

# Theories and Tropes: A Reply to Posner and Kelman

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and Richard Thaler\*\*\*

We are grateful to Richard Posner and Mark Kelman for their detailed comments on our article. Their objections come from opposite directions. Judge Posner complains that behavioral economics is not a “theory” and is indeed “antitheoretical”<sup>1</sup>; he invokes “evolutionary considerations”<sup>2</sup> in the interest of providing a unitary account of both rational and “quasi rational” behavior, as well as bounded self-interest.<sup>3</sup> Posner also thinks that rational choice theory can handle many of the problems we describe. By contrast, Professor Kelman wishes that we were less theoretical. Favoring “open-textured interpretivism,”<sup>4</sup> he thinks that behavioral economics is in a kind of “dance”<sup>5</sup> with rational choice theory, and that both dancers suffer from “hubris.”<sup>6</sup> He suggests that both approaches are mere “interpretive tropes,” providing two of many possible understandings of the “inexorably ambiguous” data.<sup>7</sup>

In these brief remarks, we reply to these different challenges. Our disagreements with Posner are far narrower than they might appear; he accepts our central claims and chooses mostly to argue that the behavior we describe, while real, deserves to be termed “rational.” Insofar as Kelman is arguing in favor of more in the way of behavioral research, we fully agree with him. Insofar as he is arguing that any approach is hubristic unless it acknowledges

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1. Richard A. Posner, *Rational Choice, Behavioral Economics, and the Law*, 50 STAN. L. REV. 1551, 1552, 1560 (1998).

2. *Id.* at 1552.

3. We will use the term “quasi rational” to refer to behavior characterized by bounded rationality and bounded willpower.

4. Mark Kelman, *Behavioral Economics as Part of a Rhetorical Duet: A Response to Jolls, Sunstein, and Thaler*, 50 STAN. L. REV. 1577, 1591 (1998).

5. *Id.* at 1580.

6. *Id.* at 1590.

7. *Id.* at 1579.

that it is merely one of several equally valid ways of viewing the world, we disagree; we believe that behavioral law and economics should be regarded as more than an “interpretive trope” and that it will help to shed light on legal problems and produce improvements in the legal system.

A small note at the outset: Both Judge Posner and Professor Kelman frequently use the word “irrationality,” and they write as if “irrationality” and “irrational behavior” are at the heart of behavioral economics and behavioral law and economics. But our article intentionally avoided that word, on the ground that it is not useful and is likely to mislead. We do far better to specify how human beings actually behave (and depart from the conventional theory) than to argue whether they are “irrational.” Hence we refer to quasi-rational agents and to bounded rationality, bounded willpower, and bounded self-interest, three ideas of special relevance to Posner’s comment, to which we now turn.

## I. POSNER

A reader of Judge Posner’s commentary might be left with the impression that there are large disagreements between us. This impression would be false. Posner accepts the most important points of our article; he acknowledges the existence of the three bounds, and he agrees that the phenomena we discuss are real and potentially important to economics and law. Much of his discussion consists of quibbling about what is really a minor matter: whether we should call a given behavior rational or not. Posner is also concerned to show the sources of the three bounds, an important subject to be sure, but one on which we are agnostic. Our goal here is to reduce the perceived distance between our views and straighten out the most important misunderstandings.

### A. *Framing the Debate*

Seeming to have learned more from the study of cognitive psychology than he lets on, Judge Posner begins with a clever framing manipulation. He says that we (implicitly) define behavioral economics negatively as “economics minus the assumption that people are rational maximizers of their satisfactions.”<sup>8</sup> He contrasts this negative approach to what he prefers, the positive “enriching” approach he claims for standard economic theorizing.<sup>9</sup> As readers of our article realize, we offer no such negative definition; in fact we often use precisely the same concept, “enriching” the standard economic model, that is offered by Judge Posner.<sup>10</sup>

Indeed, as we will make clear below, our goals and tools are remarkably similar. The primary problem Posner seems to have with our approach is our reluctance to call all behavior rational. To Posner, a model that is rational is good, disciplined, scientific, and enriching, whereas a model that is quasi ra-

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8. Posner, *supra* note 1, at 1552.

9. *Id.* at 1567.

10. *Id.*; Christine Jolls, Cass Sunstein & Richard Thaler, *A Behavioral Approach to Law and Economics*, 50 STAN. L. REV. 1471, 1475 (1998) [hereinafter “JST”].

tional is bad, undisciplined, unscientific, and subtracting. Posner seems to be using the heretofore undiscovered “rationality heuristic” (if it uses rational models, it is good). Like all heuristics, this one sometimes works well, but it can lead to systematic biases.

In this case the primary bias is one that Colin Camerer calls, in conversation, the “sufficiency bias.” Throughout Posner’s commentary, he goes through the following ritual. He discusses one of the phenomena we identify as problematic for economic theory; he offers a modification or elaboration on the standard theory that could, in principle, be consistent with this phenomenon; and then he declares victory. Posner seems to think that the fact that it is possible to tell a rational choice story consistent with the data is sufficient to establish that this explanation is the correct one. This is obviously a fallacy. In no case does he offer evidence to suggest that his preferred explanation is correct, nor even a test that would, in principle, discriminate between his explanation and ours. For those of us who believe in falsifiability, this is an unfortunate omission.

