Computational and Mathematical Modeling in the Social Sciences

Mathematical models in the social sciences have become increasingly sophisticated and widespread in the last decade. This period also has seen many critiques, most lamenting the sacrifices incurred in pursuit of mathematical rigor. If, as critics argue, our ability to understand the world has not improved during the mathematization of the social sciences, we might want to adopt a different paradigm. This book examines the three main fields of mathematical modeling – game theory, statistics, and computational methods – and proposes a new framework for modeling. Unlike previous treatments that view each field separately, this book provides a framework that spans and incorporates the different methodological approaches. The goal is to arrive at a new vision of modeling that allows researchers to solve more complex problems in the social sciences. Additionally, a special emphasis is placed upon the role of computational modeling in the social sciences.

Scott de Marchi is Assistant Professor of Political Science at Duke University. His work has been funded by the National Science Foundation, and he has published articles in the *American Political Science Review*, *Journal of Politics*, *Journal of Theoretical Politics*, and *Public Choice*. Professor de Marchi was appointed a Fellow-at-Large by the Santa Fe Institute in 1999 and is a faculty member of the Ralph Bunche Summer Institute and the Empirical Implications of Theoretical Models program. His research continues to focus on the field of computational political economy and other mathematical methods, individual decision making, the presidency, and public policy.

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SCOTT DE MARCHI
Duke University

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For Jim Barefield and Ed Christman

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Prelude

When Aeneas fled from burning Troy, he had some difficult decisions to make. His first priority was to rescue his country gods and relics, but he was covered in gore from combat and did not want to carry these sacred artifacts with his own hands. His solution was novel: Anchises, his father, could carry the artifacts and Aeneas would carry him upon his back. His second priority was to guard the safety of his wife Creusa and his son. With his heavy burden, he "satisficed" by holding the hand of his son and bidding his wife to follow him. Unfortunately, though he succeeded in rescuing the country gods and his son, he lost his wife during his flight from the doomed city.

Earning the appellation "pious" involved some cruel choices for Aeneas, but despite this offense to modern sensibilities (I daresay many of us would have tossed the country gods and told Anchises to walk on his own two feet), it is hard to blame him. Weary from battle, burdened with both his family and the country gods, it would be difficult to pay attention to everything of merit. It is not surprising that he did not even know when or how he had lost his wife.

Graduate school has some similarities. Granted, most students do not have to face a ravaging horde of Greek soldiers, nor are they surrounded by burning buildings. But the press of time is a constant weight, and one is forced to attend to some matters more than others. It is not a coincidence that if you ask students trained in the top research programs in the social sciences what their field is they may answer "mathematical methods" or even something more precise such as "game theory" or "econometrics." Most students spend a large fraction of their time learning these methods, and this comes at the expense of other sorts of work such as history and case studies. Like pious Aeneas, we make choices, and even the most heroic of us are forced to ignore many worthwhile subjects.

The important thing to note is that many of the social sciences, most notably political science and economics, have made a wager. This wager involves both time and space. From graduate students to faculty, we spend our time learning and practicing mathematical methods, in particular game theory and statistical modeling. For the journals and presses, the lion's share of space is devoted to the results generated by mathematical methods. One does not find the best journals customarily publishing case studies of individual countries, firms, or political campaigns. Nor, in the case of the top journals in political science, is much advice (either prescriptive or predictive) given to real-world political actors. Based on the 2002–2003 Report of the Editor of the *American Political Science Review*, 69% of submissions were accounted for by the formal, quantitative, or formal *and* quantitative categories; 63% of accepted articles were in these categories – this during the tenure of an editor striving for diversity.

The presumption of this book will be to examine this epistemological gamble more closely and recommend a set of changes to current practice. It is not as if every scholar has embraced the increasing emphasis on mathematical methods. The last two decades have seen many critiques, most lamenting the sacrifices incurred in pursuit of mathematical rigor. If, as the critics argue, our ability to understand the world has not improved during the mathematization of the social sciences, we might want to adopt a different paradigm. Historiography (or qualitative research) is most often presented as the alternative to the abstractions of mathematical methods. It might, say the critics, be better for the discipline to turn out area-specialists who at least know the history of their cases than to engage in bad modeling that lacks any clear connection to the real world.

