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Comment on "Excessive Ambitions (II)" (by Jon Elster)

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Any man of sound head, and practiced in wielding logic with a scholastic adroitness, might take up the whole academy of modern economists, and throttle them between heaven and earth with his finger and thumb, or bray their fungus heads to powder with a lady's fan.

de Qunicey's *Confessions of an Opium Eater*

1. Introduction

Jon Elster (2009, 2012) is a giant of political science. Even if he is writing with his tongue in his cheek, he must therefore still be taken seriously when he rejects the rationality paradigm as an explanatory tool, characterizing its proponents in social science as "excessively ambitious".

The first of his two papers in *Capitalism and Society* is a systematic attempt to demolish rational choice theory, not only in political science, but in all of the social sciences, including economics. I am asked to comment on the second paper, in which he offers Jeremy Bentham as an example of how we could usefully proceed without using either statistical methods or formal models, but I find that I cannot come to grips with the second paper without also commenting on the first.

Jean-Pierre Chiappori (2009) and David Hendry (2009) have already commented on the first paper. Hendry boils over with outrage at Elster's ignorance of modern empirical techniques in macroeconomics. I am sure that Hendry would nevertheless agree that social scientists go in for far too much mindless regression analysis — which is presumably what Elster means by curve-fitting — but the idea that all statistics should therefore be abandoned is more than a little intemperate. For example, there is a lot of statistical analysis of data in my own work but I cannot recall there being any regressions at all. However, there is no point in arguing with a critic who takes delight in admitting that he knows little or nothing of what he is criticizing. So I shall say no more about Elster's rubbishing of empirical work, and turn instead to his rubbishing of the use of formal models.

Chiappori is more laid back than Hendry in responding to Elster's attack on modeling. He agrees with Elster that the vast majority of published papers analyzing formal

models are worthless, and I am only too happy to endorse this judgment.[1] When clearing my office, I recall looking over my old and dusty copies of *Econometrica* before passing them on to clutter the office of a younger colleague. Almost none of their content was still regarded as useful in my area. A few of the formal models were still in play, but all that survived of the remaining work were the ideas that had motivated some of the authors, but which could not be published in *Econometrica* without being unnecessarily crammed into a mathematical shoe. Perhaps I am unduly optimistic in thinking that things are somewhat better now that the *American Economic Review* seems to have supplanted *Econometrica* as the leading economics journal.

I also agree with Chiappori that the problem is not so much excessive ambition, as inadequate ambition. Most of the time, social scientists simply respond to their professional incentives. If they have misgivings, they tell themselves that minds greater than their own have set the agenda and who are they to swim against the tide? I see no point in complaining about this state of affairs in social science because things are no better in physics, except that their gurus are cleverer than ours. A little googling will reveal that physicists even have Elster equivalents who are deeply distressed at the triumph of string theory in their subject.

But the fact that much formal modeling is worthless does not imply that all formal modeling is worthless. As in my experience with *Econometrica*, there is sometimes a baby in the bathwater that it would be stupid to throw away. It would be even more stupid to throw out the baby for the kind of anecdotal reasons that Elster offers. One might as well condemn aeronautics to oblivion on the grounds that strapping on home-made wings and jumping off a high building is unlikely to work out well.

2. Rationality as Consistency

The version of rational choice theory that Elster attacks was invented in late Victorian times. It is the underlying paradigm for what is nowadays known as classical economics. People are assumed to carry around utility functions in their heads that they seek to maximize (a word invented by Jeremy Bentham). This classical paradigm has been developed in modern times by adjoining beliefs to the preferences located inside people's heads. These beliefs are quantified by probabilities so that agents can be assumed to maximize expected utility.

