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Review: Rational Choice History: A Case of Excessive Ambition

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## Analytic Narratives by Bates, Greif, Levi, Rosenthal, and Weingast: A Review and Response

**Analytic Narratives.** By Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry Weingast. Princeton, NJ: Princeton University Press, 1998. 296p. \$65.00 cloth, \$22.95 paper.

The rational choice revolution in political science began in American politics in the 1970s, first influenced international relations in the 1980s, and made its way to comparative politics during the 1990s. As it moved beyond its base in American politics, rational choice theory confronted comparative and historical questions of regime transition, social conflict, democratic stability, economic development, and international governance. The application of rational choice theory to these macro concerns creates analytic problems that were less relevant when the

approach was applied primarily to microquestions. Robert Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry Weingast have written a much discussed book, *Analytic Narratives*, that develops these novel applications. Jon Elster, a seminal rational choice theorist who also has written extensively on comparative and historical problems, believes that *Analytic Narratives* expresses hopes for rational choice theories of comparative and historical processes that exceed what the approach is capable of producing. Bates and his colleagues reject Elster's critique and defend their goal of a genuine social science capable of addressing large-scale phenomena. Is rational choice history a case of excessive ambition, or is it the logical next step in the rational choice revolution in political science?

### Rational Choice History: A Case of Excessive Ambition

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In *Pathologies of Rational Choice*, Donald Green and Ian Shapiro (1994) claim that rational choice theory has poor empirical support. *Analytic Narratives* (AN) is among other things an attempt to rebut this claim (p. 231).<sup>1</sup> In my opinion, Bates et al. do not succeed in their attempts to explain complex historical phenomena in terms of rational choice. Although my comments may seem harsh, certain statements in the Introduction and the Conclusion invite the application of strict criteria. The authors claim that "the chapters in this volume . . . engage the concerns of many disciplines and should therefore command a broad audience" (p. 3). Similarly, "because the chapters engage such fundamental issues, they are of general significance" (p. 230); moreover, "they furnish deep insights in particular cases" (p. 232).

I proceed in three steps. First, I discuss each chapter to argue that the authors make serious errors of commission that undermine the credibility of the analyses. Second, I raise some general questions concerning the application of rational choice models to large-scale historical phenomena. I argue that the

contributors to AN make important errors of omission, such as failure to provide microfoundations or to offer evidence about the beliefs and intentions of the actors. Third, I try to draw some conclusions about the feasibility of the AN project.

#### INDIVIDUAL CHAPTERS

##### Greif's Chapter

The study by Avner Greif of the political economy of late-medieval Genoa offers a periodization of Genoese politics between 1099 and (roughly) 1350. From 1099 to 1154, an economically suboptimal but peaceful equilibrium obtained. Each clan invested in military strength to deter the other from becoming a controlling clan, with the result that fewer resources were available for economic cooperation abroad. (Following Greif, I assume throughout that there were two clans only.) They might still cooperate in economic ventures, but at a less than optimal level. Between 1154 and 1164, an external threat did away with the need for investing in military strength, which made more resources available for economic cooperation. From 1164 to 1194, there was semipermanent interclan warfare. After 1194, the presence of an external administrator of the city, the podestà, brought about peace and "spectacular economic performance" (p. 58). I focus on the second period (1154–64) and the final period (after 1194). As the chapter is opaquely written and marred by severe misprints (e.g., in the statement of Condition III on p. 51), I had to work hard to figure out what is being said. If I got it wrong, Greif must share the blame.

Greif's argument about the optimal nature of the

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<sup>1</sup> Unless otherwise stated, all page references are to this volume. In assessing the chapters by Bates and Levi, I also draw on some of their other writings. Although Greif refers to a forthcoming book-length version of his chapter in AN and Weingast to an unpublished book-length manuscript, I did not have access to these. Some of the issues I raise in the text may be addressed by these more extensive discussions.

second period is based on the premise that the external power will attack if and only if there is interclan warfare. Consider first the “if” part of the premise. Because the clans know that if they fight each other the future will look bleak, they need not invest in military strength to deter each other. Without an external threat, they may have to invest to raise the cost to the other of attacking. With an external threat, the benefits of attack are so small that no deterrence is needed. Consider next the “only if” part of the premise. If there is no need to fear an external attack as long as they abstain from fighting each other, the clans can devote all their resources to economic cooperation. This seems implausible. A strong external power might attack, with some positive probability, even if the clans manage to keep peace. Greif acknowledges as much on page 32. To deter attack and/or to increase their chances of winning a war against the external enemy, the clans would presumably invest some resources in military strength. Greif acknowledges as much: “Rival clans within various Italian city-states often cooperated in confronting external threats” (p. 42). This military cooperation must have come at the expense of economic cooperation. Greif seems to ignore, as far as I can see, that both investment to deter or fight an external enemy and investment to deter a rival clan must come at the expenses of resources available for economic cooperation. Although the presence of the external enemy does away with the need to invest for the latter purpose, it creates a need to invest for the former. In the absence of more information, the net effect of an external enemy on military investment is indeterminate.

I also was not persuaded by Greif’s claim that the podestà restored peace and efficiency in the final period. Consider first the argument that the presence of a third party—the external podestà—prevented the endemic interclan warfare that characterized the third period. Greif captures the strategic interaction between the two clans and the podestà by means of two games. In the analysis of the “collusion” game between one clan and the podestà against the other clan, he asserts that “after collusion has occurred, the amount by which the clan would reward a *podestà* would depend on the *podestà*’s ability to confront that clan militarily” (p. 49), because if he could not gain by confronting it, then the clan could not credibly promise to reward him.

The collusion game is embedded in a larger “podestà game,” in which the first clan has the choice between challenging and not challenging the second; the second, if challenged, has a choice between fighting and not fighting; and the podestà, if the second clan is challenged, has three choices—collude with the first clan, prevent the first clan from taking control (this means the podestà tries to take control for himself), and do nothing. For specific parameter values this game has a subgame-perfect equilibrium in which clan 1 does not challenge; clan 2 fights if challenged; and the podestà prevents if clan 1 challenges and clan 2 fights, and, if clan 2 does not fight, either colludes or does nothing. The podestà has to be strong enough to be

pivotal with a sufficiently high probability but not so strong that the first clan could credibly promise to reward him in a collusion against the second. (I simplify.)