I have the good fortune of better than adequate training in history,[1] and I can argue with some fervor that a turn to historiography would not

[1] I took undergraduate degrees in computer science and history. Because of latent schizophrenia, I completed the coursework and thesis for a Master's degree in European history at the University of North Carolina–Chapel Hill before switching to the social sciences.

be good for the social sciences. Despite its problems, I remain devoted to mathematical modeling, and the goal of this book is to improve current practice rather than to supplant it. Area-specialization and case studies are necessary prerequisites for the inspiration and understanding implicit in all good models, but in my mind they do not of themselves constitute a coherent methodology for discovering causal relationships.[2]

Required reading for those who wish to supplant mathematical methods with qualitative research should include Peter Novick's *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (1998). From the end of the 19th century to the beginning of the Cold War, history as a discipline was very similar in outlook to modern political science.[3] Novick's book lays out the history of the professionalization project in American history departments over this time period. Much like current social science disciplines, historians believed in their ability to understand causal relationships in the world and sought to give answers to pressing questions about how one prevents war between nation-states or the republican cycle of decay highlighted by political theorists such as Machiavelli.

The problem, after a century of consensus on method, was that historiography foundered upon the shoals of the objectivity question. For Novick, historians who believed in scientific objectivity never adequately answered the fundamental questions of how to tell good research from bad and neutral research from biased. Many historians, spurred on by the emergence of social history and other trends, simply did not believe that the empirical, objectivist tradition produced superior research.[4]

[2] There is an enormous literature on qualitative versus quantitative research. For an examination of some of the problems implicit in historical research from a political science perspective, good examples are Lustick (1996) and Goemans (2000).

[3] Although there was not great technical skill present in most historical research, there was a belief in empirical work and the use of history for understanding causality in human affairs. The letters of Henry Adams (at Harvard) to Herbert Baxter Adams (at Johns Hopkins), for example, demonstrate a high level of familiarity and respect for the hard sciences among practicing historians at the end of the 19th century and a belief that scientific objectivity was a worthwhile aspiration for the social sciences.

[4] For an example of an alternative approach to historiography, read Natalie Zemon Davis's *The Return of Martin Guerre* (1983). Davis's work concerns a French tale from the 16th century in which a woman discovers her husband is an imposter and takes him to court. Because the penalty was death by hanging, this was no laughing

One of the more sobering examples Novick uses to illustrate the death of objectivity in the historical profession is the case of David Abraham. The role of German industrialists in the rise of the Nazi Party was a contentious one, and Abraham, a junior faculty member at Princeton in the early 1980s, wrote a manuscript that emphasized the structural relationships in German society that precluded a more moderate outcome to the political turmoil of the Weimar state.

Unfortunately for Abraham, his abstract modeling, which was influenced by Marxist theory, did not endear him to senior researchers in the field. Despite many positive book reviews, Henry Turner at Yale University and Gerald Feldman at the University of California at Berkeley led an assault on Abraham's book. They believed that the footnotes to Abraham's monograph contained serious, willful errors. Misattributed citations, missing or incorrect quotations, and other errors were, in fact, plentiful in Abraham's work. For Turner and Feldman, these mistakes were proof of a malicious agenda that violated norms of historiography. In a book review in *Political Science Quarterly*, Turner wrote:

> Invoking the familiar primacy of economics, Abraham presents a highly reductionist version of the dissolution of the Republic and the rise of Nazism, which he explains in terms of his vastly simplified model of German society. . . . Unfortunately, Abraham's footnotes do not marshal evidence adequate to support his thesis. Informed readers will also balk at his disparagement or omission of institutions, ideologies, and personalities vital to comprehension of the German calamity. (Turner 1982, 740)

It is hard to convey how contentious this affair became. The journal *Central European History*, for example, featured an exchange between Feldman and Abraham that even included a complete list by Abraham of his errors and whether or not the corrections helped, hurt, or were neutral to his argument. The exchange appeared in press in 1985, but by then Abraham had been driven from the field. For Novick, who was Abraham's advisor, the lesson for historians was that optimism

matter. Davis had completed a screenplay on the story and found that her "appetite was whetted" for a more scholarly investigation, despite the lack of an expansive historical record on the story. Her approach to this problem is distinct from previous understandings of historiography: "Watching Gerard Depardieu [the actor] feel his way into the role of the false Martin Guerre gave me new ways to think about the accomplishments of the real imposter, Arnaud du Tilh. I felt I had my own historical laboratory, generating not proofs, but historical possibilities" (Davis 1983, viii).

about the ability to discern causality in history had been replaced by a naïve and defensive empiricism. Other than getting one's footnotes right, there was no other avenue for attacking or defending a model.

