Re: Teaching physics to biology students

Source: http://sci.tech−archive.net/Archive/sci.physics/2006−03/msg02547.html

- *From*: glhansen@xxxxxxxxxxxxxxxxxxxx (Gregory L. Hansen)

- *Date*: Tue, 21 Mar 2006 22:31:59 +0000 (UTC)

In article <1142976497.146304.306410@xxxxxxxxxxxxxxxxxxxxxxxxxxxx>,
Edward Green <spamspamspam3@xxxxxxxxxx> wrote:

> Gregory L. Hansen wrote:
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> > In article <1142950293.575497.4400@xxxxxxxxxxxxxxxxxxxxxxxxxxxx>,
> > Edward Green <spamspamspam3@xxxxxxxxxx> wrote:
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> > > Gregory L. Hansen wrote:
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> > > > Another point, mentioned about medical research which is in the same
> > > > vein as routine data runs in physics: am I right in thinking that a
> > > > significant proportion of medical research is simply statistical
> > > > investigation? A carefully controlled statistical experiment may point
> > > > to the existence of an undisclosed mechanism. A loosely controlled one
> > > > may merely be suggestive, or may do more harm than good −− since the
> > > > "suggestion" will be taken as evidence by people prejudiced towards the
> > > > hypothesis.
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> > > In some sense, *every* experiment is a statistical investigation! Even
> > > simple physical measurements must be reported with an error bar.
> >
> > True. I was thinking, as Ken suggested, of experiments which might
> > test if there were any correlation at all.

Okay, we can dispense with vague statements that exclude nothing, then.

> Now I think we might want to distinguish between experiments ... ahem, "studies" ... which merely sought causation, and those which sought effect. What's the difference? I suggest the difference may be that in a statistical study looking for an "effect", steps have been taking to reasonably randomize all uncontrolled sources of variation across the treated and untreated populations. Since we can never know all sources of variation, this can only be done by randomized assignment to treated and untreated sub−populations. This cannot be done given a pre−existing (self−treated) population. Yet I think this latter situation often occurs in published studies, taking as evidence that such studies frequently see the light of day through third party popular publication.

Causation versus correlation is more complicated than that. The classical pedagogical example is that the number of drownings in a population increases with the amount of ice cream eaten. It might not occur to the student, at first, that one doesn't cause the other; rather, both are driven by warmer weather in the summer.

Following my inclination to connect to actual research rather than sticking with hypotheticals, enough studies have shown a correlation with body fat and early mortality. Does that mean body fat causes it? We could compare health indicators of people before and after losing weight, except that losing weight involves changes in diet and exercise, which has health benefits even if no weight is lost. A convincing test would have to avoid such things. The liposuction study showed that simply sucking out the subcutaneous layer doesn't change the health indicators. There's reason to think that the visceral fat that's wrapped around internal organs has a different health effect than subcutaneous fat, so it could be that the wrong fat was studied. Maybe somebody will be able to test that directly by figuring out a way to suck out that stuff. Otherwise how can that even be tested, except through the filters of theory?

An interesting sub−population in one study was former athletes, in high school or college, who'd let themselves go. They had a higher mortality rate in later years than more sedentary people with a more stable body weight did. Now, it's a little−known fact that when Russian athletes retire, they're put on a lengthy de−training program that gradually decreases their exercise intensity, the purpose of which is to avoid health problems like perforations of heart valves that can occur if a professional athlete suddenly stops training. The Russians are somewhat ahead of us in sport science. That little−known fact suggests the hypothesis that the increased mortality has to do with the cessation of training rather than the accumulation of fat. If we hurry up and push a grant proposal through, we ought to be able to get a definitive test of that hypothesis in about twenty years.

....

> What I was suggesting, however, is that there are a significant proportion of "type I" investigations out there in the medical field. I did not define "significant", so I said almost nothing. ;−) However, I'm going to go out on a limb here. I estimate that at least 50% of published medical studies which are subsequently picked up by the popular media for sound−byte status are type−I statistical investigations.
>
> That is, with our refined classification, what I meant to suggest by my off−hand remark.

Hard to say if we just go by what's filtered through the media. They don't report things like error bars, they just report things like "NEW DISCOVERY!" Dr. "Squat" Hatfield, a sports psychologist, has followed the headlines resulting from a particular study that showed the health benefits of regular exercise, with higher intensity giving more benefits, although even low levels of exercise gave some benefit. After a few cycles through the popular media they were reporting that you don't have to exercise hard to get the benefits. They seemed to forget the part that if you don't exercise hard you won't get much benefit, and that getting great benefits is still going to be hard work. A measure of effect versus intensity had been turned into yes/no.

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"A good plan executed right now is far better than a perfect plan executed next week."
−Gen. George S. Patton
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