Re: Roberts versus Lazio on "Overaveraging"

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From: greywolf42 (*mingstb_at_marssim−ss.com*)
Date: 01/28/05

Date: Fri, 28 Jan 2005 20:07:06 GMT

Bill Rowe <readnewscix@earthlink.net.invalid> wrote in message
news:readnewscix−44350F.22020227012005@news1.west.earthlink.net...
> In article <LnzJd.6297$VA5.1171@fe07.usenetserver.com>,
> "greywolf42" <mingstb@marssim−ss.com> wrote:
>
> > Bill Rowe <readnewscix@earthlink.net.invalid> wrote in message
> > news:readnewscix−871AD6.23104719012005@news1.west.earthlink.net...
>
> <snip>

> > > Do you have any familiarity at all with basic statistics? In
> > > particular the central limit theorem? If so, it should be
> > > immediately apparent Tom's claim above is a direct
> > > consequence of the central limit theorem.
> > > And if you are not familiar with it pick up any reasonable
> > > basic text on statistics, go to the index or table of contents
> > > and find central limit theorem and turn to the referenced page.
> >
> > So sorry, but the central limit thoerem has nothing to say about taking
> > data beyond the physical capabilities of the apparatus.
>
> And my comments in no way imply the central limit theorem or any
> mathematical theorem has anything to say about the methodology of taking
> data. Such an assertion would be totally inane.

Then why did you bring it up, when the subject is taking data below the
physical resolution of the apparatus?

> But the central limit theorem does apply to analysis of data. In
> particular, it tells you what you should expect when you average
> statistically independent samples take from a distribution that has a
> finite mean and finite variance. And that is what is applicable here.

There is no "finite mean" or "finite variance" on the data under discussion.
Both are indeterminate. That is the point. Neither can be determined.
Hence, the CLT can't be applied.

> > > > Systematic errors do not affect the error bars on the statistical
> > > > results. If you know that there is a systematic error, then you
> > > > redo the experiment.
>
> > > True, and this has nothing at all to do with the comment about
> > > averaging independent observations.
>
> > I agree that Tom's approach is flawed.
>
> My comment doesn't say Tom's approach is flawed (it isn't). It merely
> states your response to Tom's comment isn't relevant to that comment.

One knows that one has systematic errors. You agree that one should re−do the experiment under these conditions. Tom claims that he can "adjust" the data for such systematic errors, and doesn't want to re−do the experiment. Thus, you disagree with Tom.

> > > > > To make them statistically independent, in this case I must
> > > > > re−apply the meter stick to the desk for each measurement (merely
> > > > > re−reading the scale without repositioning the stick would not
> > > > > give independent measurements).
>
> > > > Yes, one must actually perform each measurement... not simply
> > > > count the same measurement 'n' times.
>
> > > True, but meaningless as human beings are unable to achieve what is
> > > required.
>
> > No science was done before computers existed?
>
> The existence of science before the existence of computers has nothing
> whatever to do with my comment.

Humans took data for centuries ... and did science and experiment. This was not "meaningless."

> Humans have certain built in biases that
> are inescapable.

Not true, and unsupported in any event. See Babbage's classic work on bias. Humans can train themselves to limit bias, and can arrange for "blinded" experiments to avoid such.

But equipment has built−in biases that never change. (All equipment is built by humans.)

> They are quite incapable of achieving statistical
> independence when making repeated measurements. But that incapacity does
> not mean an incapacity to do science.

Experiments are the part of science that I am discussing.

> > LOL! Didn't you ever do experiments, Bill?
>
> I do experiments quite often and I am well aware of sources of
> uncertainty in my experiments. Your comments lead me to doubt you have
> similar experience.

So, you do experiments, even though such are "... meaningless as human beings are unable to achieve what is required." ;)

> > > They cannot help but remember what they did moments before and
> > > repeat the measurement in essentially the same way. Hence, repeated
> > > measurements made by humans one after another never really achieve
> > > statistical independence.
>
> > LOL! While computers always do things exactly the same way. So they
> > never achieve statistical independence, either?
>
> From this, I assume you've no knowledge of quantization uncertainty,
> random noise etc that cause the least significant digits of a digital
> meter to vary.

A total red herring −− as noted several times before.

> I also assume you've not familiarity with chaos theory
> where one gets unpredictable results using a deterministic algorithm on
> a computer.

Only if you build such into the program. Even "random" functions are not random, in a computer.

> In short, if you really believe what you wrote and imply,
> you clearly have little knowledge regarding what you are writing about.

Pure special plead. Zero content.

> <snip>

> > > Certainly and observer can move his viewpoint. But this isn't much of
> > > a solution in practice. Basically, what one would do is move your
> > > viewpoint until you got what you thought was the best reading.
>
> > Huh? Didn't anyone train you how to avoid optical parallax when taking
> > readings? You don't look for the "best" reading, but the middle of the
> > two extremes. Net result is no optical parallax error.
>
> And you apparently do not realize when someone does "the middle of the
> two extremes" they do this in a manner that is not independent of the
> observer and has certain bias built in that is unavoidable.

