Review: The Analytical Narrative Project

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The Analytic Narrative Project

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In *Analytic Narratives*, we attempt to address several issues. First, many of us are engaged in in-depth case studies, but we also seek to contribute to, and to make use of, theory. How might we best proceed? Second, the historian, the anthropologist, and the area specialist possess knowledge of a place and time. They have an understanding of the particular. How might they best employ such data to create and test theories that may apply more generally? Third, what *is* the contribution of formal theory? What benefits are, or can be, secured by formalizing verbal accounts? In recent years, King, Keohane, and Verba (1994) and Green and Shapiro (1994) have provoked debate over these and related issues. In *Analytic Narratives*, we join in the methodological discussions spawned by their contributions.

In one sense, the aim of our book is quite modest: We hope to clarify the commonalities in approach used by a number of scholars, including us. We do not claim to be developing a brand new method. Rather, we are systematizing and making explicit—and labeling—what others also attempt.[1] In another sense, the aim of the book is ambitious; by trying to systematize we begin to force ourselves and others to lay out the rules for doing analytic narratives and to clarify how such an approach advances knowledge. We realize that our book is only a first step and concur with another of our reviewers: "As a method, analytic narrative is clearly still in its infancy, but it has promise" (Goldstone 1999, 533).

We have an additional aim: to transcend some of the current and unproductive "tribal warfare," especially between the new economic versus historical institutionalists and between advocates of unbounded and bounded rationality. We believe that each of these perspectives brings something of value, and to different degrees the essays in our book represent an integration of perspectives. By explicitly outlining an approach that relies on rational choice and mathematical models, we do not mean to imply that other approaches lack rigor but, rather, to reveal how to apply our tools in a useful and explanatory way. We wish to join the debate, not claim an end to it.

Part of that debate is among the five of us. Achieving a minimal degree of consensus was no easy task. By including a set of individually authored essays that reveal both our commonalities and differences, we indicate the range of possible approaches while also attempting to create boundaries. The five studies all draw from the same general rational choice approach. Even Levi's norms of fairness are modeled in terms of rational choice (in fact, this is part of Elster's objection). We have accepted rational choice theory and are among those attempting to extend it in historical and comparative research. We are imperialists if that means believing, as Gary Becker did when he applied neoclassical economics to the family and to discrimination, that the domain of rational choice can be usefully enlarged. We are not imperialists if that means believing that rational choice theory is the only possible approach to historical and comparative research.

The central problem we tackle in *Analytic Narratives* is how to develop systematic explanations based on case studies.[2] King, Keohane, and Verba correctly urge us to move toward generalizing. They recommend that scholars first consider how to define the universe of cases of which their case is an element and then attempt to devise a way of drawing a sample from that universe. This is excellent advice. The problem is that, for many studies, their approach is not so easy to apply, at least in the initial stages of research. Many political scientists begin with an interest in a particular phenomenon, such as the American Civil War, the French Revolution, the cause of World War I, the fall of socialism, or the rise of the New Deal. At the beginning of research, before formulation of an account of the phenomenon, the universe of cases containing a case of this sort is not obvious. Indeed, only after acquiring a significant understanding of the phenomenon—that is, only after much if not all the research has been concluded—can a scholar have any prospect for defining the larger universe of events.

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[1] Many others write analytic narratives that combine detailed data collection with rational choice or game theory analyses. Among them are Gary Cox, Jean Ensminger, Kathryn Firmin-Sellers, Barbara Geddes, Anthony Gill, Miriam Golden, Stathis Kalyvas, Edgar Kiser, David Laitin, John Nye, Sunita Parikh, Roger Petersen, and Susan Whiting.

[2] We are hardly the first to attempt this. See, for example, Eckstein 1975, George and McKeown 1985, Lijphart 1971, and Przeworski and Teune 1970.

The five of us, representing two different disciplines, all use the analytic narrative approach to achieve insights into different puzzles in very different historical periods. A major problem we address is how to develop systematic explanations for, and extract valid inferences from, such cases. Each chapter identifies a puzzle unique to the place and period under study, offers an explanation by using the tools of analytic narrative, and lays out the more general questions raised by the specific study. Moreover, in each chapter, the author derives a model and confirms its implications with data.