One empirical question is whether the strength of the podestà was within the relevant interval. Greif tells us he had twenty soldiers (p. 53). This is a small number, but if the clans were of nearly equal strength, he might still be pivotal. Greif notes this fact, but fails to provide evidence about the relative strength of the clans. A more serious problem is that Greif does not explain how the podestà generated not only peace but also prosperity. In the second period, prosperity supposedly occurred because the clans could afford to disarm—they had nothing to gain by fighting each other. No mechanism is provided, however, for disarmament in the fourth period. The military strength of the clans is taken as given throughout the analysis, and Greif offers no argument that would exclude higher levels of military investment—and fewer resources available for cooperation—than in the first period. (We might also ask whether the high wage paid to the podestà cut heavily into these resources, but this is probably a minor factor.)

### Rosenthal’s Chapter

The second chapter is a study by Jean-Laurent Rosenthal of the political economy of European absolutism. His explanandum is nothing less than the divergent courses of French and English absolutism in the seventeenth and eighteenth centuries. To focus my comments, I limit myself to one part of that explanandum: “why per capita taxation was significantly lower in France or Spain than in England or the Low Countries” (p. 73). (Actually, I follow Rosenthal in limiting myself to the comparison between France and England.) The explanation relies on a model that links rates of taxation with gains or losses from wars financed by taxation. In brief, Rosenthal argues that tax rates were higher in countries with a unified tax system—in which either “the elite” or the king had full control over taxation—than in countries where fiscal authority was divided between them. The reason is that taxes are raised mainly for the purpose of conducting wars, the spoils from which are shared between the elite and the crown. In countries where fiscal authority is divided, neither actor can fully internalize the benefits from the taxes it raises; hence, we would expect tax rates to be lower than in a unified country.

Rosenthal assumes that “fiscal resources were used primarily to prepare for and prosecute war” (p. 69). Although he does not spell out what “primarily” means, an estimate is that “war expenditures often represented 80% of public expenditures” in early modern Europe (Bérenger 1996, 623). Whether one is justified in using 100% as a proxy for 80%, as Rosenthal does (p. 102), is not obvious to me. Rosenthal then proceeds to specify the objectives and constraints of the two actors. Both want to maximize the return from war. Both are constrained by the cost of raising resources for warfare. In a numerical exam-

ple, the cost of raising taxes is equivalent to 5% of the national product and equals the amount of taxes raised. The outcome of war affects the two actors differently, for two reasons. First, they “share the returns from winning and losing according to the extent of their fiscal control of the domestic economy” (p. 103). Second, if the war ends in a draw, which in one specification of the model happens in almost half the cases (p. 72), the elite bears all the costs of conducting war. The first mechanism is responsible for the free-rider problem mentioned above, which induces both parties to suboptimal funding of wars. The second implies that the elite might have reason to be even more reluctant than the king.

The idea of spoils divided in proportion to fiscal control is unsubstantiated, empirically and theoretically. Rosenthal does not cite any evidence that spoils were in fact divided in this way. He also does not consider alternative hypotheses, for example, that the spoils were divided in proportion to taxes raised for warfare. In one specification of the model (p. 72) the elite raises no taxes although it controls 10% of taxation and hence receives 10% of the spoils. Why would the king give away 10% of the fruits of victory to an elite that in no way contributed to it? In fact, why would the king ever give away anything? As the executive in charge of conducting the war, he was presumably in a position to retain all gains for himself. To say merely that, “after all, the king cannot hog the profits from war if he expects support from the elite” (p. 71) is to confuse *ex ante* and *ex post* decisions and to ignore the question of credible commitments. In any case, the argument fails if the elite does not raise *any* taxes for war.

The idea that the elite bears all the costs from a draw is partly defended as a substantive proposition: “The elite is . . . assumed to bear a cost from draws because it is responsible for staffing the army and the administration, both of which are made to work harder during hostilities” (p. 70). But that invites the question whether these costs would not also be incurred in case of victory or defeat. Partly (and inconsistently), however, the assumption is defended as a “convenient” (p. 70, n. 7) device to create “the key policy tension in the model: there are some wars that the king wishes to fight but that the elite does not.” Rosenthal notes that there are “other ways of creating this difference,” which do not, however “alter the substantive results of the analysis” (p. 70). But would it not be more appropriate to identify the reasons the king was more belligerent than the elite and model them directly?

The mathematical models in which these ideas are spelled out are described at three levels of abstraction. First, Rosenthal asserts that if one tries to work out the implications of the causal links described above, “it is possible to solve for equilibria in a general setting, but obtaining comparative static results requires additional assumptions” (p. 71, n. 9). Second, therefore, the mathematical appendix uses specific functional forms to model the relations between taxes and the cost of raising them, and between taxes raised and the probability of the war ending in victory, defeat, or draw.

Third, in the text Rosenthal draws on numerical illustrations of this model with specific parameter values, asserting that the “results stand up to specification changes” (p. 71). He makes no similar assertion, however, with respect to the choice of functional forms. Why should the reader have any confidence in the results?

Let us look more closely at one central result, concerning the partial explanandum described above. “In the model when either party controls the fiscal system, the tax rate is about 6.3 percent, whereas in the case in which the regime is evenly divided the tax rate falls below 5 percent, a drop in revenue of nearly one-fifth. This finding goes a long way toward explaining why per capita taxation was significantly lower in France . . . than in England” (p. 73). In a footnote Rosenthal adds: “The observed differences in rates between Great Britain and France are much larger than those found here, a finding that may well be driven by the fact that the elite was not a unified tax authority in France.” I find this statement both uncomfortably *ad hoc* and hard to square with a later statement: “Given that the current model predicts the variation of tax rates properly, it may well be unnecessary to be concerned with elite politics” (p. 89).

