

Accuracy vs. Relevance

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- *From:* "I.Vecchi" <vecchi@xxxxxxxxxxxxxx>
 - *Date:* 22 Nov 2005 07:04:33 -0800
-

This message of mine, which is a reply to [1], was rejected by spr, . Since I believe it contains some valid points, I am posting it here for the record.

carlip-nospam@xxxxxxxxxxxxxxxxxxxxxx ha scritto:

> I.Vecchi <vecchi@xxxxxxxxxxxxxx> wrote:
>
> [...]
>> They are testing a theory . If the theory truly has predictive power
>> (which btw I believe GR has although not to the extent commonly
>> claimed), it should be demonstrable in real time. The point, which I
>> will elaborate upon later, is that there is a significant difference
>> between genuine prediction and data-fitting. If they are truly testing
>> the theory's predictions, they should be able to churn out their raw
>> data, check their fit immediately and be able to claim:
>> "see, our model works, it all fits, we predicted it right" or "oh, our
>> predictive model has a problem".
>
> I think you are seriously underestimating the amount of data reduction
> required. Look at the article in Matters of Gravity 26 (available from
> <http://www.phys.lsu.edu/mog/>) to see what is involved.

If no actual prediction can be made, I wonder what GR's predictive power is supposed to be about.

>
> Note that the data analysis will be "blind," in the sense that all of
> the data reduction will be done *without* knowing the actual position
> of the GPB guide star. This will be added in only at the last step,
> precluding any chance of (deliberate or accidental) fudging.

Indeed GB-B has put some internal safety mechanism in place. I wonder whether the same can be said about other epoch-making GR observations such as Taylor-Hulse (not to mention Eddington), which I would really like to be able to examine in detail. Internal checks however are no substitute for transparency and blind protocols too may encode hidden assumptions, especially when they are not declared in detail beforehand. Big science projects are relatively new and their

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reproducibility is limited by the huge amount of resources needed to carry them out. That's already a serious problem. Moreover, as already mentioned in this thread, since the "accuracy" of the results depends substantially on expert data-adjusting, the scope for interpretative adaptation is substantial.

I wonder if I am the only one around who regards openness to critical scrutiny and testable predictive power as essential to any scientific endeavour.

By the way, in my previous post I asked some questions about "a posteriori" availability and completeness of experimental data. I would still be grateful for any feedback on that.

> [...]

>> There is another, distinct and subtler issue, which I refer to as the
>> semantic problem, i.e. the problem of mapping mathematical models into
>> measurement outcomes and viceversa, which might be relevant to discuss
>> here (cf. [1]) .

>

>> Let's start with Wigner, who, contrasting the situation in QM and SR
>> with that in GR writes ([2]) "... the measurement of position, that is,
>> of the space coordinates, is certainly not a significant measurement
>> if the postulates of the general theory are adopted: the
>> coordinates can be given any value one wants. ... Most of us have
>> struggled with the problem of how, under these premises, the general
>> theory of GR can make meaningful statements and predictions at all. ...
>> This is a point that which cannot be emphasised strongly enough and it
>> is the basis of a much deeper dilemma It pervades the general
>> theory, and to some degrees we mislead both our students and ourselves
>> when we calculate , for instance, the mercury perihelion without
>> explaining how our coordinate system is fixed in space, what defines it
>> in such and such a way that it cannot be rotated, by a few seconds a
>> year, to follow the perihelion apparent motion. There must be
>> some assumption on the nature of the coordinate system that keeps it
>> from following the perihelion. A difference in the tacit
>> assumptions which fix the coordinate system is increasingly recognized
>> to be at the bottom of the many conflicting results arrived at in
>> calculations based on the general theory of relativity."

>

>> Wigner is talking about the problem of diffeomorphism invariance in GR.
>> Now, while for well studied cases, such as a.o. the mercury perihelion
>> and structurally similar situations, physicists know how to choose the
>> coordinates so as to obtain results that fit observations, I am not
>> sure that this holds in general.

