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Comments on: The tale wagged by the DAG

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I am grateful to the editors for the opportunity to comment on Nancy Krieger and George Davey Smith's article, 'The tale wagged by the DAG', which appeared in the *IJE*'s special issue on causal analysis.[1]

Krieger and Davey Smith raise several objections to the direction taken by modern epidemiology, which they deem to be too narrowly wedded to a directed acyclic graph (DAG)–counterfactual framework. In this framework, graphical models (DAGs) are used to express scientific knowledge, and counterfactuals (or potential outcomes) are used to express queries of interest. As the article does not demonstrate concrete alternatives to current methodologies, I speculate that it is the dazzling speed with which epidemiology has modernized its tools which lies behind the authors' discomfort, and that it will subside as soon as researchers gain greater familiarity with the capabilities and flexibility of these new tools. Epidemiology, as I have written on several occasions, has been a pioneer in accepting the DAG–counterfactuals symbiosis as a ruling paradigm—way ahead of mainstream statistics and its other satellites.[2]

In examining the specific limitations that Krieger and Davey Smith perceive in DAGs, I must note that these limitations coincide precisely with the strengths for which DAGs are praised. For example, the article complains that DAGs provide no information about variables that investigators failed to include in the model. As noted in the introductory editorial in the same issue of the journal, by Davey Smith and other *IJE* editors, 'the DAG does not provide a comprehensive picture. For example, it does not include paternal factors, ethnicity, respiratory infections or socioeconomic position...'[3] This should not be taken as a limitation of DAGs or of any other scientific modelling. Quite the contrary. It would be a disaster if models were allowed to produce information unintended by the modeller. Instead, I have come to admire the ease with which DAGs enable researchers to incorporate new knowledge about new variables, or new mechanisms, when the need arises.

Model misspecification is a problem that plagues every exercise in causal inference, no matter what framework one chooses to adapt. It can only be cured by careful model-building strategies, and by enhancing the modeller's knowledge. Yet, when it comes to preventing misspecification errors, DAGs have no match. The transparency with which DAGs display the causal assumptions in the model, and the ease with which the DAG identifies the testable implications of those assumptions, are incomparable; these facilitate speedy model diagnosis and repair and, in this way, bring misspecification errors to a minimum. This is the most one can ask from a judgmental-based model.

Or, to take another example, the authors call for an ostensibly unavailable methodology, which they label 'causal triangulation' (the term 'triangulation' appears 19 times in the article).[1] In their words, in response to commentators on their article: 'In our field, involving dynamic populations of people in dynamic societies and ecosystems, methodical triangulation of diverse types of evidence from diverse types of study designs and involving diverse populations is essential'.[4] Ironically, however, the task of treating 'diverse types of evidence from diverse types of study designs and involving diverse populations' has been shown to require the DAG–counterfactual framework even for the most elementary examples analysed by Bareinboim and myself.[5] In these examples, one takes diverse types of study settings, or diverse populations, and infers causal parameters in new settings not yet studied. It is hard to imagine how more elaborate triangulations, say those integrating physiological studies on rats, evolutionary theory and knowledge of intracellular chemical reaction, could be accomplished without the machinery provided by the DAG–counterfactual framework. In other words, tools that are indispensable in solving simple problems are unlikely to become dispensable when problems become more complex.

Another conceptual paradigm which the authors hope would liberate us from the tyranny of DAGs and counterfactuals is Lipton's romantic aspiration for 'inference to the best explanation (IBE)'.[6] IBE is a compelling mantra, going back at least to Charles Peirce's theory of abduction,[7] which unfortunately has never operationalized its key terms: 'inference to', 'best' and 'explanation'. Again, I know of only one framework in which this aspiration has been explicated with sufficient precision to produce tangible results—the structural framework of DAGs and counterfactuals. See, for example, works on causal explanations and causes of effects.[8–10]

In summary, what Krieger and Davey Smith aspire to achieve by abandoning the structural framework has already been accomplished with the help and bliss of that very framework. More generally, what we can learn from these examples is that the DAG–counterfactual symbiosis is far from being a narrow 'one approach to causal inference' which can 'potentially lead to spurious causal inference'.[1] It is in fact a general and flexible framework within which a plurality of tasks and aspirations can be formulated, analysed and implemented. It is based on a rigorous logic of causation and, therefore, it is as protected from leading to spurious causal inferences as mathematics itself.

I was pleased to note that, by and large, other commentators on Krieger and Davey Smith's article seemed to be aware of the powers and generality of the DAG–-counterfactual framework, albeit not exactly for the reasons that I have described here. (I have many disagreements with other commentators as well, but I wish to focus here on the tale wagged DAG, where the problems appear more glaring.) In a recent paper, I provided a concise summary of the achievements of the structural framework and explained why one is justified in calling those achievements a 'Causal Revolution'.[11]

All in all, epidemiology has made incredible progress in the past two decades by adapting the DAG–counterfactual symbiosis as a unifying language for communicating research problems, analytical tools and research results.[10,12] To appreciate this progress, one needs only compare with the status of causal inference in fields such as economics,[13,14] where selecting covariates for confounding control is still a black magic, and where testable implications of one's assumptions are a wishful dream. The flexibility of the DAG–counterfactual language is likely to position epidemiology among the first beneficiaries of the big data and machine-learning generation. Here, inferences across populations and across experimental settings become feasible, but their management requires the machinery of structural modelling.[5] At the same time, I hope that the discomfort that Krieger and Davey Smith have expressed will be short lived and that it will inspire a greater understanding of the modern tools of causal inference.

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