Absolute B.S. Also unsupported.

Bias is not the result of optical parallax.

> > > But since we tend to do things the same way over and over again,
> > > you simply trade one bias for another.
>
> > If we follow the correct procedure, we eliminate optical parallax as a
> > systematic error.
>
> When things are done using best experimental technique, systematics
> errors can be eliminated. But this has nothing to do with the bias I and
> Tom have mentioned.

But we aren't discussing "bias" in this thread. We are discussing Tom's hilarous claims about errors systematic and errors statistical. Not bias.

Overaveraging is not related to experimenter "bias." It either *is valid* per se. Or it is *invalid* per se.

> > > > > temperature difference in the meter stick between its calibration
> > > > > and use
>
> > > > This is not systematic error, for it can be controlled. Unless the
> > > > experimenter is not competent.
>
> > > No matter how competent and experimenter is there are limits to how
> > > well any environmental factor can be controlled and measured. In the
> > > case of temperature, it is impossible to make buffer against the
> > > environment temperature and have 0 temperature gradient (so that
> > > the point at which you measure temperature is the temperature that
> > > is important) at the same time.
>
> > But there is no need for perfection. The point is that a competent
> > experimenter can control any systematic errors that might arise from
> > temperature differences. All (s)he needs to do is to limit any effect
> > to below the resolution required for the experiment.
>
> Do you not understand the difference between controlling uncertainty to
> some level and eliminating it?

"Eliminating" uncertainty is a strawman profferred only by you and Tom. One simply has to control uncertainty to below where it affects the resolution of your measurements.

> > {snip exchange uncommented by Bill}

> > > Do you not understand the difference between saying a measurement is
> > > accurate to 10 ppm because your instrument as an accuracy of 10 ppm
> > > and saying to can average 100 readings to improve the resolution of
> > > the instrument by a factor of 10 over the specified resolution of the

> > > instrument?
>
> > Of course I understand the difference. The first is real. The second is
> > wishful thinking. The resolution of the instrument does not depend on
> > how many readings we take.
>
> Exactly. And this is one of the main points made by Tom Roberts

Wrong again. Tom insists that physical resolution of the instrument depends upon the number of measurements that we take.

So, I'll ask you to declare, directly. Are claimed measurements whose absolute value is below the physical resolution of the instrument valid? Yes or no?

> > > These are two separate and distinct things. A claim of
> > > accuracy of 10 ppm using an instrument specified to have that accuracy
> > > is not suspect. A claim that resolution was improved by a factor of 10
> > > over the specified resolution of the instrument by averaging is
> > > "highly suspect". So suspect as to be considered invalid.
>
> > Yet I don't see you complaining about the COBE "variations" (a factor of
> > 10 below the physical resolution of the instrument). Or the Hipparcos
> > claims about light bending (a factor of 1000 below the physical
resolution
> > of the instrument).
>
> And your point is...? Are you suggesting I have time to review and
> comment every conceivable experiment? I assure you I don't.

The discussion is about those very experiments. Now, you have agreed with me, in general, that such a claim is invalid. Are you now telling me that you haven't looked at either the COBE data or at the Hipparcos claims?

> > > Averaging only improves resolution when measurements are statistically
> > > independent. Repeated measurements by humans don't achieve this.
>
> > And how do "non−humans" achieve this? They are even more prone to doing
> > things the same way, over and over.
>
> Perhaps you should do some research on how a given instrument works. Pay
> particular attention to discussions of noise, quantization errors,
> non−linearities etc. In short, computerized measurements do not result
> in the exact same measurement over and over again.

Sure they do. Otherwise we scrap them. I'm not talking about "noise" that might exist below the expected resolution of the instrument.

> > > And statistical independence won't always be enough even if it could
> > > be achieved by eliminating all human bias and systematic error.
>

*> > My point is that it will *never* be enough.*
>
> This certainly was not clear in your early posts.

That's because you have been so busy trying to agree with Tom Roberts, that you don't realize that you are arguing against his points.

> > > For averaging to work its magic, the central limit has to apply.
> > > And the central limit theorem does not apply to all distributions.
>
> > How do you know what the "distribution" will be, before you do the
> > experiment?
>
> You might try understanding thoroughly how the measurements are to be
> done, doing a bit of characterization of the measurement instruments and
> some appropriate statistical analysis.

That's called theory. Experiments are independent of theory. You find out if the distribution is normal by taking measurements.

> But beyond this it isn't
*> necessary to know the distribution *before* the experiment is done.*

True, but only if you then take measurements that can determine that the distribution is normal.

> But
> it is very important to know what assumptions you are making when you
> analyze the data resulting from the experiment.

On that we certainly agree.

> And any time you do
> things like computing an average, you are implicitly making assumptions
> whether you realize it or not.

And we agree on this, too.

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greywolf42
ubi dubium ibi libertas
{remove planet for return e-mail}
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