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Tendencies Versus Boundaries: Levels of Generality in Behavioral Law and Economics

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Gregory Mitchell[*]

When evidence on the truth or falsity of a proposition is ambiguous and open to multiple interpretations, psychologists warn about "biased assimilation" of the evidence to support pre-existing theories, beliefs, and attitudes.[1] Therefore, when a skeptic about the

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1. Biased assimilation is said to occur when "the same body of evidence is differentially evaluated by those on opposite sides of an issue." John W. McHoskey, *Case Closed? On the John F. Kennedy Assassination: Biased Assimilation of Evidence and Attitude Polarization*, 17 BASIC & APPLIED SOC. PSYCHOL. 395, 396 (1995). In the classic case of biased assimilation, "[e]vidence that is supportive of one's position is uncritically accepted, whereas contrary evidence is scrutinized and (subjectively) discredited." *Id.*; *see also* Cbarles G. Lord et al., *Biased Assimilation and Attitude Polarization: The Effects of Prior Theories on Subsequently Considered Evidence*, 37 J. PERSONALITY & SOC. PSYCHOL. 2098, 2099 (1979) ("Thus, there is considerable evidence that people tend to interpret subsequent evidence so as to maintain their initial beliefs. The biased assimilation processes underlying this effect may include a propensity to remember the strengths of confirming evidence but the weaknesses of disconfirming evidence, to judge confirming evidence as relevant and reliable but disconfirming evidence as irrelevant and unreliable, and to accept confirming evidence at face value while scrutinizing disconfirming evidence hypercritically."). Anderson and Lindsay differentiate two types of assimilation bias: (1) a bias to view the perceived relevance and quality of data either positively or negatively in order to assimilate the data to one's pre-existing views and (2) a hias to perceive two variables to covary either strongly or weakly in order to assimilate the covariation evidence to pre-existing views. *See* Craig A. Anderson & James J. Lindsay, *The Development, Perseverance, and Change of Naïve Theories*, 16 SOC. COGNITION 8, 17-18 (1998) (discussing "Assimilation Bias # 1" and "Assimilation Bias # 2").

public policy implications of psychological research examines the complex mix of evidence on human rationality, he may find much to support his skepticism about the use of psychology to reform the law. Likewise, an optimist about the public policy contributions of psychology may find within this same body of evidence much to bolster his optimistic view that psychological research can be used to refashion the law to better predict and regulate human behavior.

Two of my previous articles on the subject of behavioral law and economics,[2] or, as I prefer to call the field, legal decision theory,[3] and Robert Prentice's excellent discussion of my articles and of legal decision theory in general may illustrate the biased assimilation phenomenon at work.[4] Indeed, Professor Prentice and I often cite the very same works to support our different perspectives on legal decision theory—with Prentice's article emphasizing how much we know about the quasi-rationality of human judgment and decision making and my articles emphasizing how little we know in light of the complexity of the evidence.[5] Notwithstanding the possible influence of

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The good news is that a strategy of consciously considering opposing viewpoints and opposite possibilities has been shown somewhat effective in countering biased-assimilation processes. *See, e.g.*, Charles G. Lord et al., *Considering the Opposite: A Corrective Strategy for Social Judgment*, 47 J. PERSONALITY & SOC. PSYCHOL. 1231, 1239 (1984) ("In two different domains of social judgment, biased assimilation of new evidence and biased hypothesis testing, and with two different inducements, direct instructions and indirect manipulation of accessibility through stimulus salience, the cognitive strategy of considering opposite possibilities promoted impartiality." (footnote omitted)). Therefore, exchanges such as this one between Professor Prentice and myself may encourage openmindedness that decreases the likelihood of biased assimilation of the empirical evidence on human rationality and may even advance the scientific consideration of the issue: "The ability to see different sides of an issue and to evaluate each objectively is a hallmark of science." Thomas Lee Budesheim & Arlene Lundquist, *Consider the Opposite: Opening Minds Through In-Class Debates on Course-Related Controversies*, 26 TEACHING PSYCHOL. 107, 109 (1999).

2. *See* Gregory Mitchell, *Taking Behavioralism Too Seriously? The Unwarranted Pessimism of the New Behavioral Analysis of Law*, 43 WM. & MARY L. REV. 1907 (2002) [hereinafter Mitchell, *Pessimism*]; Gregory Mitchell, *Why Law and Economics' Perfect Rationality Should Not Be Traded for Behavioral Law and Economics' Equal Incompetence*, 91 GEO. L.J. 67 (2002) [hereinafter Mitchell, *Incompetence*].

3. *See* Mitchell, *Pessimism*, *supra* note 2, at 1915 n.12 (explaining my preference for the label "legal decision theory" rather than behavioralism or behavioral law and economics); Mitchell, *Incompetence*, *supra* note 2, at 78-79 (same).

4. Robert A. Prentice, *Chicago Man, K-T Man, and the Future of Behavioral Law and Economics*, 56 VAND. L. REV. 1663 (2003). Professor Prentice's primary goal in this article is to show that the behavioral law and economics' movement "retains great potential to add valuable insights to legal scholarship, despite Mitchell's withering attack" on the movement. *Id.* at 1670.

5. Professor Prentice rightly implies that I am a skeptic, and perhaps even a pessimist, about the reliability and generalizability of much psychological research, but I do not broadly view psychological research as bogus or false. *See id.* at 1718 ("Mitchell's two main points in the first article appear to be that (a) psychology is a questionable science whose methods systematically overstate limitations on human judgment, and (b) legal decision theorists have inexpertly applied its precepts to legal doctrine. He overstates both points.").

our predispositions, both of our readings of the evidence bring out valid points about the possibilities and limitations of legal decision theory.

Not yet content to call our debate a draw or to concede defeat, however, in this reply to Professor Prentice's article I offer some additional thoughts in favor of a modest approach to revising the law's assumption of rationality, as compared to the bolder approach supported by Professor Prentice. I begin by discussing how the ambiguous nature of much of the evidence on human rationality may explain some of the differences between Professor Prentice and me. Next, I turn to the larger question of whether legal decision theorists describe behavior at too general a level to be useful in the formulation of legal policy. In particular, I discuss the tendency of legal decision theorists to speak in terms of "behavioral tendencies" and how this approach to behavioral description may impede the progress of behavioral law and economics. I contend that legal decision theorists have placed too great an emphasis on finding and describing behavioral tendencies toward irrationality, without due regard for the boundary conditions on these supposed tendencies. As a result, much of the interesting and important information about the constraints on rational versus irrational behavior is consigned to *ceteris paribus*

Prentice is correct also to say that I worry that behavioral decision theory overstates limitations on human cognitive functioning and about legal decision theorists' applications of behavioral decision theory to the law. For a recent critique of social and cognitive psychology along these lines, see Joachim I. Krueger & David C. Funder, *Towards a Balanced Social Psychology: Causes, Consequences and Cures for the Problem-Seeking Approach to Social Behavior and Cognition*, 26 BEHAV. & BRAIN SCI. (forthcoming 2003). It is important to note that I am not a skeptic about the inaccuracy of the "perfect rational actor" model, as I have repeatedly emphasized in prior work. *See, e.g.*, Mitchell, *Pessimism*, *supra* note 2, at 1936 ("[M]y criticisms of legal decision theory should not be seen as an argument that human decision makers predominantly act rationally and only occasionally make computational errors."); Mitchell, *Incompetence*, *supra* note 2, at 69 ("Law and economics' perfect rationality assumption is drawn from neoclassical microeconomic theory and is refutable as an empirical matter because empirical studies often find participants whose behavior systematically deviates from economic definitions of rationality."). The fact that the rational actor model is at times grossly inaccurate does not necessarily mean, however, that existing behavioral research provides more accurate empirical models or better prescriptive guidance.

I believe it is fair to label Prentice an optimist about the reliability and generalizability of psychological research based on statements in his article. For instance, he states that

> Mitchell's own sources emphasize that psychology is a respected science with research that produces results that are often as reliable as those of medical science and that uses many of the same methods of the hard sciences. Research results in psychology are roughly as consistent as those in physics, and as reliable as many prominent findings in medical science.

Prentice, *supra* note 4, at 1719 (footnotes omitted); *see also id.* at 1771 ("Policy prescriptions based on complicated but very real facts have more promise than those based on elegant but very wrong theory."). Of course, Prentice may view himself more as a realist than an optimist, that is, as someone who is giving a realistically optimistic reading of the research.

clauses and treated as "noise" that should be ignored and controlled rather than elucidated and understood.

I. IS THE RATIONALITY GLASS HALF FULL OR HALF EMPTY?

In back-to-back articles in a recent issue of the *Journal of Economic Perspectives*, two prominent economists debate the status of the efficient capital markets hypothesis, which in its strong form predicts that prices in securities markets will perfectly reflect all relevant and available information, including inside information.[6] This debate raises important questions about whether the market weeds out irrational behavior on the part of investors, brokers, and firms and thus has important implications for how the law should regulate securities markets, as Professor Prentice and others note.[7]

In the first of these articles, Burton Malkiel surveys behavioral finance research on market "'anomalies' and statistically significant predictable patterns in the stock returns" that ostensibly refutes the efficient markets hypothesis and concludes that

> these patterns are not robust and dependable in different sample periods, and some of the patterns based on fundamental valuation measures of individual stocks may simply reflect better proxies for measuring risk.
>
> Moreover, many of these patterns, even if they did exist, could self-destruct in the future, as many of them have already done. Indeed, this is the logical reason why one should be cautious not to overemphasize these anomalies and predictable patterns. . . .

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6. Professor Fama provides perhaps the classic formulation of the hypothesis:

> The primary role of the capital market is allocation of ownership of the economy's capital stock. In general terms, the ideal is a market in which prices provide accurate signals for resource allocation: that is, a market in which firms can make production-investment decisions, and investors can choose among the securities that represent ownership of firms' activities under the assumption that security prices at any time "fully reflect" all available information. A market in which prices always "fully reflect" available information is called "efficient."

Eugene F. Fama, *Efficient Capital Markets: A Review of Theory and Empirical Work*, 25 J. FIN. 383, 383 (1970). Fama acknowledges that more detail must be added to make tbe thesis falsifiable. *See id.* at 384 ("The definitional statement that in an efficient market prices 'fully reflect' available information is so general that it has no empirically testable implications. To make the model more testable, the process of price information must be specified in more detail. In essence we must define somewhat more exactly what is meant by the term 'fully reflect.'").

7. *See* Prentice, *supra* note 4, at 1703-04 (presenting evidence on the survival of psychological biases and errors in market settings); *see also id.* at 1725 n.326 (discussing the debate over the efficiency of markets containing irrational investors). In a recent article, Professors Choi and Pritchard consider at length possible legal implications of evidence from behavioral economics on market irrationalities. *See* Stephen J. Choi & Adam C. Pritchard, *Behavioral Economics and the SEC*, 56 STAN. L. REV. 1 (2003).

