Statistical Thinking Comment Policy

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Johan Nick • 5 years ago

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Martin • 5 years ago

Dr. Harrell.

After reading Morey's "Fundamental Confidence Fallacy" (Psychon Bull Rev 2016 23:104), I'm having trouble interpreting confidence intervals (limits). Also, it seems that pooling CLs with different confidence procedures could be problematic in meta-analyses.

As a comment to a previous post, the lack of a straightforward interpretation of confidence intervals is one of the reasons that has led me to explore Bayesian analysis.

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Frank Harrell • 5 years ago

I think that true long-term collaboration with mutual respect is the answer, along with statisticians

working with subject matter experts to develop purely prospective statistical analysis plans.

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Jack Wilkinson • 5 years ago

'..and statisticians need to be stronger'. Agree! I also wonder if we should be exerting more control over the research in general. Imagine if clinicians took on more of an advisory role (identifying important research questions, identifying confounders etc) and left it to people with methodological training to actually conduct and report the research. Sure, there'd be less research, but there'd probably be more studies that were actually useful.It's increasingly unclear to me why we take on the role of eye-rolling bystanders when we could be doing more applied research ourselves, to a much higher standard.

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Frank Harrell • 5 years ago

http://www.citeulike.org/us...

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Thomas B • 5 years ago

Is there a specific reference for the Greenland (2000) article? His CV has many pubs for that year.

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Frank Harrell • 5 years ago

I've been in the same position. The IRBs need to know that apriori futile studies are not ethical, and statisticians need to be stronger. One incremental approach is to forbid the use of p-values and to only report confidence intervals for such studies, or Bayesian posterior probabilities (including the probability of similarity).

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Roy Tamura • 5 years ago

In both pharma and non-pharma clinical trials, there is often a gray area in regards to study design / sample sizes. The company or the investigator doesn't have the money to do a large Phase 3 trial but does have enough to run a non-trivial randomized trial. I always felt uncomfortable justifying a smaller sample size based on a large effect size but if I didn't do that, colleagues said that there was no chance that the study would be approved by IRB's. In pharma, there is also a huge desire to do hypothesis tests in the hope of using the study for future registration for a drug.

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Frank Harrell • 5 years ago

Sure. Many single-center randomizied trials do not have huge budgets but were approved because the frequentist statistical power was >= 0.8. However the power was >= 0.8 because a more-than-clinically-important effect size was used in the power calculation, in order to have a sample size within budget. When you power a study to detect a miracle and all you get is a clinically meaningful effect you are left with nothing.

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Unknown • 5 years ago

Can you expand on what you mean by "overly optimistic sample sizes" for single-center randomized studies? Do you mean situations where the sample size is small but the researchers are overly optimistic about what they'll be able to learn from the data?

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Frank Harrell • 5 years ago

It's wonderful to have you involved Sander. I have pointed hundreds of people to your amazing 2000 paper which serves as a perfect example of how the statistical analysis strategy affects the reliability of results in nutritional epidemiology (and is applicable to may other fields). You are perfectly correct in your comments. It's really about misinterpretation of results and lack of emphasis on confidence intervals when a study is unbiased and well executed. I tell investigators when they really want to launch an under-sized study that the power and precision will be poor but the confidence intervals will tell most of the story and need to be presented first in the paper.

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Sander Greenland • 5 years ago

The behavior of med journals regarding errors and statistical criticism is scandalous. But I disagree with the above comments about sample size and power. That a study is too small or is underpowered is by itself an invalid criticism. The fault lies instead with misinterpretation and misuse of its results. The results from a small or underpowered but otherwise well-done study can be valuable if focused on the confidence limits, not the P-value or "significance" - especially when considered in the larger research context. In particular, its results can be pooled with those from other studies to reveal more sharply what might be ambiguous from separate examinations of the studies.

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Frank Harrell • 5 years ago

Great points. I was taking those facets to be understood but should not have. Thanks for the comment. Regarding blinding (masking) I'm less worried.

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Roy Tamura • 5 years ago

I cannot imagine any statistician disagreeing with your Suggested Remedies. A point of clarification on your second list, I presume for the randomized trials on those lists, a prerequisite is that they be adequately blinded and have sufficient power for realistic effect sizes. I have seen randomized crossover and single center studies which were just too small to have sufficient power for any reasonable alternative.

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