Re: On the Incompleteness of Relativization

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- *From*: Rock Brentwood <markwh04@xxxxxxxxx>
- *Date*: Fri, 22 May 2009 20:06:52 +0200 (CEST)

In reply to:

> There are other ways to attack a paradigm that don't require
> falsifying it to show it's wrong. One can be wrong not just for what
> one says wrong, but (far more importantly) for what one does NOT say,
> that's correct... The underlying issue is what we may term the problem
> of the "Inverse Correspondence Limit". [A −> B]

Tom Roberts wrote:

> I disagree. Strongly.
>
> The reason a new theory B ...

The response (and objection), if they are to have any bearing on the discussion here, should read as follows:

> The reason a *paradigm* B needs some relationship to an older *paradigm* A is
> purely experimental: there were many experiments that supported and
> confirmed A, so for B to be valid it must meet those experimental tests
> (at least those within its domain).

(emphasis and corrections mine)

Otherwise, what you're "disagreeing" is unrelated to what you're replying to; in which case, both the reply and assessment are irrelevant. So, let us assume, for the sake of argument, that this is what you actually intended. [1]

Suppose facts A1, A2, A3 are explained by paradigm A. Paradigm B comes about and the inverse correspondence limit A −> B only has A1 in its domain.

Fact A1 maps to B1. The issue of experimental tests then are to determine whether B1 or A1 is true of the world. We do the test and find that experiment favors B1 over A1.

NOTHING more is needed [#].

To reiterate, it is only with (a2) that one speaks of "empirical verification" or "falsification". It's only here that one draws a relevant distinction between A and B; and only here, where the issue of testing comes up.

By your account, this would be all that's required.

Obviously, that account is unsound. And the reason here is simple to see: you still have the empirical fact A2 and A3 staring you in the face.

The correct statement is that it is never enough to just verify B1 vs. A1 and then stop at that and declare "job done". Without the parallels B2 and B3, respectively, of A2 and A3; the validity of B1 vs. A1 is not "job done", but "job only begun".

If the inverse arrow A −> B can only generate B1, and only has A1 in its domain, then you're facing the situation that the larger body of facts {A2, A3} that had been previously established are left untended.

In that case, one would not then be justified in simply and automatically grandfathering into B all that already been accounted for by A, nor by what had been previously grandfathered into A from whatever came before A; nor would they be justified in pretending that B somehow "subsumes" A or "contains" it.

The "subsume" and "contain" part is the completeness criterion (b).

Completeness of A −> B is a prerequisite to "automatic grandfathering". Without automatic grandfathering, all the empirical science that came before the transition to A −> B will have then been lost in the transition (unless or until re−established on a case−by−case basis) and you will have ended up taking a step backwards, even with the correction A1 −> B1.

And what's really important here is that if you only do a partial transition, you have no way of knowing off−the−cuff which subset of {A1,A2,A3,...} will have actually been included in the domain of the A −> B arrow and which one would have not. This is the golden route through which critical oversights slip −− oversights that can lead to years and even decades of being stuck on seemingly interminable problems (like the Problem of Time, as was discussed a short while back).

> In particular, there need be no "*continuous* connection" between the "formal structures" of A and B.

There always needs to be a transition of the entire infrastructure of A to B in order to ensure the grandfathering of A2, A3, ...

Without a complete A −> B transition, you have to start over on a case−by−case basis and address each item (here, A2 and A3) one at a time that had already been previously established under the old paradigm.

And what you're basically saying here is that either (a) it's okay to neglect A2 and A3 (regardless of whether it means having significant oversights), or (b) it's something we'll get around to later because it's not as important as showing B1 ought to replace A1 (which is just as dangerously unsound a point of view).

That mere fact that you took the very case−by−case−basis approach (which the article is, in fact, calling people out on for doing) in addressing each of the cases in point raised −− essentially trying to kludge your way out of each one, one−by−one, only underscores the point being made.

Apart from ignoring them, the alternative of trying to fill in the all the holes A2, A3, ... left over by the incomplete A −> B transition is the Kludge Approach. Basically that's what you're endorsing (either "Ignore" or "Kludge, after the fact") and, for that reason,

> That's an unreasonable constraint on the structure of physical theories.

.... this very assessment it, itself, is actually what's unreasonable.

> ... theories of quantum gravity could have no such continuous limit,

Moreover, raising as an example a non−existent theory that nearly 100 years of effort have failed to produce hardly serves as an argument of anything ... other than that the thing being argued is, itself, equally suspect.