## B. *Three Bounds*

### 1. *Bounded rationality.*

To his credit, Posner offers a definition of rationality: “choosing the best means to the chooser’s ends.”<sup>11</sup> But at many places he acknowledges that people often do not choose the best means to their own ends. He agrees that there is substantial evidence in favor of what he insists, in our view misleadingly, on calling “cognitive quirks”<sup>12</sup> (a subtle suggestion that these are minor and unsystematic departures from the standard model). Still, he claims that, in general, bounded rationality either can be accommodated within a more refined rational choice model, or does not upset the predictions of the standard model, or both. He claims that we are simply willing to “give up on rational-choice economics too soon.”<sup>13</sup>

Whether this is true of course depends on what the evidence shows. Posner suggests, for example, that the standard economic analysis of transportation is unaffected by people’s (“irrational”—his term) fear of flying, and that the economic analysis of voting is unaffected by (“irrational”—again his term) voting behavior.<sup>14</sup> In some contexts a standard analysis of transportation and of voting may work (although for the same reasons we gave in our article, we continue to question assertions such as that standard analysis is helpful in predicting “why the old vote more than the young”;<sup>15</sup> if in fact the young voted more, could it not be said that voting for them was more novel and thus generated greater utility?). A standard analysis cannot, however, tell us all of what we need to know. Behavioral economists would predict that

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11. *Id.* at 1551.

12. *Id.* at 1553.

13. *Id.* at 1556.

14. *Id.* at 1554-55.

15. *Id.* at 1554.

both market behavior and government behavior in the airline industry are affected by bounded rationality, as, for example, when additional safety measures are taken and mandated in the aftermath of a crash (a prediction of the availability heuristic)—even if such measures are not defensible on traditional grounds and even decrease aggregate safety (by producing a shift toward other, more dangerous modes of transportation, such as driving). We would also predict that bounded rationality (the availability heuristic, overoptimism, and framing effects) is relevant to voting behavior—and also that people are more likely to vote if they think that most people are voting (not a prediction of the conventional analysis, in fact a conundrum under that analysis, but a reasonable inference from models involving cooperative behavior<sup>16</sup>).

The behavioral economics of transportation and voting are in fact extremely interesting; the notion that all the important aspects of these behaviors are well-explained by standard economics strikes us as, well, overoptimistic.

## 2. *Bounded willpower.*

Posner agrees that self-control problems are real: “I do not doubt that there is such a thing as weakness of will.”<sup>17</sup> Indeed, he thinks that analysis of the behavior that presents such problems “may require abandoning a tacit assumption of most economic analysis—that the self is a unity.”<sup>18</sup> We share the view that people are often in struggles with themselves—struggles that the wiser part of their personality sometimes loses. (Posner suggests at one point that we “do not discuss the ‘multiple selves’ approach”;<sup>19</sup> but such temporal inconsistency of preference is precisely what hyperbolic discounting reflects.<sup>20</sup>)

We disagree, though, with Posner’s view that information provides an obvious explanation for hyperbolic discounting.<sup>21</sup> Posner suggests that people may discount sharply in the relatively near future because they know they have a particular need for cash now, whereas for times in the distant future it is hard to envision “what might make [one] pay in effect a huge interest rate to reallocate consumption.”<sup>22</sup> It is true that people may sometimes have less information about their future needs than about their present ones, but it is not clear why lack of information alone would cause them to err systematically on the side of underestimating future needs. Given the lack of information about the future, it is quite possible that the need for immediate cash will be much more intense in the future.

16. See John O. Ledyard, *Public Goods: A Survey of Experimental Research*, in THE HANDBOOK OF EXPERIMENTAL ECONOMICS 111, 121-22 (John H. Kagel & Alvin E. Roth eds., 1995).

17. Posner, *supra* note 1, at 1555.

18. *Id.*

19. *Id.* at 1556.

20. See David Laibson, *Golden Eggs and Hyperbolic Discounting*, 112 Q.J. ECON. 443, 445 (1997).

21. See Posner, *supra* note 1, at 1555.

22. *Id.*

### 3. *Bounded self-interest.*

Posner agrees that people do not act only from financial self-interest, and he appears to think that this is important to both economics and law. His chief objection to our discussion of bounded self-interest is that we do not link it with the two other bounds, making our approach to enriching the standard economic model appear disconnected and, in his view, antitheoretical. We do not see how Posner's proposed approach is more theorized, in the relevant sense, than ours, as we will now explain.