Greif investigates the growth of Genoa in the twelfth century and accounts for the puzzle of how the podestà, a ruler with no military power, resolved harmful clan conflict and promoted economic prosperity. His case has implications generally for issues of factional conflict and political order. Rosenthal models both long-term and divergent institutional change among countries and offers new insights into the relationship between war and governmental regimes by investigating the differences in taxation in France and England in the seventeenth and eighteenth centuries. Levi accounts for the variation in nineteenth-century conscription laws in France, the United States, and Prussia and finds that changing norms of fairness, resulting from democratization, influence the timing and content of institutional change. By focusing on the balance rule and how it deflected civil war in the United States, Weingast advances the program of understanding the institutional foundations and effects of federalism. Bates analyzes the rise and fall of the International Coffee Agreement; he discovers and explains why during World War II and the Cold War the United States, a principal coffee consumer, cooperated with the cartel to stabilize prices. His major finding concerns the circumstances under which a political basis for organization will trump economic competition in an international market.

Our project represents a means of connecting the seemingly unique event with standard social science methods. First, we model a portion of the critical dynamics in a way that affords tests of parts of the idea. This in itself is worthwhile. Second, we go farther and attempt to use the single case to generate hypotheses applicable to a larger set of cases. It is only in developing the account or model that we are pushed toward seeing what components of the account are testable and generalizable.

Before turning to our response to Jon Elster, it is worth considering his approach to social science. Elster's review attacks all forms of social science explanation that attempt to provide systematic answers to political, economic, and historic questions. *Analytic Narratives* is simply his current foil. Elster has a very circumscribed conception of what constitutes useful theory. He claims (1999a) that social scientists can do little more than develop a repertoire of mechanisms. His observation of "both the failures to predict and the predictions that failed" has entrenched his skepticism about the possibility of law-like explanations, a skepticism "bordering on explanatory nihilism," redeemed only by "the recognition of the idea of a mechanism that could provide a measure of explanatory power" (1999a, 2). We, in contrast, believe that although we may not be able to derive general laws, we nonetheless can develop, refine, and test theory-driven models and thus employ theory to gain deeper insights into the complex workings of the real world. Our book features both efforts to employ such models and discussions of the difficulties encountered when doing so.[3]

ELSTER'S QUESTIONS

Elster poses six questions and an additional set of concerns. The first is: "Do they agree with my characterization of analytic narrative as deductive history and with my statement that in practice this tends to mean rational choice history?" This raises two questions, one about the role of rational choice theory and one about deductive history. Yes, we use and have a preference for rational choice theory, but it is not a necessary condition for an analytic narrative. For example, one could use instead prospect theory or any systematic theory of individual choice, including nonrational theories of choice, to generate the predictions of individual behavior. Aside from prospect theory, however, the alternatives are not sufficiently developed to provide a consistent technique for generating behavior in games.

Turning to the issue of deductive history, we observe that analytic narratives are not deductive histories. In particular, we do not deduce the structure of the game from first principles. Of course, all rational choice models are deductive, but the deductive component, especially when applying game theory, assumes the existence of an appropriate game to analyze. Yet, there exists no finite list of games or any reason to believe that there is one. By the same reasoning, we should not expect to find a French Revolution game, the American Civil War game, or the Genoese game. It is at precisely this point that an analytic narrative relies on inductive methods. We take pains to explain that the process of deciding the appropriate individuals, their preferences, and the structure of the environment—that is, the right game to use—is an inductive process much like that used in modern comparative politics, by historical institutionalists, and by historians. Once that induction is complete, we can use the deductive methods to study behavior within the context of the game.

