Re: SR and GR without math

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From: sal (*pragmatist_at_nospam.org*)
Date: 10/05/04

Date: Tue, 05 Oct 2004 13:54:14 −0400

On Tue, 05 Oct 2004 16:48:25 +0000, Androcles wrote:

>
> "sal" <pragmatist@nospam.org> wrote in message
> news:pan.2004.10.05.15.13.31.116579@nospam.org...
> | On Tue, 05 Oct 2004 05:05:43 −0700, Patrick Reany wrote:
> |
> | > "Bill Hobba" <bhobba@rubbish.net.au> wrote in message
> | > news:<FOk6d.7444$5O5.5193@news−server.bigpond.net.au>...
> | >> "Paul Bramscher" <brams006_nospam@tc.umn.edu> wrote in message
> | >> news:cjchqp$438$1@lenny.tc.umn.edu...
> | >> > I've got a question regarding relativity: is there any way to state
> | >> > SR, GR, or QM fully without mathematics?
> | >>
> | >> Well it depends what you mean by fully. Are the key ideas
> | >> expressible without mathematics? − then I would have to say yes. Is
> | >> the detail expressible without it? No. Physics is written in the
> | >> language of mathematics and why it is so is a bit of a mystery − see
> | >> the following classic essay by Wigner −
> | >> http://www.dartmouth.edu/~matc/MathDrama/reading/Wigner.html. Also
> | >> see Feynman − The Character of Physical Law − page 35 Chapter 2 − The
> | >> Relation of Mathematics to Physics.
> | >>
> | >>
> | >> > I ask because of the curious problem I've encountered with Karl
> Popper
> | >> > and the claim that a scientific statement is one which is
> falsifiable.
*> | >> > But the problem with any mathematical tautology is that it is *not**
> | >> > falsifiable, not due to empirical/experimental evidence to the
> | >> > contrary, but because math can be proven to be inherently correct
> | >> > beyond the realm of the empirical.
> | >>
> | >> You have hit upon a very interesting point. A number of supposed
> | >> laws of physics such as Ohms law are in fact tautologies.
> | >
> | > You're claiming that Ohm's law has no physical content?
>

> Isn't Newton's law typically used as the _definition_ of gravitational
> force?

Which law? He had several.

> G =9.8 metres/sec^2, by observation.
>
> | Isn't Ohm's law typically used as the _definition_ of electrical
> | resistance? R = V/I, by definition.
>
> As a definition, it certainly can't be proved wrong. Many assertions
> about the way gravity behaves on physical materials can be tested, but a
> definition can't be.
>
> | Many assertions
> | about the way resistance behaves in physical materials can be tested,
> | but a definition can't be.
>
> The assertion that gravity in series add can be tested by combining the
> Earth with the Moon, at least theoretically, but the observation of
> gravity can't be.
>
> | The assertion that resistances in series add can be tested, but the
> | definition of resistance can't be tested.
>
> | Whether you'd say it "has physical content" or not is something else
> | again −− definitions contribute to the picture we have of reality, so
> | one might say that they "have content" even though they make no
> | predictions by themselves.
> Hear, here.
>
> Do you still maintain that gravity at the pole is stronger than gravity at
> the equator, thereby causing GR time contraction to counter SR time
> dilation instead of enhancing it, or are you chickening out of replying?

Chickening out? Of what? You're the one who said you were "through with considering", and apparently invested no further effort in understanding the math I'd posted.

Just to reiterate, I never intended to say that because the measured weight of something at the pole is stronger than its measured weight at the equator, it has an effect on GR's time contraction. The two effects are not directly related.

I said that the measured G force at the pole is certainly stronger than the measured G force at the equator, and that's a purely Newtonian result, and you dismissed it as being obviously wrong. On the last pass over it, you said my "upper bound" wasn't _the_ upper bound, which misses the point −− "_an_" upper bound on a value is another value which we know is at least as large as the value we're really interested in, and that's all. There's nothing unique about "an upper bound". In the case I was

examining, all I wanted to show is that a particular value −− the difference in G force at the equator versus the force at the poles due to the oblateness of the Earth, using Newtonian gravity theory, and ignoring the contribution of rotation −− could not be as large as the "lightening" effect at the equator caused by the Earth's rotation. For that, all I needed was _an_ upper bound on the difference in the gravitational field at the equator and the pole on the Earth as it is actually shaped, so long as the upper bound I found was smaller than the effect of rotation. It was. But you snipped all that, apparently because you didn't like my use of the term "an upper bound".

Again, that was a purely Newtonian calculation, which I did to try to determine if a pendulum clock will run slower at the equator. As far as I can see, it _will_, and that has nothing whatsoever to do with relativity.

Finally, GR's time−change effect between the poles and the equator has nothing to do with any non−uniformity of the measured G field at the Earth's surface. It is purely a result of the difference in altitude.