### C. *Theory and Antitheory*

As noted, Posner claims that behavioral economics is "undertheorized."<sup>23</sup> Our article should have been entitled, he says, "A Psychological Critique of Economic Analysis of Law" rather than "A Behavioral Approach to Law and Economics."<sup>24</sup> In his view, behavioral economics is "purely empirical," defined "by its subject rather than by its method," and unable to make predictions.<sup>25</sup>

We intended to create—and to show to be warranted—precisely the opposite impression. There have been many important theoretical advances in behavioral economics; a number of them are cited in our article.<sup>26</sup> Consider, for example, prospect theory, the specification of biases and heuristics, and models of fairness-related behavior—all discussed in some detail in our article. Though much remains to be done, each of these contains a set of theoretical claims. Behavioral economists interested in law would predict, for example, that injunctions will stick in nuisance cases, that participants in lawsuits will display self-serving bias, that the allocation of a legal entitlement will matter to the ultimate outcome, that laws will reflect widespread judgments about fairness organized around reference points, that the pattern of environmental regulation will reflect the use of familiar heuristics, that people will be over-optimistic about risk—all topics developed in our article. Of course it is true that there is much more to be learned and that in many contexts good predictions are difficult. To the extent that he is saying more than this, we are left with the impression that Posner's claim of undertheorization is based on Pos-

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23. *Id.* at 1559.

24. *Id.* at 1558.

25. *Id.* at 1559.

26. On bounded rationality, see Daniel Kahneman & Amos Tversky, *Prospect Theory: An Analysis of Decision Under Risk*, 47 *ECONOMETRICA* 263 (1979). On bounded willpower, see Laibson, *supra* note 20; Ted O'Donahue & Matthew Rabin, *Doing It Now or Later* (Berkeley Department of Economics Working Paper No. 97-253, Jan. 1997); Richard H. Thaler & H.M. Shefrin, *An Economic Theory of Self-Control*, 89 *J. POL. ECON.* 392 (1981). (The last two are not cited in our original article.) On bounded self-interest, see Matthew Rabin, *Incorporating Fairness into Game Theory and Economics*, 83 *AM. ECON. REV.* 1281 (1993). New theoretical work is even emerging that uses psychology to explain the anomalous behavior of stock prices. See Nicholas Barberis, Andrei Shleifer & Robert Vishny, *A Model of Investor Sentiment*, 49 *J. FIN. ECON.* (forthcoming Sept. 1998); Kent Daniel, David Hirshleifer & Avanidhar Subrahmanyam, *Investor Psychology and Security Market Under- and Over-reactions*, *J. FIN.* (forthcoming 1998) (manuscript available at <<http://www.cob.ohio-state.edu/dept/fin/journal/abstract.htm#futurearticles>>).

ner's (undefended but more than implicit) view that an essential part of a good theory is that it be a rational choice theory.

Predictions can be hard for any model, standard or behavioral, and a basic problem in judging the success of any model, rational or quasi rational, is knowing what constitutes a true prediction of the model rather than a post hoc explanation. A case in point is Posner's discussion of AIDS. In support of the rational choice model, he cites the prediction that the AIDS epidemic will increase the rate of unwanted pregnancies by "inducing a rational substitution of condoms" for birth-control pills.<sup>27</sup> But the example raises two obvious questions. First, was this really an *ex ante* prediction? Second, if so, is this really a victory for rationality? Isn't it quite possible that a rational woman who viewed both AIDS and pregnancy as undesirable would choose to use both a condom and a pill (despite the cost of using two forms of protection)? How would a rational actor model decide, *ex ante*, between this prediction and the alternative? How, then, does the rational choice approach provide a more "theorized" analysis than the behavioral approach?

Of course, predictions are sometimes robust to departures from unbounded rationality; an example discussed in our article is downward-sloping demand.<sup>28</sup> If economics only consisted of this sort of prediction, many of the bounds we discuss would not matter. But in these cases there would also be no need for (or benefit from) rational choice models. Recall that Posner defines rational as "choosing the best" in terms of benefits and costs, implying an optimal tradeoff analysis (including decisionmaking and information acquisition costs).<sup>29</sup> This optimizing model is much richer than merely buying less of a good when the price goes up; as we pointed out, even laboratory rats do this.<sup>30</sup> The richness adds precision to the theory and also renders it falsifiable (and in many cases, false; Posner's own discussion acknowledges many examples). In defense of rational choice theory, and in urging that we give up on it too quickly, Posner cites our claim that the fact that random choice (by rats or human beings) in a situation of scarcity will generate a downward-sloping demand curve shows that downward-sloping demand "is not evidence in support of optimizing models."<sup>31</sup> He declares our analysis "wrong": "Buyers do not choose randomly. Rationality is the only reasonable explanation for their reactions to changes in relative prices."<sup>32</sup> Wrong. Gary Becker's demonstration that random choice yields downward-sloping demand extends easily to almost any choice behavior.<sup>33</sup> A mix of budget-constrained consumers, some of whom just buy the brand their mother did, some of whom buy anything with Michael Jordan's picture on it, some of whom always buy the brand on the right, and so forth, will also buy less of goods whose prices

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27. Posner, *supra* note 1, at 1557.