Elster's second question is: "Do they agree that a plausible analytic narrative requires independent evidence for intentions and beliefs?" This can be read as concerning the relationship between rational choice methods and interpretive methods that tell us how individuals construct understandings of their world and give meaning to their life. Much of everyday life and of politics is about this, and rational choice theorists have for too long ignored these concerns. Luckily, there is a critical trend among a subset of rational choice theorists who have been trying to integrate interpretive and

[3] A related discussion in sociology was begun by Stinchcombe (1968, 1978) and takes its current form in a debate sparked by Kiser and Hechter (1991, 1998; also see the responses by Quadagno and Knapp 1992; Skocpol 1994; and Somers 1998).

rationalist accounts. An article by John Ferejohn (1991) exemplifies the effort, as does a recent book by David Laitin (1998). Some of our own work (Bates, de Figueiredo, and Weingast 1998; Levi 1997; Rakove, Rutten, and Weingast 1999) is self-consciously part of this enterprise.

To varying degrees the chapters in *Analytic Narratives* contribute to this program. Each of us has labored for years on the subject on which we write. Where necessary, we learned new languages; where feasible, we worked in the field. Each of us spent long periods in the archives, both public and private, and immersed ourselves in the secondary literature on our subject. Indeed, immersion of this sort constitutes a core part of the method we advocate. We seek not only to bring theory to bear upon data but also to bring data to bear upon theory. We strongly endorse the use of qualitative materials, fieldwork, and the painstaking reconstruction of events as anticipated, observed, and interpreted by political actors. Such intimacy with detail, we argue, *must* inform the selection and specification of the model to be tested and *should* give us a grasp of the intentions and beliefs of the actors.

There is also a narrower issue that Elster raises. He argues that without independent knowledge of the intentions and beliefs of the actors, the assumption of rationality adds little, resulting in explanations that are tautological. We disagree with Elster's equation of the words of actors with their intentions.[4] For example, there are two ways to read James Madison. The typical modern political theory approach takes the text and studies its ideas apart from the historical context, assessing Madison's discussion of an ideal world. In contrast, a historical approach embeds Madison in his context and suggests that he was constructing a political document designed to persuade a certain group of citizens to support the proposed revisions to the U.S. Constitution. It is clear from Madison's own writing that he did not believe in this as the ideal.

The point is that an assessment of Madison's intentions is an enormously difficult and necessarily speculative task. Because it is so difficult to judge intentions, rational choice theorists tend to rely instead on revealed preferences and behavior. Indeed, even in instances in which Elster claims we considered intentions directly, we did not do so. When Levi discusses legislative debates, she understands the legislators' public arguments as behavior; their rhetoric is often calculated and strategic, meant to attract certain constituents or change the votes of other legislators.

Thus, to the specific question of the requirement of independent evidence for intentions and belief, we give a negative answer.

Elster's third question is: "Do they agree that a plausible analytic narrative must be at the level of individual actors or, failing that, that specific reasons must be provided in a given case to explain why aggregates can be treated as if they were individuals?" Highlighting the disjuncture between individual and collective rationality, Elster takes us to task for relying on the use of aggregates: "the elite," "the North," and so forth. "Rational choice explanations divorced from methodological individualism," he maintains, "have dubious value" (Elster 2000, 693).

Whether aggregation is justified depends on the extent to which the problem of decision making within the aggregated unit can be examined separately from the interactions among such units. The question is empirical, not theoretical. Aggregation is widely used in economics and political science because it is often an appropriate assumption that leads to tractable analysis highlighting processes at the aggregate level. Firms and families, rather than rational individuals, long stood on the supply and demand side of the economist's market, and political parties—along with rational candidates—contest Downsian elections. In each case, the aggregation assumption makes the analysis tractable.

In the case of the Genoese clans, for example, there is evidence (apart from the implications of the assumption that such separation is appropriate) that directly supports decision making by the clan. The constraint of "one person, one vote" will lead us away from understanding the politics of Genoa as a republic. The same holds true with respect to other chapters. Should Bates go to the level of the individual American and his/her interest in the Cold War?

At least two of us went farther to explore the institutional mechanisms that produce (in the case of Bates) or that fail to produce (in the case of Weingast) a well-defined preference ordering. Contrary to Elster's claim, then, we do not ignore the paradox of collective irrationality but, rather, employ it as a wedge with which to open the analysis of the influence of institutions.

