      >> Baugh: >> >> >Then of course there is invariance which we
      must assume for groups of
      >> >"non−physical transformations", e.g. gauge
      transformations.
      >> >> We do not ``have'' to assume them. We are perfectly free
      to
      >> to create theories which are not gauge invariant. We assume
      >> gauge invariance, because there is a great deal of
      _physics_
      >> in that assumption.
      >
      >For the quantity to be meaningful it *must* be independent of
      >those choices we make in the description which are not
      physical,
      >i.e. choice of basis, choice of zero phase, etc.
      Once again, I said nothing to the contrary. You seem to
      missing
      the point, entirely.


Very well what then do you mean by "we are perfectly free to ...
not gauge invariant." above.



      >Otherwise we "change reality" by doing a little math. You are
      >correct in that we needn't do physics. One can create
      theories
      >which are not self consistent or unambiguously interpreted.
      If you believe that, then why are you having so much
      difficulty
      attributing physical significance to the mathematical
      quantities
      that make the theory consistent?


Because the physical significance is relative to two artificial
constructs, in this case space−time and the potential fields.
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Let x be a physical quantity, make up quantities y and z = x+y.
y and z are not physically significant alone but their relation
to each other is.

Given a theory derived using quantities y and z as elementary then of
course you cannot simply drop one.
This is what occurs when we insist on the canonical formalism
and its underlying fixed symplectic structure to derive quantum
gauge theories.

>> The entire standard model follows from gauge invariance. (I
don't
>> simply mean that gauge invariance is an additional
assumption which
>> doen't change the theory. Gauge invariance _IS_ the
standard model.)
>
>Gauge invariance "under a particular group structure".
The point being what? Try to find a different group structure
that
corresponds to physical measurements. If it were that simple,
minimal
SU(5) would have been guaranteed to work, since the standard
model
_is_ a _particular_ decomposition of SU(5). However, it did
not
work.

Your point being what? I assert that the standard model and various
extensions makes a major unfounded assumption, that the generators of
one the distinct gauge sub-groups, U(1), SU(2) and SU(3) all commute
with the the generators of another of these groups and with the Poincare
group. There are more ways to vary the total gauge group than just
tacking on more generators. And there is no specific reason to assume
that weak isospin is independent of color and hyper-charge or say spin
and momentum. To the contrary there is obviously serious interplay
there since the various gauge charges for the fundamental particles
do indeed have strong correlations.

>The Model part being the assumption of that specific group
structure
>beyond transformations of observables, e.g. SU(2) isospin
gauge and SU(3)
>color gauge, when we as yet have no mechanism for observing
isospin

>(except the one component contributing to E−M charge) or
color directly.
Nonsense. SU(3) color is believed to be an exact symmetry. You can
``observe'' the color degrees of freedom as directly as most anything
else observed (I've never ``seen'' an electron either, beyond the
predicted behavior of a signal in counter.) The color degrees of freedom
can be observed in deep inelastic scattering and electron positron
scattering. For example, the branching ratio for e+ + e− −> hadrons
vs. e+ e− −> \mu+\mu− differs for a model with and without color
degrees of freedom. By a factor of 3.

Yes but observing that one model fits data better than another is not the same thing as observing the component of that model. You cannot directly measure the color of a particle it is not a physical observable in the theory although it is treated as one in the model. I'm not disagreeing with the model, I am not saying color doesn't exist. I'm saying color is as yet a construct and its existence or non−existence is a moot point. For example, what evidence have we that rotations commute with color transformations? Design an experiment to verify this.

The existence of weak isospin also has direct physical consequencea. (In the futre, please qualify isospin with the proper qualifier, i.e., weak or nuclear. Nuclear isospin is very commony used in nuclear physics and without context, it would not be obvious to which you refer.)