28. JST, *supra* note 10, at 1481-82.

29. Posner, *supra* note 1, at 1551.

30. JST, *supra* note 10, at 1481-82.

31. Posner, *supra* note 1, at 1556.

32. *Id.*

33. Gary S. Becker, *Irrational Behavior and Economic Theory*, 70 J. POL. ECON. 1, 5, 7 (1962).

rise (because they have limited budgets). As Becker says, “[T]he negative slope of market demand curves . . . is equally consistent with individual irrationality [and individual rationality] and cannot distinguish between them.”<sup>34</sup> Consumers can be doing almost anything and this result will still hold. (Would Posner respond that whatever they do, people are “choosing the best means to the chooser’s ends”? If so, does that theory make any falsifiable predictions?)

We stress this relatively minor point because much of the evidence Posner marshals in support of rational choice models falls into just this category. We are willing to stipulate that demand curves slope down. But we agree with Becker’s analysis that this result depends not at all on unbounded rationality. We also think that for analyzing behavior related to law, it is necessary to say much more, by way of theory, than that demand curves slope down and that people choose the best means to their ends.

Posner offers a further criticism of the idea that sound predictions about law must take into account the bounds we emphasize. He says that random behavior may often be treated as rational actor behavior because “the distribution of . . . random behaviors” may have the same mean as rational actors’ behavior.<sup>35</sup> This is a common response to behavioral economics, and conceivably it could be true; but there is absolutely no reason to think it is, and (as is usually the case) none is offered by the source of the criticism. Each of the legal applications discussed in our article is an instance in which the evidence suggests that people are likely to err in a systematic direction—and hence that they will behave differently in systematic ways from unboundedly rational agents.

#### D. *Evolutionary Biology*

Following Karl Popper, Posner is enthusiastic about falsifiability as the essential feature of a successful scientific theory.<sup>36</sup> For present purposes we intend no quarrel with this idea. Posner is even more enthusiastic about evolutionary biology, and he thinks that it is important to show that behavior which departs from traditional economic assumptions is a result of evolutionary forces.<sup>37</sup> For present purposes we do not intend to quarrel with evolutionary biology either. (As we stated in our article, the notion that the bounds on human behavior are adaptive is wholly consistent with our approach.<sup>38</sup>) But it is not easy to stand, as Posner does, with both Popper and evolutionary biology. The problem is that Popper’s views have been subject to criticism in the

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34. *Id.* at 13.

35. Posner, *supra* note 1, at 1556.

36. *Id.* at 1560-61.

37. *Id.* at 1561-64.

38. JST, *supra* note 10, at 1477-78. Thus, we do not disagree with the idea that “while it may appear that the agents are acting irrationally,” it is possible that they are “processing incomplete information as well as it can be processed.” Kelman, *supra* note 4, at 1583. For instance, the use of “more general rules of thumb about when information is relevant” may be the best approach under the circumstances, “even if it results occasionally in the use of information that is not in fact probative.” *Id.*

philosophy of science, and evolutionary biology is part of the standard criticism of Popper. The reason is that evolutionary biology is an unfalsifiable theory.

We are agnostic on the question whether biology can explain the behavior we discuss in our article, but we do not think that Posner has shown that it can. Consider, for example, his claim that the endowment effect (in which opportunity costs are underweighted relative to out-of-pocket costs) or the sunk cost fallacy can be derived from evolution.<sup>39</sup> No doubt it is possible to produce an evolutionary model in which paying attention to sunk costs, or displaying loss aversion, is adaptive. The problem is that it is also possible, indeed easier, to derive another model in which ignoring sunk costs, and equating opportunity costs to out-of-pocket costs, is also adaptive (based on the standard reasoning offered to economics students who are being taught why sunk costs should be ignored and opportunity costs equated with out-of-pocket costs). Indeed, there is a long tradition in economics, going back at least to Milton Friedman, of arguing that such forces are precisely why the assumptions of economics must be true (that competition or evolution will drive out the people who don't do what they are supposed to).<sup>40</sup> It is difficult to see what conclusions should be drawn from the fact that evolution can be shown to produce a behavior *and* the absence of that behavior.

Note that we are not saying that biology is not capable of making predictions. We are simply saying that the fact that one can tell an evolution-based story for why a particular behavior might have emerged neither implies the existence of a falsifiable theory nor (more importantly) is particularly helpful. To do better economic analysis we need theories that can help us predict when sunk costs are more likely to be ignored, or when opportunity costs are more likely to be given full weight. So far, however, such insights have come more from psychology than from biology.

We agree that much of the behavior we discuss can be considered a vestige of our long held instincts. In the case of bounded willpower, this is obviously true. The instinct to eat when hungry is present in every species, but it can get one in trouble when food becomes abundant and exercise unnecessary. Ancient man needed neither Weightwatchers nor Stairmasters. Aspects of bounded rationality can also be explained, speculatively but plausibly, in evolutionary terms; the heuristics and biases literature in psychology is explicitly based on the premise that the heuristics which emerge are those that are useful on average.<sup>41</sup> Availability is highly correlated with frequency, and using the availability heuristic may be a sensible strategy in most circumstances. Behavior that has survived the test of time may or may not have a lot to do with biology, and while it would be interesting (and in some ways potentially valuable) to know, a behavioral approach to the economic analysis of law can proceed whether or not the evolutionary account is right.