Collectively, however, we urge caution about taking collectivities as actors, capable of formulating and pursuing coherent and sophisticated strategies. Each of us attempted to move to the highest level of disaggregation that was appropriate; thus, Weingast's unpacking of the Senate and Greif's treatment of free riding in the politics of Genoa.

Elster's last three questions are: "Do they agree that standard rational choice theory needs to be modified to take account of the findings of bounded rationality theory and of behavioral economics? Do they agree that it also needs to be modified to take into account nonrational motivations? Do they agree that at present there is no way to model nonrational motivations and how they interact with rational motivations, at least not in a way that yields determinate deductions?

Long a student of rationality, Elster advances important arguments regarding our use of rational choice theory. As do others, Elster recognizes that people cannot possibly perform the calculations necessary for backward induction. Instead, he urges us to assume bounded rather than full rationality. He also criticizes the assumption that actors can pursue objectives in an instrumentally rational manner, arguing that emotions and other nonrational motivations play a stronger role

[4] Despite Elster's attempt to say otherwise, how else can one read his statement that Bates through interviews and Levi through interpretation of parliamentary debates "provide direct evidence about mental states" (Elster 2000, 693)?

than we accord them. Finally, he suggests that recognizing the role of nonrational motivations questions the very possibility of modeling in the way we do.

As a team, we are divided over the first of Elster's arguments; some of us agree and find it increasingly difficult to defend the assumption of full rationality as a "necessary convenience." But even those of us discomfited by this assumption reject Elster's implication. Elster implies that inferences drawn from the use of models that incorporate such assumptions must be flawed, whereas we find some of these models highly insightful. They help us understand just how and why the balance rule worked to preserve political order in the United States before the Civil War; just how and why the rules of the International Coffee Organization influenced the allocation of the "coffee dollar" in international markets; and just how and why an official (the podestà) with just twenty soldiers brought peace and prosperity to the most powerful city-state in the twelfth century Mediterranean world. The burden falls on Elster, then, to show how the assumptions we made led us to err. Just how were we misled by them? How would the outcomes have differed had we assumed boundedly rational actors?

We have no difficultly conceding that emotional life is powerful and affects behavior. We also have no difficulty conceding that a rational choice account grounded only in material interests may be both unrealistic and analytically limited. For Elster's critique to carry more force, however, it is necessary that he show how the inclusion of nonrational motivations would improve the power of our particular analyses. We employed models to explain, not to describe; our models therefore need not capture *every* feature of human life. The ratio between the variance they confront and the variance they explain provides a measure of their success. Until Elster shows how explicit reference to emotional life and other nonrational motivations would enhance that ratio in the accounts we offer,[5] his criticism points to an omission but not to an error arising from that omission. He runs the risk of weakening the power of his explanation. At the least, his argument fails to point out errors of commission, as he suggests.

Elster has been a preeminent advocate of rational choice theory and then of considering nonrational motivations. In social science today, the attempt to develop better models of choice and action represents an extraordinarily important and exciting program, one to which we all subscribe to varying degrees. *Analytic Narratives* is, after all, dedicated to Douglass North, whose recent work focuses on developing better cognitive models (1991; also see Denzau and North 1994), and, among us, Levi in particular is identified with the project of understanding the limits of rationality and rational choice (Alt, Levi, and Ostrom 1999; Cook and Levi 1990; Braithwaite and Levi 1998).