This psychological model of human behavior seems to be taken for granted by everybody outside economics who subscribes to rational choice theory. Most economists —

[1] Elster says that nobody is likely to pay attention to an outsider like himself rubbishing "science-fiction models". What he thinks is needed is for mathematical economists to say similar things. In fact, it is not uncommon for disenchanted mathematical economists to do just that; Ariel Rubinstein (2012) is a recent example. But all anyone ever gets from blowing the whistle is a reputation for eccentricity.

including many winners of the Nobel prize — also have to be counted among those who take the classical paradigm for granted.[2] I am not so hostile as Elster to the classical paradigm, but it certainly is very badly abused in the literature. There is therefore a strong case for abandoning it for most of the situations in which it is applied, if not altogether as Elster advocates. It is therefore ironic that the classical paradigm was indeed abandoned as the official orthodoxy of the economics profession more than fifty years ago, under the powerful influence of Paul Samuelson (1947). The official orthodoxy is admittedly more honored in the breach than in the observance, but there are nevertheless reasons why there is a *neo* in neoclassical economics that one cannot just ignore.

Samuelson was explicit that one of his principle aims was to separate economics from psychology.[3] To achieve this aim, he invented (some say reinvented) the theory of revealed preference, whose modern descendant is what I like to call the theory of *attributed* preference (and belief). The idea is simply that behavior (whether actual or hypothetical) that is sufficiently consistent can be described *as if* it had been generated by maximizing the (expected) value of a utility function (relative to a subjective probability distribution).

I suspect that Elster regards the theory of revealed preference as an idle exercise in philosophical window-dressing, but if one wants to know when a formal model is a baby and when it is bathwater, it is necessary to take the theory seriously. He repeatedly complains about the failure of what he thinks of as rational choice theory to describe real-life causal relationships adequately, but fails to appreciate that neoclassical economics *reverses* the causal chain of classical economics. As a result, Elster falls headlong into what is sometimes called the Causal Utility Fallacy.

In neoclassical economics, it is not true that Alice chooses *a* rather than *b* because her utility for *a* exceeds her utility for *b*. On the contrary, modelers choose the utility function they attribute to Alice to make the utility of *a* exceed that of *b* because it is known (or hypothesized) that Alice has chosen (or would choose) *a* rather than *b*. If her behavior continues to be consistent, we may then be able to predict some of the choices she will make in the future using the utility function we have attributed to her. But nothing is being assumed about what is going on in her head except that the result of whatever is happening there

[2] They sometimes even acquiesce in the reckless accusation of hostile critics like Henrich *et al* (2004) that selfishness is axiomatic in economics. This misunderstanding presumably arises because economic agents are assumed to maximize their own utility functions. But it does not follow that they are selfish, because their utility functions may well include a component that registers a concern for the welfare of others.

[3] Which does not mean that Samuelson thought psychology should be junked, but that it should be left to psychologists, so that economists could get on with doing economics.

happens to generate consistent behavior.[4] Elster is rightly much exercised about the fact that we seldom know very much about the beliefs (or preferences) of the people we seek to represent in formal models, but one of the many virtues of the neoclassical paradigm is that we do not need to pretend that we are so absurdly well informed.

Attributing consistency to choice behavior. When is it reasonable to assume that decisions will be made in a consistent manner? Naive exponents of rational choice theory proceed as though the answer is all the time — a view that mainstream psychologists regard as ludicrous. But it does not follow from the fact that consistency fails in many situations that it fails in all situations. So what are the situations in which consistency should not be expected to fail? If there are babies in the bathwater, this is where we can find them.

When talking about experimental work, I identify three criteria that need to be satisfied simultaneously before one might reasonably expect rational-choice models to predict in the laboratory (Binmore 2006):

> The problem to be solved is not too hard;
> The incentives for choosing well are adequate;
> Sufficient time is available for trial-and-error learning or adjustment.

