Re: Skolem's Paradox and why is math the way it is?

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From: J.E. (*troubled6man_at_yahoo.com*)
Date: 10/16/04

Date: 16 Oct 2004 13:48:34 −0700

"Shmuel (Seymour J.) Metz" <spamtrap@library.lspace.org.invalid> wrote in message news:<414f9bf0$45$fuzhry+tra$mr2ice@news.patriot.net>...
> In <39d6e584.0409200810.610c9430@posting.google.com>, on 09/20/2004
> at 09:10 AM, troubled6man@yahoo.com (J.E.) said:

[snip]

> >I'm interested in what works,
>
> ZFC works. Other approaches also work, and also have the
> characteristics that you're objecting to.

So using IF−logic has the same problems? Do you have a reference for that? I thought IF−logic wasn't axiomizable? I thought IF−logic was descriptively complete (i.e. only INTENDED models are valid models)?

> >and I wonder why math goes down this road to "uncountable".
>
> Because it works. Because trying to avoid it doesn't work. Because
> it's elegant.

Back to your claims that nothing else works. Have you seriously tried IF−logic or are you just defining past failures as proof of present and future failure?

> >I have to have an intended model in physics,
>
> Why? Do you have an intended model of your own brain? Do you have an
> intended model of a slide rule[2]? Do you have an intended model of
> the scratch paper that you use? Mathematics is a tool that happens to
> be useful; you don't need a model to apply it. In fact, if you want to
> model Mathematics then you have to use Mathematics to do so, which
> would defeat your purpose.

We make formal statements to each other. We don't have a recepticle

in our brain that passes sets and predicates back and forth. Both parties have to make a logical model, and I don't know what you mean when you say they can avoid this. A good way to faithfully share a theory is to teach it to someone that you believe will make an equivalent model, or to first make it so that only the intended models are valid models.

> >I have to have an intended model in physics, because in the end I
> >need operational definitions that relate to things we do in the lab.
>
> You don't need a model for operational definitions.

Have you ever sat down with a mathematician in a modern physics lab and dealt with hard core skepticism about what an electron is. The way out of that room is to demonstrate the observational affect of many electrons and help them contruct a theory about themselves about what is and is not an electron based on experiments they have seen and observations they have done. Then you send them out into the wide world telling them to always compare their theory to new experiments and observations and revise their theory as requried. If you have a better way to deal with such a student that actually results in them practising science, by all means, tell it to me.

> >The "religion" of physics is that the universe is predicatble
>
> When was Bohr overthrown? For that matter, classical systems are often
> nonlinear and thus subject to chaos.

If there was no predictable pattern at all, then it wouldn't be science. I can't tell if you are deliberately trying to misinterpret what I say. Line spectra of atoms are very predicatable. If you can't follow something on the time and length scales at which it is working based on your experiment, then you study the conserved ADN observable results.

> >The better physicists actually compare the generalized
> >patterns to things they hadn't seen before and make sure it matches
> >up. Does that make sense why I care?
>
> No. The better Physicists don't try to model the models of the models
> ... of their tools. They learn what tools are available and how to use
> them.

We can argue forever on whether I'm wasting my time. We can't all do the same things if we aren't making progress. I can stake my career on my hopes of better results, and no one has to follow me. I've seen a lot of great minds not make the progress I'd have expected, so I've come to doubt the tools. Especially when the axioms I was taught in school (comprehension and specification) turned out to be outright wrong and the alleged replacements I can't find anywhere with a non−circular definition.

> >*Lagrangian forumlations are convienences, no one sees one anywhere.*
>
> *The conventional formulation of Mathematics is a convenience. It's*
> *elegant and it works, which is as much justification as you have for*
> *the Lagrangian formulation of mechanics.*

Using predicates defined in a preexisting set theoretical universe to define the set theoretical universe is convient? Isn't just assuming all your theorem are true even more convient and just as scientific? Langrangian mechanics can never be used again as far as I care, this isn't engineering, this is science and QM is the best game so that's what we should be figuring out how to make work.

> >*I think it is actually a waste of time, since the hamiltonian*
> >*quantum approach works all the time.*
>
> *I've never seen a Hamiltonian on the hoof either.*

Have you tried to make a universal quantum theory. If you don't look you can't see in science.

> >*but this does seem off topic to me,*
>
> *It was a rhetorical question, to show that you don't actually apply*
> *the criteria that you ask Mathematicians to apply.*

If a physicist lapsed into circularity, I'd call them on it to, don't assume I don't disapproce of many things that some physicists do. I'm just tired of you assuming that if some physicists do something, that I do it too.