Yes, thanks for the qualifier, I'll be more specific in future. But as you say weak isospin has direct physical consequences, I simply say e.g. you cannot observe w2 vs w1 vs w3 where w3 is the component used in the E−M charge formula. We observe w3 in that we observe the distinction between neutrinos and their dual leptons. But this in fact is an observation of charge and mass. I wouldn't replace the weak isospin component of the model without a better one, but it is in this sense ad hoc. Before we get too involved in the question of "why is weak isospin symmetry broken?" we should consider more carefully how its restoration fits in with the other symmetries e.g. translations and rotations and boosts. In short, why are we looking for a Higgs particle?

The conserved vector current hypothesis (CVC) os probably one of the most aspects of the standard model. It predates the standard model by more than 20 years. It says that the weak interaction is a triplet of vector currents which differ by a rotation of weak isospin. The classic test is a measurement of ft_values for superallowed fermi decays

(0+ -> 0+). In fact, if the weak coupling constant G_f is a constant,
you've proven the same thing, since you have 3 different currents
all with the same coupling. That defines a symmety such as weak isospin.

>But in addition there are ad hoc assumptions which break the gauge >symmetries.

So what? Everything is ``ad hoc'' until it's either shown to be
incorrect or explained by something less ``ad hoc.''

But my point is that we introduce "ad hoc" assumptions to explain phenomena which are not directly observed but rather are components of the model. It is like asking what is the mechanism by which the aether shrinks measuring rods in the pre-Einsteinian interpretation of the M-M experiment.

The standard
model and general relativity explain a great deal more with a great many fewer ad hoc assumptions. By an yardstick ever used in any physical theory, the quantities in the standard model are justifiably considered physical.

Possibly you are correct. I may be drawing a line between "physical" and "conceptual" which must be drawn somewhere and I may simply be drawing it too conservatively for your tastes. The issues get fuzzier the more extreme the experiments in which we "observe" quantities. The point I was making was that the Standard Model is justifiably called a model and not a theory because there are elements which are not directly observable, that thus should not be set in stone before considering alternative models. The level at which the Standard Model predicts phenomena, the theory built upon this model, is the level at which we may directly observe hadrons and leptons. When we then consider quarks, is it meaningful to assume the operator defining momentum commutes with the projection operators which distinguishes a red top quark? Certainly with confinement we cannot readily translate them.

>The model aspect is the assumption of an underlying dynamic structure
>where these symmetries are restored.
Again, so what? (You have that backwards by the way - the dynamical
structure is the broken symmetry. The underlying structure is the
fundamental part.)

Not quite what I meant. I was speaking to dynamic structure at both levels, symmetry and symmetry broken. As for example the dynamics of atoms below the level of the dynamics of crystal vibrations. Hence the "underlying" qualifier.

>It's of course not a bad assumption given the success of the
>resulting theory.
On that basis, one could say it's the best assumption in history.

>Yes, good point. I'm thinking more in terms of GR wherein one has >recognized the need to impose gauge invariance within a prior theory.
>(Though the term "gauge" wasn't used.)
But general relativity is a classical theory. The gauge symmetry
is evident in maxwell's equations, but in the context of maxwell's
equations cnnot be used to predict any physics. That's why the potentials in classical theory are artifacts. In quantum theory,
the potentials become interactions coupled to fields.

I mean, look, this is about the aharanov-bohm effect. If you can demonstrate how to obtain the shift in the interference pattern using just the E and B field, please do.

I am by no means arguing that you can ignore the A field in the field theory. (classical or quantum). But in the case of the A-B effect we do not observe phase, we rather observe the shift of interference which we interpret in terms of relative phase shifts for paths. So saying the A field affects phase doesn't quite mean we observe the A field indirectly in the A-B experiment. It is two levels of abstraction deep rather than the one level deep for E and B which we observe by observing forces on charged particles.

By the same token, the necessity of including ghost particles in the successful quantum field theories is not an argument for their physicality. The success of the theory argues that they must be included in the mathematical calculations but not that we should take their "reality" seriously and go looking for ghosts.