For example, when Elster points out that rational choice does not work very well in voting models, my response is to wonder how we ever got into a state where scholars convinced themselves otherwise. It is a commonplace of the subject that a voter in a big election has no incentive for thinking very much about how to vote because the probability that a single vote will be pivotal is negligible. On the other hand, when I toured the world offering advice on the design of big-money telecom auctions, it was reasonable to assume that the billions of dollars up for grabs would incentify telecom companies to spend much time and money making sure that their bids were properly thought out (Binmore and Klemperer (2002)).[5]

However, it is not the role of incentives that I want to emphasize but that of trial-and-error adjustment, which gets short shrift from Elster, who tells us that it reduces to either reinforcement or selection. He then offers two ancient references by distinguished authors: firstly to a case in which a particular form of reinforcement learning fails to converge on rational choices, and secondly to a case in which a particular conception of economic

[4] Biologists say that the mental or physiological processes that generate some particular behavior are its *proximate* causes, which they distinguish from the evolutionary considerations that explain why other behaviors failed to survive in the long run. The latter considerations are said to be *ultimate* causes of the behavior. Neoclassical reasoning has a similar flavor. Confusing neoclassical and classical reasoning is therefore like saying that an animal's survival in the long run depends on its knowing its reproductive fitness and actively maximizing this quantity.

[5] Although telecom companies were not nearly so good at figuring out what a telecom license was worth to them.

selection has turned out to be overly ambitious. Such anecdotal reasoning would be unacceptably brief even if rational choice theory had not enjoyed its biggest empirical successes in evolutionary biology.[6] In fact, a new subject called evolutionary game theory has emerged since Elster last looked at the literature. In this new subject, it is shown that sensible models of trial-and-error adjustment do converge on Nash equilibria much of the time — for example, in games with a unique strict Nash equilibrium. The simplest case is that of the one-shot Prisoners' Dilemma in which 90% of experimental subjects end up defecting after experiencing 10 trials against a randomly chosen new opponent each time.[7]

In brief, Elster's rejection of trial-and-error adjustment as a mechanism capable of generating consistent choice behavior is wildly cavalier. It is true that we would have to classify almost all current, rational-choice literature as bathwater if we insisted on the presence of some such mechanism before congratulating ourselves on having found a baby capable of quantitatively accurate prediction, but this is what I am advocating.

Bayesianism. Leonard Savage — the founder of Bayesian decision theory — would have been as outraged as Elster at the widespread use of his theory of expected utility in finance and macroeconomics. Savage understood perfectly well that the consistency in choice required by his theory is hard to come by, and so argued that its use should be confined to *small worlds* in which all possible future contingencies can be fully assessed before they occur so that consistent plans can be made (Binmore 2009, page 130). He says that it would be "preposterous" and "utterly ridiculous" to use his theory in large worlds (Savage 1954, page 16). Nevertheless, Nobel prizes are awarded to rational choice theorists who do precisely that.[8] I am happy to join Elster in classifying this work as bathwater, but he would throw out the use of expected utility theory in small worlds as well. It is true that ordinary people are not very good at making consistent decisions even in small worlds when the feedback is noisy, but we sometimes need — as in designing auctions — to deal with decision-makers who can afford to employ mathematicians to keep their affairs in order, or with people or animals whose behavior has been shaped by long experience of a fixed environment. For such small-world applications, Bayesian decision theory is a very beautiful baby indeed.

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[6] I observe in passing that Elster's rejection of equilibria in mixed strategies would also apply in biology. But nobody nowadays thinks that mixed equilibria can only make sense with Elster's naive interpretation. Strategies appear to be used with different probabilities in what biologists call polymorphic equilibria because an animal population can break down into different groups whose members get the same fitness payoffs in equilibrium but are programmed to play different strategies.

[7] The common claim that people cooperate in the Prisoners' Dilemma—thereby refuting rational choice theory—is therefore mistaken. Even subjects playing for the first time defect 49% of the time on average (Camerer 2003). See the surveys of Ledyard (1995) and Sally (1995).

[8] For example, to the work of Merton and Scholes in 1997.