> Any truly repetitive and exploitable pattern that can be discovered in the stock market and can be arbitraged away will self-destruct.[8]

Malkiel thus predicts ultimate success for the efficient markets model despite behavioral finance's challenge: "I suspect that the end result will not be an abandonment of the belief of many in the profession that the stock market is remarkably efficient in its utilization of information."[9]

In the second article, Robert Shiller offers a very different perspective on the strength of the evidence against the efficient markets hypothesis:

> After all the efforts to defend the efficient markets theory, there is still every reason to think that, while markets are not totally crazy, they contain quite substantial noise, so substantial that it dominates the movements in the aggregate market. The efficient markets model, for the aggregate stock market, has still never been supported by any study effectively linking stock market fluctuations with subsequent fundamentals.[10]

Shiller thus concludes that "[w]hile theoretical models of efficient markets have their place as illustrations or characterizations of an ideal world, we cannot maintain them in their pure form as accurate descriptors of actual markets."[11] Indeed, "efficient markets theory may lead to drastically incorrect interpretations of events such as major stock market bubbles."[12]

At first glance, these conclusions seem strikingly at odds. Upon looking more closely, however, one sees that Professors Malkiel and Shiller really do not disagree about the inaccuracy of the efficient

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8. Burton G. Malkiel, *The Efficient Market Hypothesis and Its Critics*, 17 J. ECON. PERSP. 59, 71-72 (2003); *see also id.* at 60 ("I conclude that our stock markets are far more efficient and far less predictable than some recent academic papers would have us believe."). Professor Malkiel notes that

> [t]he efficient market hypothesis is associated with the idea of a "random walk," which is a term loosely used in the finance literature to characterize a price series where all subsequent price changes represent random departures from previous prices. The logic of the random walk idea is that if the flow of information is unimpeded and information is immediately reflected in stock prices, then tomorrow's price change will reflect only tomorrow's news and will be independent of the price changes today. But news is by definition unpredictable, and, thus, resulting price changes must be unpredictable and random. As a result, prices fully reflect all known information, and even uninformed investors buying a diversified portfolio at the tableau of prices given by the market will obtain a rate of return as generous as that achieved by the experts.

Id. at 59. In other words, under the efficient markets hypothesis, stock prices should be unpredictable, and so evidence of predictable changes in stock prices refutes the hypothesis but should also provide profiteering opportunities to those who can successfully predict the price changes.

9. *Id.* at 80.

10. Robert J. Shiller, *From Efficient Markets Theory to Behavioral Finance*, 17 J. ECON. PERSP. 83, 90 (2003).

11. *Id.* at 102.

12. *Id.* at 101.

capital markets hypothesis, but simply differ in their views about the importance of the behavioral finance evidence. Whereas Malkiel argues that the efficient markets model, despite its failures, provides a better investment guide than the collection of anomalies found within behavioral finance, Shiller argues that behavioral finance research falsifies the efficient markets hypothesis in several important respects regardless of whether the anomalies revealed through this research translate into profit opportunities.[13]

Similar evaluative processes are at work in the recent assessments of legal decision theory by Professor Prentice and me. Professor Prentice emphasizes how thoroughly behavioral research refutes the rational actor hypothesis embraced by law and economics, and he provides an impressive array of examples of this refutation. I accept that the rational actor hypothesis has been refuted, but I question the robustness of these refutations and argue that their implications for the law are not clear (i.e., Prentice emphasizes the bounds on rationality, while I emphasize the bounds on irrationality.)[14] The enabling factor behind our equally credible but

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13. *Compare* Shiller, *supra* note 10, at 101 ("In judging the impact of behavioral finance to date, it is important to apply the right standards. Of course, we do not expect such research to provide a method to make a lot of money off of financial market inefficiency very fast and reliably."), *with* Malkiel, *supra* note 8, at 60 ("[T]he evidence is overwhelming that whatever anomalous behavior of stock prices may exist, it does not create a portfolio trading opportunity that enables investors to earn extraordinary risk adjusted returns."). Malkiel further writes:

> What I do not argue is that market pricing is always perfect. After the fact, we know that markets have made egregious mistakes, as I think occurred during the recent Internet "bubble." Nor do I deny that psychological factors influence securities prices. But I am convinced that Benjamin Graham was correct in suggesting that while the stock market in the short run may he a voting mechanism, in the long run it is a weighing mechanism. True value will win out in the end. Before the fact, there is no way in which investors can reliably exploit any anomalies or patterns that might exist. I am skeptical that any of the "predictable patterns" that have been documented in the literature were ever sufficiently robust so as to have created profitable investment opportunities, and after they have been discovered and publicized, they will certainly not allow investors to earn excess returns.

Malkiel, *supra* note 8, at 61 (citation omitted). Shiller further writes:

> [T]he mere fact that anomalies sometimes disappear or switch signs with time is no evidence that the markets are fully rational. That is also what we would expect to see happen even in highly irrational markets. (It would seem peculiar to argue that irrational markets should display regular and lasting patterns!)

Shiller, *supra* note 10, at 102.

14. Several years ago Keren and Wagenaar made the useful point that there are bounds on both our rationality and irrationality. *See* Gideon Keren & Willem A. Wagenaar, *On the Psychology of Playing Blackjack: Normative and Descriptive Considerations with Implications for Decision Theory*, 114 J. EXPERIMENTAL PSYCHOL.: GENERAL 133, 157 (1985) ("The players observed in the present study could be well described by what Simon has termed as *bounded rationality* or quasi rationality. Their intuitions are clearly limited and error prone. Yet, they are also setting constraints on the use of wrong beliefs and heuristics which may potentially be misleading. . . . To make the description more complete, we would also characterize the players

different perspectives on legal decision theory is the complex nature of the evidence on human rationality.

Good illustrations of the ambiguous implications of this body of evidence are found in instances where Professor Prentice and I rely on precisely the same research to reach different conclusions. As a first example, consider the case of whether financial incentives improve performance on tests of rational thinking, an important question because such tests most commonly employ hypothetical as opposed to real payoffs.[15] Professor Prentice cites a literature review by Camerer and Hogarth on the effects of financial incentives on rational behavior for the proposition that "'[t]he most common result is that incentives did not affect mean performance.'"[16] This proposition supports Prentice's view that, often, studies using hypothetical payoffs yield the same results as studies using real payoffs. I, on the other hand, rely on the same work for the proposition that, "as Camerer and Hogarth

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as being guided by what might be termed as *bounded irrationality*. We speculate that this latter description can be generalized beyond the limited context of playing blackjack." (citation omitted)).

15. For example, many experiments on decision making under risk use hypothetical gambles to examine whether subjects' preferences are consistent with the predictions of subjective expected utility theory or whether they show systematic deviations from the predictions of how a perfectly rational actor would act under expected utility theory. *See, e.g.*, Colin Camerer, *Individual Decision Making*, *in* THE HANDBOOK OF EXPERIMENTAL ECONOMICS 587, 589 (John H. Kagel & Alvin E. Roth eds., 1995) ("In the psychology experiments, subjects are often not paid according to their performance, or are paid small amounts; stimuli have natural labels that may induce nonmonetary utilities; subjects do not always make repeated choices under stationary replication; treatments are sometimes created by deceiving subjects; and so forth. As a result, many economists discount evidence from the psychologists' studies.").

16. Prentice, *supra* note 4, at 1751-52 (citing Colin F. Camerer & Robin M. Hogarth, *The Effects of Financial Incentives in Experiments: A Review and Capital-Labor-Production Framework*, 19 J. RISK & UNCERTAINTY 7, 22 (1999)). Prentice further states:

> In some judgment and decision tasks, incentives often hurt performance. In some areas where concentration and attention are important, financial incentives often do improve decision making by increasing the subject's motivation and/or attention, but Stone and Ziebart note that "financial incentives are no panacea for eliminating decision biases. Instead, incentives appear to increase the extent of attention given to a task, but also to increase potentially distracting emotions." Kuhberger and colleagues list several examples of important areas where no significant differences were found between hypothetical and real decisions, concluding "the general consensus among psychologists seems to be that hypothetical choices give a reasonable, qualitatively correct picture of real choices." They then pursued their own study and found that similar preference reversals regarding framing were obtained using both hypothetical and real decisions for both small and large payoffs. These results are supported by the numerous studies cited above where laboratory results were confirmed in the field.

Id. at 1752 (footnotes omitted); *see also id.* at 1748 ("[S]tudies generally show that there is not much difference between laboratory decision making and real world decision making." (footnote omitted)). He does note some evidence of differences in results, however. *Id.* at 1748 n.449 ("There certainly are studies showing differences in some contexts when incentives are introduced." (citations omitted)).

emphasize, the existing data [on the effect of financial incentives] cannot properly be summarized in simple, dichotomous terms. Sometimes decisions with material consequences are the same as decisions without material consequences; sometimes they are different."[17]

On this point, it is worth noting that Vernon Smith, who, as Prentice indicates, was a co-recipient with Daniel Kahneman of the 2003 Nobel Prize for economics,[18] has recently written that "[a]nyone who doubts that payoffs can and do matter has not looked at the evidence. What is not predictable by any theory is what situations will be sensitive at what payoff levels and what situations will not be sensitive at the levels commonly used."[19] Smith's statement accords with my intended point: much remains to be learned about material incentives and rational behavior, and, just as we should not assume incentives are a panacea for eliminating judgmental biases and decision errors—Prentice's point—we should likewise not ignore the possible effects of incentives.

In another example of our divergent interpretations of the same evidence, Professor Prentice emphasizes the empirically informed, yet primarily theoretical, analysis by Kerr, MacCoun, and Kramer to the effect that group deliberation will often not prevent the appearance of psychological biases.[20] In contrast, I emphasize the empirical analysis by Kerr and his colleagues to support the proposition that there are no simple conclusions when it comes to groups and psychological biases: "In some cases, group deliberations and collective decisionmaking moderate bias, in some cases they have no apparent net effect, and in some cases they amplify bias."[21] Again,

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17. Mitchell, *Incompetence*, *supra* note 2, at 119; *see also* Camerer & Hogarth, *The Effects of Financial Incentives in Experiments: A Review and Capital-Labor-Production Framework*, 19 J. RISK & UNCERTAINTY 7, 8 (1999) ("The extreme positions, that incentives make no difference at all, or always eliminate persistent irrationalities, are false.").

18. *See* Prentice, *supra* note 4, at 1775 n.585.

19. Vernon L. Smith, *Method in Experiment: Rhetoric and Reality*, 5 EXPERIMENTAL ECON. 91, 101-02 (2002).

20. *See* Prentice, *supra* note 4, at 1715 ("Similarly, in 1996 Kerr and his colleagues reviewed all the studies they could find regarding differences in individual and group bias, and concluded that there is little difference between group and individual bias." (footnote omitted)). Prentice cites to portions of the Kerr, MacCoun, and Kramer article drawing conclusions from a theoretical analysis (based on the social decision scheme model) of how different types of groups are likely to be affected by the biases of individual group members. *See id.* at 1715 nn.273-75 (citing Norbert L. Kerr et al., *Bias in Judgment: Comparing Individuals and Groups*, 103 PSYCHOL. REV. 687, 713-14 (1996)).

21. Mitchell, *Pessimism*, *supra* note 2, at 2004. Kerr, MacCoun, and Kramer draw the following conclusion from their review of then-existing empirical data:

> The central question of this paper has been, "Which is more likely to make a biased judgment, individuals or groups?" Our overview of the relatively small and diverse

I caution against simple extrapolations from the evidence, while Professor Prentice focuses on the fact that group deliberation does not provide refuge for proponents of the rationality assumption.