Yet, Elster's program fails what Kenneth Shepsle once called "the first law of wing-walking," which holds that you do not let go of something until you have a grip on something else. We do not throw out models based on rationality just because we agree that there are nonrational aspects of choice. We are likely to learn quite a bit in the next ten years about how to extend choice-theoretic methods to include emotional and nonrational elements. The approach will be much stronger for this. But we do not believe that this now requires abandoning our current tools. Nonrational approaches to choice are not yet far enough along to provide an analytic approach that challenges or extends the traditional choice framework, at least as applied to empirical research outside the laboratory (see, however, Jones 1999). There are, nonetheless, scholars who are beginning to integrate expressive and rational bases of action. For example, Elster's own work on emotions and social norms (1991a, 1999a, 1999b) explores a range of useful phenomena and mechanisms that will prove useful. Sunita Parikh (in process) explores riots as events whose explanation requires understanding a mix of motivations and behavior. Thus, in contrast to Elster's implied thesis (in question six) that "there is no way of modeling nonrational motivations and how they interact with rational motivations, at least not in a way that yields determinate deductions," we believe that there are good first models of how noninterest-based and even nonrational motivations might interact with rational motivations (e.g., Bates, de Figueiredo, and Weingast 1998; Ensminger and Knight 1997; Levi 1997; Lupia and McCubbins 1998). As these new choice-theoretic approaches become more defined and complete, we believe they can be easily integrated into the analytic narrative framework.

UNCERTAINTY

Having attempted to respond to Elster's questions, we now turn to another major criticism Elster makes in the text, our purported failure to deal adequately with uncertainty. He invites us to impale ourselves upon one of two horns, both chosen by him. On the one hand, he criticizes us for failing to employ models that take into account the effect of uncertainty; on the other, he brands such models as "artificial."

Elster lumps together the separate issues of uncertainty and incomplete information. We use the concept of uncertainty in our chapters more than he recognizes. Consider, for example, Greif's analysis, which takes into account uncertainty about who will win the war, whether the emperor will come, whether the podestà and one clan will ally, and so on.

We are more cautious in applying models of incomplete information, however. Because of their complexity, explicit reliance on Bayesian updating (which we doubt people use in reality), and artificiality, we felt that we should apply them only when failing to treat uncertainty would imply ignoring a central feature of the puzzle under investigation. In most instances, we found we could avoid the use of such models. We, like Elster, believe that political actors occupy a terrain clouded by uncertainty, but we also believe it is char-

[5] Elster attempts to do this in his 1999 books, but he does not demonstrate there or in his review how his claims produce different analyses than the ones we offer.

acterized by sharp institutional features and powerful political forces. Events that take place in such settings *do* possess a high degree of contingency. When we give insufficient weight to the role of chance, we deprive ourselves of the opportunity to assess the level of confidence with which we can advocate our explanations. If we succeed in apprehending the major forces at play, then the systematic component of our explanations should prevail over that which is random.

"JUST-SO" STORIES

Elster suggests we are guilty of the very sin we try assiduously to avoid, namely, writing "just-so stories." We admit that it is difficult to elude this problem. In the analysis of a single case in biology—or social science—a just-so story can establish the plausibility of the theoretical perspective, but we resisted the temptation to stop at that point. Instead, we laid out the basis for our analytic decisions, indicated the deviations from the predictions of our initial models, and applied the criterion of falsifiability to our hypotheses.

"We identify the actors, the decision points they faced, the choices they made, the paths taken and shunned, and the manner in which their choices generated events and outcomes" (Bates et al. 1998, 13–4), and we do this in each of the single-authored chapters. We attempt to make clear the preferences and to model the outcome of choices. By this means, we aim to offer both a recognizable historical representation and an explanation of significant institutional arrangements and changes. The analytic narrative approach gains credibility when the equilibria of our models "imply the outcome we describe" (p. 12).

Moreover, "when history contradicts the model, so much the worse for the model" (Goldstone 1998, 533). For example, when a model based only on self-interest fails to account for the change in citizen reactions to substitution and commutation, Levi employs an alternative that permits her to explore the implications of the more narrow with the broader rational choice model. In the Bates chapter, the failure of the chain store paradox model highlighted the significance of high fixed costs in the production function of coffee. The Stigler/Pelzman model also foundered, but its internal inconsistency drove Bates to recognize the significance of security interests and the political role of large corporations.

Contrary to what Elster argues, we do not employ formal theory to construct just-so stories. The theories are tested against the stories, and they can and do fail. From their failure, we then learn about the case. We use deductive theories for inductive purposes. As our introduction and conclusion describe, each of us goes back and forth between the model and the data, testing our ideas against reality.

