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1

Introduction

The rise of formal theory and the credibility revolution are two of the great developments in social science over the past half century. With these advances, the potential for productive dialogue between theory and empirics has never been greater.

So it is distressing that, in political science, theory and empirics appear to be drifting apart. Ironically, these two developments, which should be drawing scholars together, have instead been dividing them.

The credibility revolution has sensitized social scientists to the challenges of separating correlation from causation and forced us to reckon with the plausibility of the causal interpretations routinely given to empirical estimates (Angrist and Pischke, 2010; Samii, 2016). But it has also opened up a schism.

On one side are scholars concerned that the pursuit of credible causal estimates is displacing the canonical goal of understanding important political phenomena. The papers we write, they argue, seem no longer to be about the questions that motivate us. "Why does this important thing happen?" has been replaced by "What is the effect of x on y?" Scholars with this worry might agree with some of the credibility revolution's critique of prior practice. But they fear it has gone too far. They see adherents of the credibility revolution as dismissive of what can be, and indeed what has been, learned by empirical scholars employing other approaches. The credibility revolution, they hold, unnecessarily limits the scope of evidence that is considered legitimate. We have let a fetish for a particular kind of credibility distract us from our true goals. Political scientists have expressed these worries in various ways. Clark and Golder (2015) describe the rise of "radical empiricism" divorced from theory. Huber (2013) laments that, because "good causal identification is not always possible on questions of central importance," the credibility revolution has led political scientists to "excessively narrow the range of questions we

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ask." And Binder (2019, p. 419) warns that "prioritizing identification strategies risks losing sight of the theoretical and analytical interests that motivate the research."

On the other side are scholars who have embraced the credibility revolution, arguing that much of the canonical quantitative work in political science offered only what Gerber, Green, and Kaplan (2014) call "the illusion of learning." For these scholars, there is no point in tackling questions that cannot be answered well. We should instead focus on questions accessible to credible research designs. Samii (2016, p. 941) describes the "prevailing convention in political science" prior to the credibility revolution as "what we might call mass production of quantitative 'pseudo-general pseudo-facts' through multiple regression." And Sekhon (2009, p. 503) argues that "without an experiment, a natural experiment, a discontinuity, or some other strong design, no amount of econometric or statistical modeling can make the move from correlation to causation persuasive. This conclusion has implications for the kind of causal questions we are able to answer with some rigor."

This schism recalls the earlier divide opened up by the rise of formal theory and its increased focus on model building (Green and Shapiro, 1994; Friedman, 1996; Walt, 1999). Like the adherents of the credibility revolution, early rational choice theorists, as Green and Shapiro (1994, p. 3) describe, "[did] not contend that traditional political scientists have studied the wrong phenomena," but rather that they "have studied the right phenomena in the wrong ways."

In the role of today's critics of the credibility revolution were those worried that a fetishization of mathematical elegance was distracting political scientists from the goal of generating insights that were genuinely useful for explanation or suitable for empirical assessment. Green and Shapiro (1996, p. 54) lamented that "empirical progress has been retarded by what may be termed method-driven, as opposed to problem-driven, research." What is interesting or useful, critics asked, about narrow models built on assumptions that bear, at best, only a distant relationship to reality? For instance, in his critique of formal models in international relations, Walt (1999, p. 9) argued that "[a] consistent, precise yet trivial argument is of less value than a bold new conjecture that helps us understand some important real-world problem . . . a logically consistent but empirically false theory is of little value."

Lined up to oppose such critics were those arguing that formalization allows scholars to avoid errors of logic and achieve greater transparency. Responding to Walt, Powell (1999, p. 98) argued, "[e]ven if tightening the

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connections between assumptions and conclusions were all that formal theory had to offer, this would be a very important contribution." Cameron and Morton (2002) point to three virtues of formalization: seeing with clarity which assumptions drive which results, avoiding mistakes of logic through rigor, and achieving a kind of unity or coherence by eschewing hypotheses that depend on contradictory assumptions.

These two schisms foreshadowed today's deepening divide between theorists and empiricists. While, in principle, nearly everyone agrees that theory and empirics ought to work together, in practice, each side feels the other often doesn't hold up its end of the bargain. On the one hand, a group of theoretically minded scholars is baffled and dismayed by the empirical turn toward research designs for credibly answering narrow causal questions. Why, they wonder, are empiricists obsessed with carefully answering uninteresting questions, rather than doing work that speaks to theoretical questions? On the other hand, a group of empirically minded scholars is similarly baffled and dismayed by theorists' focus on abstract models built on, from their perspective, demonstrably false assumptions. Of what use, they wonder, can such models be for explaining the world or guiding empirical inquiry?

