Re: Trying to find significant factors in experimental results

Source: http://sci.tech−archive.net/Archive/sci.stat.edu/2008−04/msg00022.html

- *From*: Rob <rtshilston@xxxxxxxxx>
- *Date*: Sun, 13 Apr 2008 07:12:25 −0700 (PDT)

Rich,

Thanks for you response. I've interspersed my comments below.

Best wishes,

Rob

On Apr 11, 1:22 am, Richard Ulrich <Rich.Ulr...@xxxxxxxxxxx> wrote:

> On Thu, 10 Apr 2008 14:27:21 −0700 (PDT), Rob <rtshils...@xxxxxxxxx>
> wrote:
>
> > Hi,
>
> > I've asked a 100 subjects to assess a quantity. For simplicity,
> > assume I've asked them to estimate the length of a piece of wood.
> > I've also asked each subject a number of questions (sex, age, whether
> > they're short sighted or long sighted, if they were glasses, contact
> > lenses or nothing).
>
> > My hypothesis is that the results are independant of all question
> > answers.
>
> Sometimes, these examples "for simplicity" turn
> out to be misleading. The standard hypothesis
> has to do with an interesting *difference* or effect,
> so there is an extra problem of trying to figure if
> you really want to reverse things, or if you just asked
> the question in a clumsy way.

In this case, the actual task is to judge distance based on looking at a photograph. My hypothesis is that subjects ability to judge distance is not related to their vision.

> But I will take the question as it is asked.

Thanks!

> Entertaining the hypothesis of no−difference usually
> requires much larger samples than other experiments
> with the same factors and outcomes. That is because
> you can *never* establish that there is no difference
> at all − which is almost impossible, philosophically,
> for observational data − You can only establish that the
> difference is too small to be interesting.
>
> This is what is done in bio−equivalence studies, which
> you might want to look up, for comparison.
>
> With multiple factors, you are stuck with providing
> confidence limits for each of the observed differences,
> and then arguing that they are each small. If there is
> any suggestion that these might combine ("bad vision,
> no glasses"), you also need to discredit those things
> that anyone else might argue for.

Very clear, and thanks for pointing me towards some more reading.

> > How can I prove this? As a first attempt, I've partitioned the
> > subjects into every possible partition, based on the question answers,
> > where I've still got a minimum of ten subjects in the smallest
> > population. I then ran ANOVA for the two formed populations, and
> > repeated for every possible partition.
> >
> > This strikes me as being a bit clumsy, but I'm not sure how else I can
> > do this. My reading of Factorial ANOVA suggests that every population
> > needs to be of the same size, and so this isn't possible with my data.

> That is surely not the case. The most powerful
> tests have equal group sizes; and cross−classifications
> need cell sizes that are proportionate if the tests are
> to remain independent and "unconfounded" with each
> other. There still can be tests.

I see. So fringe populations (eg short sighted, colour blind, wearing contact lenses against those who don't) are less powerful as the population is enormously mis−blanced? I presume this is because there might be a larger inter quartile range (or similar) within the small population because of the lower quantity of results?

> > Can anyone point me in the direction of a technique that could be used
> > to analyse my data?
>
> If you don't want separate confidence intervals for
> every difference that anyone might argue for, then
> you may need to be more explicit about the variables
> and the problem.

I'd be incredibly greatful if you would look closer, but I'd like to take such discussion out of the news group. If you're happy, I will email details of the experiment directly.

> −−
> Rich Ulrich
>
> http://www.pitt.edu/~wpilib/index.html

.