There is no such thing as quantum gravity and there never will be any such thing as quantum gravity [2]. It is the academic equivalent of vaporware. Gravity is fundamentally semi−classical.

This, too, is a matter that has been raised here quite a few times over the past several years. Indeed, in recent times, one of the moderators of this group has published on the very question of whether "quantum gravity is even necessary". Though he may not be at the point of recognizing the imminence of any No Go theorem for quantum gravity, the awareness is slowly starting to dawn on people that there may very well be a No Go result for fully quantized theories of gravity in 4 or more dimensions.

(Some of this is discussed in more depth in the articles under the "Diosi Interpretation" header; along with two important arguments that serve to explain the impossibility of the very notion of a fully quantized gravitational theory. The only reason you can get away with things in 3 dimensions is because there are no exterior solutions: the Weyl tensor vanishes).

> Again I disagree. This is physics, not math,

No, the question of completeness is a question of logic, not physics; and one which (frankly) a physicist is not qualified to address, while wearing the hat of Physicist. Yet, more importantly: it is a question which physics must abide by and is therefore highly relevant to physics. So it holds precedence over anything you may do, while wearing the Physicist hat. It is a higher authority which even Nature must report to and conform to.

It's no different than the question of consistency [3]. This, too, is a question that is purely mathematical. But it would be foolhardy to declare that, on that account alone, it is "not relevant to physics?, much less that (taking the opposite tack, as you just did) the question "is physics, not math"! As anyone who has ever come face−to−face with a No Go Theorem knows, it sure is relevant to physics! And physics must abide by it. But at the same time, it is certainly not a question of physics, but simply one of logic and math!

In particular, Nature will be bound by whatever No Go result actually comes to be found for any fully quantized theory of gravity in 4 or more dimensions.

Now, even with all that said, the question of the A −> B arrow (for the specific case of A = non−relativistic physics, and B = relativistic physics) does have an important bearing on physics; and in ways you haven't even begun to consider or appreciate −− ways that make the criterion of a *continuous* transition mandatory. [4]

> What is needed is that ALL THE EXPERIMENTS (within B's domain) that supported and confirmed A also support and confirm B,

That is, what you're saying is that one only needs B1, one can ignore A2 and A3 −− and pretend that these facts (which may have long since now been established in either A or whatever came before A) can be ignored.

No point of view that leads to this kind of conclusion −− which you just stated −− is tenable. What it basically amounts to is what those recent television shows on PBS on Negative Plasticity were referring to −− being caught in a serious gully of a Negative Plasticity rut. One establishes a paradigm B, which correctly accounts for corrections (A1 −> B1) of some results; and then, instead of acknowledging that they have dug a gully too deep to make for any semblance of A2−>B2 and A3−>B3, decide to take on the point of view that A2 and A3 somehow "don't count" or that they can somehow be "accounted for" by "writing them in by hand" as Kludged versions of B2 and B3 −− after the fact.

A2 and A3 must always be accounted for; and established either with testable corrections (case (a2) in the original article) or as is (case (a1) in the original article). There is no way to argue yourself into the B1−only box on this. If B2 and B3 are not available, then the question of A1 vs. B1 testing becomes moot.

If B2 and B3 can only be recovered on a case−by−case basis, this is a mere consolation and will do for the time being; but it would be a consolation which would not hold favorably to an approach that is more systematic. In particular, if one finds arrows A −> B that have domains that contain not just A1, but also A2 and A3, then this takes precedence over whatever other explanation or account of B2 or B3 may have been concocted by hand.

That is ALL that is required.

The very moment in time you see this frame of mind in the history of any field, no matter what that field is, is the moment that you know that those following the paradigm have, indeed, become stuck in a terminal rut.

It is never all that's required. Representations for B2 and B3 are always required.

If I were free to adopt the tact, "the (revised) definition of paradigm B is that which pertains to what we formerly called A1 in paradigm A, and only A1 (so that I may exclude the empirical results A2 and A3, which were established in paradigm A)", then you might as well also exclude A1, while you?re at it and define paradigm B to be the empty set.

An authority that would try to Clintonize their way out of dealing with A2 and A3 by redefining the word "is", is impeached, on those grounds alone.

In determining the weak points of any paradigm −− as part of the larger process of moving beyond it and resolving some of the seemingly unresolvable tangles that have plagued the field for the past 100 years −− there is one clear−cut objective test that stands out over all others and serves as a sure guide. And that is, that the closer you get to the weak points ...

NOTHING ... HUMAN HISTORY ... ALL THE EXPERIMENTS ... ALL ...

.... the louder becomes the objection.