There are more modern examples of the continuing crisis in historiography. Michael Bellesiles's book *Arming America: The Origins of a National Gun Culture*, which presented the argument that gun culture in early American society was not as widespread as believed, won the Bancroft Prize when it was released in 2000. Much like Abraham, Bellesiles riled opponents of a different political stripe, and upon scrutiny, it was discovered that much of the data underlying the book's quantitative analysis was either misused (in the case of probate data) or entirely missing from the archives. Despite these glaring problems, the question remained about whether or not his core argument was valid. Ultimately, like Abraham, Bellesiles was forced from the discipline, resigning his post at Emory University under pressure from the trustees at the end of 2002. The Bancroft Prize for his book was rescinded shortly thereafter.[5]

Although I do not believe that Novick has much of a remedy for historiography, I do accept his diagnosis of the problem. If a particular methodological paradigm is to survive, a large majority of practicing scholars has to believe that the costs involved in training and research are merited. Simply put, the output of a methodology has to be superior results, at least compared to existing alternatives. The question economists and political scientists should ask is whether or not Novick's history of the erosion of the belief in objectivity among historians holds any lessons for us.

Despite the enormous successes made possible by the mathematical approach – the Arrow, McKelvey, and Schofield work on social choice is an excellent example – many critics, rightfully, want to know what the last decade has produced. The argument that I will present in this book is that the practice of mathematical modeling is due for a revision. In particular, existing methods are brittle when confronted with complex problems, and there is a genuine lack of correspondence

[5] A special issue of the *William and Mary Quarterly* (2002) featured essays by Bellesiles and several other historians that examine the controversy and its implications for historiography.

between deductive models, on the one hand, and empirical tests of these models, on the other.

There are additional problems unique to each of the two major subfields within mathematical methodology. Game theory, for example, has a troubling answer to the question "Is game theory meant to predict what people do, to give them advice, or what?" As Camerer (2003, 5) notes, many game theorists believe that "game theory is none of the above – it is simply 'analytical,' a body of answers to mathematical questions about what players with various degrees of rationality will do. If people don't play the way the theory says, their behavior has not proved the mathematics wrong, any more than finding that cashiers sometimes give the wrong change disproves arithmetic." Although there are examples of formal modelers tackling real-world problems, such as the interesting work of Groseclose, Milyo, and Primo on topics that include the dollar value of a House of Representatives seat, campaign finance, and empirical measures of media bias, many game theorists do not believe that their work needs an empirical referent.[6]

Statistical methodology in the social sciences has its own set of problems that mirrors the opening passage in Dickens's *A Tale of Two Cities*. We have increasingly sophisticated forays into Bayesian and nonparametric techniques. At the same time, replication continues to be problematic, especially as the complexity of statistical methods increases. Recently, the laudable goal of linking formal theory with statistical models has received renewed attention in the research of Signorino and others. Yet, most published research continues to ignore the most basic tenet of statistical work, which requires out-of-sample testing to validate a model.[7] Never before has training in statistical modeling been so widespread in graduate departments around the nation. So, too, has suspicion deepened, as many researchers have adopted Achen's (2003) admonition that a model with more than three independent variables is immediate cause for concern.

[6] On the value of a seat, see Groseclose and Milyo (2004a); on campaign finance, see Primo and Milyo (2004); and, on media bias, see Groseclose and Milyo (2004b). Behavioral game theory also tries to put game theory on a more empirical footing – Camerer's book provides a nice introduction to the field. One also might visit Roth's Web site at http://www.economics.harvard.edu/∼aroth/alroth.html.

[7] For an excellent statement on statistical modeling that also happens to make this point on the neglect of out-of-sample work, see Good and Hardin (2003).

While some might question whether or not mathematical methodology is in need of revision, it is the case that a sense of unease permeates the social sciences. Those who do not practice these methods are deeply suspicious of the validity of results generated from mathematical models. And those that do practice one field of mathematical methodology are often just as suspicious about the other fields. I will argue that at least some of this suspicion is warranted, and the goal of this book is to provide a set of tools designed to increase transparency and improve modeling. Part of this enterprise involves a constructive critique of existing practice. Despite the widespread belief that the problems that beset mathematical methods are idiosyncratic to each subfield, I will demonstrate that there are a set of underlying problems that span subfields (including analytic, empirical, and qualitative).

