Re: Field theory for continua

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From: Van Jacques (*vanjac12_at_yahoo.com*)
Date: 11/03/04

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Date: Wed, 3 Nov 2004 15:57:44 +0000 (UTC)
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Igor Khavkine wrote:
> On Wed, 27 Oct 2004 15:56:18 +0000, Van Jacques wrote:
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> >> On 2004−10−22, Van Jacques <vanja...@yahoo.com> wrote:
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> >> > I recall Goldstein's "Classical Mechanics" does Lagrangian and
> >> > Hamiltonian mechanics for particles, then tries but fails to do
> >> > continua in the last chapter−−a rather pathetic finish to an otherwise
> >> > great text. One can now
> >> > include continua and use differential geometry.
> >
> >> I'm not sure how Goldstein "fails to do continua" in the last chapter.
> >> I don't believe the treatment is erroneous, but it is brief compared to
> >> the amount of space devoted to other topics.
> >
> > Goldstein fails in two respects:
> > Since one wants to be able to introduce the EM field interacting with
> > (continuous) matter in MHD and plasmas, one must treat everything
> > relativistically (and have Lorentz invariance) from the start.
> > Non−relativistic treatments create many problems, making the introduction
> > of the EM field almost impossible, since the EM field is intrinsically
> > relativistic. I wonder what physicists were thinking when trying to create
> > Newtonian theories of MHD and plasmas. You can't mix one group of eqns.
> > which are Lorentz invariant with another group that is Galilean invariant
> > without creating a mess and introducing serious errors at the start.
>
> I don not think that doing magnetohydrodynamics was one of Goldstein's
> goals for writing that chapter. Neither is his goal to do an exposition of
> continuum mechanics. As best I can tell, the intent of the last chapter of
> Goldstein's book is to introduce the reader to the generalities of the
> formulation of classical field theory, as well as introduce some classical
> fields that will later be used in relativistic quantum mechanics and QFT.

This is what he fails to do, IMO, and for the reasons I gave in
a post above. He mixes up the Lagrangian coordinates and the Cartesian
coordinates. The Lagrangian coords are the field, and the Cartesian
coords

are the independent variables, as they are in EM and all other field
theories on spacetime. He tries to made the Lagragian coords the
independent
coordinates, and the Cartesian coords. into the fields, which leads
him to a mess and conceals important relations, and prevents the
theory from going anywhere.

> *Also, referring to your other post discussing Goldstein, I can only say*
> *that his choice of notation has nothing to do with the validity of what he*
> *is trying to say. And as far as I know, his treatment contains no serious*

If you mean he manages to get the right non−rel eqns. in the end,
its true that he does, but at great cost.

> *errors.*
> *Also, his use of index and vector notation as opposed to*
> *the more modern index−less one is no more a draw back than the use of*
the
> *same notation for treating classical mechanics of point particles.*

It can be done without vectors at all, which is how Maxwell's eqns.
were
first written, in x,y,z components. Then with 3 vectors they had a
great advance in notation, which made things compact,
and helped them see relationships more easily.
Now we have 4−vectors and differential geometry, which helps
further. One can always reduce it to 3 + 1 or to 4 separate eqns.

Misner, Thorne, and Wheeler's "Gravitation", Lightmann et al "A Problem
Book In Relativity and Gravitation", the Choquet−Bruhat et al "Analysis
Manifold and Physics" are good examples to see how things can be better
understood with these tools. Its not just notation. There are
many important new ideas in differntial geometry.

> >> *I've found*
> >> *that the use of differential geometry in physics is dictated more by the*
> >> *culture of a given subfield instead of whatever is fashionable in modern*
> >> *mathematics. Continuum mechanics can be done quite well with the more*
> >> *orthodox use of vector analysis. In fact, this is what is done in most*
> >> *texts on fluid dynamics and elasticity theory (for example the classic*
> >> *texts by Landau and Lifshitz).*

But these are old and need to be translated into new notation and new
ideas.
Soper's "Classical Theory of Fields" contains many of the ideas that
I am planning to present, and he uses 4−vectors, since it is all
relativistic, but he does it all with indices and no differential
geometry.
If you can find his book, it has a lot of excellent work which
should have been put in text, IMO.
I have written up my work in papers which I would be happy to send

to anyone who is interested.
(which noone has yet read, and I hope to upload to the archives
when I can find someone to vouch for me−−I have a PhD and published
papers in Ap J, but I don't think that is enough as I am no longer
at a university).

> > I thought that the tools of differential geometry and forms were well
> > established by now. They included vector analysis, but they clarify
> > everything. Esp. Stokes thm. and multiple integrals, which are especially
> > important in dealing with continua.
> >
> > I gave the example of the EM field, which is a mess without the use of
> > forms. Writing Maxwell's eqns. in 4D as dF = 0, F = dA, and d*F = *J, and
> > p dimensional integrals over p−forms should be standard. I hope things
> > like curl(A) and div(B) have been abandoned in favor of dA and *d*B. We
> > can always project onto a 3D subspace of spacetime if necessary.
>
> I would disagree with your assumption. Basic differential geometry is not
> part of the regular undergraduate curriculum for pretty much any
> discipline, except perhaps mathematics. Vector analysis is, but it is hard
> to make the connection between the two without prior exposure.