As a final illustration of our differing interpretations, Professor Prentice notes that Vernon Smith's work on experimental markets reveals that feedback and market experience do not completely extinguish irrational behavior,[22] whereas I cite Smith's work to show that behavior in single-decision and repeated-decision settings does differ at times and that some learning does occur in market settings.[23] Recent evidence on the issue of whether markets foster rational behavior further illustrates the difficulty in reaching simple conclusions in the rationality-irrationality debate. In two sets of studies into the impact of market forces on the appearance and survival of anomalous decision-making behavior, the economist John List found that level of experience in the marketplace mediates rationality. In one set of studies of a naturally occurring market, List found that prospect theory predicted the behavior of inexperienced consumers while neoclassical microeconomic theory predicted the behavior of experienced consumers.[24] In a second set of market studies, List found evidence of an endowment effect that dissipated

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> empirical literature suggested that there was no simple empirical answer to this question. Even when we restrict our attention to particular bias phenomena (e.g., framing effects, preference reversals), there was frequently little consistency in the direction (i.e., sign) and magnitude of observed relative bias, *RB*.

Kerr et al., *supra* note 20, at 713.

22. *See* Prentice, *supra* note 4, at 1704 ("Even the repeat games of experimental economist and Nobel Prize winner Vernon Smith demonstrated that the stock market, the market most likely of all markets to be efficient, often is not. . . . Even repeat professional players in the financial markets—players who have time and incentive to perform well—are subject to many of the heuristics and biases that have been identified in lay persons by one-shot laboratory experiments." (footnotes omitted)).

23. *See* Mitchell, *Pessimism*, *supra* note 2, at 1978 & n.144; Mitchell, *Incompetence*, *supra* note 2, at 165-66 & n.299.

24. JOHN A. LIST, NEOCLASSICAL THEORY VERSUS PROSPECT THEORY: EVIDENCE FROM THE MARKETPLACE 24-25 (Nat'l Bureau of Econ. Research, Working Paper No. w9736, 2003) ("In this study, I make use of three clean tests that pit neoclassical theory against prospect theory. I test the three hypotheses with data generated from a naturally occurring market. Examining trading patterns and bid/offer schedules in actual auctions for *everyday* consumables yields several unique insights. First, prospect theory is found to have strong predictive power for inexperienced consumers across both the trading and auction treatments. Second, for those consumers that have had a considerable amount of exchange opportunity in the sportscard marketplace, neoclassical theory predicts reasonably well, as I find sharp evidence that behavior approaches the neoclassical prediction for experienced agents. In light of the extant body of psychological evidence that reports limited transfer of learning across tasks, these results are quite surprising. The tentative conclusion regarding the underlying learning process at work is that agents with intense market experience have learned to part with entitlements, suggesting attenuation of the anomaly appears to take place on the sell side of the market rather than on the buy side."), http://www.nber.org/papers/w9736.

appreciably with trading experience, with behavior converging toward the neoclassical prediction as experience intensified.[25] Thus, while market forces may not entirely extinguish irrational behavior, these forces may have important positive effects on behavior.

Despite the different lessons we draw from the mixed evidence on rationality, and these examples do not exhaust our differences in interpretation,[26] Professor Prentice and I do agree in some important

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25. John A. List, *Does Market Experience Eliminate Market Anomalies?*, 118 Q.J. ECON. 41, 70-71 (2003) ("In this study, I depart from traditional experimental investigation by observing actual market behavior. Examining behavior in four field experiments across disparate markets yields several unique insights. First, the field data suggest that there is an overall endowment effect. Second, within both institutions—observed trading rates and explicit value revelation—I find strong evidence that individual behavior converges to the neoclassical prediction as trading experience intensifies. . . . These results provide initial evidence consistent with the notion tbat market experience eliminates market anomalies.").

It should be noted that Professor List is not among the group of economists who refuse to accept tbe findings of behavioral economics, but he does seek to test the generalizability of experimental results. Sometimes his tests reveal strong support for bebavioral economics' amendments to rational choice theory. For instance, in a study of undergraduates and experienced futures and options floor traders, Haigh and List found that, "while both traders and students fall prey to MLA [myopic loss aversion], traders fall prey to MLA to a *greater* extent than students. . . . [O]ur findings suggest tbat expected utility theory may not model professional traders' bebavior as well, and this finding lends credence to behavioral economics and finance models, which are beginning to relax the rationality assumptions used in standard financial economics." MICHAEL S. HAIGH & JOHN A. LIST, DO PROFESSIONAL TRADERS EXHIBIT MYOPIC LOSS AVERSION? AN EXPERIMENTAL ANALYSIS 9 (Univ. of Maryland, Dept. of Agric. & Econ. Resources, Working Paper No. 02-18, 2002), http://www.arec.umd.edu/publications/papers/working-papers-PDF-files/02-18.pdf.

For a recent study finding that repeated play within an experimental market reduced tbe hindsight bias, see Tarek El-Sehity et al., *Hindsight Bias and Individual Risk Attitude Within the Context of Experimental Asset Markets*, 3 J. PSYCHOL. & FIN. MARKETS 227, 233 (2002) ("Our results do not support the conjecture tbat traders in an experimental asset market are prone to hindsight bias in remembering their price predictions. Moreover, our results show that bindsight bias is not generally present; rather, it was moderated by the metbodology in use. This result may be content-specific. In studies witb almanac questions, hindsight bias seems robust, but in our experimental approacb, personal experience and feedback about financial performance may crowd out the bias. Another explanation for our finding may be the within-subjects design itself, more specifically an asymmetry in the ability to draw on prior information.").

26. Two others areas of debate merit brief mention. The first concerns the "experimental conversations" program of research that arose out of Kahneman and Tversky's own concerns about the settings in whicb so many cognitive heuristics are tested. *See* Mitchell, *Pessimism*, *supra* note 2, at 1980-81. Whereas Prentice reads tbe research on the effects of conversational cues to be "irrelevant to experiments regarding many of the heuristics and biases in the K-T tradition" but "particularly relevant to the dilution effect," Prentice, *supra* note 4, at 1706, I read this research to be relevant to the strength of the evidence for several of tbe biases and errors that legal decision tbeorists discuss. *See* Mitchell, *Pessimism*, *supra* ncte 2, at 1983-84 (citing research on tbe influence of conversational conventions and cues on the use of base rate information, the fundamental attribution error, susceptibility of eyewitnesses to misleading questions, the conjunction fallacy, primacy effects, framing effects, judgmental overconfidence, and the dilution effect).

respects on the lessons to be learned. We agree most broadly that economic models of behavior are often quite inaccurate and that psychological models are sometimes more accurate. Nevertheless, we disagree on the conditions under which we can declare "K-T Man" a better model than "Chicago Man." In other words, we agree that revision or replacement of the rational actor model is appropriate, but we debate the details of how, when, and to what extent such revisions should occur.[27]

I take a cautious approach to prescription that seeks to target the persons who may be most prone to irrationality without adversely affecting those who are not. I therefore suggest a few possible procedural reforms that might have this effect,[28] an approach similar

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Also, I do not believe the evidence is quite as clear as Prentice suggests that behavior in within-subjects experiments often mirrors behavior in between-subjects experiments. *See* Prentice, *supra* note 4, at 1685 ("In short, psychologists often study the same phenomena with both between-subjects and within-subjects studies and usually (but not invariably) find similar results," (footnote omitted)). *But see* Daniel Kahneman & Shane Frederick, *Representativeness Revisited: Attribute Substitution in Intuitive Judgment*, *in* HEURISTICS AND BIASES: THE PSYCHOLOGY OF INTUITIVE JUDGMENT 49, 72 (Thomas Gilovicb et al. eds., 2002) ("Between-subjects and factorial designs often yield different results in studies of intuitive judgment."). In fact, if within-subjects and between-subjects designs did commonly yield similar results without changes to the experimental design, we would probably see many more within-subjects designs because they require fewer subjects and often less time, increase the power of tbe study, and allow the subject to act as her own control within tbe study—all very attractive features. *See* Elizabeth J. Heillier, *Within-Subject Designs*, *in* LABORATORY PSYCHOLOGY: A BEGINNGER'S GUIDE 39, 45-46 (Julia Nunn ed., 1998). However, for some of the very reasons that Prentice notes, such as demand characteristics and carryover effects, *see* Prentice, *supra* note 4, at 1680-81, subject behavior in within-subjects designs does often differ from that in between-subjects designs, making within-subject designs less common in heuristic and bias research. *See* Albert Erlebacher, *Design and Analysis of Experiments Contrasting the Within- and Between-Subjects Manipulation of the Independent Variable*, 84 PSYCHOL. BULL. 212, 212 (1977) ("Many experiments have shown, however, that the two experimental designs do not always yield similar results."); Paul A. Klaczynski et al., *Goal-Oriented Critical Reasoning and Individual Differences in Critical Reasoning Biases*, 89 J. EDUC. PSYCHOL. 470, 472 (1997) ("Research on the biased application of reasoning strategies has typically been conducted with between-subjects designs. Reasoning biases are demonstrated when one group readily assimilates the enhancing information to their existing beliefs and a second group uses complex reasoning to reject threatening information." (citations omitted)); *see also* J. MERRILL CARLSMITH ET AL., METHODS OF RESEARCH IN SOCIAL PSYCHOLOGY 267 (1st ed. 1976) ("In social-psychological experiments, within-subjects designs are rare. This is because in many research situations there is a strong probability that participation in one condition will influence, or 'contaminate,' a subject's response to the second condition."); Erlebacher, *supra*, at 212 ("Most experiments in personality or social psycbology tend to be performed with a between-subjects design.").

27. *See* Prentice, *supra* note 4, at 1720 ("In other words, to some extent we are now arguing about details. Mitchell essentially admits this. He does not argne that the Chicago Man model in any way approximates how people actually act. He does not reject the psychological analysis of law. It is the details he quibbles over, and the scientific work of nailing down those details will be ongoing for a long, long time." (footnotes omitted)).

28. *See* Mitchell, *Incompetence*, *supra* note 2, at 132-35 (discussing possible "do no harm" reforms, including statistical training for judges, presentation of evidence in frequency and

to the "asymmetric paternalism" approach recently discussed by Colin Camerer and his colleagues.[29] Professor Prentice, however, believes that I have been "unduly timid in failing to embrace numerous other policy prescriptions offered by legal decision theorists."[30] Eventually I may overcome my admitted prescriptive timidity, but I believe we still have much to learn about the conditions under which we act rationally and irrationally before we attempt wide-scale reforms, as Camerer and his colleagues likewise suggested recently:

> The scientific consolidation of psychological findings into a new brand of behavioral economic theory breathes new life into the rationales for paternalistic regulation . . . . In a sense, behavioral economics extends the paternalistically protected category of "idiots" to include most people, at predictable times. The challenge is figuring out what sorts of "idiotic" behaviors are likely to arise routinely and how to prevent them, while imposing minimal restrictions on those who behave rationally.[31]

One way to obtain a better idea of when we do and do not act like "idiots" is through the meta-analytic synthesis of existing judgment and decision-making research, another important point on which I believe Professor Prentice and I agree.[32] A single meta-analytic study, using systematic techniques for the estimation of effect sizes and effect parameters over collections of studies, may provide

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probability formats, jury instructions that encourage active, open-minded thinking, and the exposition requirements to debias risk assessments).

29. *See* Colin Camerer, Samuel Issacharoff, George Loewenstein, Ted O'Donoghue & Matthew Rabin, *Regulation for Conservatives: Behavioral Economics and the Case for "Asymmetric Paternalism,"* 151 U. PA. L. REV. 1211, 1212 (2003) ("Our purpose in this Article is to argue that in many cases it is possible to have one's cake and eat it too. We propose an approach to evaluating paternalistic regulations and doctrines that we call 'asymmetric paternalism.' A regulation is asymmetrically paternalistic if it creates large benefits for those who make errors, while imposing little or no barm on those wbo are fully rational. Such regulations are relatively harmless to those who reliably make decisions in their best interest, while at the same time advantageous to those making suboptimal choices." (footnote omitted)).

