Seminar 5 10.00-11.00

Lieberson and Horwich (2008) argue that it is necessary to address and evaluate alternative causal explanations as a way of reaching consensus about the superiority of one or another theory. Does this mean social science researchers should always discuss alternative causal explanations in their research/ publications in order to increase the credibility of their arguments?

Bhrolcháin and Dyson suggest a set of causal criteria that are primarily based on examples with a single leading cause. The authors then suggest that more complex cases could be dissected and examined via the same criteria; studied with other analytical processes (i.e. historical); and/or assessed with many consequences of a causal hypotheses. With the ultimate goal of causal inference as generating hypotheses about causal pathways and mechanisms - and myriad ways to go about that - **how does one know if their approach/perspective is appropriate (in terms of being a student, not a professional)**. "[T]here is no single criterion or even any combination that, if met, is sufficient by itself to establish a relations" (Bhrolcháin & Dyson, pp. 27, 2007). With concrete, yet abstract criteria and highly scrutinized causal hypotheses, does a student ever truly know if their casual inference(s) is effective (in cases of unaided work) or do causal inferences slowly improve with experience?

Are there specific methods and research designs capable of incorporating the causal criteria proposed by Bhrolcháin and Dyson (2007: 25)? For example, to what extent could cross lagged structural equation modeling incorporate the proposed criteria?

Would it make sense to apply the Bhrolcháin and Dyson (2007: 25) causal criteria at the individual level, as a less expensive alternative to experiments?

How can one design a good study linking demographic variables to a social outcome? For example the "youth bulge" problem - can disproportionate numbers of youth in a society really be linked to higher rates of violence? Is it more logically consistent to focus not on demography as a causal factor, but whatever political or social event produced the bulge in the first place?

Should we completely disregard Mill's small N Methods on the basis that they do not permit a 'real world' approach, which can feature a range of interaction effects?

Regarding Lieberson's piece on Small N's and Big Conclusion, while his criticisms of Mills' methods are explicit, I find his last part rather unclear: what can researchers do to improve studies employing Mills' methods in those instances when Mills' methods might be appropriate forms of inquiry?

How do we establish causality when we have a treatment but lack a control group to compare it to, much like the situation we have in the mid-term assignment. What is the best way to proceed? Should we try and form a control group to compare the treatment too or should we do pre/post treatment testing to measure the difference?

At one point in the paper by Krieger and Smith discuss inference to the best explanation. After summarizing Lipton's observations of Ignaz Semmelweis' work with respect to causality Krieger and Smith go on to state, "'prediction' does not garner special consideration because opposing hypotheses may still both predict a given phenomenon (e.g. disease rates higher in groups exposed vs not exposed to X), but not be equally 'lovely'.

Perhaps erroneously I had supposed that prediction was the highest form of the scientific endeavor—but maybe a better way to characterize what we're really after with causality is just explanation? In my mind, if we really understand something then we can predict it, but maybe prediction is an ancillary or even utopian aim.

Seminar 5 11.00-12.00

How do we know if a variable is really exogenous?

It seems that, in sociology, making causal claims is hardly feasible given the many assumptions, which probably can't be made in most cases, and the fact that many research projects are bound by only a few cases. Is, in social science, exploring causes of effects a utopian undertaking? To what extent is making a causal claim only one's own belief in a given theory? (The more I read about causality and causal inference, the more sceptical I am about papers which talk about causal effects).

This week's readings highlighted alternative methods of causal inference, such as inference to the best explanation and looking at factors including time order and contiguity. How do we find a balance between using statistical methods and more logic-based methods of causation? Is it a question of picking the best method for each project or can a combination be used?

The article "The tale wagged by the DAG" by Nancy Krieger argues that DAGs cannot provide insight into the omitted variables, and encoded assumptions in DAGs might not be sound, and thus it is not a sufficient method to grasp the causal effect. And then they argues that the "systematic triangulation of evidence" should be employed. But is it actually possible to employ triangulation in social science, not in epidemiology? And even if it is possible, does it complement the limitations of DAGs?

Krieger and Smith (2016) talks about the triangulation of evidence based on data from different methods to draw "lovelier" explanations. And they emphasize how the "triangulation of evidence from empirical studies whose methodological assumptions, limitations, biases and errors (which inevitably affect all studies) are uncorrelated" (Pp. 1797, parentheses in original). I understand what triangulation is, but I am confused by the idea of uncorrelated methodological assumptions. When using triangulation, why do we want the assumptions of the methods to be uncorrelated?

The Krieger and Smith article has mentioned causal triangulation and I was wondering if we could talk about this in the class. My understanding is that they suggest comparison across cases and across different subject groups in order to test the results of a given study (proposed causes of a given effect). So is causal triangulation a systematic review of findings from similar studies with different design or is it a mixed method approach where we aim to explore a given issue from different angles and through different methods?