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39. Posner, *supra* note 1, at 1562-63, 1565.

40. MILTON FRIEDMAN, *The Methodology of Positive Economics*, in *ESSAYS IN POSITIVE ECONOMICS* 3, 21-22 (1953).

41. *See* JST, *supra* note 10, at 1477-78.



E. *Smaller Points*

We do not intend here to respond to all of Posner's particular claims. In many places he has raised good questions for further research, behavioral and otherwise.

1. *The ultimatum game and the endowment effect.*

Posner claims that our analysis of the ultimatum game is merely a "labeling of the result of the game."<sup>42</sup> He prefers an analysis that depends on what he calls "negative altruism" and "signaling."<sup>43</sup> We are not sure that we understand the difference between his negative altruism explanation and our own; here is another case where Posner's "enriched" version of economics is simply a form of behavioral economics. Where we part company has to do with signaling. If the signaling explanation has any content, then it would seem to predict that Responders would accept small offers (such as the fifty cents offered by the least generous of the Proposers in our study) as long as their actions were completely anonymous (not known to the Proposer or anyone else including the experimenter.) Of course, these are exactly the conditions in our experiments (as well as the other ultimatum game experiments in the literature). How then can their actions be called signaling—unless they are signaling to themselves?

Posner also has various explanations for why we might observe the endowment effect within a rational choice model.<sup>44</sup> Though he claims that these explanations are falsifiable, he offers neither evidence in support of them nor tests he would accept as valid. For example, he points out that an endowment effect might be rational for a good with no substitutes, and that this observation might explain the large discrepancies that are observed between willingness to pay and willingness to accept for environmental goods.<sup>45</sup> However, this observation cannot explain the difference in buying and selling prices for the coffee mugs in the mugs experiments (since the mugs were for sale in the campus book store). This suggests that the absence of substitutes cannot be the only explanation for the observed behavior.

Posner also suggests that the outcome of the mugs experiments might be explained by rational habit formation.<sup>46</sup> He does not say exactly how this works; one possibility is that we are used to thinking, correctly, that we like the things we own because we picked them out ourselves and have subsequently become accustomed to them. In the mugs experiment, this is not the case, but perhaps we act, out of habit, as if the mug had been in our possession for a long time. This is an interesting speculation, though no empirical support is offered on its behalf. More important, suppose this explanation is true. This in no way alters the behavioral analysis. Recall that the mugs ex-

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42. Posner, *supra* note 1, at 1564.

43. *Id.* at 1564-65.

44. *Id.* at 1565-67.

45. *Id.* at 1566.

46. *Id.* at 1566-67.

periments were designed to test (i.e., falsify) a prediction of behavioral economics, namely that the ultimate allocation of property rights would depend on the initial assignment of those rights even when transaction costs were zero. The experimenters were unable to reject this prediction. The “prebehavioral economic analysis” (to use Posner’s phrase<sup>47</sup>—one we like!) of this experiment was surely that the mugs market would be identical to the token market; that is, the Coase theorem would hold. The behavioral economic analysis is that the granting of property rights will affect the allocation of those rights. Posner’s “habit formation” hypothesis leads to exactly the same prediction as the behavioral analysis, and hence to rejection of the Coase theorem. We are not sure about the hypothesis, but we are pleased to agree on the prediction, and we are looking forward to a revised treatment of the Coase theorem in the next edition of *Economic Analysis of Law*.

## 2. *Crime, Optimism, and Childbirth.*

Posner has various responses to our interpretation of Jon Gruber’s empirical study of childbirth benefits.<sup>48</sup> Many of these responses are acknowledged in our original discussion; we expressly stated that the endowment effect is not the only possible explanation for the empirical findings. One response that we did not mention—and that strikes us as particularly odd—is Posner’s objection to our stressing the role of one variable (the endowment effect) while retaining the rest of the rational choice apparatus. Why, he asks, do we not think that other behavioral factors were not also at play?<sup>49</sup> The answer, of course, is simplicity. We add complexity one step at a time just as he does. When he suggests that signaling, asymmetric information, risk aversion, altruism, and other traditional rational choice add-ons provide an alternative explanation for some phenomenon, he does not say why another factor is not also present. We do not challenge this way of proceeding. Users of both methods are forced (because of bounded rationality) to enrich their models one step at a time. It is, of course, possible to combine two features in one analysis. This is exactly what we have done in our sunk cost ultimatum game. We used the mental accounting of sunk costs to predict that Responders would demand even more (closer to half the amount to be divided) if they had put up the money themselves. We then attempted to falsify this prediction, unsuccessfully.

Still, Posner grants that we are “on to something.”<sup>50</sup> He just predicts that rational choice plus evolutionary biology will prove more useful in understanding these phenomena. Time will tell.