30. Prentice, *supra* note 4, at 1763.

31. Camerer et al., *supra* note 29, at 1218; *see also id.* at 1254 ("To sum up: asymmetric paternalism helps those whose rationality is bounded from making a costly mistake and harms more rational folks very little. Such policies should appeal to everyone across the political spectrum and can potentially shift the debate from one about wbether or not paternalism is justified, to one about whether the benefits of mistake prevention are larger than the harms imposed on rational people. The idea is designed to foster debates about paternalism on these empirical terms. Creating a sharp empirical debate may, in turn, encourage social scientists and lawyers to generate new answers.").

32. Altbough Professor Prentice properly notes tbat meta-analysis has its limitations, Prentice, *supra* note 4, at 1689 ("For example, several critics of [null hypothesis significance testing] have supported greater use of confidence intervals, model fitting, and meta-analyses, although tbese statistical methods have tbeir own limitations." (footnotes omitted)), he does not affirmatively deny its potential value and he does cite favorably findings from meta-analytic studies. *See id.* at 1691-93. For a discussion of criticisms of meta-analysis, see R. Rosenthal & M.R. DiMatteo, *Meta-analysis: Recent Developments in Quantitative Methods for Literature Reviews*, 52 ANN. REV. PSYCHOL. 59, 66-68 (2001).

more useful information for policy-making purposes than hundreds of individual studies.[33] So Prentice and I can each cite numerous studies whose individual statistically significant results seem to provide strong support for our respective views—as we each do—but in the end these studies provide limited information about subsets of people acting in subsets of situations, and numerous inferences and judgments must be made to extend the findings more broadly. Although meta-analysis does not obviate the need for such inferences and judgments, it provides a much more systematic and reliable basis for them than qualitative literature reviews.[34]

Perhaps most importantly, Professor Prentice and I agree that predictable individual and situational differences exist in rational behavior and that legal decision theory must take these differences into account. Thus, it makes no sense for legal decision theorists to speak of legal actors as if they are all "equally incompetent": individuals are not equally likely to fall prey to cognitive illusions, and situations are not equally likely to elicit irrational behavior. On this point, Professor Prentice asserts that a careful reading of legal decision theory would lead one to conclude that legal decision theorists

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33. *See* Rosenthal & DiMatteo, *supra* note 32, at 61 ("Meta-analysis allows the combining of numerical results from a few or many studies, the accurate estimation of descriptive statistics and the explanation of inconsistencies as well as the discovery of moderators and mediators in bodies of research findings."). For examples of just such useful meta-analyses, see Jay J.J. Christensen-Szalanski & Cynthia Fobian Willham, *The Hindsight Bias: A Meta-analysis*, 48 ORGANIZATIONAL BEHAV. & HUMAN DECISION PROCESSES 147 (1991); Anton Kühberger et al., *The Effects of Framing, Reflection, Probability, and Payoff on Risk Preference in Choice Tasks*, 78 ORGANIZATIONAL BEHAV. & HUMAN DECISION PROCESSES 204 (1999); Anton Kŋhberger, *The Influence of Framing on Risky Decisions: A Meta-analysis*, 75 ORGANIZATIONAL BEHAV. & HUMAN DECISION PROCESSES 23 (1998); Jennifer K. Robbennolt, *Outcome Severity and Judgments of "Responsibility": A Meta-analytic Review*, 30 J. APPLIED SOC. PSYCHOL. 2575 (2000).

Although a meta-analysis can be quite useful in organizing findings from a large number of studies, note that a large number of studies is not a prerequisite to meta-analysis. *See* MARK W. LIPSEY & DAVID B. WILSON, PRACTICAL META-ANALYSIS 7 (2001) ("We hasten to add, however, that meta-analysis does not require large numbers of studies and, in some circumstances, can be usefully applied to as few as two or three study findings.").

34. *See* HARRIS COOPER, SYNTHESIZING RESEARCH 2 (3d ed. 1998) ("A social scientist performing a research synthesis makes numerous decisions that affect the outcomes of his or her work. Each choice may create threats to the outcome's trustworthiness. Therefore, if social science knowledge contained in research syntheses is to be believable, research synthesists must be required to meet the same rigorous methodological standards that are applied to primary researchers."); Rosenthal & DiMattteo, *supra* note 32, at 61-62 ("Reviews of research have been valuable to many fields, but when presented and described only qualitatively, the results of conflicting studies can be confusing. Qualitative or narrative methods approach controversy by listing and describing conflicting findings, and sometimes by trying to group or otherwise configure those that have various types of results or outcomes. Yet, it may be all too tempting for authors of narrative reviews consciously or unconsciously to select and describe studies to support their own understanding of the literature and/or their own established theoretical positions.").

have always believed in the situation- and person-specific nature of irrational behavior. In other words, my argument against the "equal incompetence" model of thought—the cognitive model that I contend was developing within legal decision theory—is a fight against a straw man.[35] Jeffrey Rachlinski similarly accuses me of the "cheap academic stunt" of "[m]ischaracterizing a line of scholarship and then attacking that mischaracterization."[36] Although Professors Prentice and Rachlinski accept my case for the context-dependence of irrationality, they contend that legal scholars already knew this and had been acting as if they knew it.[37]

I find my indictment on this point a bit hard to swallow when overly broad and empirically dubious claims about behavior continue to appear within the legal decision theory literature, such as in the following examples (on top of the numerous examples I cited in my previous articles):[38]

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35. Prentice, *supra* note 4, at 1722 ("Mitchell's second article, his *Equal Incompetence* article, erects and tears down a giant straw man. He claims that while economists err by assuming that man is always rational, legal decision theorists err by assuming that man is always irrational. Contrary to this claim of assumed 'equal incompetence' on behalf of all people in all situations, legal decision theorists recognize individual and situational variations." (footnote omitted)).

36. Jeffrey J. Rachlinski, *The Uncertain Psychological Case for Paternalism*, 97 NW. U. L. REV. 1165, 1167 n.18 (2003).

37. *See* Prentice, *supra* note 4, at 1722 ("Contrary to [Mitchell's] claim of assumed 'equal incompetence' on behalf of all people in all situations, legal decision theorists recognize individual and situational variations."); Rachlinski, *supra* note 36, at 1167 n.18 ("To be sure, the author of these studies successfully refutes the claim that context or incentives do not affect the magnitude of the cognitive phenomena that psychologists have observed. Inasmuch as no one has ever advocated such a position, however, it is hard to see the value of such a refutation.").

38. In both of my articles in question, I quote at length numerous empirical claims within legal decision theory that I found troubling. *See, e.g.*, Mitchell, *Pessimism*, *supra* note 2, at 1917 n.15, 1918-20 n.18; Mitchell, *Incompetence*, *supra* note 2, at 69 n.2, 70-72 nn.4-7, 84-85 n.41. In these articles, I acknowledge that not all legal decision theorists share exactly the same views and that at various points we find some cautionary statements to accompany the bold statements. *See, e.g.*, Mitchell, *Pessimism*, *supra* note 2, at 1915 n.12 ("Of course, not all legal decision theorists will subscribe to every claim made by every other legal decision theorist, and no single legal decision theory yet exists. To limit confusion and increase the fairness of my remarks, I attempt to provide as much detail as possible on who has made what particular claims, including direct quotation of the statements at issue."); *id.* at 1933 n.44 ("Legal decision theorists generally express caution in the introductions and conclusions of their articles, often with respect to policy arguments, but seem to abandon this caution at other points in the same articles and particularly with respect to empirical claims, making at times very broad, unqualified statements about the scope of research findings."); Mitchell, *Incompetence*, *supra* note 2, at 85 n.41 ("We see at times in the works of legal decision theory some equivocation on the strength of the claim of equal incompetence, but the basic belief in the universality of flawed information-processing and inferential processes seems to prevail in the end."). Prentice himself notes my caveats. Prentice, *supra* note 4, at 1721 ("Indeed, in his more candid moments, Mitchell admits that 'legal decision theorists generally express caution about their endeavor and note the preliminary nature of much of their work . . . .' " (quoting Mitchell, *supra* note 2, at 1933) (footnote omitted)).

> Psychologists, however, offer evidence that people rely on heuristics (rules of thumb) rather than investing in information, make decisions using unconscious biases, and frequently fail to respond to changes in incentives. These imperfections in cognition, memory, and decisionmaking occur regardless of the decisionmaker's education or socio-cultural background.[39]

> [R]esearch demonstrates that [these biases] apply to all of us, not just a benighted unfortunate few. Further, while these biases do not invariably operate in all cases, they are fairly resistant to correction through education. So, there is little or no correlation between these biases and education or intelligence.[40]

A scholar who took these statements at face value, even with their vague qualifications, would obtain an incorrect understanding of the effect of incentives, education, intelligence, and culture on rationality, because these statements understate the importance of these variables.[41]

In fact, after characterizing my concerns as overblown, Professors Prentice and Rachlinski both acknowledge some overreaching within the ranks of legal decision theory. In Professor Prentice's words, some legal decision theorists may have fallen into "the overadvocacy trap,"[42] and, in Professor Rachlinski's words, "the notion that people make systematically erroneous choices . . . has been overlearned and overapplied by legal scholars."[43] So it turns out that

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39. Stephanie Stern, *Cognitive Consistency: Theory Maintenance and Administrative Rulemaking*, 63 U. PITT. L. REV. 589, 589 (2002) (footnotes omitted).

40. Tamara Piety, *"Merchants of Discontent": An Exploration of the Psychology of Advertising, Addiction, and the Implications for Commercial Speech*, 25 SEATTLE U. L. REV. 377, 404 (2001) (footnote omitted).

41. *See* Mitchell, *Incompetence*, *supra* note 2, at 87-94 (discussing evidence that education can improve performance on some rationality tests); *id.* at 94-98 (discussing evidence that intelligence and susceptibility to several cognitive biases are related); *id.* at 114-19 (discussing evidence that financial incentives may, in some cases, affect the frequency or magnitude of irrational behavior); *id.* at 132-35 (discussing evidence on ways to debias the person or environment); *id.* at 147-56 (discussing evidence of cultural variation in rationality).

42. Prentice, *supra* note 4, at 1773-74 ("Ardent supporters of legal decision theory have no doubt painted an enthusiastic picture of the potential that it has for informing legal analysis, and some may have fallen into the overadvocacy trap." (footnote omitted)); *see also id.* at 1722 ("This certainly does not mean that errors have not crept into the policy precepts of legal decision theorists, or that they will not do so. But it is the proponents of K-T Man, rather than those of Chicago Man, who have a fighting chance of usefully formulating legal doctrine based on how people actually make decisions.").

43. Rachlinski, *supra* note 36, at 1168. Later Professor Rachlinski adds

> Legal scholars have either misunderstood or misapplied behavioral decision theory. The psychological research itself is not designed solely to uncover error; it is designed to identify how people think. The chief lesson of behavioral decision theory is not people make bad choices but that they do not rely on rule-based systems, such as deductive logic or expected utility theory in making decisions.

Id. at 1206-07.