>

> This is certainly a major issue in quantum gravity. But in classical GR,
> it is not, or at least need not be.

I think the distinction is spurious. See below.

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- > The basic point to remember is that
- > *actual observations* are diffeomorphism-invariant.

What you refer to as "actual observations" are measurement outcomes. The statement that measurement outcomes are diffeomorphism invariant can be viewed as either meaningless or erroneous.

- > We do not observe,
- > for instance, the "coordinate value of the position of Mercury"; we observe
- > things like "the round trip time of a radar pulse from a fixed location
- > on Earth to Mercury and back, as measured by an atomic clock at that
- > location," or "the angle between the light arriving from Mercury and
- > that coming from a reference star, as measured at a particular telescope
- > at a time determined by a clock at the location of that telescope."

Clock readings are measurement outcomes.

- > Such
- > quantities do not depend on any choice of coordinates. To compare GR
- > to observation, what you do is to compute (in, say, the post-Newtonian
- > approximation) the predictions for such *observables*, and compare them
- > to the the actual observations.

OK. Here comes the key point.

Expanding around the classical limit (or any other limit for that matter) means that you are picking a privileged frame to begin with. Now that's something that you should not be able to do in GR. I mean, such a choice of coordinates is inherently meaningless in GR and yet it plays a crucial role in the approximation you obtain.

Actually, when Wigner sets out to find a solution for the diffeomorphism-invariance problem in GR, he does so by trying to build a theory of space-time measurement, which is unavoidably quantum. The problem however, is intrinsic to GR:

"In relativity theory, the state is described by a metric which consists of a network of points in space-time, that is, a network of events, and the distances between these events. If we wish to translate these general statements into something concrete, we must decide what events are, and how we measure the distance between events" ([2]).

Wigner's events are obviously instances of what you refer to as "actual observations".

So it seems to me that when we talk about any experiment confirming GR we are actually referring to a quantum gravity model (GR + an implicit space-time measurement model), where the observer's frame is already there to break diffeomorphism invariance.

The scope of validity of such implicit model is debatable.

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- > Better, rather than just comparing GR,
- > you look at a more general model (the *parametrized* post-Newtonian
- > approximation, for instance), and find the best fit for your free
- > parameters; you can then compare the result to GR, and at the same time
- > get a good estimate for how good the fit is.
- >
- > For a simple example, take a look at Boddener and Will, Am. J. Phys. 71
- > (2003) 770, "Deflection of light to second order: A tool for illustrating
- > principles of general relativity." The authors discuss the deflection of
- > light by the Sun, with detailed computations in Schwarzschild, isotropic,
- > and harmonic coordinates. They show that, as you say, the *coordinate*
- > predictions differ, and give a careful explanation of the equivalence of
- > the *physical* predictions.
- >

Thank you for your suggested readings. However I believe that your counter-argument misses the key point of Wigner's argument. Wigner points to a deep problem of GR, i.e. its semantic incompleteness and its reliance on a tacitly assumed space-time measurement model. I do not see how your argument addresses that issue.

I still have a question. I remember some posts you wrote about galaxy rotation curves and the problem they pose for GR. Are you sure that's not a quantum gravity issue? I mean, are you sure you can tell when what you call GR fades and quantum gravity becomes relevant?

Best,

IV

[1]

<http://groups.google.com/group/sci.physics.research/msg/f3bae0de448eee61>

[2] E. Wigner "Relativistic Invariance and Quantum Phenomena" Rev. Mod. Phys. 29, 255 (1957)

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• *Follow-Ups:*

◆ *Re: Accuracy vs. Relevance*

◇ *From:* Juan R.

◆ *Re: Accuracy vs. Relevance*

◇ *From:* donstockbauer

• Prev by Date: *Re: Any connection between Gravity and Electro Magnetism?*

• Next by Date: *Re: "Where Is The Kinetic Energy of a Bullet Stored?"*

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