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47. *Id.* at 1552.

48. *Id.* at 1568-70.

49. *Id.* at 1569-70.

50. *Id.* at 1570.

### 3. *Empirical Evidence.*

Posner says that we make “exaggerated claims for the empirical robustness of behavioral economics.”<sup>51</sup> What, he asks, is the theoretical or empirical basis for supposing that the experimental environment is similar to the real world? This is a perfectly reasonable question that behavioral economists have been busy exploring. For example, the original research on both mental accounting and prospect theory was experimental. Yet these concepts have proven useful in explaining field phenomena as diverse as the asset allocation decisions of pension plan participants, the behavior of cab drivers, and the equity premium puzzle.<sup>52</sup> We await similar tests of the alternatives Posner favors.

One of the best examples of doing precisely what Posner seems to want is the study of self-serving bias by Linda Babcock, Xianghong Wang, and George Loewenstein.<sup>53</sup> Posner misinterprets the results of this study. He is right, of course, that there is nothing surprising in the fact that each side in a lawsuit will publicly adopt self-serving analogues. And he grants that there is such a thing as a role bias. But he is wrong in concluding that this study adds nothing, or that the survey respondents are just posturing to the researchers (as well as to their counterparts).<sup>54</sup> What the authors show is that the answers the two parties give to the survey help explain the incidence of strikes. Why, if the two sides are just posturing, would their “meaningless” answers help explain their actual behavior? It seems that, while posturing to the other side (or even to the researchers), the bargainers end up posturing to themselves in a way that they have difficulty overcoming. We also refer readers once again to the study’s own discussion of the problem of strategic behavior and the evidence against this explanation for the study’s findings.<sup>55</sup>

Posner is also incorrect in asserting that the empirical findings on whether parties bargain around court orders “vindicate rather than challenge rationality.”<sup>56</sup> Posner reasons that “[i]f a case that has become final through exhaustion of appellate remedies could have been settled, because the remedy sought by the plaintiff would cost the defendant more than it would benefit the plaintiff, the case would have been settled earlier—at the latest after the judgment in the trial court and before the appeal.”<sup>57</sup> We fail to see this reasoning. For if the defendant thinks there is a good chance a verdict for the plaintiff will be overturned on appeal, it will have no reason to settle the case prior to appeal,

51. *Id.*

52. See Schlomo Benartzi & Richard H. Thaler, Risk Aversion or Myopia? Choices in Repeated Gambles and Retirement Investments (Nov. 8, 1997) (unpublished manuscript, on file with the *Stanford Law Review*); Colin Camerer, Linda Babcock, George Loewenstein & Richard Thaler, *Labor Supply of New York City Cabdrivers: One Day at a Time*, 112 Q.J. ECON. 407 (1997); Jeremy J. Siegel & Richard H. Thaler, *Anomalies: The Equity Premium Puzzle*, 11 J. ECON. PERSP., Winter 1997, at 191.

53. Linda Babcock, Xianghong Wang & George Loewenstein, *Choosing the Wrong Pond: Social Comparisons in Negotiation that Reflect a Self-Serving Bias*, 111 Q.J. ECON. 1 (1996).

54. Posner, *supra* note 1, at 1573.

55. Babcock et al., *supra* note 53, at 17-18.

56. Posner, *supra* note 1, at 1572.

57. *Id.* at 1571-72.

even if the plaintiff's remedy would cost the defendant more than it would benefit the plaintiff; the defendant is hoping to have to pay nothing. In any event, we offer the empirical findings on settlement not to challenge "rationality," but to suggest that motivations other than material self-interest help to account for behavior relevant to law—a point to which Posner offers no objection.

#### 4. *Bureaucrats and debiasing.*

Posner's commentary on our normative analysis is misplaced. We never suggested that experts would be charged with determining the populace's "authentic preferences."<sup>58</sup> We do not think people have orderly preferences, so charging anyone with determining them would be a meaningless assignment. Nor do we suggest that bureaucrats would be immune from bounded rationality. Indeed, we emphasize precisely the opposite position.<sup>59</sup>

Posner also faults us for not advocating education and psychiatry to cure "cognitive quirks and weakness of will."<sup>60</sup> In some circumstances, debiasing techniques may work for instances of bounded rationality, and this is an important area for further investigation. We mention several specific applications in our article.<sup>61</sup> Perhaps education and psychiatry can help with bounded willpower as well, but the problem has been with us at least since the time of Adam and Eve, and we are not optimistic about a cure any time soon. In any case, while educators and psychiatrists are doing their best to reduce the impact of bounded rationality and bounded willpower, we will continue our efforts to model these phenomena and learn more about how they affect the economics of the law, and also about how their harmful effects might be reduced. We hope that no one will attempt to cure bounded self-interest.