As a result of this mutual puzzlement and dissatisfaction, these two groups are pulling apart—going about their own business and viewing the other with increasing skepticism. This widening gap threatens the link between theory and empirics that is essential to the social scientific enterprise.

The moment is ripe to draw these two groups back together. Formal theory and the credibility revolution are natural partners that, together, can support a richer and more productive dialogue between theory and empirics than has ever before been possible in political science.

However, as a discipline, we are not currently prepared to realize this potential. Empiricists and theorists alike are too quick to dismiss one another's enterprise. We all need a better framework for thinking about how the two fit together. Each side needs to better understand what kind of knowledge the other is trying to create, and how they go about it. Only with this understanding will theorists see how to make their models genuinely useful to empirical inquiry and empiricists see how to structure their research in ways that speak to theoretically meaningful questions.

This book provides such a framework. We explain to empiricists why theorists build the sorts of models they do, the kind of understanding such models provide, and how such models generate insight that is vital for interpreting empirical evidence. We explain to theorists why empiricists use the sorts

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of research designs they do, the kind of quantities credible research designs estimate, and why those quantities are exactly what is needed to speak to theoretical questions. And we give both sides a way of thinking about how these two activities together underpin the accumulation of social scientific knowledge.

We do this through both conceptual analysis and detailed examples. Some of the ideas may be familiar. Others will be new. We organize and synthesize them in ways we believe are conceptually clarifying. One payoff of this book, then, is to help scholars better understand how their own research fits into the overall enterprise of political science and what that enterprise entails. This is the work of Part I.

But conceptual understanding, though important, is not our end goal. We want this conceptual understanding to have a practical payoff for research. We believe this book will give scholars, from first-year PhD students to seasoned veterans, a lens that brings into focus opportunities for substantively important contributions that might otherwise be missed. More than anything else, understanding and pursuing these intellectually exciting opportunities for real synergy will bring us back together and improve our discipline. This is the work of Part II.

Our argument starts with the observation that theoretical implications are always all-else-equal claims. This means holding all else equal in empirics is important, for two reasons. First, if the empirical analysis has not held all else equal, we don't know whether the reason for disagreement between a theoretical implication and an empirical estimate is that the theoretical mechanism is not at work or that the empirical estimate and theoretical implication are about different quantities. Second, an empirical finding is a better guide for theorizing when it is about the sort of all-else-equal relationship theoretical models produce as implications.

For an example of how holding all else equal is important for assessing theory, consider the empirical finding that members of the House Appropriations Committee secure more pork than those not on the committee (e.g., Lazarus, 2010). This finding is often taken as confirmatory evidence for the theoretical claim that congressional rules grant committee members outsized influence (Shepsle and Weingast, 1987). But that finding is actually not very informative about the theory. The theoretical implication is an all-else-equal one. But the empirical comparison fails to hold all else equal—for instance, we might worry that the sort of member who can secure a coveted seat on Appropriations might also be the sort of member who would have captured more

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federal funding regardless. When we use the tools of the credibility revolution to take the all-else-equal caveat more seriously, things don't look as good for the theory. For instance, Berry and Fowler (2016) compare the spending garnered by the same member before and after gaining a seat on Appropriations and find the bump from joining Appropriations is negligible.

For an example of how formal theory is particularly useful for interpreting empirical findings that hold all else equal, consider the empirical literature showing that exogenous events outside of the control of incumbent politicians (e.g., natural disasters) affect electoral fortunes. These all-else-equal results are often interpreted as evidence of voter irrationality (Wolfers, 2002; Achen and Bartels, 2004; Leigh, 2009; Healy, Malhotra, and Mo, 2010). But an implication of Ashworth, Bueno de Mesquita, and Friedenberg's (2018) formal model of electoral accountability is that, all else equal, even when voters are rational, incumbent electoral fortunes are responsive to natural disasters. A natural disaster gives voters an extra opportunity to learn about an incumbent's quality (e.g., how well they responded to a global pandemic). If, absent new information, most incumbents expect to win reelection, this new information will create more downside than upside risk. Hence, the empirical fact that, all else equal, disasters hurt incumbent electoral fortunes on average, does not necessarily mean that voters are irrational.

