Why Are All the Interesting Questions Hard to Answer?

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The discipline of political science is in the throes of a "credibility revolution" that orients scholars to focus their efforts on generating credible estimates of causal relations in the social world. At the same time, it has become folk wisdom in the discipline that the more interesting a research question, the harder it will be to answer convincingly. Only in the rarest of circumstances is evidence relating to the most interesting political phenomena *causally identified*,[1] meaning that the evidence indicates a credible causal relationship. I am told that in Harvard's economics department, this trade-off between "interesting-ness" and "identification" is called the "Glaeser indifference curve" in honor of Edward Glaeser's predilection for pointing it out. Here I will call it the *causal identification frontier*. Whatever the name, social scientists who wrestle with endogeneity, selection problems, and confounding variables will recognize this trade-off immediately.

The tension between interesting and answerable causal questions underlies an ongoing debate in the discipline of political science about the primacy of rigor versus relevance, and the value of "problem-driven" versus "method-driven" research. This trade-off exists in other fields, notably economics, but seems to be particularly acute in political science. One piece of telling satire portrays a professor discouraging an overly-earnest student from seeking a Political Science PhD with the line: "None of the important questions can be answered using the most rigorous tools of quantitative statistical analysis. The only questions we can answer are ones that nobody cares about."[2] Based on candid conversations at conference hotel bars, this bluntly but accurately captures the frustration of at least some political scientists with the side-effects of the

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[1] In statistics, a model is "identified" if it produces unique point estimates. In the causal inference literature, it has taken on a new meaning: it denotes a model where the estimated effect of one variable on another can be interpreted causally.

[2] https://www.youtube.com/watch?v=idHQoCUfPZ4, 1:28, accessed 11/21/2015.

credibility revolution.

Is there necessarily a trade-off between interest in our questions and credibility in our answers? What can be done to improve the rigor of answers to interesting questions, or to make questions that can be answered rigorously more interesting? To answer these questions, I offer a theory that provides potential answers to two prior questions: (1) Why are interesting questions interesting? And (2) why are interesting questions usually hard to answer? The answer to both questions is, in a word, politics.

Interesting questions are interesting because they arrest the attention of humans, and humans pay more attention to certain types of social interactions than others. The particularly interesting human interactions are called "politics," and the more political the politics, the more interesting humans tend to find them. This interest derives from the common psychology of humans and is most likely a result of evolutionary pressures that helped humans survive in small groups by selecting for those humans that were most attentive to social interactions characterized by high stakes, secrecy, and violence. Thus, the most interesting questions to most humans relate to the most political aspects of human interaction.

Interesting questions are hard to answer because they are about politics that matter, meaning that they have high-stakes consequences for who gets what. Credible causal inference from data is generally only possible when a *treatment variable*[3] of interest in a researcher's theory is assigned at random in the real world, either by a researcher or by a natural process. This is easy enough in situations where politics are non-existent, because no one cares if researchers randomize treatments that affect outcomes with few consequences for who gets what. However, if individuals with "skin in the game" believe that the treatment might have important political consequences, then they are unlikely to be content with random assignment by researchers or nature. Instead, those who stand to gain or lose will try to manipulate the assignment of treatment for their own benefit. The more important the treatment, the more actors will try to game it. Moreover, these same actors will probably attempt to obscure their actions, making measurement difficult. Strategically-induced endogeneity and measurement error make credible causal inference difficult or impossible.

If people are constantly trying to manipulate the assignment of consequential (and therefore interesting) treatments, then credible causal inference about the effects of treatments is only possible in three circum-

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[3]I borrow the language of treatments from the causal inference literature which in turn borrowed it from medicine. It's just an easy way to denote the key causal variable of interest to a researcher.

stances: (1) when political actors choose to manipulate some factor in a way that assigns it randomly, (2) when actors cannot manipulate the factor, (3) when actors could manipulate it but don't bother to try. The first is rare, but can be extremely instructive about interesting questions and the answers are often practically relevant. The second is also instructive about interesting questions, but the answers tend not to be very practically relevant, nor are they easy to generalize. The third is only possible with boring questions, except in rare cases.

This theory clarifies that political science is particularly besieged by causal inference problems precisely because of its focus is on politics. The parts of the discipline addressing questions most interesting to outsiders, where credible answers would find the widest audience, are the precisely those areas where credible answers will be hardest. Other fields of social science will face the same trade-off to the extent that they attempt to provide causal answers to questions about who gets what. Scholars in the hard sciences will not generally face these problems — the molecules involved aren't trying to game anything — which explains why scholars in these fields make substantial scientific progress despite only passing familiarity with modern statistical technology for causal inference.

This view may seem overly pessimistic. Can it really be that only the boring questions are tractable? The answer is "mostly yes," but realizing this will help attune researchers to the exceptions where causal inference is possible on interesting questions. It will also help political scientists understand why some questions that arrest the attention of their colleagues cannot be answered as rigorously as they would like.