If you're not really completely through considering, then consider this: IF the Earth were not rotating (or were rotating very slowly, like the Moon), then, according to GR:

1) A clock at the top of a tower would run faster than one at the Earth's surface −− AND the measured G force on the tower would be a little smaller than the force at the Earth's surface.

2) A clock at the bottom of a mine shaft would run SLOWER than one at the Earth's surface −− BUT the measured G force at the bottom of the shaft would _also_ be smaller than the force at the Earth's surface.

Does this sound contradictory to you?

In simple terms, the different rates are due to the difference in gravitational potential, not the difference in local G field strength.

> Whether you'd say it "has physical content" or not is something else
> again −− definitions contribute to the picture we have of reality, so
> one might say that they "have content" even though they make no
> predictions by themselves.
>
> The predictions of relativity do not seem to have contributed much to
> our picture of reality, do they?

Oh, I don't know about that. They've provided lots of entertaining material to argue over, which certainly enhances MY picture of reality.

> "Thence we conclude that a
> balance−clock at the equator must go more slowly, by a very small
> amount, than a precisely similar clock situated at one of the poles
> under otherwise identical conditions" was quite definitely a prediction,

> wasn't it? What should we do with a theory that makes incorrect
> predictions, sal?

Figure out why they're wrong, and then decide whether the theory can be patched up easily enough to keep using it.

Do you discard a computer program which turns out to have a bug in it? No, of course not. You fix the bug and keep using it. If you can't fix the bug, you document it, and that turns it into a "behavior" or "feature" rather than a "bug". Same thing with theories.

> Embrace it and shore it up with more stupidity from
> the same source, or abandon it? An idiot would shore it up, of course,
> by claiming some physical phenomena that was actually false, such as
> gravity being stronger at the poles, thereby LYING and being an enemy of
> the search for truth. You are that idiot, are you not? Yes, I'm calling
> you a LIAR, sal.
>
> What are you going to do, sal?
> Snip it?
> Deny it?
> Ignore it?
> Call me names?
> Pretend you can't see it?

No, I'm going to tell you that I think you misunderstood my use of the term "an upper bound" and as a result of that misunderstanding, you failed to follow the math I posted. You then apparently got mad and started throwing insults.

I usually ignore your insults, mostly because I find some of your posts interesting.

> I'm quite used to all those, I've had years of experience writing to
> this ng.
> Occasionally, I meet someone with enough courage to admit they are wrong
> and Einstein was wrong, but such strong characters are few. They'd
> rather bluster and squirm and lie, those people with weak, disgusting
> characters, the Dr. Jekyll's to moortel's Mr Hyde. His alter−egos are
> everywhere, and reflect the same character.
>
> | > No one can think of an experiment in which the predictions of Ohm's
> | > law, relating voltage, resistance, and current, could be other than
> | > the Ohm's law prediction on its domain of applicability?
> |
> | Again, AFAIK Ohm's law makes no predictions at all −− it just gives a
> | name to the ratio of the voltage to the current in a region of space
> | through which charges are moving.
>
> Actually, that is balderdash.
>

> (+12V)−−−−−−−−−−/\/\/\−−−−x−−−−−−−/\/\/\−−−−−−− (−12V)
> 10k R?
> What value of R would I use to obtain a voltage of zero at x, if I
> cannot use what Ohm's law predicts? Is not Ohm's law predicting 24 = 10k
*> * I so the current will be 0.0024 amperes? If you do not think so, tell*
> me by what method you would select R. Intuition, perhaps? Yes, I would
> use intuition also, but only from familiarity.

As far as I know, it is Ohm's law which defines resistance of a conductor, as I already said. Since the current is the same through the two resistors (by ... Kirchoff's law, is it? Anyway, one of those other laws which actually makes predictions), we can use Ohm's law to conclude that if the voltage at x is zero, then the resistances must be equal. BUT how do you TEST that assertion? If the voltage is zero in the middle, then the resistances must be equal BY DEFINITION.

In other words, we "measure" the value of a resistor by measuring the voltage across it at a number of different currents. If the ratio is linear, we say it's a "pure resistor" and note the number we measured, which we call its "resistance"; the number is the one determined by Ohm's law.

If you disagree with that, please tell me how you measure the property of "resistance" _without_ using Ohm's law to do it. As far as I know, you can't, because resistance is defined by Ohm's law −− not merely described by it.

(In the lab, of course, we actually determine the resistance by looking at the paint job. If we want a 10K we look for one with a brown, a black, and an orange stripe, and if we care much about its value we either look for a gold band on the end, for a 5% resistor, or a white band followed by a red band for a 2% resistor. We hardly ever actually "measure" the value of one after we get it out of the drawer unless we're really feeling anal.)

> Androcles
>
>
> | The assertion that the resistance of a path through a uniform
> | conductor is linear in the path length and inversely proportional the
> | cross sectional area of the path is testable, but it does _not_ follow
> | from Ohm's law.

−−
I can be contacted through http://www.physicsinsights.org