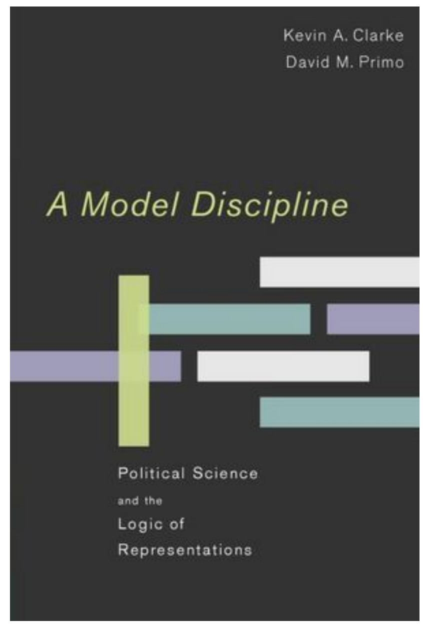
Models as maps: The search for better explanations of political phenomena

A Model Discipline continues the long-running debate on the role of formal, mathematical models in political science and whether purely theoretical work should be published in top journals. **Benjamin Lauderdale** finds the larger points in the book compelling despite disagreeing with its arguments on what should actually be demanded of theoretical models.

A Model Discipline: Political Science and the Logic of Representations. Kevin A. Clarke and David M. Primo. Oxford University Press. January 2012.



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What is the role of modeling in political science? More fundamentally, what is a model? *A Model Discipline: Political Science and the Logic of Representations* argues that models are best thought of as maps. A map is a tool that is useful with respect to a particular purpose. The authors note that a subway map, a highway map and a local road map can provide different abstractions of the same city, each of which are useful for answering certain questions. None of the maps are more fundamentally true than any other; however, a given map's tradeoffs between parsimony, verisimilitude and other attributes might be improved upon with respect to a particular purpose.

This metaphor of models as maps is presented as an antidote to what the authors take to be the dominant paradigm for understanding models in political science, that of "hypothetico-deductivism" (H-D). H-D is the villain of the book. It is the notion that theoretical models are used to derive empirical predictions, and that the models are "tested" by comparing those predictions to empirical data using some suitable empirical model. In the authors' usage, a theoretical model is something like a game-theoretic model of crisis bargaining, and an empirical model is something like a linear regression model with variables describing a set of crises. The authors document the decline of the H-D model of science in the philosophy of science over the last four decades, but provide ample evidence that this perspective still pervades the way that political scientists present research.

As the authors repeatedly emphasize, theoretical models are objects that logically deduce conclusions from a set of assumptions. If their assumptions are true, their conclusions will be true. If their assumptions are not true, their conclusions may or may not be. This undermines hypothetico-deductivist theory testing because there is nothing to test. We cannot verify the model, because any given prediction could arise from alternative models. We cannot falsify the model, because we already know that its assumptions will never be exactly true as descriptions of the world.

Much as the ubiquitous prisoners dilemma (a theoretical model) links a set of assumptions to a set of predictions, the ubiquitous linear regression model (an empirical model) links a set of assumptions to a set of estimands. In the authors' view, empirical models are more than just a set of mathematical relationships between abstract variables. They also include the substantively informed decisions that govern variable selection and other specification choices (p 107-109). We can compare different empirical models in terms of predictive power, but this is a test of model usefulness rather than of the models themselves.

In the end, the authors' position is presented as ecumenical: let theorists be theorists and empiricists be empiricists. Their larger points about the role of models are, I believe, very compelling. However, at several points in the text there are less compelling arguments about what should be demanded of theoretical models in political science. This book is written in the context of a long-running battle about the role of formal, mathematical models in political science, and whether purely theoretical work should be published in top journals (p 180). I suspect that the authors want us to conclude that all criticism of formal modeling derives from the flawed H-D perspective. However, I would argue that criticisms really reflect a widespread view that key behavioral assumptions, embedded in nearly all such models, have well-documented limitations as descriptions of behaviour.

Everyone seems to agree that a valid way to criticize the usefulness of a particular empirical model is to point to ways that inaccuracies in assumptions are consequential for conclusions. However, the authors claim that while assumption checking is "crucial" for empirical models, it is "decidedly unimportant" for theoretical models (p 109). We should ignore whether assumptions are realistic, because they are never perfectly so (p 99). This is a fallacious argument: statistical assumptions in empirical models are never exactly right either, yet it is indeed "crucial" to subject them to scrutiny. There is no reason why this standard should not be applied to theoretical models.

Fundamentally, this is a question of usefulness, the very standard that the authors argue for. The authors argue that theoretical models are useful because they explore the space of possible explanations for political phenomena. But some parts of that space are just more interesting than others: a map of Narnia is of little use when one lives in London. All else equal, both theoretical and empirical models are more useful when their assumptions are more credible.

Criticism of model assumptions is vital to the process of searching for better explanations of political phenomena. It is part of the dialogue between theoretical and empirical work that must occur if we are to make collective progress. I want to emphasize that this perspective is not in conflict with the authors' argument that theoretical models can be valuable even when their predictions are not immediately subjected to empirical investigation. Few would deny that theoretical statistics is a useful field, even when it simply explores general properties of certain assumptions, models, or estimators. Demanding that each and every such result be presented with an application is indeed mistaken. But most research using statistical methods is applied in orientation, and most formal modelling in political science is as well. Both readers and journal reviewers are right to demand that model assumptions are substantively plausible, and that the consequences of potential violations are discussed.

Anyone who is a political scientist, or who aspires to be one, has to decide what kind of research is worth doing. Moreover, we have to decide what to make of the research that we read in the published literature and that we are asked to review. The target audience of this book is primarily the people making these decisions. However, for that audience of political scientists, the issues taken up in this book are very important. There are few political scientists who would claim they are happy with the corpus of publications in the field, but there is enormous disagreement about what exactly is wrong with existing research practices. Clarke and Primo set forth one view of what is wrong, and how we should proceed. As this review indicates, I did not agree with everything they wrote; however, considering their arguments has helped me better understand my own perspective. It therefore meets the authors' standard: I did find it useful.

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research interests primarily include US political institutions and behavior, particularly the Supreme Court, Congress and public opinion. He is especially interested in methodological problems related to the measurement of policy preferences across time, issues, and institutions. More information about his research can be found at his website. Read more reviews by Ben.

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