Elster (1989b, 3–10; 1999a, 1–47) himself makes "a plea for mechanisms," for the necessity to explanation of identifying the causal mechanisms. Elster argues that dozens of potential mechanisms potentially underpin human behavior, which he takes to mean that there is no way to predict ex ante which mechanism will come into play or, if many are at work, which will predominate. If this correctly characterizes Elster's position, then only after the fact can one suggest which mechanisms work. As a consequence, his method is far more susceptible to "curve fitting" than is our own.

We believe that rational choice offers a superior approach because it generates propositions that are refutable. Being subject to standard methods of evaluation—such as the out-of-sample testing of predictions and the systematic pursuit of falsification—the models we employ are not mere just-so stories.

THE CASE STUDIES

Elster finds much to which he objects in our book. By his own admission, however, much of his specific critique of the chapters focuses on secondary issues. What underlies this kind of criticism, however, is his belief that "to be analytic is above all to be obsessed with clarity and explicitness, to put oneself in the place of the reader and avoid ambiguity, vagueness, and hidden assumptions" (Elster 2000, 691). He argues that "not many, if any, of the chapters in AN live up to his demand," (p. 691). We beg to differ. We feel confident that most readers will find that each author produced an analytic narrative which both advances knowledge and produces generalizable and falsifiable implications.[6] And that formal reasoning was key to these contributions.

Rather than go through each chapter and debate each of Elster's specific second-order charges, we addressed many of his more general and theoretical critiques in our answers to his six questions. There remain a few points to which we need to respond.

Greif's Chapter

Elster argues that the analysis does not support the assertion that external threat or internal innovation released resources for use in cooperative endeavors. He is absolutely correct; the analysis does not support his assertion. But it never attempted to do so. The focus is on a different issue, namely, the *motivation* to cooperate, not the *ability* to do so.

Rosenthal's Chapter

At no point does Elster contest Rosenthal's claim that the conclusions follow from his model or the fit between the implications of the model and the historical records. Changing the division of spoils, the "war bias" of the crown, or the incentives of the elite to free ride would indeed change the predictions that issue from Rosenthal's model and lead to convergence between the policies of England and France, just as Elster claims. But then the modified model would be contradicted by fact and thus proven wrong!

[6] We all draw on related research available in published and forthcoming material. See, for example, Bates 1997, Levi 1997, Greif n.d., Hoffman and Rosenthal 1997, and Weingast n.d.

Levi's Chapter

Elster's condemnation of Levi's logic rests more on misunderstanding and honest disagreement than on pinpointing logical flaws in her argumentation. Elster maintains that "abolition of replacement and introduction of universal suffrage were part of the same ground swell—not that the latter was the cause of the former" (Elster 2000, 688); this is Levi's explanation, too. He critiques her for holding a different view than his about the relationship between fairness and rationality and a distinct interpretation of the actors' preferences. Elster correctly chastises her for inconsistent wording; price is indeed an object, even in the considerations of the wealthy. But he does not seem to disagree on what is easier or less costly.

Weingast's Chapter

Elster questions Weingast's principal argument that Americans constructed antebellum political stability in part on the balance rule. His main criticism is that pairing occurred because southerners blackmailed northerners by holding the admission of northern states hostage to the admission of southern ones. This assertion supports rather than contradicts Weingast's thesis. The South held up admission of free states precisely to maintain a balance, and it used its sectional veto to do this. Elster's comments therefore add to Weingast's argument by showing how the balance rule was self-enforcing during this period.

Bates's Chapter

Elster criticizes Bates for not showing how the consumer side of the market enforced the agreement. Elster appears to be looking for a unilateral form of intervention; indeed, he cites one attempt (by the U.S. government) and conjectures regarding the possibility of another (by U.S. corporations, acting in Central America). But the very form of the explanations advanced by Bates rest on game-theoretic reasoning; rather than seek forms of unilateral imposition, he seeks behaviors that would prevail in equilibrium.

In short, our responses underscore a larger problem with the review: Elster's repeated failure to engage with the main points made by the essays or his misreading of them.

