Discussion of
"Causal Inference without Counterfactuals"
by A.P. Dawid

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1 Introduction

Professor Dawid has presented a thought-provoking analysis of causal inference, and has certainly caused us to think hard about these matters.

There are three main topics that we would like to comment on: the structure of models, the object of inference and the philosophy of inference. The desire to make a causal inference leads one to a particular class of models. From the model (and the data) an inference need be made. The model, and an associated parameter of interest, directs the possible type of inference. We then must decide on a reference set (or population) to which the inference will be made. All of these pieces work together in an inferential philosophy. There are many choices to be made at each stage of the process (model, parameters, inference). Dawid insists that such choices, and inferences, must be based on strict principles that can be verified empirically. We believe that such a program is so overly rigid that, in the end, science will not be served.

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2 Inferring from ...

An individual counterfactual model necessarily involves unobservable quantities. These quantities can lead to *unidentifiable* models such as (1). When faced with such a model, we would normally think that the statistician would try to refine the model to make valid *individual causal effects* inferences possible, if such inferences are the desire of the experimenter.

One way of doing this is to shift the target of inference to *average causal effects*. Then model (8), which is free of nonidentifiability baggage, can be used. To us, this is a way out of the problems inherent in model (1). Let us look at this switch of inferential target a bit more closely.

To switch, one is forced to place assumptions on the structure of the parameters, thus bringing in a "metaphysical component" that Dawid finds so distasteful. But, in a sense, this is a reality of inference. When faced with an unwieldy model, we have to make assumptions to obtain usable inferences.

One assumption that results in average causal effects becoming the inferential target is that of *treatment unit additivity (TUA)*, which Dawid does not like. But, there is another road to average causal effect, based on the thinking of the 18^{th} century philosopher David Hume (1748):

> It appears, then, that this idea of a necessary connection among events arises from a number of similar instances which occur, of the constant conjunction of these events; nor can that idea ever be suggested by any one of these instances surveyed in all possible lights and positions.

Following Hume, the causal inference is necessarily shifted from the individual to the group. This eliminates any counterfactual problems because, at the group level, the counterfactual is observable (one group did not get aspirin).

Dawid's insistence on empirical verification would reject the above line of reasoning. Such an insistence not only severely restricts the range of possible models, but it also may disregard the scientific input of the subject matter expert (who may insist that TUA is entirely plausible for the experiment at hand).

A crucial point is that, if we can reduce the inferential target to one based only on marginal distributions, then we can provide a reasonable inference (to us, this means that we are working with an identifiable model). As Dawid rejects TUA (and presumably the argument based on Hume) as metaphysical, he applies Bayesian decision theory to reduce the inference to a marginal one. However, in doing so, he has substantially changed the inferential target. The primary target of inference is $Y_t(u) - Y_c(u)$, the individual difference, which is unobservable. Using either TUA or Hume, this target becomes $\theta_t - \theta_u$, the average causal effect. Dawid's decision theory argument leads to the inferential target being $u_0|\text{Treatment} = t$, the distribution of the response given that the treatment was t. While this may be a reasonable target of inference, it may not be the one that the experimenter cares about.

3 Inferring to ...

Whatever the chosen target of inference, an inference must be drawn. Some of us think in terms of populations or reference sets, often described by the experimenter. For example, in Dawid's Example 6, we can specify a number of reference sets. We have usually left the choice of such to the experimenter, whose greater subject matter knowledge can be used to choose the appropriate frame of inference.

3.1 Empirical Verification

Relying strictly on empirical verification, Dawid deals with the shortcomings of a model like (1) by invoking a principle known as *Jeffreys Law* to decree what types of inferences are allowed from nonidentifiable models.

Jeffreys Law is the *Likelihood Principle* in another guise. The Likelihood Principle states that if $\mathbf{x}$ and $\mathbf{y}$ are two sample points such that the likelihood $L(\theta|\mathbf{x})$ is proportional to $L(\theta|\mathbf{y})$ for all θ, then the conclusions drawn from $\mathbf{x}$ and $\mathbf{y}$ should be identical.

Since the landmark paper of Birnbaum (1962), the likelihood principle has been the focus of much debate. It is probably fair to say that with the exception of the strictest Bayesian, most statistical practice violates the Likelihood Principle. Why this is so is perhaps best explained by Berger and Wolpert (1984)

> We emphatically believe that the LP (Likelihood Principle) is always valid, in the sense that the experimental evidence concerning θ *is* contained in $\ell_X(\theta)$ (the likelihood function). Because of limited time and resources, however, interpreting or making use of this evidence *may* involve use of measures violating the LP.

This sentiment may be closest to what most statisticians feel. There are compelling arguments for embracing the likelihood principle but, in reality, we need to go beyond it. We must use, among other things, metaphysical assumptions, to thoroughly evaluate an inference.