[...]
>I would also say that in the modern treatment of both classical and >quantum electro-dynamics these "models" are ignored almost entirely

>which is the point of the formal language of gauge theory.
I beg to differ. Allow me to quote a line from the first volume of
``Quantum Fields and Strings: A Course for Mathematicians'', Witten, E.,
et al, Chapter 1, Classical Field Theory:

``Rather, we can couple [a relativistic particle] to fields,
specifically to an electromagnetic field (abelian connection)
and to a gravitational field (variable lorentz metric).''

The electromagnetic field here is A_u. By ``potential function,''
they refer to the type of potential one would write in a galilean
invariant theory, e.g., U(r) = e^2/r.

In the modern treatment one works with propagators and Feynman diagrams. The field values at each point in space are replaced by the interactions of various physical and virtual particles. The very distinction between "classic" QFT and "modern" QFT is exactly the abandonment of this picture of field values at every point of a space-time manifold. Rather the notation for creation and annihilation operators, a(x) a*(x) is reinterpreted as a parameterization of the acts of creation and annihilation of quanta. Space-time becomes a *parameter manifold* instead of a physical one.

[...]
>gravitation you then have in the gauge transformations of standard EM
>a variation of the geometry of the 5 dimensional K-K manifold.
Sure. So what?

The point again is the comparison of interpretation for the same quantity in two distinct models of the same theory.

>In the K-K theory this is changed to a variation of which submanifold
>one projects onto to define space-time (at zero phase).
I think you're getting carried away.

Quite possibly.

Kaluza klein theory is simply
a 5−d wave equation that has _exactly_ the same solution as the
4−d wave equation x a phase. The phase is not observable. Moreover,
the radius of the compactified dimension was (is) considered to be
physically significant.

Yes relative to the scale of the other four dimensions and then in
that they are comparable via the common metric. In which case the
A field is this relationship. The radius is significant relative to A
and the A field is significant relative to this radius. Neither alone
is meaningful.

>Essentially one re parametrizes the this 5−d manifold so that the
>x_mu in one case is x'_\mu, a mixture of x_mu and theta in the other.
In which case, it can't ``project'' out at ``zero phase.'' The
phase transforms. It's unobservable. That's not the same as
being zero.

Yes but the "vacuum" case defines the zero phase modulo choices of
gauge. Fixing the gauge is precisely the choice of coordinates on
this 5−d manifold. The constraint $(x,y,z,t,phi) = (x,y,z,t,0)$
defined the projection to which I referred.

[...]

>One can always add enough dimensions so that the geometry is fixed
>and the dynamic forces are merely the non−geodesic components of
>the curvature of the sub−manifold we identify as space−time.
That's the point of invariance. One is seeking a theory in
which there are _no_ forces, per se.

No, the theory remains unchanged under this alteration of the underlying
"model". Rather one is choosing one of many models for the same theory
in which there are _no_ forces, per se. This is the meaning in my
mind of the equivalence principle, not that the forces go away but
that the forces are defined only relative to the geometry et vis versa.

>This is what M−Theory is all about. But I assert that this is
>introducing too many artifacts into the theory when we quantize.

I would assert that no one knows what are artifacts and what deserve status as a genuine part of theory until experiments tell you which is which. In the case of E&M, the four potential _is_ an artifact in _classical_ theory. It would be an artifact in quantum theory, too, were it not for experiments that cannot be explained using E and B.

Don't confuse the mathematical structure for the theory. The theory as I am using the term is the system of predictions. The E and B fields are part of the theory as the predicted forces on test particles. The probability distribution defining the interference pattern in the A−B experiments is part of the theory. Below this is the A field which is utilized to calculate both of these elements. I wouldn't attempt to guess how you could calculate these elements without utilizing A or some equivalent in a more general theory. I am not arguing for its elimination form the tools we use to "do electrodynamics" I am simply stating that it is a quantity of a distinct type from E B and |psi(x)|^2.

[...]

>I'm suggesting that the equivalence principle is not fully
>integrated into such theories.

Sure they are. The standard model treats all mass as inertial mass.

