therapeutics.

Statistical Thinking Comment Policy

Comments are welcomed. Be informed, informative, respectful, and criticize ideas, not people. Please read our Comment Policy before commenting.

Dimiter Toshkov • 4 years ago

I find the distinction between exploration/description and 'inference' unhelpful, although I realize it is quite common in the statistical literature. First, the first kind of research involves inference as well, e.g. from an observed measure to a case attribute (measurement) and from variable to concept. Second, the second kind of research seems to lump together descriptive inference from sample to population with causal inference, which are of course rather different (and, as you point out prediction being yet another kind of inference). So why not just say that all scientific research involves inference, and go on to specify the inference we mean: measurement, descriptive, predictive, causal, etc.?

1 ^ | v • Reply • Share ›

Frank Harrell Mod → Dimiter Toshkov • 4 years ago

Thanks for the comments. Can you isolate which phrase implied that descriptive inference is similar to causal inference? I'd like to correct that. On your first point, assessment of measures and case attributes, and variables to concepts, without saying more, is decidedly not inference. The term 'inference' is reserved to mean reasoning from a sample to a population or from a sample to the data generating mechanism that produced the sample.

^ | v • Reply • Share ›

Dimiter Toshkov → Frank Harrell • 4 years ago

Thanks for your reply.
1) The phrase 'Sometimes this is called population inference...' evokes sample to population inference (to me at least), which I would consider descriptive (what else)?
2) The common meaning of 'inference' is much broader and covers any reasoning that leads from what you observe directly to what you do not observe directly. In this sense it applies to inference from an observed sample to a population, from observed associations to causal relationships, but also from observed variables to unobserved concepts, etc.
The problems with a narrow understanding of 'inference' are that, first, it suggest to researchers that the other processes do not involve reasoning from what you observe to what cannot be directly observed (hence, it comes with inherent uncertainty and error). And then, inference from a sample to a population and from a sample to the data generating mechanism (~ causal inference?) are quite distinct to be covered by the same label 'inference'. To my mind, this lack of terminological clarity partly explains why inference about a regression coefficient is too often confused with inference about a casual effect.

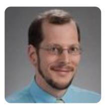
1 ^ | v • Reply • Share ›

1 ^ | v • Reply • Share ›

Frank Harrell Mod ➦ Dimiter Toshkov • 4 years ago

While you make some excellent points, statistical inference is always defined as inference from the specific to the general, from the sample to the population, from the sample to the process generating the sample. *Descriptive* is reserved to make sample-specific without an attempt to generalize. Whenever I say *inference* I mean to imply *statistical inference*.

^ | v • Reply • Share ›



David C. Norris, MD • 4 years ago • edited

Frank, I read your post more as a collection of loosely connected assertions than an integrated philosophy of statistics. I don't appreciate a set of core organizing principles, such as a coherent underlying philosophy of science would lend. Indeed, I venture to assert that any attempt to develop a philosophy of statistics apart from a philosophy of science must inevitably fail. The attempt would flounder like a theory of the quadratic equation making no recourse to the Complex plane. In the constricted 'tunnel vision' of the Real line, this theory ends up seeing confusing distinctions between equations that do have roots and equations that don't—just as you've perceived a distinction between *exploratory* and *inferential* statistical practice.

To move beyond a merely suggestive metaphor, consider the *logical* alternatives to fully embedding a 'PhilStat' within a 'PhilSci'. One possibility is that your PhilStat will make allowances (by invoking special problems presented by statistics and probability, say) that a sound PhilSci would not: "Well, because I'm a statistician, you see, I get to do X even though it's not okay in science generally." This, of course, would look like special pleading, and no one would tolerate such a libertine PhilStat. The other possibility is that you exclude from statistical practice certain behaviors and attitudes that are entirely acceptable within science at large. In this latter case, you'd end up with a dour and prudish PhilStat not unlike the impoverished view of medical science that I have criticized elsewhere: https://medium.com/@davidcn....

What happens if we re-examine your exploratory-inferential dichotomy, in light of some of Karl Popper's ideas? Possibly the most stunning and profoundly useful feature of Popper's evolutionary epistemology is its placing human knowledge on a continuum with animal knowledge. In his book *Objective Knowledge*, Popper examines the ends of that spectrum as follows:

> "…the main difference between Einstein and an amoeba is that Einstein *consciously seeks for error elimination*. He tries to kill his theories: he is consciously critical of his theories which, for this reason, he tries to *formulate* sharply rather than vaguely. But the amoeba cannot be critical *vis-à-vis* its expectations or hypotheses; it cannot be critical because it cannot *face* its hypotheses: they are part of it. (Only objective knowledge is criticizable: subjective knowledge becomes criticizable only when it becomes objective. And it becomes objective when we *say* what we think; and even more so when we *write* it down, or *print* it.)" [1979 rev. ed., pp 24f; emphasis is Popper's]