In my opinion, the model does not go any way at all toward explaining the observed differences. The fit is poor (as Rosenthal admits), and the model is very artificial (as I have argued). In addition to the objections mentioned above, I cannot see any reason the same parameter values should obtain for all countries. One might expect, for instance, that the resources needed to raise a given amount of revenue (and for a given BNP) would be smaller in England than in the somewhat more sparsely populated France. Also, can one assume that all armies are equally good at converting money into probability of winning? This objection obtains both for purposes of comparative statics and for the purpose of determining the level of mobilization in the enemy.

### Levi's Chapter

The third chapter, by Margaret Levi, is a study of nineteenth-century conscription in France, the United States, and, more briefly discussed, Prussia. The explanandum is “the disappearance of various ways of buying one's way out of military service if conscripted: commutation (a fee paid to government) and substitution and replacement (payment to someone else to take one's place)” (p. 109). The explanation relies on “the micromotives of four sets of key actors: government policy-makers, the army, legislators and constituents” (p. 111). As Levi notes later, a fifth actor plays a considerable role in explaining the disappearance of buying-out and its replacement by universal conscription, namely, the largely disenfranchised poor, who “rioted against commutation, making the practice sufficiently costly that government acceded to their demands” (p. 135). Although the explanation is presented as “derived from rational choice and relying on the logic of game theory” (p. 144), the chapter does not



draw on game theory at all and deviates from standard rational choice theory in one important respect.

Let me first address some empirical shortcomings of the section on France. I was surprised to read that after Thiers gave a speech in the 1848 constituent assembly advocating the maintenance of replacement, “the vote of the *Assemblée Nationale* was 663 against and 140 in favor of his position” (p. 129). In fact, the vote was 663 against and 140 in favor of a proposal by Deville to ban replacement. The book-length account by Levi (1997, 95) has the correct version, so the version in AN must be due to an accident of compression. Other mistakes or ambiguities, however, are common to the book and to the chapter in AN. They concern the timing of the introduction of universal (male) suffrage, the timing of the abolition of buying-out, and the causal link between the two.

Levi inconsistently dates the introduction of universal suffrage in France to 1875 (p. 111) and to 1884 (p. 130), but the dates of 1848 or 1871 would be equally or more plausible. In the wake of the February revolution, the constituent assembly of 1848 was elected by universal suffrage. The constitution that it adopted also included a provision of universal suffrage. It was reintroduced in 1871, after defeat in the war against Prussia and the fall of Louis Napoleon. As for the changes in military service, Levi writes that “the military defeats of 1870 and the Commune of Paris were to sound the death of replacement” (p. 129). Hence, on her chronology, there could be no causal link between universal suffrage and universal conscription: “Since full male democracy was not achieved until 1884, direct political pressure by those who had come to believe themselves most harmed by the inequities of the system cannot explain the change in governmental norms about military service” (p. 130). Note, however, that she asserts the opposite in an earlier and more theoretical section: “The preferences of the government policymakers and the low-wealth population are the same, thus helping to account for the policies that emerge when the franchise is significantly extended” (p. 118). A similar passage (p. 115) is quoted below. As these statements are not explicitly made about France, I postpone discussing them until later.

Chronology and the facts suggest a close connection between the two reforms. With regard to 1871, the chronology is consistent with the idea 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, but that the two sprang from the same popular sentiment. I know too little about this episode, though, to affirm that there was a link of this kind. I feel more confident in asserting a strong link during the first part of the 1848 revolution. Levi’s treatment of this event is confusing. Although she correctly asserts (in her 1997 book) that the constituent assembly voted against a ban on replacement, she also says in AN that “the members of the *Assemblée Constituante* did overwhelmingly declare support of the abolition of replacement” (p. 128). The two statements can be reconciled by noting the drastic change of mind of the assembly after the June insur-

rection of the Paris workers. In the first draft by the constitutional commission, presented to the assembly on June 19, 1848 (four days before the insurrection), Art. 107 declared starkly: “Le remplacement est interdit.” After the insurrection, the assembly canceled this measure and a number of other radical proposals, notably the right to work and progressive taxation. In this case, it seems clear that universal suffrage and universal subscription were part of the same ground swell, broken only when the bourgeoisie came to see the workers as a threat rather than an ally.

My main complaints with the chapter, however, are logical rather than empirical. I do not agree with Levi’s construction of the preferences of the various social groups concerning military service. First, “the traditional elites and wealthy . . . would prefer a professional army and, second, a system in which they could buy their way out through commutation since, in principle, price is no object. Their third choice would be a system of substitutes or replacements, which would require them to incur some search costs as well as pay a fee” (p. 114). This seems illogical: Search costs and fee add up to a price, which may or may not be higher than in the second case. In the second case the fee is set by the government, in the third by the market, and there is no a priori reason market fee plus search costs should exceed the government-set fee. Also, if “price is no object,” why mention search costs at all? Levi also claims that “the preference ordering of unemployed and low-skilled workers and landless peasants should be, first, a volunteer army, then substitution, then universal conscription, and, last commutation” (p. 114). I do not see why this group would prefer universal conscription over commutation. If commutation coexists with lottery, as it did throughout this period, then they would prefer this system (which gave them a chance of not being selected) to universal conscription.

Levi writes in summing up:

If [substitution and replacement tend to dominate universal conscription for everyone], why does so much of the population begin to object to directly purchased exemptions? It may be that, in fact, commutation, substitution, and replacement are not Pareto optimal. As government requires more men to serve, substitution and commutation may increase the probability of having to serve for those who can afford neither of those options. The more people who buy themselves out of the pool of eligibles, the deeper into the pool the army reaches. Thus it may not, after all, be an efficient system (pp. 114–5).

But this argument applies only to commutation. Also, even commutation *is* Pareto optimal, although, unlike replacement, it is not a Pareto improvement compared to lottery-based conscription.

These preferences are constructed on the assumption that people assessed the alternatives “on purely economic grounds” (p. 114). Levi argues, however, that fairness considerations also entered into the formation of preferences. The existing arrangement

may also begin to be perceived as an inequitable system. Those who cannot afford to purchase substitutes face the possibility of having to become soldiers when they would

rather not; they compare themselves unfavorably both with those who have purchased exemptions and with those who become substitutes instead of draftees and thus receive additional money. . . . If such groups begin to dominate the electorate numerically or resist with riots, then policy change will result (p. 115).