That alone is not the E.P. It is the consequence of the E.P. in the geometric formulation of GR. It doesn't matter as much at the classical level but when we attempt to quantize there is a constraint which picks one underlying model from the other i.e. the constraint: Forces := 0. This constraint when we quantize must either commute with the relevant observables or the consequence of its non−commutativity must be reckoned with. It is the same as when one works "off the mass shell" in quantum particle dynamics. And this I assert is part of the reason quantization of GR has been unsuccessful. Of course I can criticize all day at others failures, but I don't expect too much attention unless I can offer something more constructive i.e. a successful theory of quantum gravity. To this I can only say give me some time.

[snip]
>I'm suggesting that reifying space−time as a physical 'brane' in
>some larger dimensional space is going to lead to non−renormalizable

>divergences pretty-much no matter how cleverly it is quantized. I
>don't have a proof.
Either way, it wouldn't matter, since I'm not going to cheerlead
for string theory. I'm perfectly willing to believe it, if there is
experimental evidence that agrees with predictions unique to string
theory, but so far there aren't any (with the possible exception
of some qcd sum rule, which has been validated, but as far as I
know, hasn't been derived from quantum field theory). Since ed
witten knows a great deal more about string theory than I do, I'll
leave the believing up to him for the time being.

I even hesitate to call it a "theory", rather a class of models
possibly leading to theories but, again as I assert still incorporating
what I see as fundamental conceptual flaws.

>I simply have an intuition based on my belief that we shouldn't
>"quantize" the mathematical artifacts of our exposition but rather
>the physical observables of the theories.

The aharanov-bohm experiment demonstrates that A is not an artifact.

I don't exactly disagree but cannot agree. Rather I view A as being
a composite entity representing a mixture of the physical and the
artificial.

So far, you've said a lot of stuff that is rather straigt forward and
uncontoversial, but have not explained the aharanov-bohm effect using
only E and B, which you seem to think is possible.

Again I do not seek to "explain" the aharanov-bohm effect. It is
and empirical effect and I must in any alternative or improved theory
be sure it is predicted or my theory dies on the vine. But said
hypothetical theory needn't predict or incorporate any feature of
the electro-magnetic vector potential provided the A-B effect is
predicted accurately. I can't imagine a theory which would not
incorporate some analogue of the A field. But I can't see any
reason to assume this is necessarily the case.

Let me further state that elimination of the A field is not my goal.
I'm not trying to "explain the A-B effect using just the F field."

I'm not trying to explain anything. I'm specifically trying to quantize gravitation. I don't specifically recall how we got on to this topic.

> If you want to join in such a discussion, you might help out eugene stefanovich on sci.physics.research, who has similar ideas and took his argument there after being shot down here. The two people arguing with him on sci.physics.research seem to have more patience.

See previous paragraph.

> >> The gauge degrees of freedom have immediate physical sgnificance.
> >
> >Yes but not as physical degrees of freedom.

> I think you should compare theory to experiment a bit more. A four-vector has four degrees of freedom. The photon has two. How does one account for the two missing degrees of freedom?

By noting that the photon in actuality has a huge number of degrees of freedom which we call infinite for want of an exact number. We pick out two of these for the photon's polarization because we are factoring the photon's irrep of the Poincare group in a specific way. The very fact that we do not see four polarization modes for the photon demonstrates that this factorization: 4-vector x scalar wave function, is not correct and in fact not irreducible.

The very reason we factor in this way is because we are working from a model of "fields over a space-time manifold". The very fact that we must impose additional constraints to get the observed two polarization modes is precisely the "kludge" we introduce to fix this less than ideal model.

> The condition d_u A^u = 0, reduces the number to three. That makes the photon a spin 1. The remaining three degrees of freedom are the three possible polarizations. In order to conserve charge, it must be possible to insure that the time component vanishes. That imposes the additional constraint which eliminates the longitudinal polarization. The absence of a longitudinal polarization means the photon is massless. The two degrees of freedom which remain are the two transverse polarizations.

Yes yes yes, I'm well versed in QED though a bit rusty, it's been a while since I parsed through the derivations. But keep in mind I am not talking about alternative field theories but alternatives to field theories. As long as you are working within a field theory framework, all you've said is 100% correct. It none the less is not especially relevant to my points.