Your exploratory/inferential distinction seems to me open to examination along much the same lines. In exploratory data analysis, we tend to allow inbuilt expectations to drive our investigations, without much conscious insight. For example, in graphical data exploration we use the edge detection circuitry built in to our retinas, plus additional preconscious image processing within the brain. At the other end of this spectrum, we might bring our whole analysis into consciousness and make it objective in the form of a *script* that can be bootstrapped. (Note also how splendidly Popper's parenthetical remark above applies to Bayesian priors!) I do think we have a true continuum here, too. Consider the vast middle ground occupied by researchers who know a handful of Stata commands, and freely employ off-the-shelf statistical procedures (propensity score adjustment, stepwise variable selection, Cox proportional hazards model, etc.) without much conscious insight into their substantive content or their connection with a realistic conception of the DGP. I see little purpose served by statistical practitioners' observance of an artificial exploratory-inferential distinction. Statisticians will do far better to ask themselves constantly: "Am I acting more like an amoeba right now, or Einstein?"

You seem to have employed the exploratory-inferential distinction to prop up the idea that inference "generalizes" from a sample to the DGP that gave rise to it. Karl Popper famously called inductive inference "a myth", which is a very difficult thing for most statisticians to stomach because (like you) they regard 'inductive inference' as synonymous with 'statistical inference'. Popper's arguments on this point are multifarious, and richly varied, and I won't dare to summarize them here. But let me quote at length from Günter Wächtershäuser's essay, "The Uses of Karl Popper" https://philpapers.org/rec/..., as he offers "caricatures" of inductivism and deductivism. I think you might like to consider whether you buy the bill of goods under the former heading, and whether the latter perspective might offer a more coherent outlook within which to understand and advance your philosophy of statistics. Even if you reject the deductivist view, by doing this explicitly you could well add clarity to the views you laid out here.

Before I come to my scientific story, let me briefly summarize and contrast the major tenets of inductivism and of Popper's deductivism (*LSD*, *RAS*, *BG*, *CR*). I begin with a caricature of inductivism in the form of eight theses:

1. Science strives for justified, proven knowledge, for certain

truth.

2. All scientific inquiry begins with observations or experiments.

3. The observational or experimental data are organized into a hypothesis, which is not yet proven (context of discovery).

4. The observations or experiments are repeated many times.

5. The greater the number of successful repetitions, the higher the probability of the truth of the hypothesis (context of justification).

6. As soon as we are satisfied that we have reached certainty in that manner we lay the issue aside forever as a proven law of nature.

7. We then turn to the next observation or experiment with which we proceed in the same manner.

8. With the conjunction of all these proven theories we build the edifice of justified and certain science.

In summary, the inductivist believes that science moves from the particulars to the general and that the truth of the particular data is transmitted to the general theory.

Now let me give you a caricature of Popper's theory of deductivism, again in the form of eight theses:

1. Science strives for absolute and objective truth, but it can never reach certainty.

2. All scientific inquiry begins with a rich context of background knowledge and with the problems within this context and with metaphysical research programmes.

3. A theory, that is, a hypothetical answer to a problem, is freely invented within the metaphysical research programme; it explains the observable by the unobservable.

4. Experimentally testable consequences, daring consequences that is, are deduced from the theory and corresponding experiments are carried out to test the predictions.

5. If an experimental result comes out as predicted, it is taken as a value in itself and as an encouragement to continue with the theory, but it is not taken as an element of proof of the theory of the unobservable.

6. As soon as an experimental result comes out against the prediction and we are satisfied that it is not a blunder we decide to consider the theory falsified, but only tentatively so.

7. With this we gain a deeper understanding of our problem and proceed to invent our next hypothetical theory for solving it, which we treat again in the same way.

8. The concatenation of all these conjectures and refutations constitutes the dynamics of scientific progress, moving ever closer to the truth, but never reaching certainty.

In summary, the Popperian deductivist believes that science moves from the general to the particulars and back to the general—a process without end. Let me inject a metaphor. I might liken the Popperian view of science to that of a carriage with two horses. The experimental horse is strong, but blind. The theoretical horse can see, but it cannot pull. Only both together can bring the carriage forward. And behind it leaves a track bearing witness to the incessant struggle of trial and error.

^ | v • Reply • Share ›

Related

- [Commentary on Improving Precision and Power in Randomized Trials for COVID-19 Treatments Using Covariate Adjustment, for Binary, Ordinal, and Time-to-Event Outcomes]
- [Musings on Multiple Endpoints in RCTs]
- [EHRs and RCTs: Outcome Prediction vs. Optimal Treatment Selection]
- [p-values and Type I Errors are Not the Probabilities We Need]
- [Information Gain From Using Ordinal Instead of Binary Outcomes]