Behavioral economics. It is odd that Elster should express some enthusiasm for behavioral economics, because it is here that the practices he likes least are most deeply entrenched. Behavioral economists do indeed criticize neoclassical economics a great deal, but in seeking to model social or other-regarding preferences, they reveal themselves as having reverted to the classical paradigm when they fit psychologically interpreted utility functions with many parameters to laboratory data using the curve-fitting techniques that Elster so despises. Nor should their use of the as-if methodology championed by Milton Friedman (1953) appeal to Elster. It is true that it does not matter what principles guide the construction of a model, provided that the model predicts data well, but the behavioral social-preference school is no better at predicting new laboratory data than Friedman was at predicting new macroeconomic data (Binmore and Shaked 2009).

3. Jeremy Bentham

In his second paper, Elster looks at various problems in institutional design with a view to showing how it is possible to proceed without formal models or quantitative analysis. His chief exemplar in this enterprise is Jeremy Bentham — whose mummified corpse was seated next to me when I joined the Jeremy Bentham Society for dinner some years ago,[9] Bentham is certainly deserving of admiration, but I am astonished that Elster should choose Bentham of all people to personify his hostility to formal modeling and statistical analysis.

Bentham was infamous in Victorian times for advocating exactly what Elster does not like — a quantitative approach to social reform. It is Bentham and his followers that Charles Dickens was attacking in *Hard Times* when Mr Gradgrind tells little Louisa never to wonder: "By means of addition, subtraction, multiplication, and division, settle everything somehow, and never wonder." His reputation as a supposedly heartless technocrat is kept alive to this day in such works as A. N. Wilson's *Victorians*, in which the author applies the word *benthamite* to any economic development of which he disapproves, so that Bentham is held responsible not only for all the crimes of authoritarian socialism, but also for the excesses of laissez-faire capitalism.

However, it is necessary to turn from the real Bentham to the program for a "more modest and more robust political theory" that Elster refers to as Benthamite to examine the

[9] Bentham was a founder of London's University College, which honors his last will and testament to the extent of displaying his "auto-icon" in a glass cabinet for all to see. I think history has been unkind to Bentham by awarding the intellectual laurels for utilitarianism to John Stuart Mill, who seems to me much talk but little substance (Binmore 2011). Why Harsanyi (1977) gets so little credit among philosophers for actually doing what Mill is said to have done is an ongoing mystery.

kind of reasoning that Elster believes superior to "addition, subtraction, multiplication, and division".

4. Institutional Design

Elster argues that social scientists in general should abandon any attempt at positive institutional design. We should instead confine our attentions to negative institutional design — arguing for reforms merely on the grounds that they *can't hurt and might help*. In advocating this point of view, Elster is inspired by Ely (1980, pages 102-103), who advocates an "antitrust" as opposed to "regulatory"" approach, not just to economic affairs, but to affairs in general. Rather than pushing for substantive results, Ely would intervene "only when the 'market', in this case the political market, is systematically malfunctioning".

We economists do not enjoy the luxury of being able to take such a relaxed attitude to the troubles of the world. We see far more markets malfunctioning than is possible for those like Elster, who cannot or will not use formal models that reveal how the markets would be running if they were not subject to systematic abuse. And the last thing we need to deal with these abuses is more of the fuzzy antitrust legislation invented by lawyers who know little or nothing of the industries the legislation is meant to control. What we need is precise regulation tailored to particular industries, put together by people who know the industry well enough to be able to construct and calibrate a proper model of its workings. Elster believes that such competence is not available, but what does he know about it? I have myself been involved in the design of numerous big-money telecom auctions and in various European regulatory cases. I have also advised on institutional design within the British National Health Service, where Elster will find many fellow travelers whose inability to distinguish babies from bathwater continues to cause much pain and suffering. Huge sums of taxpayers' money are wasted on worthless reforms implemented without any preliminary modeling or testing. In such microeconomic situations, Elster is wrong that competent professionals are unavailable. The scandal is not that they pretend to an expertise they do not have, but that vested interests so often prevent their expertise being usefully employed.[10]

What for? Elster tells us that we should not worry too much about exactly what conception of the good a reform is intended to promote, because such questions have no definite answers and those who promote a particular version of the good often have a private axe to grind.