Professors Prentice and Rachlinski express some of the same concerns that motivated my articles.[44]

Given these common concerns about "overadvocacy," how can it be that I view many claims by legal decision theorists as cavalier and neglectful of important contextual influences on behavior, while Professor Prentice defends these same claims as careful and duly mindful of contextual influences?[45] Part of the answer lies, no doubt, in our different readings of the underlying empirical evidence that translate into different degrees of certainty about the state of our knowledge, as discussed above, but I believe there is another explanation as well. A large part of the gap between our perspectives derives, it seems to me, from different comfort levels with the concept of "behavioral tendencies" and different views about the appropriate level of generality for describing behavior. Understanding these differences may help explain why I am less willing to infer legal prescriptions from the psychological research than Professor Prentice.

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44. *See* Mitchell, *Pessimism*, *supra* note 2, at 2020-21 ("Progress is unlikely, however, so long as legal decision theorists fail to specify the boundary conditions on their empirical claims and instead settle for overly broad generalizations about nonrationality to compete with the economic theorists' overly broad generalizations about rationality."); Mitchell, *Incompetence*, *supra* note 2, at 138-39 ("Although the criticisms directed here at legal decision theory may mistakenly lead some readers to conclude that this Article's primary purpose is to defend conventional law and economics and trivialize behavioral law and economics, the real goal is to encourage greater fidelity to existing empirical data and encourage a more sophisticated approach to the study of legal behavior. Legal decision theory pushes legal scholars and lawmakers to examine the behavioral assumptions that underlie legal concepts and why these assumptions were chosen. This inquiry may be tremendously valuable.").

Of particular note, Professor Rachlinski and I agree that an empirical demonstration of heuristic processing should not be equated with poor judgment or decision making. *See* Mitchell, *Pessimism*, *supra* note 2, at 1973 ("Recall that the use of cognitive heuristics is adaptive, for heuristic processing often leads to the same result as does more systematic information-processing that requires greater effort or resources. Indeed, heuristic processing may lead to the normative result with a high degree of frequency." (footnotes omitted)); Mitchell, *Incompetence*, *supra* note 2, at 108 ("[I]t is the experimenter who may well be committing an error by labeling a subject's choice irrational without first examining the subject's motives and beliefs about the decision situation." (footnote omitted)); Rachlinski, *supra* note 36, at 1219 ("The road from identification of a heuristic to the use of that heuristic to justify supplanting individual choice is much longer than has been noted in the literature thus far. Even though psychological research on judgment and choice suggests that people usually use heuristics to make choices, the reliance on heuristics does not necessarily suggest that people make chronically bad decisions. People's ability to learn good approaches to problems might ensure that they can make good choices most of the time.").

45. *Compare* Mitchell, *Incompetence*, *supra* note 2, at 139 (Legal decision theorists' "important inquiry needs to be conducted using data rather than mere words, and claims to empirical truth need to be made less cavalierly."), *with* Prentice, *supra* note 4, at 1724 ("Because legal decision scholars are necessarily sensitive to the fact that the complex human reasoning and judgment processes are anything but universal and uniform, Mitchell is unable in either of his articles to meaningfully challenge a single policy prescription by any legal decision scholar.").

II. TENDENCIES, *CETERIS PARIBUS* CLAUSES, AND BOUNDARY CONDITIONS

As a partial rebuttal to my criticism of legal decision theorists' aggressive behavioral claims, Professor Prentice emphasizes that legal decision theorists often speak in terms of behavioral tendencies rather than universal patterns of behavior:

> Contrary to this claim of assumed "equal incompetence" on behalf of all people in all situations, legal decision theorists recognize individual and situational variations. Some of Mitchell's own cited sources make this explicit by stating that the psychological evidence shows that people "frequently" or "often" think in ways that depart from accepted norms of rationality.[46]

Professor Prentice is correct that talk of behavioral tendencies—talk of how "people" (in general? all? most? some?) "often" or "frequently" act—surely does admit of some individual or situational variation in behavior, for if all people do not always act the same way, then there must be some behavioral variation. We find such talk of behavioral tendencies throughout the empirical legal literature, including in some of my own work,[47] because in the behavioral sciences we discover

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46. Prentice, *supra* note 4, at 1722 (footnotes omitted); *see also id.* at 1726 ("Mitchell cites no legal decision theorist who has told his or her audience that educated and uneducated people *always* fall prey to the same illusions, and I cannot imagine that one has done so. Nor does Mitchell cite any legal decision scholar who has based a policy prescription upon such an assumption. Mitchell could have cited several theorists who have claimed that educated and uneducated people *usually* fall prey to the same illusions at roughly the same rates, and Mitchell would have great difficulty discrediting such statements."). I previously noted concern about the use of vague qualifiers to condition the claims of legal decision theorists:

> Inferring generalizations about behavior from aggregated data may be a defensible, if imprecise, practice, but only when this inferential leap and its basis are made clear. Virtually all of the claims of the legal decision theorists are general-type propositions inferred from aggregate-type propositions without explication as such, and without any discussion of how findings of statistical significance alone can support the generalizations. One finds, at most, linguistic hedges, such as the data "suggest" some effect or some effect "generally" occurs, but not outright admissions that legal decision theory is founded on generalizations that are shakily inferred from aggregated data in between-subjects experiments.

Mitchell, *Pessimism*, *supra* note 2, at 1969 (footnotes omitted).

47. *See, e.g.*, Kevin Barton, *Game Over! Legal Responses to Video Game Violence*, 16 NOTRE DAME J.L. ETHICS & PUB. POL'Y 133, 138 (2002) ("Several empirical investigations on the effects of violent video games link such games to children's behavioral tendencies."); A. Mechele Dickerson, *A Behavioral Approach to Analyzing Corporate Failures*, 38 WAKE FOREST L. REV. 1, 3 (2003) ("Given the well-established behavioral tendency for actors to be overconfident about the risk that bad things will happen, it is somewhat predictable that directors will be unwilling to place firms in bankruptcy since doing so acknowledges that they lack the ability to save the business or, even worse, may have made decisions that contributed to the firm's financial crisis." (footnotes omitted)); Reid Hastie, *Emotions in Jurors' Decisions*, 66 BROOK. L. REV. 991, 1002 (2001) ("A person in an angry emotional state would have an increased 'action tendency' to behave aggressively, sympathy would evoke protective and restitutive tendencies, fear would instigate escape or self-protective behaviors, and so forth. Either account would imply that

tendencies and probabilities, rather than universals and certainties.[48] Hence, the use of tendency talk is not *per se* objectionable.

Nevertheless, speaking in terms of behavioral tendencies can have negative consequences, particularly for an applied field of work such as legal decision theory. The overriding problem is that this tendency talk obscures the complexity of the relations among behavior, cognition, motivation, emotion, and the environment. Given what we now know of important boundary conditions on judgmental biases and decision-making errors (i.e., with what strength and frequency, for whom, and in what situations are these tendencies likely to hold?), to say that people tend to exhibit this or that tendency without expressing the boundary conditions on it omits important information from a policy-making standpoint. The point I have been

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inducing an emotional state would result in a systematic behavioral tendency, and that such a tendency could influence legally relevant judgments." (footnotes omitted)); Mitchell, *Pessimism*, *supra* note 2, at 2011 ("The precise words used in a law or a contract and the subject matter or domain of the law or contract, not to mention the characteristics of the legal actors and characteristics of the larger social context, may dramatically affect whether and to what extent different behavioral tendencies (most typically in the form of different risk preferences) are observed across positive and negative framings of formally equivalent judgment and choice options."); Janice Nadler & Mary R. Rose, *Victim Impact Testimony and the Psychology of Punishment*, 88 CORNELL L. REV. 419, 448-49 (2003) ("In many settings in which it is possible to evaluate the quality of decisions, the addition of certain types of information leads to a less reliable, lower-quality decision. This is because human decision makers tend to make systematic errors in the way they combine pieces of information." (footnote omitted)); Prentice, *supra* note 4, at 1666-67 ("A 'mountain of experiments' performed in psychology and related disciplines, much of it in the 'heuristics and biases' tradition founded by psychologists Daniel Kahneman and Amos Tversky, demonstrate that people tend to deviate systematically from rational norms when they make decisions." (footnotes omitted)); Rachlinski, *supra* note 36, at 1170 ("People tend to base judgments of frequency on 'the ease with which instances of occurrences can be brought to mind.'" (footnote omitted)); Susan J. Stabile, *Another Look at 401(k) Plan Investments in Employer Securities*, 35 J. MARSHALL L. REV. 539, 564 (2002) ("Over-investment in employer securities represents another significant problem with 401(k) plans. Given the freedom to do so, participants invest disproportionately in the stock of their employers. They do so because of various behavioral tendencies that are not susceptible to change through increased education efforts.").

48. The use of descriptive abstractions, in the form of people tend to do this or that, becomes unavoidable once outside the realm of biography or limited historical description. While the social sciences often strive for universal laws of behavior, the complex and changing nature of the world makes finding such laws extremely difficult, if not impossible, and leaves contingent and incomplete descriptions of behavioral regularities perhaps the best that we can do (i.e., we must speak in terms of imperfect rather than perfect correlations between variables). *See, e.g.*, Robyn M. Dawes, *The Nature of Human Nature: An Empirical Case for Withholding Judgment—Perhaps Indefinitely*, 16 POL. PSYCHOL. 81, 92 (1995) ("In conclusion, we simply don't know enough beyond our benchmark knowledge of what people are like to make generalizations about human nature."); Harold Kincaid, *Confirmation, Complexity and Social Laws*, 2 PSA: PROCEEDINGS BIENNIAL MEETING PHIL. SCI. ASS'N 299, 306 (1988) ("The social sciences can, I believe, produce laws, at least on some occasions. Yet, *ceteris paribus* clauses, and domain-specific generalizations predominate. Perhaps their prevalence suggests not that laws do not exist but that laws are only part of social science explanation.").

pressing in my articles on legal decision theory is that there are few indisputable and broad tendencies when it comes to irrational behavior. As a result, it makes little sense, and can be misleading, to speak of depersonalized, decontextualized irrational tendencies.

In some cases, legal decision theorists do attempt to work out the details of the rationality research when applying it to the legal system, as Professor Prentice ably demonstrates with his work on the possible role of self-serving bias in accounting.[49] But we also find within legal decision theory listings of one or more "cognitive illusions" that people "tend" to fall prey to under some vaguely specified conditions, followed by speculation about how these illusions might affect legal behavior and how the law might want to respond them.[50]

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49. *See* Prentice, *supra* note 4, at 1741-44.

50. Others share this perception. *See* Russell B. Korobkin & Thomas S. Ulen, *Law and Behavioral Science: Removing the Rationality Assumption from Law and Economics*, 88 CAL. L. REV. 1051, 1058 (2000) ("In the early stages of the movement, legal scholars have been able, by and large, to make important strides by hypothesizing that empirical and experimental findings published by social science researchers apply to actors subject to legal commands. To progress beyond the current initial stage of scholarship, legal scholars will have to conduct more empirical and experimental work of their own to test whether these hypotheses are in fact true in the particularized settings they study." (footnote omitted)); *see also* Rachlinski, *supra* note 36, at 1225 ("The new field of behavioral law and economics is likely entering a new phase that recognizes the deeper meaning of the research in cognitive psychology. Scholars will come to realize that merely identifying how a cognitive error might play out in a legal context is not sufficient to support a change in law or policy.").