Is Lieberson and Horwich's (2008) jury trial model an effective apparatus in dealing with some of the methodological limitations of the social sciences?

Why are the methods of agreement and difference by Mill (1872) useful for making causal inferences?

Seminar 5 12.00-13.00

Do we always need explanatory mechanisms for any convincing case on causality to be made? Also, what features should quantitative research have to make valid explanations on mechanism?

My question for this seminar: what is the difference between probabilistic and deterministic approach in social research?

For small N's, according to Lieberson, the probabilistic approach is difficult to apply because it require large sample size, and the Mills' determinist approach of method of agreement and difference has some dangerous assumptions such as couldn't handle more than one determinant variable, interactional effects, etc. I wonder, then, in the case of small N's, what would be a good approach? Should we give up in trying to find causation or correlation in this type of study since both the approach of probabilistic and deterministic are problematic? For historical events or phenomenon that are extremely rare, such as revolution, can we examine carefully all the possible variables within the event and deduce logically, and internally not through comparison, with the evidence we have at hand to reach a satisfactory conclusion that suggest certain degree of correlation, if not causation? If we find some possible contender independent variables that could explain the dependent variable, say, exploited and underprivileged angry peasant class and the outcome of revolution, can we infer that this is, or these are, a possible variable that "cause(s)" the "this" revolution even if there are not much other cases of revolution and their context varies?

My question for this week's seminar is: Is there an alternative to Mill's method for small-N studies?

What is the Boolean method proposed by Ragin (1987) for dealing with somewhat larger samples used in comparative and historical research mentioned in the S. Lieberson (1991) reading?

Personally, I find the aspiration to discover the causes of effects to be a particularly unhelpful approach to social research – description and probabilistic theories are, for me, both more flexible and more relevant to accurately depicting and explaining a complex social world that permits multivariate causal patterns in nearly every large-scale policy or phenomena.

For my question, I'd love to discuss more the role of other possible methods in causality like KKV's "process tracing." Can this improve our understanding of causal mechanisms, through a process of approaching not only confounding variables but also additional observable implications?

The conclusion that methodological triangulation is the best option for causal inference does not sound realistic when thinking about limits of funding and time constraints of researchers in the real world. Is it still better to strive for triangulation given these constraints (and necessarily compromise this with lower quality research) or use the time for concentrating on one method, be it using DAG or “inference to the best explanation”?

Krieger and Smith's (2016) plea for a more holistic and historical approach to studying causation is very intuitively appealing. However, I wonder about how their expanded approach, incorporating triangulation, would have any sense of limits or measurement. The reason why social scientists don't talk about why everything causes everything else is not because they don't believe that there's a causal effect (by deciding to eat breakfast today, I have contributed 2 pounds to my college's food suppliers, which, combined with the actions of a hundred other students this morning, has had an extremely small causal impact on their salaries, which has an even smaller impact on how they lead their everyday lives etc...) but because it's hard to confirm its presence and its scale. In addition, the problem with trying to use diverse study designs "involving different methodological assumptions" is that these assumptions all invalidate each other. Basically, I really like Krieger and Smith's proposal but I find it unfeasible. What is the best defense of their argument?

While I understand the desire to try and prove causal relationships in sociology, particularly as a way to be taken more seriously by policy makers, etc., there is ample evidence that true causality can never be proved in social research. Instead of making causal statement and glossing over the assumptions needed to support the findings, it seems to me that it would be more productive to gather as much evidence as possible to declare a strong association and leave it at that. Purporting weak arguments does more harm to the discipline as a whole than putting forth strong, non-causal arguments.

The discussions surrounding causality are intriguing, but it surprises me that they are so incredibly recent (as is much of the statistics). How do we evaluate research from past centuries (say, the achievements of people like Pierre and Marie Curie in the natural sciences or Aristotle's theoretical endeavours (see https://plato.stanford.edu/entries/aristotle-causality/#FouCau)) or even just a few decades ago seen as there seems to be so much development in the field nowadays? Aren't we just reformulating what Aristotle said over 2000 years ago and much of modern science has intuitively understood as causality in the past 150 years? And isn't some of the discourse just reflecting fluctuations in the trends such as SEMs or the demographic "accounting method" or the predominant use of regression analyses at a certain time?

I am curious about simulation-type causation, e.g. as is extensively applied in environmental studies (global warming etc.). Is simulation thoroughly unfit for social sciences? Obviously agent-based modelling is somewhat similar to simulation studies, although they seem completely useless for recovering relevant parameter estimates or out-of-sample prediction. Are there other attempts and visions on this?