[10] One vested interest admittedly consists of *incompetent* professionals who make a living out of pretending to be experts on subjects on which it is not possible to be an expert in the absence of a proper model.

In none of my cases of institutional design did anyone involved have the luxury of asking themselves what was morally Good or Right, and then languishing in dismay at rediscovering Elster's observation that Moral Authorities offer mixed advice. In designing telecom auctions, my first question was always: What are we trying to achieve? What is our social welfare function? To which the government officials would always respond by pointing to a number of desirable but not fully compatible objectives such as promoting competition, assigning licenses efficiently, and raising revenue. I would then agree that these objectives are indeed desirable, but what are the government's priorities when these objectives conflict? To which the answer was always that we hire technocrats like you to make such technical decisions. But it should not be people like me who make such momentous decisions—literally involving billions of dollars—but the politicians who have been elected for this purpose.[11]

Elster would say that the indeterminacy in objectives should persuade social scientists to leave such (small-world) problems for others to solve. I think, to the contrary, that social scientists should use their expertise to help our principals remove any indeterminacy or inconsistency in their objectives. To this end, one can ask what their attitude would be towards a variety of scenarios and then employ the theory of revealed preference to help remove inconsistencies in the views expressed, and finally to summarize the adjusted preferences with a social welfare function. Elster would not approve because one cannot carry through such a program without using a model of some kind to link the scenarios that a principal is asked to evaluate. But then he would presumably still be handing out telecom licenses to fat cats for peanuts—as was done in the "beauty contests" that preceded the advent of auction design in the telecom industry—on the grounds that one should only implement reforms that will not do any harm and might do some good.

When social scientists offer advice on less immediate problems — like the issues in jury design that Elster cares about or the need for health reforms that trouble me — one should perhaps be even more scrupulous in sorting out the precise aims of the exercise. As Elster eloquently explains, people often have hidden motives for advocating or opposing reforms — motives that they conceal under a simulated concern for some aspect of the public good. (I particularly dislike gurus who choose not to reveal their religious reasons for opposing assisted suicide but instead cherry pick the data that they offer in support of their opposition.) But rather than walk away from such political realities as Elster advocates, I think social scientists should have the courage to expose intellectual dishonesty wherever they find it.

Our own prejudices that we contrive to hide from ourselves deserve the same treatment. The conversational style that Elster attributes to Bentham is particularly vulnerable to

[11] For example, to maximize revenue from an auction, it would be optimal to sell just one license, thereby creating a monopoly in the relevant branch of the telecom industry. But competition is best served by selling as many licenses as the market will bear.

the latter kind of self-deceit. For example, Elster wants to convince us that we should worry more about the diversity of jurors than their ability. Perhaps he is right — but how easy it is to deceive oneself and others when one is allowed to cherry pick the case studies and formal arguments offered for and against such propositions.[12] I believe that the standard methodology of science is our best defense against such dishonesty — the same science that Elster believes social scientists should abandon. Without the kind of self-discipline in which scientists are trained, people inevitably pay more attention to facts or studies that tend to confirm their inbuilt prejudices and neglect or ignore those that point in the opposite direction.[13]

5. Moral aptitude?

Elster quotes Bentham to the effect that institutions need to protect the active, intellectual and moral aptitudes of the decision makers. He is pessimistic about improving intellectual aptitudes,[14] and focuses on what can be done to improve juries and similar bodies by removing or minimizing influences like self-interest, passion, prejudice, and bias on jurors' active and moral aptitudes. In taking this line, he enters the territory of what is called mechanism design in economics.[15] Why does Elster not mention mechanism design at all? Perhaps because it uses formal models in a manner that his critique does not recognize. Not even physicists confine themselves to the kind of naïve positivism that Elster thinks is necessary in order for modeling to have explanatory worth. As an undergraduate, I was taught the mathematics of two-dimensional, incompressible, non-viscous flow along with numerous other formal models that were never intended to predict anything because they have unrealistic assumptions. So why were we taught them? Because nobody ever solved a complicated problem without being able to solve simpler problems first. In brief, models do not need