According to Levi, the fairness motive may generate “indirect costs,” which salvage “the supply and demand model by expanding the notion of cost” (p. 115).

There are two senses in which considerations of fairness may be taken to generate costs, depending on whether those who harbor such feelings have the right to vote. If they do not, fairness-motivated riots by the disenfranchised may certainly generate costs for government. But if they do, should we say that the feeling of being unfairly treated represents a cost for the low-wealth enfranchised? To answer in the affirmative—as Levi seems to do—would be uncomfortably close to the Ptolemaic devices chastised by Green and Shapiro (1994), that is, people derive psychic benefits from doing their duty. If the feeling of unfairness is conceptualized as an emotion of resentment or envy, then it is inappropriate to view it as a mere cost (Elster 1998b, 1999). If it is conceptualized as a belief that a certain arrangement is wrong, then it need not go together with any subjective feelings of pain at all. Note that in the former case we would expect the resentment of poor conscripts to be directed toward poor replacements rather than toward the rich conscripts whom they replace, on the principle that envy presupposes the belief that “it could have been me” (Elster 1999, chap. 3, section 3).

### Weingast's Chapter

The fourth chapter, by Barry Weingast, is an attempt to explain political stability in antebellum America. The central argument can be stated quite briefly. From the Missouri Compromise in 1820 to shortly before the Civil War, American political life was stabilized by a convention that new states would be admitted to the Union in pairs, a slave state and a free state at the same time, thus ensuring (given initial parity) that neither the South nor the North would be able to dominate.

If there were few sustained attacks on slavery—despite its constant presence below the surface as a source of discord—it was because institutions restrained those with the motives to launch such attacks. In light of the potential risk of changes in slavery rights, the South sought to institutionalize some form of *credible commitment* for the North not to change rights in slaves. The specific form of credible commitment was the convention of equal representation or regional balance in the Senate, giving both regions veto power and, in particular, the South a veto over any policy affecting slavery (pp. 165–6, emphasis in original).

The argument is spelled out in a game-theoretic model that, to the extent I understand it, does not add much to a verbal presentation. If I do not fully understand it, it is not because it is technically complex (it is simpler than other models in the book), but because it introduces a somewhat mysterious plethora of players. I may have missed something, or the exposition may be

the victim of excessive compression from an unpublished book-length manuscript. I shall focus on the simple verbal argument presented above.

What is the evidence for the existence of a convention? Weingast does not cite any relevant statements by contemporaries. Although he claims that “Jefferson clearly understood the principle and its implications” (p. 178), the only backing is a statement by a modern scholar to the effect that an equilibrium obtained. Weingast asserts that “contemporaries” under the second-party system clearly understood sectional balance” (p. 178), but no such contemporaries are cited. The claim that “John C. Calhoun . . . understood this principle” is supported only by a reference to Calhoun’s proposal for two concurrent presidents, one elected in the North and one in the South, each with the power to veto national legislation. If Weingast is unable to produce explicit statements by contemporaries to the effect that there was a convention and that they relied on it, then why should we believe there was one?

An answer might be found in a compelling pattern of joint admission of states to the Union. There is, however, no such pattern. Weingast overstates his case: “As revealed by table 4.1, for the next thirty years [after the Missouri Compromise], states entered the Union as pairs” (p. 155). The table shows that the pairs after 1820 were (1) Arkansas and Michigan in 1836 and 1837 and (2) Florida-Texas and Iowa-Wisconsin in, respectively, 1845 and 1846–48. (As a matter of fact, the pairing was more regular before the Missouri Compromise.) This is not a compelling pattern.

If we look at some individual episodes, we may also doubt the existence of a convention. Following the Missouri Compromise, for instance, John Quincy Adams wrote in his diary: “I have favored the Missouri compromise, believing it to be all that could be effected under the present Constitution, and from extreme unwillingness to put the Union at hazard” (cited after Miller 1996, 190). This sounds like a simple one-shot bargain, not the creation of a durable convention. The Arkansas-Michigan admission may also have been little more than a piece of blackmail: “To a degree, as a congressman said, the admission of each state was held hostage to the admission of the other; but really, as another congressman observed, it was more one-sided than that: the admission of the free state of Michigan, about which there was no serious problem, was held hostage to the admission of Arkansas, with its provision of perpetual slavery” (pp. 210–1).

The one-sided nature of the relation between the two parts of the country is underlined by the fact that the North held a majority in the House of Representatives throughout the period. Although Weingast asserts that “the balance rule protected Northerners against the dominance of national policymaking by the South” (p. 151), this is inconsistent with his claim of the “veracity of an important prediction of the model, namely, that antislavery measures should be able to pass the House of Representatives but not the Senate” (p. 168). The North had no need for a Senate veto of antinorthern bills from the House, because its majority in the lower chamber ensured that no such bills would

pass. This feature of the situation is more consistent with one-sided blackmail than with a quid pro quo.

Because I am not a specialist on the period, I cannot offer a persuasive alternative to Weingast's account. Given the facts that he adduces, however, I cannot see that the convention-based explanation is superior to one based on a succession of bargains. The alleged convention is left dangling. What is the mechanism by virtue of which it enabled credible commitments? In a volume that generally tries to spell out the game-theoretic foundations of credibility, it is surprising to see that Weingast does not specify the sanctions that supported the convention. The obvious answer is that if the North had tried to impose its will on the South in matters of slavery, the South would have broken out of the Union. But that standing fact could equally have sustained a succession of deals.

### Bates's Chapter

The final chapter, by Robert Bates, is an account of the operations of the International Coffee Organization (ICO) from 1962 to 1989. The main explanandum is the stability of the producer-consumer agreement against free riders on the consumer side. Why did not large American coffee roasters, such as Folgers, break the agreement by purchasing coffee from illicit sources at lower prices? An additional question raised in the chapter concerns the internal organization of the ICO. Here, the explanandum is the allocation of quotas to producer countries.