> >It is the combination of this artificial format along with the gauge
> >degrees of freedom which together "have immediate physical significance."
> I have no idea what that means. Are you saying the electromagnetic
> field is not a vector field? If it is, then I just described the
> two degrees of freedom.

I'm saying that as long as you work within the format of fields over a space-time manifold then you must incorporate this additional artifact. I'm saying that field theory itself incorporates an additional artifact, the fixed division of space-time and field. It is comparable to the fixed division of space and time prior to SR. Upon unification we loose the artifact of "absolute universal time". I assert that in a unification of space-time-field you likewise loose the artifact of "absolute vacuum". The A field then will take the same stage as the boost parameters distinguishing different frames, i.e. as a relationship between alternative decompositions of the same physical quantities into components.

> >As a simple example consider a periodic physical degree of freedom. Now
> >force that into a flat model by describing it as a rotation in two >space. You must then append the radial scaling as a gauge degree of >freedom and you have a gauge constraint R = 1 e.g. by which the point >object is rotating on a unit circle in your model. The periodicity is >physically significant hence to get your point to be periodic it is
> >necessary to impose the gauge condition which requires you address that
> >gauge degree of freedom.
> That's entirely different. What you are describing is hidden variable theory. There is nothing hidden about a gauge field.

No. I am not talking hidden variables. I'm talking appending mathematical variables in order to embed the physical variables in a particular form of mathematical construct. To identify the physical variables you then must impose additional mathematical constraints. That my dear is gauge, not "hidden variables".

[...]

>> It's the freedom to make a gauge transformation that has no
>> physical significance.
>
>Right.
So, what's the problem? Do you also think the lack of an absolute
position in spacetime implies the measurement of a distance is an artifact?

No of course not. But it means the specification of position. The values of the position is not physically meaningful. The difference in positions defines the physical distances.
It is the distinction between "real points of space" and space as a construct to express relations between physical objects.

[...]
>Implicit in your exposition is an underlying canonical structure.
And?

See below ***

>[p,q]=i, and the underlying inhomogeneous group ISO(3,1) wherein
>p^2 is an invariant.

No, it doesn't. The mass is casimir operator of the poincare group, just like the spin.

Check again. The Casimir invariant is the contraction of the Killing form with the products of pairs of (representations of) generators.

I^2_rho = B^{ab} rho(G_a G_b).

The Killing form for ISO(3,1) is null for the momentum operators.
It is because ISO(3,1) is a "singular" group (not semi−simple) that it has no unique quadratic invariant.

The squared mass is in fact the additional quadratic invariant
which can be appended to the Casimir invariant.

I'^2 = I^2 + \lambda m^2

*** And the non−uniqueness of this invariant is lost if you
ever so slightly perturb the defining relations of ISO(3,1).
The perturbation yields either SO(4,1) or SO(3,2) in each case
of which the full Casimir invariant is a sum of mass and total
relativistic spin. The value of \lambda above becomes fixed.

The canonical structure goes hand in hand with the singularity of
ISO(3,1) and hence the independence of mass from spin. Mass^2 and
spin^2 alone will cease to be central elements of the group.

Now you can still work within a format of a variably curved space−time
manifold. However our choice of a *flat tangent space* instead of
say a *tangent pseudo−sphere* is not based on any physical assumptions
but rather one of mathematical convention. Replace the tangent
space and tangent Poincare group with a tangent pseudo−sphere and
tangent SO(4,1) group and you have an entirely different format.
In that format we would not define mass but a unified mass−spin.

>Mind you I don't have a replacement theory in which to demonstrate
>a counter example (yet) but I cannot consider the one instance as
>a demonstration that no such theory is possible.

Quite honestly, I can't figure out what _kind_ of a possible theory you
could be talking about, or even the degree to which it would be funda−
mental. What you seem to be saying is that you can find a different
representation for the same group structure. But that's irrelevant.