[12] In the case of judges being allowed to over-rule juries, I notice that Elster chooses not to draw attention to the numerous cases in which judges have perverted the course of justice for political reasons. If I were also allowed to cherry pick, I would quote the case in which a British judge instructed a jury vetted by MI5 that a high-profile whistle-blower was guilty *by definition*—but the jury found him not guilty anyway. Through such unusual but important incidents do we sustain our freedom.

[13] A good example is provided by the claim made by behavioral economists that the "endowment effect" is a robust finding (for example, Knetsch, Tang and Thaler, 2001). Had they actually surveyed the large literature, they would have found, with Plott and Zeiler (2005), that the endowment effect is observed only in about half of the papers.

[14] For example, Elster argues that since we cannot reliably choose democratic decision-makers for their ability, "the question of a diversity-ability trade-off does not arise". One might similarly argue that since Alice cannot reliably assess how much Bob loves her, she should only consider the size of his feet and other such objective facts when deciding whether to accept his proposal of marriage.

[15] Leo Hurwicz, Eric Maskin, and Roger Myerson were awarded the Nobel Prize in Economics for their work on mechanism design in 2007.

to be based on realistic assumptions to be useful. Indeed, as has often been pointed out, a model whose assumptions were entirely realistic would not be a model any more — it would be indistinguishable from the real thing.

Mechanism design. To quote David Hume (1739) on the general issue:

> In constraining any system of government and fixing the several checks and controls of the constitution, every man ought to be supposed a knave and to have no other end in all his actions than private interest.

Hume is not saying that all men are always knaves. He is saying that, when designing an organization, one should accept that if power *can* be abused, then it *will* eventually be abused. Elster is telling us the same thing when he quotes John Liburne's warning to Cromwell's soldiers not to grant their delegates too long in office because "standing water though never so pure at first, soon putrifies".

Those who initiate abuses usually invent stories that allow them to justify their abusive behavior to themselves; and once a climate of abuse has become habitual, even those initially endowed with a good measure of "moral aptitude" find it hard to resist the disapproval of their fellows by trying to climb out of the basin of attraction of the abusive convention. The recurrent scandals over politicians' expenses often have a farcical flavor, but there is no humor to be found in the reports of the ill treatment of helpless orphans or old people in care homes, or the callous neglect of patients in public hospitals.

Insofar as it is possible to deal with these problems at all, Hume argues that organizations should take the eventual emergence of knavery for granted and set up rules and incentives that minimize the extent to which knaves can prosper. Mechanism design takes up Hume's challenge by designing games in which the agents to whom power is delegated are treated as players. The checks in the constitution are the rules of the game. These are used to prevent a player going off the rails in situations that the designer can effectively monitor and evaluate. However, it is the controls that are more important, since these apply to decisions that the designer cannot monitor, or does not know how to evaluate. To get the players to act in accordance with the designer's aims rather than their own in such situations, it is necessary that the payoffs of the game be carefully chosen to provide the right incentives. The long-run behavior of the agents is then predicted by finding a suitable Nash equilibrium of the game on the assumption that all the players "act like knaves" in seeking only their own personal interest.

Notice that in order to insulate a new institution against corruption — to the extent that this is possible—we need to be able to take a shot at guessing how its proposed constitution would be subverted if operated by knaves seeking only their own self-interest. The model used for this purpose—whether formal or informal—will make the unrealistic assumption that everybody involved is a knave. It will not predict actual behavior in the

short run if the institution is put in place because very few people start out with knavish inclinations: it will never predict anything at all if its conclusions lead the designer to reject the design because it is too vulnerable to corruption. We would then have a very good reason for congratulating ourselves on having constructed a model incapable of predicting anything — except in a hypothetical world that we chose not to create.