Of the problems detailed in this book, the most severe is the curse of dimensionality. In the nonparametric statistics and artificial intelligence literatures, the "curse of dimensionality" is incredibly important, but it is not well known in the social sciences. In brief, the curse states that for any interesting problem, one should count the size of the parameter space needed to model the problem, paying special attention to how large this space becomes as the problem increases in size. If the parameter space implied by a naïve encoding of the problem is huge, one must resort to domain-specific information and a good dose of cleverness to surmount the curse of dimensionality. A brief example will clarify this informal definition.[8]

In the social sciences, preferences are almost always the subject of assumption rather than study. We simplify preferences by imposing *a priori* that for most human decisions, preferences are unidimensional, single-peaked, symmetric, and so on.[9] There is little justification for these assumptions, so why do we make them? Mathematical convenience is the typical answer, but this masks a more serious difficulty. Without simplifying assumptions, many of our models would produce different or unpredictable results.

[8] An excellent overview of this problem for statistical models is found in Chapters 4 and 8 of Harrell (2001).

[9] Note that assumptions of this type go well beyond more fundamental (and defensible) axioms such as well-ordered preferences and transitivity.

To be more concrete, imagine you were in an expensive ice cream parlor and had never before tasted ice cream. In addition to the flavors of ice cream, you have the option of adding sprinkles, nuts, syrups, and the like. All told, you have 10 flavors of ice cream and 10 different optional ingredients and want to test every possible flavor so that you could determine a preference ordering. This natural enough desire would probably bankrupt the store (and require you to do some shopping for larger clothes), as $10 \cdot 2^{10}$ possible combinations (recipes) exist. Unless one imposes limiting assumptions on the nature of preferences, there are no shortcuts possible – you would need to test every flavor if you wanted to be certain about your preference ordering. In many cases, you would feel justified in asking for this huge number of samples, because most everyone would agree that although sprinkles and marshmallows taste great singly, in combination they might be too sweet. Recipes are one example where the different dimensions of choice are nonseparable. We do not independently sample each ingredient, arrive at a set of ideal points, and then throw them all together in a pot.

This problem worsens if the ice cream parlor subsequently adds ingredients. Imagine you had just completed the extensive taste tests outlined above and then strawberries were provided as a new option. Would you be able to somehow "save" the results of your previous search, or would you have to begin an entirely new set of tests?[10] Few of us would think that adding strawberries to a hot fudge sundae, for example, would improve the sundae, whatever our preference for strawberries. It is easy to see that as the number of ingredients increases, the size of the resultant parameter space for ice cream recipes expands exponentially – and this is not a good thing![11] In the context of recipes, making the assumption that preferences are always separable would be quite odd, and would likely lead to equally odd results. One should instead depend upon domain-specific knowledge about cooking to simplify matters, but it may not be obvious how to go about this.[12]

[10] This exercise is left to readers, especially for those who like ice cream.

[11] I will argue throughout this book that trying to understand a problem like preference formation, without assuming away the complexities of the phenomenon (e.g., nonseparability), is a very important activity despite the ugly combinatorics involved.

[12] Domain-specific knowledge is information about the problem under consideration. Unidimensionality, for example, is appropriate to some contexts and not others – for

Ice cream recipes aside, how ubiquitous is the curse of dimensionality? Some readers will immediately point to statistical work, where the curse of dimensionality appears in a nearly equivalent form. Often, our data are insufficient for testing the huge parameter spaces implied by our independent variables and modeling choices. Like the preferences literature, empirical modelers often resort to limiting assumptions (e.g., linearity of the functional form) to derive results. We rightfully question these results due to their dependence upon atheoretic modeling choices and data mining.

The curse of dimensionality is not, however, limited to statistical work. Game theoretic work falls prey equally often. Assumptions are also parameters, and the structure of game theory comes at the price that results are conditioned upon the values chosen for these assumptions. Additionally, not just any assumptions will do, as formal modelers have to find a way to fit problems into the encoding of game theory (i.e., an extensive or normal form representation of strategies, explicit utility functions, and backwards induction as the solution algorithm). Many "games" do not fit comfortably within this encoding; as a consequence, technical assumptions end up doing a great deal of heavy lifting in many formal models. The intellectual process involved in finding a set of assumptions, choosing an equilibrium concept, and choosing an abstract game to produce an outcome desired *a priori* is not different in kind from the curve fitting of some empirical researchers.

It is important to go beyond criticism, however. The more important objective of this book is to provide both a framework for evaluating models and a set of tools designed to deal with the problems sketched in this prelude. The curse of dimensionality highlights the difficulty of using mathematical models to study complex phenomena. Contributing to this difficulty is the gap between analytic models and empirical tests; it is not a coincidence that as we extend our reach to investigate more complex phenomena, concerns have grown about the quality of our results. One consistent answer to these difficulties is to keep modeling simple, such that one can understand

recipes, it would be inappropriate. In all cases, one has to justify assumptions by the final performance of the model, not by appeals to abstract and untested notions about rationality or mathematical simplicity.

and test all the moving parts in a given model. This position is elaborated quite well by Axelrod (1984), but it is not surprising that his advice is largely ignored by scholars attempting to "push the envelope." The main question is how to build more complex models of behavior without sacrificing the ability to subject the results to exacting scrutiny.