Bates begins by asking why there was any need for a cartel at all. Why did not the dominant producer, Brazil, try simply to deter other producers from entering the market? Although Selten's chain-store paradox shows that entry deterrence fails when there is full information and a finite number of successive entrants, deterrence can work when there is private information or an infinite number of periods. Bates does not address the strategic use of private information to develop a reputation for toughness to deter entrants (Fudenberg and Tirole 1991, 370). He asserts, wrongly, that reputation effects arise in infinite-horizon games (p. 204). Instead, he argues that the entry of African producers into the coffee market during World War II, whom Brazil could not exclude after the war, turned the market into a one-shot game. Once the African producers had incurred the high fixed costs of planting coffee trees, Brazil could not credibly threaten to drive them out of business by underselling them: "They would be reluctant to uproot their plantations, and instead would simply reduce their inputs of labor" (p. 205). The argument is obscurely presented and relies on an economic model that is unstated rather than explicit.

Next, Bates tells the story of U.S. involvement in ICO. The apparent paradox—why a consumer nation would enter into an agreement to raise prices—is explained by security reasons. After the rise of Castro, "the governments of Brazil and Colombia were able to link the defense of coffee prices to the defense of hemispheric security" (p. 205). Consumer concerns and

electoral preoccupations in the House of Representatives delayed U.S. adherence to the agreement but could not prevent it. Although Bates takes the delay as providing an argument against the realist school of international relations, one might equally well take the ultimate acceptance of the agreement as support for that school.

To explain the stability of the cartel, Bates focuses on the consumer side. In his book-length study of the subject, Bates (1997, 145–50) argues against the propositions that producers were kept in line by Brazilian hegemony, by U.S. enforcement (but see below!), or by redesign of the pricing system to deter free riding. Instead, he argues that stability among the consumers induced stability among the producers. The key element in the explanation is the discount on bulk purchases that Brazil and Colombia offered to large American roasters. The passage linking the discount to the stability of the agreement goes as follows:

[The] reasoning focuses attention on the relationship between those who demanded the agreement (the large coffee producers) and those who organized its supply (the coffee roasting firms). Examining their behavior in a collusive equilibrium, we find that when a large roaster, like General Foods, *helped the U.S. government to police illicit shipments of coffee from the competitive fringe*—say, Central America—it drove up the price of those coffees. The differential between the price of Colombian and other coffees then declined. Given the differentials in quality between Colombian and other mild coffees, buyers switched to Colombian coffee. As more buyers turned to Colombian coffee, Colombia's dependence on any one given buyer of its coffee declined. The strategies pursued by the parties in the collusive equilibrium thereby lowered the costs to producers of implementing threats to the roasting firms. The reduced costs to Colombia of canceling its contract with any given firm rendered Colombia's threats . . . more credible. The collusive agreement could therefore become self-enforcing (p. 215, emphasis added).

Once again, the economic model is verbal and implicit rather than explicit, which makes it hard to judge the validity of the explanation. It may be true, or it may not.

The phrase that I emphasized differs sharply from a passage in which Bates (1997, 147) denies the efficacy of U.S. enforcement:

From time to time, the United States followed a second tactic. It signaled to the "competitive fringe" that should they not adhere to the coffee agreement, they would not be eligible for benefits from the Alliance for Progress. While the United States used its aid program to offer inducements and sanctions, little evidence suggests that these measures affected the behavior of nations in the coffee market. Rather, the evidence suggests that such blandishments lacked credibility.

Like the chapter in AN, the book nevertheless asserts the role of the United States in policing illicit shipments (Bates 1997, 152).

One might try to eliminate the contradiction by a suitable interpretation of the claim that the roasters "helped" the United States police illicit shipments. Perhaps the roasters interfered directly in, say, the Central American countries where such shipments



might originate? In that case, the roasters would achieve what the United States could not do on its own by credible threats. This suggestion must be rejected, however, in light of the statement that, “from Colombia’s point of view, the discount in the price of coffee to General Foods bought compensating advantages: the help of a large roaster in securing the United States’ enforcement of the International Coffee agreement, thereby checking opportunistic behavior by the competitive fringe—and raising the average price of coffee” (p. 214). On a normal reading of this phrase, the roasters *ensured* U.S. enforcement rather than *substituted* for it. I have read and reread Bates’s chapter in AN as well as his book, but I may have missed something. Even if I have, the need to go through tortuous exegesis to detect exactly what is being asserted does not support the claim that this is an “analytic” narrative. For me, 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. As I have indicated, not many, if any, of the chapters in AN live up to this demand.

In a final section, Bates discusses the internal operations of the ICO. Even when supplemented by other writings (Bates 1997; Bates and Lien 1985; Lien and Bates 1987), the argument is opaque. Bates asserts that one can explain the 1982 adoption of quotas by the ICO in terms of the voting power, as measured by the Shapley value, of individual members. The mechanism for proposing quotas and the constraints on such proposals remain, however, mysterious. In one article (Lien and Bates 1987), there is a reference to a proposal that failed and to a proposal that succeeded, but there is no mention of who made them. By comparing equation 4 in AN (p. 225) and equation 10 in Lien and Bates (1987), we can infer that the successful proposal must have been made by Colombia. (I cannot understand, however, how equation 4 can relate “Colombia’s Shapley value to its quota” [p. 225]. Presumably, it regresses the quotas of all countries on the Colombian proposal against their Shapley values.) It might seem (p. 222) that the failed proposal was made by Brazil, but this is inconsistent with the figures offered. On the one hand, under Brazil’s proposal, it “would secure over 33 percent of the market” (p. 222); on the other hand, under the failed proposal, Brazil would obtain 30% of the market (Lien and Bates 1987, 631). Or perhaps these percentages are not comparable; I cannot tell.

Bates also asserts (p. 224) that producer countries could make a claim for a quota according to their average exports in one of two three-year periods in the recent past. Which of these two periods they chose was up to them; presumably all countries chose the period in which they had the larger share. If average coffee exports are measured in absolute numbers of tons, then the sum of the individual claims might in theory exceed the total to be allocated. If measured in percentage share of exports, then they must exceed the total when each country can choose the most favorable period. Bates never tells us, however, how much the claims

constrained the proposals. Presumably, they must have left some scope for bargaining—but how much?