#### 5. *Equity premium puzzle.*

Posner suggests that we have succumbed to the hindsight bias; he says we conclude that people are irrationally risk averse from the fact, known only in hindsight, that stock returns have been so high.<sup>62</sup> We should note that we are not responsible for declaring the high returns on equities a "puzzle." The puzzle was declared by rational choice theorists Mehra and Prescott, who were admirably attempting to make the theory of investment decision-making falsifiable by asking how large an historical equity premium would be consistent with reasonable (in their view) levels of risk aversion.<sup>63</sup> They decided that the returns up to the mid 1980s, when they wrote their article, were too high. Since their article was written, the equity premium in the United States

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58. *Id.* at 1575.

59. JST, *supra* note 10, at 1475, 1541, 1543-45.

60. Posner, *supra* note 1, at 1575.

61. JST, *supra* note 10, at 1504, 1527, 1544.

62. Posner, *supra* note 1, at 1572 n.41.

63. See Siegel & Thaler, *supra* note 52, at 192, for a discussion. The original article is Rajnish Mehra & Edward C. Prescott, *The Equity Premium: A Puzzle*, 15 J. MONETARY ECON. 145 (1985).

has been even higher than in the past, and a substantial equity premium has been found in most countries around the world.<sup>64</sup> These facts provide out-of-sample tests of the equity premium puzzle.

## II. KELMAN

Professor Kelman has considerable sympathy for behavioral economics. His basic complaint is that far from providing a “fuller, more accurate” account of human behavior, behavioral economics should recognize that it is merely one of “a variety of interpretive traditions” engaged in making up “stories” about social practices.<sup>65</sup> Kelman thinks that behavioral economics and rational choice theory are “bound together in a form of rhetorical duet or ritualized dance.”<sup>66</sup> In his view, the two therefore stand together in “irreconcilable mutual dependence.”<sup>67</sup> Kelman also contends that behavioral economics mostly offers “anecdot[es].”<sup>68</sup> In Kelman’s view, this is not so much a criticism as a suggestion about the need to avoid “hubris.”<sup>69</sup> In light of “the rich, inexorably overwhelming data with which we have to deal,” “open-textured interpretivism” is all that we have.<sup>70</sup> His complaint is that behavioral economists, and the behavioral approach to law and economics, claim to provide more than that.

### A. Tropes?

It is not clear to us why Professor Kelman prefers “interpretive tropes” to what behavioral economists are trying to do. Kelman does not show that analysts are unable to develop and to test hypotheses about the relationship between incentives, including law, and human behavior. We hope to have suggested a number of promising examples in our article. To be sure, in many areas we know much less than we should; as we emphasized throughout the article, one of our most important goals in this project is to outline an extended research agenda. The solution to this problem is to do more investigating in order to figure out which hypotheses are true. There is nothing hubristic about that.

Kelman thinks that our hope that behavioral economics can supplement standard economics is wrong because they are in “irreconcilable mutual dependence.”<sup>71</sup> We do not understand this objection. While it is surely true that behavioral economics relies greatly on traditional economic tools (since it is, indeed, merely an approach to doing economics), behavioral economics is in-

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64. See Siegel & Thaler, *supra* note 52, at 193-94. The equity premium has risen since 1985 because of the exceptionally high returns on stocks. See *id.*

65. Kelman, *supra* note 4, at 1580.

66. *Id.*

67. *Id.*

68. *Id.*

69. *Id.* at 1590.

70. *Id.* at 1591.

71. *Id.* at 1580.

tended as an improvement because it contains more realistic assumptions about human behavior.

Of course Kelman is right to say that some imaginable debates amount to stylized rhetorical patterns, or "moves," involving those who are not and those who are committed to traditional rationality assumptions. Psychologists concerned with bounded rationality may claim to have found hindsight bias; those committed to unbounded rationality may say that what appears to be hindsight bias is really not that at all, but rather is a normative judgment about the appropriate legal regime. Behavioral economists may point to the results of the ultimatum game as suggesting that people will sacrifice some money to punish someone who they think has mistreated them; traditional economists may reply (as Kelman does in their name) that people care about various psychic goods and are willing to sacrifice their material self-interest to obtain these goods.<sup>72</sup> And so on.

Some debates may indeed have this form. But taken in this stylized way, the disagreements between the two camps are awfully tedious. The real task is to be clear about both the hypothesis being tested and the data, and to go back and forth between them to see whether the hypothesis in question can be falsified.

Consider a few examples. Of course there is nothing to prevent economists from saying that because people care about "psychic goods," the results of the ultimatum game are no surprise. But this response has a major defect. It either makes rational choice theory incapable of generating predictions at all (because whatever happens can be said to have been a result of the relevant utility function, constructed *ex post*), or it converts rational choice theory into a primitive form of behavioral economics. It is plainly better to do what behavioral economists are doing, that is, to be explicit about the bounded nature of self-interest and to see what hypotheses fit the data and might be used to make predictions, and then test them. Indeed, it is worth remembering that the ultimatum game was invented precisely to test ideas about resisting unfair behavior.<sup>73</sup>

Or consider Kelman's claim that hindsight bias is not what it appears to be and the results may well reflect a form of rational Bayesian updating or normative judgments about liability schemes.<sup>74</sup> This is an intriguing claim that is difficult to evaluate without seeing the relevant experiments, but Kelman does not say anything to undermine the central empirical work on hindsight bias. Consider, for example, Baruch Fischhoff's early study, where people were asked to say how likely certain events were to happen during

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72. *Id.* at 1585 n.24.