> >*We make models to explain the data we see.*
>
> *You make models, we make theories. You put terms in a Hamiltonian*
> *because they give results you like; we put axioms in a theory because*
> *they give results we like.*

I have a lab to tell me when I'm wrong. If you are just making stuff up without even trying to have a well−defined foundation, then I have no idea what at all you are doing. If you first made some non−circular axioms, then you could say that you are exploring the logical consequences of the axioms. But I haven't yet met someone in math that successfully explained that that was what they were doing.

> >*I can show you experiments where there is a lack of a particular*
> >*type of a neutrino, and where there is a presence of a particular type.*
>
> *No, you can only show me experiments that you *interpret* as showing*
> *the lack of a particular type of a neutrino, and and as showing the*
> *presence of a particular type.*

In my model, yes. You can make your own model and compare that to the data. The sensible thing to do is to choose experiments to distinguish between different models so that the number of models doesn't get too large.

> >*If I use a model that "appears" to have uncountably many parts but*
> >*no one can test (even in theory) more than a "special" countable*
> >*subset, then someone else can merely take my model and remove*
> >*the non−observable parts and call their model a simpler model.*
>
> *They can try, just as they can try to devise a quantum theory that*
> *doesn't involve phases that are, in principle, unobservable. You've*
> *had nearly a century to do it; I won't hold my breathe. Sometimes*
> *unobservable aspects are unavoidable, in Physics even more than in*
> *Mathematics.*

Why do you keep assuming these are unrelated? You honestly expect physicists to make a model without unobservable things in it if they start with math with unobservable things. One makes a model of the whole universe, then one examines in detail the parts that describe the part of the universe that is what part you will observe in teh observation part of an experiment that you devised to compare your model to another person's model. Each model makes a prediction about what will happen. You do the experiment, you compare the results to the models. Best model wins that round, new challegers will likely come up, that's science. I don't know what you are talking about. The model needs to have the parts that correspond to the observations, it doesn't need the rest. To put it another way, the models with the same predicted observations can be considered scientifically equivalent.

> >*Why should I let someone do that to me?*
>
> *Because the Universe is the way it is, not the way that you would like*
> *it to be. Why should you let someone stick you with a metric signature*
> *of + − − − when + + + + is so much nicer?*

I can make a model in a + + + + metric, just as I can make a model where the sun goes aroung the earth. The connections between the underlying space and the physics will be different (e.g. newton's law don't hold (even approximately) if the sun goes around the earth), the usual choices are made to make the models easy, but we've got experimental results to keep us in check from over simplifying.

> >*I will apologize personally for any opaqueness to outsiders in*
> >*physics. I think the first goal of physics today (not the magazine)*
> >*should be to clean up physics education and communication with*
> >*outsiders.*
>
> *Well, I'd consider a workable theory of Quantum Gravity to be more*
> *important, but that may be more difficult. And Physics education isn't*

> *messed up in a vacuum; education is messed up across the board. The*
> *educational situation in English, History and Mathematics is at least*
> *as dismal as in Physics. Further, some of the opaqueness is*
> *unavoidable.*

I'm not worried about opqueness about details, it's the fundamental stuff. Like most people think that SR says that you'd die of old age before reaching stars that are 100 million light years away. That's because they learn parts of theory (no FTL travel) and not the whole theory (with length contration that squishes that 100 million light years into a much smaller distance for the relativistic traveller). And most people think that if preparing a series of particles into energy eigenstates can result is series of different position measurements, that this means that quantum mechanics can't apply to everything, which just has no basis. And thousands of physicists say sensational things to the press (like implying that quantum teleportation doesn't require having specially prepared equipment in each seperate location), to just confuse people more, it not surprise that individual physicists sometimes get the science wrong too, if we commuincate that badly outside of the peer reviewed journals.

> *>Does it really make me a bad person to ask this question?*
>
> *No, just confused. Mathematics is not about what things mean, but*
> *about what can be proven. ...*

Do you really honestly think that you have a proofs of more real numbers than you have proofs themselves. In IF−logic you have descriptive completeness, but there isn't a proof theory that turns out the theorems. Isn't that a better place for creative mathematicians anyway, rather than sticking to a finite axiomization doings things that could have been done by computers? Math and physics used to be closer together and I think things were better then.