Although the Shapley value is uncorrelated with the votes cast on the two successive proposals, *and uncorrelated with the allocative quotas in the proposal that failed*, it is correlated with the allocative quotas in the successful proposal.

The resolution of the paradox may lie in the following: quotas are established in stages. In the ICO, a proposal is first negotiated, then proposed, and then voted on. It is during the period of negotiations that each nation exercises its bargaining power to shape the proposed allocation. When the proposal is subsequently voted on, however, the nations then appear simply to determine whether they do better or worse under the proposed quota than under the status quo (Lien and Bates 1987, 635).

The claim is not that the Shapley value allows us to explain the outcome of prevote bargaining (and of the subsequent vote). It is the weaker claim that *if* the outcome of prevote bargaining is correlated with the Shapley value, then the proposal will gain the requisite majority. Is this analysis an application of “rational choice theory” (Bates and Lien 1985, 555, 559)? Apart from the indeterminacy just noted, the use of the Shapley value to explain the allocation of quotas is a deviation from the use of noncooperative game theory in the rest of the book and in the earlier parts of Bates’s chapter. The microfoundations of cooperative game theory are by definition shaky.

I am also puzzled by the following statement:

Under the rules of the agreement, bigger producers were endowed with greater numbers of votes; intuitively they should therefore have possessed more power. Equation 5 provides the test we seek. As seen in that equation, the relationship between the voting strength of a nation and its Shapley value is indeed nonlinear; it is quadratic, such that as the number of votes controlled by a nation increases, its Shapley value rises more than proportionately (p. 225).

This seems to be confusing two claims. First, larger nations had more votes (see Bates 1997, 138, for details). Second, the relation between voting strength and power is quadratic, such that when the number of votes rises, the power rises more than proportionately. Even if the Shapley value has been a linear function of the number of votes, the larger producers would have had more power. Further puzzlement is provided by the fact that Bates uses statistical estimation to determine the Shapley value as a function of voting strength, whereas I would have thought that the relation obtains by definition.

It is quite possible that most of these puzzles are just (severe) problems of exposition, not of reasoning. It may seem petty to dwell on them, but I believe the programmatic nature of the volume justifies my doing so.

## SOME GENERAL ISSUES

The volume is also beset by some more general and perhaps more serious problems. The objections cited above mainly concern second-decimal issues. Even if



the authors had gotten them right, there would still be a number of first-decimal questions to be asked.

### The Assumption of Rational Actors

The authors uniformly assume that the main actors acted rationally. With the exception of Levi and Rosenthal (who, albeit not in his formal mode, gives some place to the causal efficacy of religion), they also assume that the actors were motivated by their material interest. In general, of course, rational choice theory can easily accommodate nonmaterial or nonselfish interests. What matters is whether the actors pursue their goals in an instrumentally rational manner, not whether these goals are defined in terms of material self-interest. Yet, even when expanded to include broader goals, rational choice theory is often inadequate because people may not conform to the canons of instrumental rationality.

Let me distinguish between two issues. First, we may oppose rational choice theory as applied by the authors to the paradigm of bounded rationality (e.g., Nelson and Winter 1982) and to the research program variously labeled behavioral economics or quasi-rational choice (Thaler 1991). In the chapters by Greif and Rosenthal, actors (individual and collective) are endowed with the capacity to make very complicated strategic decisions. There is no reference to the fact that for most people the ability to engage in strategic reasoning is severely limited. Consider, for instance, French President Chirac's disastrous call for elections in June 1997. I conjecture that his coalition lost because voters understood that if he wanted early elections, it was because he knew something they did not, something which made him believe that he would lose if he waited. By calling early elections, he revealed what he knew, or at least *that* he knew something, and therefore gave them a reason to vote against him.

The point is not that this was a failure of rationality on Chirac's part, but that the failure is a very common one. Experiments on the Winner's Curse (Thaler 1992) show that people are very bad at anticipating how behavior may reveal beliefs. In Chirac's case, he did not anticipate that the voters would infer his beliefs from his behavior. In the Winner's Curse, subjects fail to anticipate the beliefs that they themselves will be able to infer from the behavior of others. In both cases, there seems to be a limit to how many inferential steps people can make ahead of time. It is not that it is particularly difficult; it just does not seem to come naturally.

Why, then, should I believe that Greif's Genoans were capable of reasoning through nested games of backward induction, complete with discounting over an infinite horizon? Also, why should I believe in Greif's assumption of exponential discounting when there is "compelling" evidence (Barro 1999, 1126) that time discounting is hyperbolic rather than exponential? It will not do to say that the hypothesis of exponential discounting is a useful first approximation, since it predicts behavior that is qualitatively different from what we would expect on the hypothesis of hyperbolic

discounting. By contrast, the hypothesis of quasi-hyperbolic discounting (Laibson 1997) is a useful first approximation.

Second, the departure from rationality may be much more radical if the actors are motivated by fairness concerns or by emotions. The Ultimatum Game (Roth 1995) is a standard example of a situation in which people are willing to take nothing rather than something, either because they react to being unfairly treated or because they are motivated by emotions of envy or anger. Weingast's chapter, for instance, ignores completely the emotional charge of the slavery issue. Commenting on the congressional debates over slavery before the Civil War, Miller (1996, 184, emphasis in original), observes that "the slavery *interest* has transmuted into the glorious Southern *cause*." As he documents, the thinking of the slave owners and their political representatives was seriously distorted by passion. (This may have made their blackmail more credible than it otherwise would have been.) Levi does not ignore these nonrational concerns, although she misdescribes them as rational. Although I know more about seventeenth-century French literature than about the politics of the period, even my scattered knowledge of the latter confirms the overwhelming impression conveyed by the former, namely, that the ruling elites were obsessed with glory and honor. To understand, for instance, the deep pleasure they took in humiliating their rivals, we must read Saint Simon's *Mémoires* rather than *Games and Decisions*.