Particularly problematic is the listing of supposed irrational tendencies, especially the endowment and framing effects and status quo bias, in support of arguments to change default rules, because default rule changes may implicate many groups of people and types of situations for which these tendencies have not been shown to hold with any real strength or frequency. *See, e.g.*, Edward J. Janger & Paul M. Schwartz, *The Gramm-Leach-Bliley Act, Information Privacy, and the Limits of Default Rules*, 86 MINN. L. REV. 1219, 1243-44 (2002) ("According to well documented empirical findings, most people define value by focusing on changes (gains and losses) relative to some reference point. Research into frames has also found that most people react more decisively to avoid losses than to obtain gains. Put simply, the pain caused by the loss of $100 is greater than the joy caused by the gain of $100. Reconsider, then, the privacy notices that imply that consumers may have already opted out or that opting out will accomplish little. These GLB Act notices present a reference point that suggests to consumers that only inaction is needed or that at best only relatively small gains are available from opting out. Finally, to raise an additional tactic of the GLB Act privacy notices, some notices state that consumers who opt-out may fail to receive 'valuable offers.' This notice creates a frame that points to opting out as leading only to a loss. By creating a perceived entitlement, the financial institution seeks to discourage opt-out. Due to the power of frames, a 'notice plus opt-out' approach may prove unable to alter a lemons equilibrium and have virtually no information forcing effect. To express this idea more completely, a law requiring notice and an opt-out default may fail to induce much bargaining so long as the better informed party still controls the language and form in which the actual data are conveyed. In light of this critique, a notice and 'opt-in' regime might at first appear to be a better choice to create an information forcing default. Because consent must be procured, the burden shifts to the financial institution to convince a customer to permit disclosure. The financial organization must therefore explain the benefits of action, which has the effect of flipping the frame. Opt-in creates an entitlement in the privacy of personal information, and the customer must be induced to give it up." (footnotes omitted)); Cass

Behavioral tendencies become little more than a menu of behaviors ready to be ordered up when a particular type of judgment or decision is encountered, with little regard for how the larger social setting or characteristics of the decision maker may alter the tendency. Thus, a

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R. Sunstein, *Switching the Default Rule*, 77 N.Y.U. L. Rev. 106, 112 (2002) ("Where the Coase Theorem blunders is in suggesting that no matter the initial allocation of the entitlement, people will bargain to the same result. The Coase Theorem fails to account for the fact that the initial allocation seems to create an endowment effect. When the endowment effect is at work, those who initially receive a legal right value it more than they would if the initial allocation had given the right to someone else. There is a great deal of evidence to this effect." (footnotes omitted)); *id.* at 133 ("Echoing the emerging orthodoxy in behavioral law and economics, I have argued that the default rule might well matter. If the legal rule has an endowment effect, it is potentially important to ultimate outcomes, even in the absence of transaction costs. The principal qualification here is that in some domains, workers and employers might order their affairs with little or no reference to legal rules. I have also urged that a switch in the default rule, to an initial allocation in favor of employees, might have the fortunate result of ensuring that important information is disclosed to employees—a corrective to what seems to be a 'fairness heuristic' by which people identify likely legal rules. By itself this is an argument in favor of the switch.").

With respect to legal decision theorists' use of research on the endowment effect (i.e., possession may increase value), Russell Korobkin recently wrote:

> As might be expected when legal scholars import into their work a concept developed by and primarily studied in other disciplines, the sophistication with which the endowment effect has been used to address legal policy questions varies substantially across the literature. Legal scholars have universally grasped the most important positive implication of the endowment effect—that legal entitlements will not change hands as often in the free market as the status irrelevance assumption implies—and often have successfully employed this insight to revisit long-standing arguments about normatively appropriate legal policy. However, two important subtleties sometimes escape legal scholars' attention: (1) that the existence and extent of the endowment effect is context-dependent (and, in addition, not fully understood) and (2) that the explanation for why the endowment effect exists, which is also not well understood, should often affect its normative implications.

Russell Korobkin, *The Endowment Effect and Legal Analysis*, 97 NW. U. L. REV. 1227, 1229 (2003).

For a detailed discussion of problems with the attempt to use the judgment and decision-making research to justify enterprise liability ("EL") in product liability law, see James A. Henderson, Jr. & Jeffrey J. Rachlinski, *Product-Related Risk and Cognitive Biases: The Shortcomings of Enterprise Liability*, 6 ROGER WILLIAMS U. L. REV. 213 (2000). Henderson and Rachlinski conclude that "[t]he chief mistake made by EL's newer advocates lies in oversimplifying the lessons of cognitive psychology." *Id.* at 256.

Of course, some of the early work listing cognitive biases and errors and then speculating about their legal import were meant more to provoke debate than offer definitive prescriptions. *See, e.g.*, Christine Jolls et al., *A Behavioral Approach to Law and Economics*, 50 STAN. L. REV. 1471, 1474 (1998) ("The unifying idea in our analysis is that behavioral economics allows us to model and predict behavior relevant to law with the tools of traditional economic analysis, but with more accurate assumptions about human behavior, and more accurate predictions and prescriptions about law. Certainly a great deal of work would be necessary to justify a final evaluation of most of the topics pursued here; there is fertile ground for future research, both theoretical and empirical, and one of our principal goals is to suggest the directions in which that research might go."). Nonetheless, these works lack the kind of careful consideration of contextual influences that Professor Prentice demonstrates in his discussion of accountants and self-serving bias. *See* Prentice, *supra* note 4, at 1741-44.

judgment about liability that calls for ex post reflection on a defendant's ex ante assessment of the risk of danger is said to elicit the tendency toward "hindsight bias"; a judgment about the liability of a defendant whose omission caused harm is said to elicit the tendency toward the "omission bias"; a judgment about a defendant who engaged in an unconventional but not extraordinarily risky course of action is said to elicit the tendency toward "normality bias"; and so on.[51]

This menu approach to behavioral description and prediction follows from the manner in which basic psychological research is conducted and reported. In a good experiment, a psychologist isolates the impact of a particular variable of interest on some behavior of interest and eliminates to the greatest extent possible other behavioral influences and complicating factors. In this way, unconfounded causal conclusions may be drawn.[52] Real world messiness must necessarily be sacrificed for clarity and control.[53] The psychologist then reports how the variable of interest affected the behavior, often using tendency talk to do so, as in "the subjects showed an irrational tendency to take into account nondiagnostic information in their judgments" (which is also known as the dilution effect).[54]

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51. The hindsight bias refers to "the tendency to overestimate the degree to which one would have been able to predict the outcome of an event or the answer to a question after one has received feedback about one's prediction or after the event has occurred." Elizabeth Creyer & William T. Ross, Jr., *Hindsight Bias and Inferences in Choice: The Mediating Effect of Cognitive Effort*, 55 ORGANIZATIONAL BEHAV. & HUM. DECISION PROCESSES 61, 61 (1993) (citation omitted).

The omission bias refers to "the tendency of people to find more blameworthy bad results that stem from actions than bad results that stem from otherwise equivalent omissions." Robert A. Prentice & Jonathan J. Koehler, *A Normality Bias in Legal Decision Making*, 88 CORNELL L. REV. 583, 587 (2003) (footnote omitted). For a recent reconsideration of the evidence on the omission bias, with particular attention to omission bias in vaccination decisions, see Terry Connolly & Jochen Reb, *Omission Bias in Vaccination Decisions: Where's the "Omission"? Where's the "Bias"?*, 91 ORGANIZATIONAL BEHAV. & HUM. DECISION PROCESSES 186 (2003).

The normality bias refers to "the tendency for people to react more strongly to bad outcomes that spring from abnormal circumstances than to otherwise identical outcomes that spring from more ordinary circumstances." Prentice & Koehler, *supra*, at 588 (footnote omitted).

52. *See* James Hampton, *The Between-Subjects Experiment*, *in* LABORATORY PSYCHOLOGY: A BEGINNER'S GUIDE, *supra* note 26, at 15, 21 ("When an experimenter chooses to vary some aspect of the experimental task, in order to observe its effect, what is manipulated is known as the *independent variable*. . . . The effect that is measured is likewise called the *dependent variable* because in this case the value of the measured variable is hypothesised to *depend* on the experimental conditions.").

53. *See id.* at 25 ("In an ideal world, each experiment would have a well motivated independent variable, and everything else would be exactly the same between conditions.").

54. *See* Robert A. Prentice, *The Case of the Irrational Auditor: A Behavioral Insight into Securities Fraud Litigation*, 95 NW. U. L. REV. 133, 161 n.158 (2000) ("Under the 'dilution effect,' nondiagnostic information dilutes diagnostic information, leading to less accurate judgments than where diagnostic information alone is available." (citations omitted)).

Try to transport these behavioral tendencies outside the laboratory, however, and one quickly realizes that they carry tremendously heavy baggage in the form of *ceteris paribus* clauses into which all of the complicating factors have been shoved.[55] To get a feel for the size of this problem, choose any behavioral tendency catalogued within legal decision theory, insert the words "all other things being equal" into the definition of the tendency, and then try to enumerate all of the "other things" that must be equal (i.e., held constant) for this tendency to express itself.

Professor Prentice provides a good example to work with when he describes the omission bias:

> [S]tudies show that when intent, injury, *and all other factors are held constant*, jurors will punish more severely defendants whose acts were active rather than passive (for example, the physician who unplugged the life support machine as opposed to the physician who failed to plug it in when she had the chance to).
>
> This is evidence of an *omission bias*.[56]

Now what other factors besides intent and (level and type of) injury must be held constant for the omission bias to appear? A good number, as it turns out: (1) the normality of the circumstances leading up to the plaintiff's injury, since Prentice and Koehler have shown that the normality bias may swamp the omission bias;[57] (2) the means by which the omission bias is measured, since Connolly and Reb have shown that omission bias is not robust across measurement methods;[58] (3) the nature of the particular values at stake, since Tanner and Medin have shown that omissions to act may be viewed more harshly than acting when the omissions fail to protect values

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55. Daniel Hausman provides a good description of *ceteris paribus* clauses as they appear in economics:

> Explicit or implict *ceteris paribus* clauses are pervasive in economics. People do not always buy more of x wben the price of x decreases. The generalization holds only "other things being equal" or *ceteris paribus*. Not everybody wants more wealth, but economists have held that the generalization holds, *ceteris paribus*. When government imposes price controls, shortages do not always arise, but, *ceteris paribus*, they do.

Daniel M. Hausman, *Ceteris Paribus Clauses and Causality in Economics*, 2 PSA: PROCEEDINGS BIENNIAL MEETING PHIL. SCI. ASS'N 308, 308 (1988).

56. Prentice, *supra* note 4, at 1739-40 (first emphasis added).

57. *See* Prentice & Koehler, *supra* note 51, at 643 ("In fact, the normality bias is so strong that it swamps the influence of another well-documented bias—the omission bias—when the two biases push in different directions." (footnote omitted)).

58. *See* Connolly & Reb, *supra* note 51, at 199 ("These studies show that two features of earlier measures—the truncation of probability response scales, and the asymmetry of open-response matching scales—could well have produced inadvertent bias in the earlier studies. . . . Measures that in one form show substantial vaccine aversion show exactly the reverse after apparently harmless modification, and intendedly convergent measures of the same construct fail even rudimentary tests of consistency.").

associated with moral obligations to act;[59] (4) the point of view from which an omission is judged, since instructing subjects to consider how persons affected by an omission would feel reduces the omission bias;[60] and (5) the age of the subjects, since children appear to be less prone to the omission bias.[61] It should also be noted that these are just the factors that we now know should be held constant to increase the chances of the omission bias appearing; there surely are others waiting to be specifically identified.[62]

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59. *See* Carmen Tanner & Douglas L. Medin, *Protected Values: No Omission Bias and No Framing Effects*, 10 PSYCHONOMIC BULL. & REV. (forthcoming 2003).