As an example of what can happen when legislators fail to ask themselves how their social designs can be gamed, consider the mistake made by the US Congress in 1990 when it passed an act intended to ensure that Medicare would not pay substantially more for its drugs than private health providers. The basic provision of the act said that a drug must be sold to Medicare at no more than 88% of the average selling price. The problem was created by an extra provision which said that Medicare must also be offered at least as good a price as any retailer. This provision would only work as its framers intended if drug manufacturers could be relied upon to ignore the new incentives created for them by the act. But why would drug manufacturers ever sell a drug to a retailer at less than 88% of the current average price if the consequence would be that they must then sell the drug at the same price to a huge customer like Medicare? However, if no drugs are sold at less than 88% of the current average, then the average price will be forced up!

Elster might also contemplate the work of the psychologist Gerd Gigerenzer (2009) and his colleagues on the efficacy of public health policy in screening men for prostate cancer. Or the lives saved by Al Roth (2012) by his design for incentive-compatible kidney donation pairings between people willing to donate a kidney to a relative but find that it is only compatible with some unrelated victim of renal failure. How would such work be possible without a model to help assess what *would* happen if a reform *were* introduced?

6. Conclusion

It is easy to endorse Elster's condemnation of most papers that appeal to rational choice theory, but he is wrong to argue that all such papers are worthless. More importantly, he is wrong to imagine that returning to the methodology of Jeremy Bentham would make it adequate to base such an intemperate conclusion on some anecdotal accounts of cases where the abuse of rational choice theory has led to unhappy outcomes. There are babies among the bathwater that deserve to be saved from Elster's reforming zeal.

Elster would be advised to look at the areas in which rational choice theory works sufficiently well — both in the field and the laboratory — to count as a branch of social engineering. He may not be enamored with computerized or regulated micro-markets, but the fact that new designs in this area often work extremely well is a fact that cannot simply be ignored. The big success of game theory in evolutionary biology is also a phenomenon deserving of attention. Instead of throwing away such successes along with the dross, we

need to ask how and why rational choice theory works in these contexts and not in others, so that we can set ourselves the modest task of expanding the domain in which rational choice theory — modified as necessary — can be successfully applied.

In so doing, it is necessary to take the foundational works on the neoclassical paradigm seriously. These foundational works reduce rationality to consistency of choice. Before applying rational choice theory, the question for the modeler should therefore always be whether there are reasons to suppose that the agents being modeled are likely to be consistent in the context under study — a task at which Elster would probably be very good. Some examples of contexts in which we should not expect consistency of choice are the large worlds of macro-economics and finance — to which Elster might reasonably add most large social and political institutions (Savage 1954, page 16).

What should we do in the many worlds in which we cannot reasonably expect consistency of choice? Elster's answer is that we should return to the methodology of Jeremy Bentham, whom Elster offers as a champion of discursive argumentation. I agree that when one does not have a worthwhile model to assist in assessing reforms, one must assess the reforms without a model as best as one can — although my own champion of the discursive approach would not be Bentham, but the great David Hume. But doing without axioms and theorems does not make it intellectually respectable to cherry pick bits and pieces of data that support one's prejudices while ignoring other data that points in the opposite direction. On jury design, for example, I dislike the authoritarian inclination that leads Elster to endorse societies that allow judges to over-rule juries, but I would be open to persuasion if shown a reasonably comprehensive survey of relevant case studies, or an analysis of why those who argue against over-ruling juries are mistaken. Elster may be right on this and other issues of institutional design, but the kind of rhetoric that satisfies those already converted is unlikely to persuade a skeptic like me. In brief, Elster argues in favor of taking the science out of social science, but I think we need to keep the science in social science, even when we are too ignorant to be able to write down any useful equations.

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