The alternative to the rational actor is not, however, someone who is in the complete grip of passions and completely ignores concerns of security and self-interest. It would be as absurd to deny that the French absolutist kings were preoccupied with revenue as to assert that this was their only concern. They were, in a word, reward-sensitive. That is not to say, however, they were reward-maximizing. Nonrational concerns could often distort their thinking or shorten their time horizon so as to undermine the instrumental efficiency of their behavior. Yet, once their material interests were seriously threatened, they were capable of facing reality. The social sciences today, however, cannot offer a formal model of the interaction between rational and nonrational concerns that would allow us to deduce specific implications for behavior. As mentioned earlier, the idea of modeling emotions as psychic costs and benefits is jejune and superficial. The fact that emotion can cloud thinking to the detriment of an agent's interests is enough to refute this idea. Also, if (say) guilt were simply a psychic cost, then agents should be willing to pay for a pill that would eliminate the emotion—but in reality they would, I believe, feel guilty about doing so.

### High Levels of Aggregation

Some of the explananda in the volume are vast historical entities: the development of absolutism, the stability of antebellum politics, the secular expansion of the Genoan economy. Some of the actors that enter into the explanations are huge collectives—clans, the elite,

North, South, and so on. When these are endowed with beliefs and goals and are assumed to engage in complex strategic calculations, credulity breaks down. Rational choice explanations divorced from methodological individualism have a dubious value. Although there may be collective actors who can for practical purposes be treated as individuals, the ones who appear in AN are not—or have not been shown to be—among them.

The most fundamental question is, of course, whether aggregates can coherently be said to have preferences at all. Although social choice theorists have argued that preference aggregation is a fragile business, there may be special cases in which it can be justified. We may be entitled, perhaps, to treat firms as individuals to the extent that they are subject to hard market constraints. Even larger aggregates such as nations may be forced by their own statements to behave consistently. Suppose there is a majority cycle of policies A, B, and C, and that procedural rules lead to the adoption of A, which is then stated publicly as the position of the country. If some of those who formerly preferred C over A change their preference because of the cost to the country of being perceived as inconsistent, even procedural rules that would originally have led to the adoption of C may now simply confirm A. This particular story may not apply to any of the cases in AN. To be taken seriously, however, any account that imputes goal-oriented behavior to aggregate entities has to explain why we should expect consistency in their behavior.

### Little Concern for Intentions and Beliefs

One would expect rational choice theorists to take the idea of choice seriously, by looking for evidence that the agents whose behavior they want to explain did in fact have the goals and the beliefs they describe. To some extent, Bates and Levi provide direct evidence about mental states, drawing on interviews (Bates) and parliamentary proceedings (Levi). Weingast tries but fails. The other chapters neglect this aspect of social-scientific explanation. One might say, perhaps, that they are ruthlessly instrumentalist in the Milton Friedman tradition. But to explain observed behavior by intentions and beliefs that are imputed rather than documented is acceptable only if (1) the sources do not permit us to establish intentions and beliefs directly, (2) the observed empirical fit is very good, (3) other implications of the imputed intentions and beliefs are deduced and verified, and (4) plausible alternative explanations are given a good run for their money and then rejected. At the least, I do not feel confident that the chapters by Greif, Rosenthal, and Weingast satisfy these criteria.

Much of applied rational choice theory is a combination of just-so stories and functionalist explanation. One constructs a model in which the observed behavior of the agents maximizes their interests as suitably defined, and one assumes that the fit between behavior and interest explains the behavior. Suppose that higher education tends to make people pay more attention to the future, and paying more attention to the future

tends to make people better off. It is then a tempting step to conclude that people choose higher education *in order to* reduce their rate of time discounting (Becker and Mulligan 1998), or (in a more general version) that this particular benefit of education *explains* why it is chosen. It is, however, a temptation that should be firmly resisted, in either version (Elster 2000, 26–9). Unless one can demonstrate an intention (first version) or a causal feedback loop from the consequences of the behavior to the behavior (general version), the coincidence of behavior and interest may be only that—a coincidence.

### No Concern for Uncertainty

Except for a minor feature of Weingast's chapter, all the models in AN assume full information. In the real world, of course, high-stake politics is permeated by uncertainty. No model of political behavior that ignores this fact can be successful in predicting outcomes. There are at least five kinds of uncertainty. The first is brute factual uncertainty (will there be a major earthquake in greater Los Angeles over the next decade?). The second is higher-order uncertainty about the cost of resolving first-order uncertainty (do I have time to ascertain the enemy's position before going into battle?). The third is strategic uncertainty due to multiple equilibria (do cartel members play tit-for-tat or sudden death?). The fourth is uncertainty due to asymmetric information (is my opponent irrational or only faking?). The fifth is uncertainty due to incomplete causal understanding (will tyrannical measures imposed by a dictator make the subjects more compliant or less?). The compound effect of these (and perhaps other) forms of uncertainty will, in most complex situations, tend to be overwhelming. At the same time, existing models of decision making under uncertainty, of equilibrium selection, and of games with asymmetric information tend to be very artificial. One might say, perhaps, that it is to the credit of the AN authors that they stay away from these models. Yet, I think the proper conclusion would have been to eschew formal modeling altogether.

### CONCLUSION

It is not clear to me what an analytic narrative is. Although the authors deploy rational choice theory, they assert that analytic narratives also could derive from “the new institutionalism . . . or from analytic Marxism” (p. 3). In the absence of specific examples, I shall understand an analytic narrative as a *deductive explanation of individual historical facts*, with “fact” being taken in a broad sense that may include long-term developments. In practice, this means rational choice explanation, for the simple reason that rational choice theory is the only theory in the social sciences capable of yielding sharp deductions and predictions. The theory rests on maximization theory, the beauty of which is that it typically yields a unique maximum.

I have argued that the contributions in AN fail to sustain this research program. I have also hinted that,

over and above flaws of execution, the failure may be due to the intrinsic nature of the program. As in the case of sociobiology (Kitcher 1985), deductive rational choice modeling of large-scale historical phenomena may be a case of excessive ambition. But this analogy may be imperfect. In the case of sociobiology, we know from first principles that patterns of animal behavior must have an explanation in terms of biological fitness. The main objection to sociobiological just-so stories is that they are premature, not that some story of this kind will not eventually come to be found. In the seventeenth century Pascal ([1660] 1995, 22) made a similar observation about the Cartesian attempt to create a mechanistic biology: "In general terms one must say: 'That is the result of figure and motion,' because it is true, but to name them and assemble the machine is quite ridiculous—pointless, uncertain, and arduous."