It would be if that is what I was saying. Rather I'm saying one
can find a different group structure for the same physical theory
(or one very close to the same so as to agree to the level of current
experiment) and that said different group structure may in fact
remove some of the ad hoc assumptions. This after all is what
occurred in SR. It is a matter of looking at what worked in past,
why it worked specifically, and applying this to current theory.
That is exactly why I'm making issues of "what is" and "what isn't"
physical. Otherwise fixating on certain conceptual elements can
inhibit progress in exactly the same way that the concept of absolute
time which was built into the theories prior to SR made it damned hard

to see SR when they had the new transformation group right in front of them.

> Physics is represetation independent. One representation can be transformed into a different one. Reality might look different to a lightcone observer than to us, but who cares?

Exactly my point. So how much of our current theory is "choice of representation". It's hard to see from within the theory especially given its success. But the success of the whole theory doesn't mean all the elements of the model used to derive it should be held sacrosanct. Which should are those which are directly tied to physical observables and those one or two levels abstracted should be taken less seriously as "physical". In an attempt to improve the theory one needs to identify what is representation dependent so that one can distinguish between a new theory and the old theory with new representation.

> >At the level of the group there is no gauge transformation, only a >choice of frame.
>
> Nonsense. Gauge transformas are unitary transforms. Explain to me how a unitary transformation is not ``at the level of the group.''

Reread my prior post, you missed the point. BTW, unitarity is a function of representation, let us rather say "compact". But not all gauge transformations are. Certainly the gauge transformation generated by A -> A + epsilon grad(x^2) is not compact. Any group can action can be made unitary in an infinite dimensional representation so you point about unitarity is not very helpful.

More to the point, the part you cut out is where the sense lies. If you consider carefully the quantization of gauge systems as for example explained in:
"Quantization of Gauge Systems" by Henneaux & Teitelboim

The emergence of the gauge degrees of freedom result from the additional variables you must add to embed the system description within a canonical phase space. You get a group larger than the group of physical transformations because you insist on this canonical language. The result is the additional gauge degrees of freedom and the inability to separate the physical from the gauge until you impose constraints. We then execute canonical quantization and must deal with the constraints and gauge degrees of freedom.

What I'm saying is that if we reject this formula and instead quantize* the group of physical transformations directly then no extra gauge variables emerge.

*(By quantize the group I mean quantize the system on which this group acts, i.e. select appropriate irreducible representations and define the translation metric which yields the physical predictions.)

The result will not be a quantum field theory. Rather it will be a quantum mechanics over a large possibly infinite dimensional space which is the correspondent to the Fock space of the field theory minus the ghosts and virtual particle modes which are appended to retain gauge covariance in the canonical formalism but which have no physical significance.

The result will most certainly give us something distinct from bosons. Rather something which may approximately resolved into bosonic quanta in the low energy limit.
For a physical analogue consider spin waves or phonons in a B−E condensate.

Sorry got pretty detailed here. My point is then that the resulting group will be frame transformations without the additional gauge transformations.

Keep in mind that although we refer to e.g. the SU(2) weak−isospin group as a "gauge group" it is rather the group of SU(2) transformations semi−independently over the points of space−time which contains the gauge group elements. Executing an SU(2) rotation which transforms an electron into a neutrino is certainly not a gauge transformation.

>It is when we project the group representations into >the infinite dimensional representation of arbitrary fields over a >manifold that we get the infinity of choices which constitute choices of >gauge. By excising this model of fields over a base manifold
>we remove those non−physical gauge components without loosing any of
>the physics.

> One ``removes those non−physical gauge components'' by insisting
> the physical results are covariant and finite. That doesn't imply
> you remove the physical gauge components.

One in the standard formulations "removes those non−physical gauge
components" by fixing the gauge. I'm suggesting "removing" them
by not including then in the first place, by working in a formulation
which does not a priori insist we "canonicalize" the system.

And pray tell what is a "physical gauge component"?
(Perhaps our semantics are diverging, I'm referring to
"component of the degrees of freedom"
some of which are free because they are choices
of gauge and others because we freely varying the values of
physical observables.)

−−
Regards,
James Baugh
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