These two examples show how formal theory and credible empirical research work together, whether for assessing a particular theoretical implication or interpreting a particular empirical finding. Even greater progress can occur when this partnership is sustained in a back-and-forth over time, as illustrated by the literature on the economic causes of civil war.

Theorists of civil conflict have long argued that grievance drives rebellion and that a thriving economy, by alleviating grievance, might reduce conflict (Gurr, 1970). In an important early quantitative contribution, Fearon and Laitin (2003) provide cross-country evidence that per capita income is negatively correlated with the onset of civil war. But they also found that other factors thought to affect grievance (e.g., ethnic divisions, lack of democracy) are not correlated with the onset of civil war. Hence, they argue that a different theoretical interpretation is needed. They suggest that governments in wealthier countries have greater capacity to control territory, which limits the conditions conducive to insurgency.

Scholars working in the tradition of the credibility revolution question how informative these empirical findings are about any of the theories. Theoretical implications about the relationship between the economy and civil

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conflict are all-else-equal claims. But these cross-country comparisons do not plausibly hold all else equal (Miguel, Satyanath, and Sergenti, 2004). For instance, perhaps a high risk of civil conflict harms the economy by causing capital flight or deterring foreign investment. And, indeed, findings from research that takes the all-else-equal caveat more seriously don't look as good for the theories. For example, Bazzi and Blattman (2014) find no relationship between plausibly exogenous shocks to the world prices of a country's commodity exports and civil conflict in that country.

The story doesn't end there. One plausible interpretation of Bazzi and Blattman's null finding is that economic mechanisms are unimportant. But theory suggests both a reinterpretation and a path forward for empirical scholarship. The reinterpretation comes from observing that, while commodity-bundle price shocks might hold all else equal, their effect also likely reflects competing mechanisms. For instance, in addition to the grievance mechanism, Grossman (1991) models a predation mechanism that works in the opposite direction—all else equal, positive economic shocks might exacerbate conflict by increasing the value of the territory over which the rebels are fighting. The path forward is suggested by Dal Bó and Dal Bó's (2011) theoretical model that differentiates two types of commodities. Shocks to the price of labor-intensive goods should affect wages and thus primarily activate grievance-type mechanisms. Shocks to the price of capital-intensive goods should instead primarily activate predation-like mechanisms. Dube and Vargas (2013) take up this idea empirically in the context of conflict in Colombia. They estimate how conflict changes differentially in coffee-producing and oil-producing municipalities in response to shocks to world coffee and oil prices. Their results agree with the theoretical implications—all else equal, negative shocks to the price of labor-intensive coffee increase conflict in municipalities that produce coffee relative to those that don't and negative shocks to the price of capital-intensive oil decrease conflict in municipalities that produce oil relative to those that don't.

For all the progress made over the course of this exchange between theory and empirics, it also exemplifies a common lament among critics of the credibility revolution. The literature seems to have been diverted from studying broad questions about the sources of civil conflict to narrow questions about the sources of civil conflict in the municipalities of Colombia. But this worry is misplaced. The narrowing of focus allowed for greater clarity of thought, a tighter link between theoretical implications and empirical estimates, and a disentangling of mechanisms. Moreover, with these insights in

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place, scholars were then able to again broaden the scope, but without sacrificing the deeper connection between theory and empirics. For instance, several subsequent studies use research designs similar to Dube and Vargas's to analyze the same mechanisms, using data on economic and conflict activity measured at the level of fifty square kilometer grid cells spanning multiple countries, and produce similar findings (Berman and Couttenier, 2015; Berman et al., 2017).

Looking across these examples we see why the credibility revolution and formal theory are natural complements. The essence of formal theory is the crafting of models that embody mechanisms and reveal the all else equal implications of those mechanisms. The essence of the credibility revolution is the crafting of research designs that make credible the claim to have held all else equal, at least on average—exactly what is needed to assess and guide theory.

A major theme of this book is that such exciting opportunities for a deeper connection between theory and empirics lie waiting throughout political science, on topics ranging from elections to civil war to bureaucratic politics to international organizations. Our ambition is that this book, itself a joint effort by empirical and theoretical researchers, will better equip readers to discover these opportunities. And, along the way, we hope it helps both groups gain a deeper appreciation for what their colleagues are up to, and why it matters.

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