73. See Werner Güth, Rolf Schmittberger & Bernd Schwarze, *An Experimental Analysis of Ultimatum Bargaining*, 3 J. ECON. BEHAV. & ORG. 367, 367-68 (1982). Güth, Schmittberger, and Schwarze, after defining the ultimatum game, proceeded to test people's behavior in the game. See *id.* at 373-77.

74. Kelman, *supra* note 4, at 1583-84.

Nixon's trip to China, and then asked to remember their earlier predictions.<sup>75</sup> Their remembered probabilities were higher for things that happened. This is surely a bias. Or consider the work by Colin Camerer and others on the "curse of knowledge," in which subjects who are told the actual earnings for a company are unable to give good estimates of what other subjects, who do not know the outcome, will predict.<sup>76</sup> Of course there may be some studies that attribute some behavior to hindsight bias where some other interpretation is possible, or even plausible.

With respect to the equity premium puzzle, Kelman thinks that a surprisingly high equity premium gives people the profitable opportunity to sell bonds to unwitting investors and use the proceeds to buy stocks, making a killing.<sup>77</sup> Although this critique is based on standard economic thinking, the analysis is flawed. The opportunity Kelman identifies is not a true arbitrage opportunity (meaning without risk); as we suggested in our article, there are costs to arbitrage in equity markets.<sup>78</sup> Investment bankers could try what Kelman suggests, at their own risk, assuming they could convince investors that their bonds were risk-free (despite the risks the banks were taking on). If enough people could do this, then the equity premium might shrink. But this is precisely the kind of question that behavioral economists have been successfully addressing, namely, how do the forces of competition and arbitrageurs interact with quasi rational agents?<sup>79</sup>

Kelman is right to emphasize the importance of sorting out possible alternative explanations and of understanding what kinds of effects are at work in different settings. Our basic point is that we have important empirical issues here, and no reason to think that we are left only with "interpretive tropes" or "dances." Dancing has its place, but in this context, people should stop dancing and get to work.

## B. *Incompleteness*

Sounding a bit like Posner, Kelman also complains that behavioral economics is an incomplete theory.<sup>80</sup> He objects, for example, that behavioral economics cannot predict the domain in which quasi rationality will be important; that we do not know when and how much people will sacrifice their material interest for the sake of fairness; that the relationship between legisla-

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75. Baruch Fischhoff, *Hindsight ≠ Foresight: The Effect of Outcome Knowledge on Judgment Under Uncertainty*, 1 J. EXPERIMENTAL PSYCHOL. HUM. PERCEPTION & PERFORMANCE 288, 297 (1975).

76. Colin Camerer, George Loewenstein & Martin Weber, *The Curse of Knowledge in Economic Settings: An Experimental Analysis*, 97 J. POL. ECON. 1232 (1989).

77. Kelman, *supra* note 4, at 1586-87.

78. JST, *supra* note 10, at 1485.

79. See Jeffrey Pontiff, *Costly Arbitrage: Evidence from Closed-End Funds*, 111 Q.J. ECON. 1135 (1996); Thomas Russell & Richard H. Thaler, *The Relevance of Quasi Rational Behavior in Competitive Markets*, in RICHARD H. THALER, QUASI RATIONAL ECONOMICS 239 (1991); Andrei Shleifer & Robert W. Vishny, *The Limits of Arbitrage*, 52 J. FIN. 35 (1997).

80. Kelman, *supra* note 4, at 1580, 1586-90.

tive behavior and widely shared fairness norms is ambiguous;<sup>81</sup> that the use of the availability heuristic to explain environmental law leaves many gaps in understanding the demand for environmental regulation. Generally speaking, we agree. There is a lot of work to do. To be sure, we already have far more than “counterstories.” Matthew Rabin, for example, has developed a model of fairness behavior,<sup>82</sup> and there is a good deal of empirical work (cited and discussed in our article) in this domain. The role of the availability heuristic in producing regulation has already received considerable attention (again, examples are cited in our article). This is not to say that all of Kelman’s questions have been answered; as we have repeatedly said, we think there is a lot more to do.

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Our discussion of the legal examples just mentioned, and the others in our article, was intended both as an effort to make new progress in behavioral law and economics, and, equally importantly, an effort to suggest directions for future research. One of our central points is that behavioral law and economics is “incomplete” at this extremely early stage. A great deal remains to be learned about the actual (as opposed to hypothesized) relationship between law and human behavior. We hope that Posner and Kelman, both occasional past practitioners of behavioral economics, will join the many people now engaged in that endeavor.

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81. Kelman misunderstands us when he suggests that we believe that legislators “seek reelection above all.” Kelman, *supra* note 4, at 1590. We have only assumed this for purposes of analysis.

82. Rabin, *supra* note 26.