I wouldn't expect them to necessarily be taught to undergrads. I was
thinking of physics grad students and scientists.

> I also take issue with your claim that since modern diff.geo. notation is
> more compact and more elegant that it is more powerful. First of all,
> "powerful" is not a well defined term. Instead one can look at its
> utility. But before discussing utility, a specific purpose must be stated.
> If the purpose is to express all the dynamical equations of your theory as
> compactly and elegantly as possible? Then yes, I'd say it's useful. If the
> purpose is to perform a calculation in some geometry where space and time
> are naturally split, then I'd say that regular vector analysis is more
> useful. If the purpose is to perform some numerical simulation, then
> explicit coordinate choices must be made and everything must be calculated
> in components. For this case I'd say that the abstract index−less
> formalism is one of the most useless.
>
> In conclusion, is modern diff.geo. notation universally useful? No. Is it
> universally useless? Also no. Just as many other things, it falls
> somewhere in between. To advocate the use of one notation or formalism
> over another, the intended purpose must be stated and a strong argument
> for the advantages for this particular purpose must be made.
>
> >> Although I still find your equations a little hard to decipher,
> >
> > What is hard to decipher? d = exterior derivative, /\ = exterior product.
> > I use \x to denote the direct product in the EMT, but if you are familiar
> > with classical field theory you should know how T is calculated from the
> > Lagrangian. I will leave out \x as I guess it doesn't help. I assume that
> > one has been thru the field theory of the EM field. If not, this post

> > won't make sense, but all physicists should be familiar with that.
>
> I find your equations hard to decipher because you make many claims
> without justification that are not obviously true, although they very well
> might be. At least they are not obvious to me. I've taken a crack at
> sorting some of your equations out in another branch of this thread, your
> clarifications are welcome.

I now understand what you mean, and will try to clarify.
I have also sent you some email and a copy of the most important
paper, which you should have by the time you read this.

> >>I think all of this is well known by now, even if it is usually expressed
> >>in different language.
> >
> > Is it? The only place I have seen it expressed in any language is in
> > Soper's "Classical Field Theory", and he left a lot to be done.
>
> I don not know of where to find a thorough relativistic treatment of
> continuum mechanics coupled to E&M. But some of this is worked out in
> Chapter IV of Landau's Classical Field Theory (vol. 2) and Chapter XV of
> his Hydrodynamics (vol. 6). Also, take a look at Chapter VIII of
> Electrodynamics of Continuous Media by Landau and Lifshitz (vol. 8). There
> the equations for non−relativistic MHD are worked out. You have a point
> about possible pitfalls of using a Galilean theory for matter coupled to a
> Lorentzian theory for the E&M field. However, the E&M equations can also
> be reduced to a set of Galilean equations when the limit c −> oo is taken
> into account. This amounts to dropping the terms corresponding to the
> electric displacement current and the Faraday effect.

I am aware of EM with c −−> oo, but I don't like it. e.g.

the Alfven speed v^2_a = B^2/n non−rel vs B^2/(B^2 +n) relativistic.
As n −−> 0, the 1st gives the incorrect v^2_a −−> oo, the 2nd v^2_a−−>
1 = c.
Lichnerowitz, Andre has published a lot of excellent work on
relativistic
magnetohydrodynamics, and there are a few papers here and there
on continua with EM fields. But it is an area where there is still work
to be done.

> > The point I am making is that the theory is much more powerful in the
> > field theoretic form. This is how things should be done in modern
> > physics−−it is how classical EM is done. The knowledge that A is the
> > vector field for the field theory of EM, and that F = dA, and L = −
> > (F|F)/2 is important and a powerful tool for problems in EM, as well as
> > for integrating it with the rest of physics and for making progress.
> >
> > The same is true for continua. Everyone should know that the field theory
> > of continua has 3 scalar fields, the Lagrangian coordinates, which come
> > from the eqn. of continuity d*J = 0, and the Lagrangian L = − r + L_em,

> >
> > where r = total energy density, and L_em = Lagrangian for any EM fields.
>
> More powerful? That's an ambiguous statement that I discussed above.
> I think it would be a safe guess that anyone working on a particular
> theory knows what the relevant dynamical variables of that theory are.
> But the choice of dynamical variables is not unique, you can also do a
> change of variables. In E&M, all the equations can be expressed either in
> terms of A or F. A is the variable with respect to which the variational
> problem is constructed, but even then. I could define something like A' =
> *A, or A' = A + dV, or A' = (A|.), or other possibly non−linear field
> redefinitions. Just as with E&M, all hydrodynamical equations can be
> expressed in terms of the current density j or as you do in terms of the
> Lagrangian coordinates, or perhaps Eulerian coordinates or some other
> field definition. Once again, you must state your goal and argue why your
> choice of field is best.
>
> Igor

I agree, and hope my paper helps to convince you of some of what I say.

I thank you again for taking an interest and spending the time
going over this stuff. I hope you stick with it just a little
while longer, when you have time anyway.
If nothing else it is good practice in relativistic thinking and
mechanics. Most of this so far is trying to lay some groundwork.
This is the result of many years of work by me, and I found it all
to be rewarding. I hope you will too.

Van