60. *See* Jonathan Baron, *Value Analysis of Political Behavior—Self-Interested : Moralistic :: Altruistic : Moral*, 151 U. PA. L. REV. 1135, 1150-51 (2003) ("Omission bias is somewhat labile. It can be reduced by the instructions to take the point of view of those who are affected, e.g., 'If you were the child, and if you could understand the situation, you would certainly prefer the lower probability of death. It would not matter to you how the probability came about.' " (footnote omitted)).

61. *See* Mitchell, *Incompetence*, *supra* note 2, at 157 n.270 (discussing developmental studies of the omission bias and other cognitive biases).

62. In one view, the *ceteris paribus* clause contains, in the Duhem-Quine sense, the "auxiliary hypotheses and approximations introduced to give us some sort of measurable implications in our theoretical models." Neil de Marchi & Jinbang Kim, *Ceteris Paribus Conditions as Prior Knowledge: A View from Economics*, 2 PSA: PROCEEDINGS BIENNIAL MEETING PHIL. SCI. ASS'N 317, 318 (1988); *see also* DANIEL M. HAUSMAN, THE INEXACT AND SEPARATE SCIENCE OF ECONOMICS 306-07 (1992) ("Piere Duhem . . . pointed out that one never tests significant scientific propositions by themselves. Testing an hypothesis involves deriving a prediction from a conjunction of many propositions, of which the hypothesis is only one. Even if one could capture formally the requirement that the hypothesis be essential to the deduction, there would still be the problem that a predictive failure could be due to the falsity of one of these other propositions. Consequently, one can always 'save' any given hypothesis by casting the blame on some other claim."). Viewed in the Duhem-Quine sense, it becomes impossible to identify all of the *ceteris paribus* conditions (i.e., auxiliary assumptions) that might account for the failure of a hypothesis.

Even without attributing much practical bite to the Duhem-Quine thesis, *see, e.g.*, DEBORAH G. MAYO, ERROR AND THE GROWTH OF EXPERIMENTAL KNOWLEDGE 456-59 (1996) (discussing how scientists in practice may overcome the Duhem problem), it should be apparent that it can be difficult to predict, *a priori*, which features of the environment must be held constant for a behavioral tendency to generalize. A recent example from legal decision theory illustrates this difficulty. Early work by Kahneman, Schkade, and Sunstein on how jurors set punitive damages assumed that an averaging of individual mock juror decisions provided a fair representation of the decisions that actual deliberating juries would reach. Later work showed, however, that actual deliberating mock juries are likely to be more unpredictable and possibly more extreme in their punitive damage awards than a process of simply averaging individual juror awards would have suggested. *Compare* Daniel Kahneman, David Schkade, & Cass R. Sunstein, *Shared Outrage and Erratic Awards: The Psychology of Punitive Damages*, 16 J. RISK & UNCERTAINTY 49, 68 (1998) ("There is no reason to believe that our main findings would be altered by the process of group deliberation."), *with* David Schkade, Daniel Kahneman & Cass R. Sunstein, *Deliberating About Dollars: The Severity Shift*, 100 COLUM. L. REV. 1139, 1148 (2000) ("Our earlier study did not . . . involve deliberating juries, and a natural question was whether deliberating juries would produce similar or quite different results."), and Schkade et al., *supra*, at 1143 ("[A]s compared with the median of individual predeliberation judgments, deliberation significantly increases high dollar awards, increases high punishment ratings, decreases low punishment ratings, and modestly increases low dollar awards. To summarize a complex

So we see that a behavioral tendency like the omission bias, when its *ceteris paribus* clause is unpacked, can be a rather fragile contingency likely to exert its influence only under very circumscribed conditions. This is not to say that the omission bias is unimportant, because even phenomena that have small or limited behavioral effects may at times have large negative consequences.[63] It is to say, however, that when we have empirical data establishing the contingent nature of a behavioral tendency, surely we should acknowledge these limiting conditions rather than bury them in (typically implicit rather than express) *ceteris paribus* clauses:[64] "There being no way to avoid ceteris paribus, the second-best strategy is to detail what is in the pound and lay it bare for examination."[65] Just as importantly, we should recognize how little we know about many of these limiting conditions, as indicated by the above discussion of the complex and multiple effects of incentives, group deliberation, and market interactions on rational behavior. Otherwise, the prescriptions drawn from legal decision theory run the risk of being just as unrealistic and predictively inaccurate as prescriptions drawn from law and economics' unrealistic assumption of perfect rationality.

In sum, rather than lowering my concerns about overreaching within legal decision theory, the possibility that tendency talk is becoming the lingua franca of the field heightens my concerns. Tendency talk permits us to repeat the findings from psychology in terms that, while not universalistic, still sound like tidy empirical generalizations with broad implications. Certainly these behavioral tendencies have a specified context in their definitional statements, namely, the *type* of judgment or decision that will evoke the tendency. This truncated contextualism, however, omits some of the most

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analysis, it follows that *deliberating juries produce even more unpredictability than was observed for statistical juries.*"). Thus, the element of group deliberation—a factor originally consigned to the *ceteris paribus* clause—turned out to be quite important to this line of work.

63. *See* Mitchell, *Pessimism*, *supra* note 2, at 1959 ("Factors having small but statistically significant effects in the laboratory may pale in comparison to the force of other factors in real world settings. Conversely, it may be possible to isolate those particular situations in which cognitive biases exert considerable negative causal force on particular people, even if the effect size appears small." (footnote omitted)).

64. My *Equal Incompetence* article discusses numerous studies that demonstrate how particular individual and situational characteristics predictably affect the likelihood that irrational tendencies will express themselves. *See generally* Mitchell, *Incompetence*, *supra* note 2.

65. de Marchi & Kim, *supra* note 62, at 323. The impoundment metaphor flows from Alfred Marshall's famous statement about how economists may simplify the study of a complex problem: "In breaking it up, he segregates those disturbing causes, whose wanderings happen to be inconvenient, for the time in a pound called *Caeteris Paribus*." ALFRED MARSHALL, PRINCIPLES OF ECONOMICS 366 (C.W. Guillebaud ed., 9th ed. 1961) .

interesting and important features of the person and environment that cause irrational tendencies to dissipate or strengthen.

Professor Prentice does advance two arguments that lessen, if not completely remove, the threat of misleading inferences being drawn from legal decision theorists' talk of vaguely delimited behavioral tendencies. First, he correctly notes that some studies find substantial numbers of subjects who act irrationally, and, in such cases, we can be confident that the irrational tendency is strong.[66] Unfortunately, such pervasive patterns of behavior are not always found (or perhaps I should say, fortunately, since we are talking here about *irrational* tendencies), as demonstrated by Professor Prentice's own empirical research.[67] Instead, we find that irrational "tendencies"

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66. *See* Prentice, *supra* note 4, at 1683-84 (discussing research where large majorities of subjects seemed to violate rationality norms).

67. A recent empirical study by Professors Prentice and Koehler demonstrates how the behavior of a subset of experimental subjects can result in the finding of a behavioral tendency, in this case the "normality bias," or the "tendency for people to react more strongly to bad outcomes that spring from abnormal circumstances than to otherwise identical outcomes that spring from more ordinary circumstances." Prentice & Koehler, *supra* note 51, at 588 (footnote omitted).

Using two between-subjects experiments, Prentice and Koehler asked groups of subjects to read scenarios in which a doctor or stockbroker either acted or omitted to act in a way that constituted either a departure from or towards what would be normal or conventional behavior for the scenario in question (e.g., in one experimental condition in the first study on medical malpractice, subjects read about a doctor who, during the course of treatment, changed a cancer patient's treatment from a conventional to an unconventional treatment protocol). *See id.* at 622-25 (describing method and materials for first study on medical malpractice). The scenario informed subjects that the doctor's patient or the stockbroker's client experienced a negative outcome (death or a significant loss of money), and then subjects were asked, among other things, to judge the liability of the doctor and stockbroker. *Id.* at 628-29 (describing method and materials for second study on stock loss); *id.* apps. A, B (providing wording of scenarios and questions presented to subjects). In other words, Prentice and Koehler used an experimental design in which two levels of two independent variables (the normal/abnormal manipulation and the active/passive manipulation) were crossed to form four different scenarios in each experiment, and then, after reading one of the four scenarios, each subject would respond on dependent variables, including the key dependent variables of assigning liability and possible damages for negligence. *See id* at 629-31. For a discussion of the benefits of between-subjects designs in experiments, see Prentice, *supra* note 4, at 1679-86.

Prentice and Koehler found that subjects who read about the doctor or stockbroker who pursued an abnormal approach (i.e., pursued the less conventional route to treatment or investing) were more likely to hold the doctor or stockbroker liable as compared to subjects who read about the doctor or stockbroker who pursued a normal approach (i.e., the normality/abnormality manipulation exerted a statistically significant main effect on ratings of liability in both experiments). *See* Prentice & Koehler, *supra* note 51, at 626 ("In stark contrast to the results for the active/passive variable, the results for the normal/abnormal variable yielded significant differences on the key questions."); *id.* at 630 ("jurors were significantly more likely to find the financial advisor negligent when the stocks were unconventional than when the stocks were conventional"). The active/passive manipulation exerted no statistically significant effect on subjects' responses, and no interaction of the active/passive and the abnormality/normality variable is reported. *See id.* at 625 ("The results showed that jurors did

not treat deaths that resulted from the physician's active deed . . . any differently than deaths that resulted from the physician's passive deed . . . ."); *id.* at 629 ("Data from the Stock Loss experiment showed that the active/passive variable played little, if any, role in jurors' verdicts or the size of damage awards."). Prentice and Koehler thus concluded that subjects exhibited a tendency toward the aforementioned "normality bias," and they noted a possible legal implication of this bias: "a preference for that which is normal may translate into large penalties for unlucky or negligent mavericks and innovators, and smaller penalties (or even exoneration) for equally unlucky or negligent rule-followers." *Id.* at 644.

The following tables summarize the number and percentage of subjects within each experimental condition who held the doctor or stockbroker liable for negligence. (The published report of these experiments does not specify the precise number of subjects who participated in each condition, but Professor Koehler provided me with these numbers through correspondence. E-mail from Jonathan J. Koehler, Distinguished Teaching Associate Professor, University of Texas, to Gregory Mitchell, Assistant Professor, Florida State University (Mar. 24, 2003, 8:34 p.m.) (on file with author). Row totals were calculated from the reported percentages of participants who found the defendant negligent within each specific experimental condition. The article does not report raw numbers of participants finding the defendant negligent within each specific experimental condition hut rather reports only percentages for each condition. Accordingly, the raw numbers below for each condition were calculated by multiplying the number of participants in each condition by the reported percentage of negligence votes in each condition; due to possible rounding errors, these raw numbers may differ slightly from the actual number of participants who found the defendant negligent in each condition.)