CHARACTERISTICS OF ARGUMENTS

Elster's criticism of our work is, and was intended to be, powerful. It is a "scorched earth" review, and we have tried to resist the temptation to respond in kind. However, we have, on occasion, felt compelled to answer some of his criticisms with a tone that matches his, especially since much of the power of Elster's critique is due to the substitution of *his* premises for our own and then showing that our results do not follow. He advances his premises so diffidently that it may not appear reasonable to object, but there is little diffidence in the manner in which he claims to clinch the subsequent argument, even though the argument that fails is his, not our own.

Elster often substitutes his opinions for our judgments. Admitting that he is not an expert on French politics of the period, he nonetheless feels free to reconstruct the preferences of social groupings, as depicted by Levi. If he cannot see that it would be reasonable to suppose that the groups prefer one form of recruitment to another, then perhaps it is because, unlike Levi, he has not immersed himself in the necessary evidence. Although he admits he is "not a specialist" (Elster 2000, 690) on nineteenth-century American politics, Elster nonetheless feels free to "doubt the existence" (p. 689) of a convention regarding the admission of states to the Union. He is entitled to challenge our logic, and we cannot simply resort to our detailed knowledge of the cases as our defense. But given that we attempted to combine reasonable claims with in-depth research, it is incumbent upon Elster to demonstrate how his alternative and abstractly derived construction matters empirically. At issue is more than a question of taste. At issue is a better or worse explanation of actual events.

Elster repeatedly seeks to ensnare his prey in double binds. On the one hand, for example, he calls for the clarity that only, he claims, a formal model can provide; on the other hand, he then attacks a model—"to the extent I understand it" (Elster 2000, 689)—for failing to add "to the verbal presentation" (p. 689). On the one hand, he criticizes a chapter for deviating from "standard rational choice theory" (p. 695); on the other hand, he condemns the authors for depicting actors as being fully rational. He endorses collective action theory but less than a page later brands collective action theory for being "rococo (or is it baroque?)" (p. 695). The authors are thus damned if they do and damned if they do not.

Elster cites distinguished figures, long dead, to add authority to his arguments, but he often does an injustice to their actual words and meaning. For example, Elster enlists Pascal, applying to our enterprise the phrases applied by Pascal to the mechanistic biology of Descartes: "ridiculous—pointless, uncertain, and arduous." When he wrote these words, Pascal had abandoned philosophy for faith. For our part, we are engaged in secular pursuits, and we take inspiration from history's vindication of Descartes; "mechanistic biology" has proven fertile, yielding major advances in medicine and medical engineering.

Rhetoric helps Elster forcefully communicate his low opinion of our work. The tone is Olympian and harsh, and it diverts attention from his failure to engage with the substance of our cases and our method.

CONCLUSION

Elster criticizes us at three levels. First, he claims that we fail to execute the program we propose; that is, we fail to apply skillfully or persuasively formal theories to elucidate complex cases. We believe him wrong and encourage readers to explore the case studies and decide for themselves.

Second, Elster argues that our framework is itself flawed or, at best, premature; he chides us for "excessive ambition" in attempting "deductive rational choice modeling of large-scale historical phenomena." His real opponent is rational choice theory. But as a critic of the analytic narrative approach, Elster fails to engage with the main purposes of our book: How can we build and test systematic explanations based on case studies? How can we move from the world of a problem to be studied to a test to be administered? Moreover, the analytic narrative method easily affords substituting more general modes of "choice" for the explicitly—and limited—rational choice game theory. Put simply, we believe that the debate over choice versus rational choice is orthogonal to the issues we raise.

The burden is on Elster to explicate a better methodological alternative that can actually inform empirical research. This means demonstrating how the weaknesses of rational choice adversely affect the use we make of it. Unless Elster can show how the assumptions of rationality lead us to conclusions that are wrong, he is merely restating obvious truths rather than uncovering material errors. Unless he can offer us a better set of tools, we shall proceed with those we have.

Finally, Elster criticizes *Analytic Narratives* because we espouse an ambition to a genuine social science. We believe that generalizations are possible and that many have emerged in the social sciences. Our project represents an attempt to bring some analytical tools to the task of studying unique case studies, a question long of interest to political scientists.

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