To adhere to empirical verification and the limitations imposed on inferences by Jeffreys Law leads inexorably to a Popperian view, as Dawid explains:

> My approach is grounded in a Popperian philosophy, in which meaningfulness of a purportedly scientific theory, proposition, quantity or concept is related to the implications it has for what is or could be observed, and, in particular, to the extent to which it is possible to conceive of data that would be affected by the truth of the proposition, or the value of the quantity. When this is the case, assertions are empirically refutable, and considered 'scientific'. When not so, they may be branded 'metaphysical'.

However, this view is based on a philosophical orientation that is outmoded and has been rejected by virtually all mainstream philosophers of science.

3.2 Popper is Out

The "Popperian" philosophy that grounds Dawid's approach was part of the much larger *Logical Positivist* philosophical movement that had great currency up to perhaps 40 years ago. Its main tenet is that meaningful propositions must be either analytic (mathematical) or empirically falsifiable or verifiable by possible sensory observations. Karl Popper emphasized falsifiability and, for example, famously directed an attack against Marxism, arguing that it was unscientific and just a matter of faith. To that extent positivism served a useful purpose. It helped to rid the intellectual arena of much philosophical and pseudo-scientific dross. It was like a breath of fresh air. Logical positivism has also been influential in science. For example, behaviorism is based on the idea that we can observe behavior but we cannot directly observe other people's minds. Therefore behavior, but not the mind, is a fit subject of scientific study.

Starting in about 1950 logical positivism was subjected to a withering series of criticisms and has now entirely lost favor among philosophers. The attack was based primarily on the logical work of W.V. Quine (1961) and the historical work of Thomas Kuhn (1970), with much help from many other thinkers and researchers. The criticisms demonstrated that the logical positivist program was too rigid and technically unworkable and that logical positivism did not represent the actual practice of scientists. If held to the rigid standard of Popperian philosophy little or no actual science would get done. The demise of logical positivism has had the beneficial effect of expanding the horizons of scientific pioneers. For example, cognitive science has now replaced behaviorism as the leading orientation in psychology.

3.3 Counterfactuals are In

Among the many technical problems facing logical positivists was what to do about counterfactual. Certainly many counterfactuals are unverifiable and do not seem to be scientifically meaningful. For example, "If I had been born in China, I would now be able to speak Chinese." On the other hand many other counterfactuals seem clearly to be meaningful and indeed true. For example, "If Nixon had not resigned, he would have been impeached." The fact is that counterfactuals are indispensable in many areas but attempts to analyze them in terms of direct observation foundered. The problem of how to understand them is still a matter of philosophical controversy. Probably the most widely accepted view today is that of David Lewis cited by Dawid. Lewis analyzes counterfactuals in terms of other possible worlds–ways that things could have been but aren't. Anathema to the logical positivists and Dawid.

3.4 A Fatal Flaw?

Dawid's use of tendentious vocabulary clouds his argument and obscures the motivation for his views. For example, besides the questionable empirical versus metaphysical distinction, Dawid rejects a view he terms "fatalism."

> Many counterfactual analyses are based, explicitly or implicitly, on an attitude that I term fatalism. This conceives of the various potential responses $Y_i(u)$, when treatment i is applied to unit u, as pre-determined attributes of unit u, waiting only to be uncovered by suitable experimentation. (It is implicit that the unit u, and its properties and propensities, exist in dependently of, and are unaffected by, any treatment that may be applied.)

If by this Dawid means that the world and its objects exist independently of our attempts to know them, then this view is quite respectable and usually goes under the rubric "realism." And it seems that even Dawid sometimes embraces a "fatalist" view, as he says, "Nature is surely utterly indifferent to our attempts to ensnare her in our theories." "Fatalism" seems to be a highly misleading name for a rather commonplace and obvious idea. If Dawid means something else by his use of "fatalism," then we fear he is attacking a "straw man" view that no one holds.

4 And Finally..

Clearly there is something right about the positivist approach in general. Certainly we want our scientific theories to be verifiable or falsifiable in some sense, but it turns out that verifiability and falsifiability are much more flexible, elastic, and looser notions than the logical positivists supposed. The upshot is that we need to take a more tolerant approach to verification and falsification and abandon the kind of tendentious and rigid distinctions that the logical positivists, and following them Dawid, use. Scientific theories are not verified or falsified by direct observation or crucial experiment, except in very rare instances. Theories are accepted or rejected by scientists on the basis of how well they explain selected sets of data, how elegant, simple, and useful they are, how well they do against competing theories, etc. In fact, in his discussion of Barndorff-Nielsen's paper, Dawid (1976) expressed a similar sentiment when he said (our italics)

> A constant theme in the development of statistics has been the search for justification for what statisticians do. To read the textbooks, one might easily get the distorted idea that "Student" proposed his t-test because it was the Uniformly Most Powerful Test of a Normal mean, but it would be more accurate to say that the concept of UMPU gains much of its appeal because it produces the t-test, and *everyone knows the t-test is a good thing.*

Everyone knows that the simple, elegant, and useful t-test is a good thing because it has performed admirably for almost 100 years. In the interest of science, performance counts for more than rigidly adhering to philosophical principles.

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