Now ... on the issue of allied field of Quantization (the other A −> B arrow), since you brought it up:

The question of Quantization is not a vague open issue as you've made it out to be. This matter is well−understood and there has been a large body of literature developed in the field −− with Landsman taking the lead on much of it. An entire theory exists, called "Quantization Theory", which addresses this issue in depth; and in which (indeed) the notion of a continuous correspondence limit of all infrastructure lies at the center.

What you're calling "unreasonable" is already the way Quantization works. It is therefore also the way the prospective parallel field of "Relativization" will also need to work.

In any field theory, no matter what the theory, the state space will always be the space of classical solutions, one and the same. The classical solutions are embedded, one−to−one, within the quantum state space in such a way that generate a dense subspace (namely, the subspace of coherent states).

Whether it's gravity or anything else; even here, one must respect the Correspondence Limit. This remains the case, regardless of what geometric or quasi−geometric representation you have. The classical states (and the manifold, itself) will still be there −− as the coherent states of the quantum theory.

If you can't even define the coherent states (a good example is to ask what the coherent states of Loop Quantum Gravity supposedly are [5]) then this is a good sign that something's amiss and that the prospective theory is dead on arrival.

These issues are discussed in greater depth in Landsman's 1998 "Mathematical Topics Between Classical and Quantum Mechanics" (an abridged Landsman may be found under "Between classical and quantum", arXiv:quant−ph/0506082 v2 25 Jul 2005). Indeed, the very mapping you say is "unreasonable" is carried out on the entire infrastructure of the classical & quantum theories item by item. Section II "Quantization and the Classical Limit" lays the groundwork of the basic elements of Quantization Theory.

The only new feature a quantized field theory can ever bring to the classical field theory is the addition of a non−trivial "transition probability" structure, as Landsman refers to it.

That means, for instance, to devise a theory that couples a 1−form field and 0−form field, one needs only to (a) actually solve the classical field theory in full, and (b) define the transition probabilities between the members of this solution space. The posing of a non−trivial transition probability structure (that is, one which does not reduce to the Kroenecker delta) is the only thing that will distinguish the quantized theory from the classical theory.

Among other things, it also means that whatever qualitative features that emerge in field theory that (even now) are widely believed to be quantum in essence (e.g. running of the couplings, renormalization group, Landau pole, confinement, asymptotic freedom, infrared slavery) are not! They must all exist at the classical level, as well and, indeed, be classically rooted.

This, in fact, has been one of the main points of a whole series of articles posted here over the past few years; where, on a case−by−case basis, we can actually see where and how it is indeed so that each of these elements are all rooted in classical field theory, not quantum theory.

To the degree you find a theory of gravity without a "classical limit", it will by that very fact fail the correspondence limit test and even the B −> A arrow will be suspect (soundness and empirical validity, itself), never mind the question of completeness.

Notes:
[1] That is, we'll assume for the sake of argument you're actually talking about entire paradigms, like Relativistic vs. Non−Relativistic physics or Quantum vs. Classical physics and not just "new theories" that may be posed within any of these paradigms; and will ignore the attempted topic switch from "paradigm" to "new theory".

[2] Indeed, the only known consistent combination of gravity with quantum theory is probably Carmeli's *SEMI*−classical theory of gravity, which comprises a classical metric and a quantized connection represented as a SL(2,C) gauge field on the classical Lorentzian spacetime. Even this has problems when coupled to matter, but produces a perfectly legitimate quantization for exterior fields, non−linearity and all.

[3] e.g. the mathematical consistency of any classical (or quantum) field theory with linear constitutive laws; or the mathematical consistency of fully quantum theory of gravity, itself.

[4] If you believe in Hawking, Hartle and company (and you should, since the basic premise is correct and well−argued; see the highlighted passage under http://federation.g3z.com/Physics/index.htm#SignatureChange); the initial cosmological singularity is a boundary between a Lorentzian and Riemannian domains. This boundary has a Galilean signature −− quite literally, a 3−dimensional surface of simultaneity that marks "time 0". Hence, the question of devising a continuous transition for the "relativization" arrow A −> B is immediately relevant to the question of how one effects a non−singular transition across the signature−changing boundary. The two issues are not just intimately related, but are different incarnations of the same issue. The only resolutions in the literature at present are deficient in this way: all balking on the question, thus making necessary the use of Columbeau Theory. This is the most immediate application that the question of a continuous transition to the Galilean limit of gravity actually has, and one of the reasons why it's important.

[5] That is, "the coherent states of what would be loop quantum gravity, if any such theory could actually be devised". Since the question is vacuous, the point is moot.

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