Rational choice history is in a worse situation than that of either mechanistic biology in the seventeenth century or of sociobiology today. The analogy would be appropriate if it were mainly a question of refining the theories and of gathering more evidence. One could refine theory by incorporating bounded rationality and quasi-rational choice, so as to match more closely the way in which actual decisions are made. One could gather more evidence by paying careful attention to sources that illuminate the beliefs and goals of the actors. When we look at the public justifications given for the votes in the first French constituent assembly, for instance, it appears that all deputies had lofty public-spirited motivations. When we read the letters they wrote to their wives, it is apparent that some voted against bicameralism or against an absolute veto for the king because they feared for their life if they did not (Egret 1950). After the publication by Alain Peyrefitte (1994, 1997) of the notes of his conversations with de Gaulle, we can distinguish between what the latter said for public consumption and what really animated his behavior. There is no reason to think that de Gaulle was also being strategic in these conversations, whose substance has been confirmed by his son, Admiral Philippe de Gaulle. By proceeding on these two fronts, one would approach the ideal of explaining behavior by deductive models that rely on (1) realistic assumptions about the way agents make their decisions, given their mental states, and (2) assumptions about their mental states that are independently verified rather than stipulated.

Unfortunately, this will not take us very far. I do not want to insist on the issue of uncertainty. If we can identify the actual beliefs of the agents, we need not worry too much about indeterminacy in rational belief formation. I also do not want to insist on the problem of aggregation. We can proceed from the bottom up to explain the behavior of aggregates of agents, for example, by putting together individual-level explanations of the voting behavior of all members of an assembly to explain the overall vote. In many cases, the voting procedure, including the ways in which alternatives are help up against each other, can simply be taken as parametric. The main obstacle to analytic narratives,

understood as rational choice history, arises at the level of motivations.

As suggested by my earlier discussion, I want to make two claims. First, nonrational motivations are important and pervasive. Wars have been lost because soldiers were taught that it was dishonorable to take defensive measures (Dixon 1976, 54–5). Analyses of why some individuals harbored Jews in Germany or German-occupied countries during World War II whereas others did not suggest that a major factor was that the former were *asked* by someone to do so (Varese and Yaish 1999). On a rational choice account, this would be a matter of information: To harbor someone, you first have to know about their existence. On an alternative and perhaps more plausible account, it is a matter of the emotional difficulty of refusing a face-to-face request. In war trials after World War II, individuals accused immediately after liberation were sentenced much more severely than those tried for identical crimes two years later (Elster 1998a), which most plausibly can be explained by appeal to the dynamics of anger and hatred. These are scattered examples, which could be multiplied indefinitely.

Yet, if we embrace the most abstract characterization of analytic narrative as deductive history, rather than rational choice history, such facts are not necessarily fatal to the project. To the extent that emotions—their triggering and their dynamics—can be modeled in a way that yields definite predictions, they can be incorporated into an analytic narrative. My second claim, however, is that we do not know how to construct such models. We do not know how to predict the behavior that will occur when an individual is entirely in the grip of an emotion. Fear, for instance, can lead to fight, flight, or freezing, and we do not know which will be triggered in a given situation. We may not even be able to predict which emotion will be triggered. If A favors B at the expense of C, will C feel envy toward B or anger toward A? Also, we do not have good models of the trade-offs at work when emotion and rational pursuit of a goal coexist as motivations. (On all these points, see Elster 1999.)

If pressed, I might have to choose between two versions of the latter claim. Do I mean, à la Pascal, that deductive modeling of emotions is a premature project? Or do I want to state, more strongly, that such modeling will forever remain beyond our reach? Although I incline toward the second answer, my reactions are tempered by previous attempts to legislate a priori what science cannot do—often followed, decades or centuries later, by scientists who carry out the allegedly impossible task. Nevertheless, two reasons make me believe that deductive history will forever remain impossible. First, the micromechanisms, if and when we find them, are likely to be of very fine grain. Second, however assiduously we search the historical record, the evidence is unlikely ever to match the fineness of grain of the mechanisms.

All this is not to say that rational choice theory cannot illuminate historical analysis, as long as its claims are suitably modest. Collective action theory has changed forever the way social scientists and historians



think (or ought to think) about rebellion, revolution, and related phenomena. Hobbes, Tocqueville, and Marx may use language that reminds us of modern discussions of the free-rider problem, but formal analysis is needed to bring out its relation to the subtly different phenomena modeled in the game of Chicken or the Assurance Game. Montaigne and Descartes may have understood at a qualitative level that iterated interactions differ importantly from one-shot interactions, but they did not and could not anticipate game-theoretic results about the conditions under which behavioral differences are likely to arise. Modern analyses of credibility and precommitment have revolutionized our understanding of strategic behavior. The idea of burning one's bridges has always been known, but only after Schelling (1960) has the motivation for such behavior been fully understood. Again, examples could be multiplied indefinitely.

The need for modesty appears in two ways. First, as I have been at some pain to emphasize, one should avoid the postulate of hyperirrationality. Collective action, iterated games, and credibility are simple ideas that can be and have been refined to yield rococo (or baroque?) constructions that no longer bear any relation to observable behavior. To be useful, they have to be constrained by what we know about the limitations of the human mind. Second, because formal analysis has nothing to say about the motivation of the agents, it cannot by itself yield robust predictions. Although it is extremely useful to know that the structure of material interests in a given case is that of a one-shot Prisoner's Dilemma, that fact does not by itself imply anything about what the agents will do. If they have nonmaterial or even nonrational motivations, they might behave very differently from the noncooperative behavior we would expect if they were exclusively swayed by material interests. If they are in fact observed to cooperate, then we will have to search for nonmaterial or nonrational motivations. Rational choice theory tells us what to look for, not what we will find.

In closing, to focus the discussion I might put six questions to the contributors to AN. (1) 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? (2) Do they agree that a plausible analytic narrative requires independent evidence for intentions and beliefs? (3) 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? (4) 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? (5) Do they agree that it also

needs to be modified to take into account nonrational motivations? (6) 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?

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