Thus, I do not believe that mere ignorance accounts for the existing problems in mathematical modeling in the social sciences. Rather, the complexity inherent in many problems of interest has hampered our ability to generate models with clear empirical referents. In this book, I will integrate computational modeling into existing methods and demonstrate how many classes of problems demand a shared approach that includes computational modeling.[13] Computational methods are poorly understood (and sometimes poorly utilized) in the social sciences, despite an increasing presence in both training and research. Yet, it is my contention that computational modeling offers several advantages over traditional modeling strategies when confronted with a variety of games and decision contexts.

THE BOOK IN A NUTSHELL

There are three components to this book. The first builds a framework for evaluating models. Whatever the methodological orientation of a model, one should ask the following questions:

1) What are the assumptions/parameters of the model? Do the values chosen for the parameters come from qualitative or empirical research, or are they chosen arbitrarily (i.e., for convenience)? More important still, do the assumptions spring from a consideration of the problem itself, or are they unrelated to the main logic of the model?
2) Is there any assurance that the results of the model are immune to small perturbations of the parameters; that is, is there an equivalence class where the model yields the same results for a

[13] At the broadest level, computational models are numerical experiments where one uses computers to simulate a problem rather than solve it deductively – Monte Carlo statistical methods are one familiar example.

neighborhood around the chosen parameters? Or, is the model brittle?

3) Do the results of the model map directly to a dependent variable, or is the author of the model making analogies from the model to the empirical referent? Although toy models[14] have their place in developing intuition, they are difficult to falsify, and even more difficult to build on in a cumulative fashion.
4) Are the results of the model verified by out-of-sample tests? In this book, it will be argued that the only appropriate out-of-sample tests for a model are either

   a. a large-N statistical approach that tests the model directly;
   b. a logical implication derived deductively from the model.

5) Is the parameter space of the model too large to span with the available data? This, as noted earlier, is the curse of dimensionality, and one should never neglect the importance of bean counting. To cope with large parameter spaces, did the author of the model derive a domain-specific encoding, provide a feature space,[15] or use theory in other ways to lessen the impact of the curse of dimensionality?

Topics 1–3 are covered in Chapter 1 of this book. In addition, Chapter 1 presents a comprehensive statement on epistemology that justifies the above framework. Topics 4 and 5 are covered in Chapter 2, which also introduces the concept of feature spaces and their role in surmounting large parameter spaces. Examples using currency adoption and the security studies literature on militarized interstate disputes illustrate the main concepts.

While the first two chapters focus on how to assess models, Chapters 3 and 4 focus on the second component of this book: computational

[14] Toy models are defined here as a class of simple models without any unique empirical referent. For example, the iterated prisoner's dilemma (IPD) is a simple game that investigates cooperation. It seems unlikely that all of human cooperation is a two-player contest with the exact strategy set of the IPD, and there is enormous difficulty in analogizing from the IPD to actual human behavior with enough precision to do any sort of predictive work.

[15] Feature spaces will be covered in Chapter 2. Feature spaces use domain-specific information (i.e., theory) to reduce the dimensionality/complexity of a problem.

methods and their role in addressing more complex phenomena. The use of computational methods makes it easier to build models that directly map to empirical tests. The main topics are:

1) How do game theoretic and computational models differ? Illustrations will be drawn from the artificial intelligence and combinatorics game theory literatures.
2) How does one "break up" a problem into smaller pieces, thereby overcoming the curse of dimensionality? The concepts of component games and idiosyncratic utility functions are examined in detail.
3) How does one use statistical work or logical implications to verify the results of a computational model (to the degree this is possible)?

In addition to these questions, these chapters provide a gentle introduction to the skills needed for computational modeling. Topics include programming languages, good programming style, and testing computational results.

The final component of the book provides two lengthy illustrations of the main concepts of the previous chapters. Chapter 4 presents the first example, which builds a complete encoding for a complex alliance game. Unlike most game theoretic models, the alliance game presented here has infinite strategies, four or more players, and the possibility of cooperation between different, endogenously created coalitions. Chapter 5 returns to the problem of the ice cream store and nonseparable preferences. Unlike situations in which one has enough high-quality data to do out-of-sample statistical work, studying nonseparable preferences requires the creation of logical implications to leverage existing survey data.