Malpractice Study (N=211)	Active Rx	Passive Rx	Row Totals
Conventional Rx	48% (27/56)	50% (26/51)	49% (53/107)
Unconventional Rx	68% (38/56)	62% (30/48)	65% (68/104)
		"Irrational" % Δ:16%	

Stockbroker Study (N=92)	Active Invest Strategy	Passive Invest Strategy	Row Totals
Conventional Stock Mix	45% (10/23)	48% (11/23)	46% (21/46)
Unconventional Stock Mix	68% (16/23)	57% (13/23)	63% (29/46)
		"Irrational" % Δ:17%	

Two methodological points should be explained. First, in these experiments, bias is defined as a statistically significant deviation from perfect verdict consistency across the scenarios. That is, a bias would be found if there appeared to be a systematic difference in the ratio of liable/not liable verdicts between the groups. A normality bias is found if such a systematic difference existed and there are more verdicts for liability when the doctor or stockbroker took an unconventional rather than conventional approach (i.e., normal or conventional approaches are judged less harshly). *See, e.g.*, Joachim Krueger, *The Bet on Bias: A Foregone Conclusion?*, 9 PSYCOLOQUY 1, ¶ 4 (1998) ("The theoretical notion of rational or unbiased reasoning assumes the feeble status point of a point specific null hypothesis, whereas bias lies in any significant departure from this point. [Subjects] have ample room to err, but only one place to be correct. Not surprisingly, NHST reveals that [subjects] 'significantly' miss the point of no bias. With this asymmetric testing, there is a growing conviction that people are cognitively limited or miserly. Investigators demonstrate bias by detecting it. They rarely attempt to detect rational judgment."), *at* http://psycprints.ecs.soton.ac.uk/archive/00000595.

Second, because of the use of a between-subjects design, no individual subject made liability judgments for each of the scenarios, and therefore no individual subject actually changed verdicts depending on whether a normal or abnormal course was taken by the defendant. *See, e.g.*, Mitchell, *Pessimism*, *supra* note 2, at 1948-50 (discussing the inferences required in a between-subjects design to argue that an individual acted irrationally). The best we can do is assume equivalence of persons across all of the conditions and then infer that the normality/abnormality manipulation caused the aggregate verdict totals to differ between the

can be christened even when less than a majority of the research participants act irrationally.[68] Moreover, even these "strong" behavioral tendencies are strong only in the numerical sense and not necessarily in the broader contextualist sense. That is, such findings remove some concerns about complicating individual difference factors that might be contained in a *ceteris paribus* clause, but they do not remove concerns about *ceteris paribus* environmental factors.

Second, Professor Prentice notes that some of the laboratory findings of bias and error have been replicated using subjects other than college students and sometimes in naturalistic settings.[69] Field work provides the best test of whether important complicating factors hide in the *ceteris paribus* clauses that accompany experimental

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groups (i.e., we assume that random assignment to experimental conditions balanced out or controlled for any individual differences so that we may attribute the differences across groups to the experimental manipulations). *See* Camerer, *supra* note 15, at 633 ("Many people think within-subjects analysis is the only proper analysis in choice experiments, because [expected utility] requires consistency of individual preferences. But, of course, between-subjects tests are equally legitimate (though less powerful) if the subjects in different groups can be presumed to have the same distribution of tastes, up to sampling error, because they were drawn from a single population.").

If we assume equivalence across subjects in each of the experimental conditions, then we find that at least 46% of the subjects in each experiment held the defendant liable for negligence regardless of whether the defendant acted normally or abnormally (i.e., across all conditions, at least 46% of the subjects voted for liability), and at least 35% in each experiment held the defendant not liable for negligence regardless of whether the defendant acted normally or abnormally. (The proper focus is on the row totals without regard to column totals, because Prentice and Koehler report only a main effect for the normality/abnormality variable; therefore, the column totals associated with the two levels of the active/passive variable may be collapsed and combined due to the lack of an effect for this variable (i.e., we may act as if this variable had not been tested and ignore the data categories associated with the passive/active variable)). In each experiment, we may infer that sixteen to seventeen percent of the subjects "switched" their votes from not liable to liable solely in response to being told that the defendant's approach was unconventional rather than conventional (i.e., we must assume that the persons who voted for liability in the unconventional treatment/stock mix conditions would have voted against liability in the conventional treatment/stock mix conditions). It is this approximately seventeen percent of the subjects that accounts for the statistically significant percentage of "irrational switches," which in turns leads to the finding of a tendency toward normality bias; the fact that the majority of subjects did not make this irrational "switch" does not lead to a corresponding finding of a tendency toward rationality.

68. *See, e.g.*, Krueger & Funder, *supra* note 5, at 18 ("As sample size increases, the precision of measurement is improved, and more robust statistical tests are employed, ever-smaller effect sizes pass the threshold of significance. In some cases, this allows biases to reach significance even when the modal response is identical with the demands of the normative model." (footnote omitted)); William H. Riker, *The Political Psychology of Rational Choice Theory*, 16 POL. PSYCHOL. 23, 36 (1995) ("None of the experiments displaying inconsistencies in choice portray all subjects as inconsistent. For experimenters to recommend the abandonment of expected utility theory when the experiments themselves show that many people—often well over half, as in the preference reversal experiments—are indeed expected utility maximizers is to ignore the evidence that the experimenters have themselves created.").

69. *See* Prentice, *supra* note 4, at 1698-1702, 1726-29.

results, and so Professor Prentice does a wonderful service in collecting research on whether people "in the wild" express the same irrational tendencies as experimental subjects.[70] This work possesses great utility in the effort to build better models of behavior than the perfect rationality model.[71] The relative ease of experimental work results in too little field work relative to experimental work, and close inspection of this field work often confirms the view that environmental features do matter, and that no simple conclusions flow from rationality research.[72]

The problem with *experimental* replications that use professionals and experts as subjects, rather than the usual sample of undergraduates (i.e., replications in tbe lab rather than the field but using the "real world" target populations as the subjects), is that these replications likewise yield no simple answers, sometimes providing strong support and sometimes weak support for the bias and error portrait of cognition. This mixed evidence is the natural result of the fact that individuals differ significantly in their propensities to act rationally.[73] Indeed, to be faithful to the data, Professor Prentice must describe these findings in terms of behavioral tendencies of varying strengths and specificity that imply unknown *ceteris paribus*

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70. *Id.; see* Edwin A. Locke, *Generalizing from Laboratory to Field: Ecological Validity or Abstraction of Essential Elements, in* GENERALIZING FROM LABORATORY TO FIELD SETTINGS 3, 7-8 (Edwin A. Locke ed., 1986) ("[W]hat is needed when trying to determine the legitimacy of generalization is *the identification of the essential features of field settings that need to be replicated in the lab* (that is, essential subject, task, and setting characteristics). Just what is essential cannot necessarily be known in advance; discovering these essentials is an inductive procedure though plausible hypotheses can be used as guidelines. . . . A key function of lab studies then would be to identify and isolate these essential features so that they could be reproduced in field settings. If generalization succeeded, this would support the validity of the features isolated. If generalization failed, it would imply that one or more essential features had been omitted.").

71. For an excellent use of field work to compare the relative explanatory power of the standard economic model (expected utility) and behavioral model (prospect theory), see Colin F. Camerer, *Prospect Theory in the Wild: Evidence from the Field, in* CHOICES, VALUES, AND FRAMES 288 (Daniel Kahneman & Amos Tversky eds., 2000). With respect to the transactions considered, the behavioral model arguably did a better explanatory job than the economic model. *See id.* at 299 ("[P]rospect theory is a suitable replacement for expected utility because it can explain anomalies . . . and can *also* explain the most basic phenomena expected utility is used to explain.").

72. *Compare, e.g.*, Werner F.M. De Bondt & Richard M. Thaler, *Do Security Analysts Overreact?*, 80 AM. ECON. REV. 52, 57 (1990) ("The same pattern of overreaction found in the predictions of naive undergraduates is replicated in the predictions of stock market professionals."), *with* Michael P. Keane & David E. Runkle, *Are Financial Analysts' Forecasts of Corporate Profits Rational?*, 106 J. POL. ECON. 768, 797 (1998) ("The evidence in this paper strongly supports the view that professional stock market analysts make rational forecasts of earnings per share for the companies they follow.").

73. *See* Mitchell, *Incompetence*, *supra* note 2, at 87-105, 139-60 (discussing research on various individual differences in rationality).

constraints: "statistical experts are sometimes as prone to committing the conjunction fallacy as lay people"; "increased knowledge often correlates with *increased* overconfidence"; "experts often are no better than laypeople at making predictions"; "judges, although less susceptible than jurors, are subject to 'a strong hindsight bias effect"; "professional blackjack players tend to be subject to the omission bias."[74]

Most importantly, the replication of statistically significant findings of biases and errors in college students in "real world" populations should not lead to the conclusion that these replications have practical, real world significance. While the replications may in some instances be of practical significance, a finding of statistical significance inside or outside the lab does not guarantee practical significance.[75] Only an examination of the circumstances under which the effect is found can lead to an assessment of practical significance.[76]

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74. Prentice, *supra* note 4, at 1727-28 (footnotes omitted).

75. For a discussion of the distinction between statistical and practical significance, and the possible pernicious effects of the pervasive use of statistical significance testing in psychology, see Mitchell, *Pessimism*, *supra* note 2, at 1954-60. The essential problem arises from psychology's heavy reliance on null hypothesis significance testing ("NHST") to demonstrate behavioral tendencies: because NHST focuses on averages across groups and is sensitive to sample size and not merely effect size, it is possible that fairly small real differences in behavior between groups or fairly small percentages of subjects within each experimental group expressing irrational behavior will lead to a rejection of the null hypothesis and a finding of an irrational tendency. *Id.*

Professor Prentice's response to my concerns about NHST does not dispute the need for a distinction between practical and statistical significance, but he does argue that there is evidence that many findings of statistical significance also possess practical significance. *See* Prentice, *supra* note 4, at 1687-89.

76. For example, the practical significance of research into jury bias must be evaluated in light of the consistent finding that strength of evidence is generally the best predictor of jury decisions. *See* Dennis J. Devine et al., *Jury Decision Making: 45 Years of Empirical Research on Deliberating Groups*, 7 PSYCHOL. PUB. POL'Y & L. 622, 684-86 (2001) ("Strength of evidence (SOE) is a global term referring to the quantity and quality of evidence presented by the plaintiff/prosecution during a trial. There is no doubt that SOE has an effect on jury verdicts—the real issue is to what extent. . . . Overall, theoretical ambiguity regarding what makes a case 'compelling' and the lack of an accepted metric for its measurement make it difficult to quantify precisely the impact of SOE on jury decisions. Nonetheless, efforts to manipulate SOE or capture its natural variation have produced large and robust effects on jury verdicts and postdeliberation verdict preferences as well as evidence of interaction with other variables. . . . In summary, there is ample evidence supporting the conclusion that SOE is the primary determinant of jury verdicts in criminal trials in most circumstances, but it remains to be determined how important SOE is relative to the many irrelevant biasing factors that may influence jury verdicts." (citations omitted)); *id.* at 701 ("Furthermore, studies that have observed bias attributable to procedural and/or participant characteristics have tended to involve ambiguous evidence. In particular, biasing factors (e.g., pretrial publicity) have been found to have little to no impact when SOE is weak or very strong and have their greatest influence on jury decisions when SOE is moderate."); Michael J. Saks, *What Do Jury Experiments Tell Us About How Juries (Should) Make Decisions?*, 6 S. CAL. INTERDISC. L.J. 1, 19 (1997) ("[E]very researcher who does mock jury experiments is acutely aware that the case facts need to be pretested and adjusted lest they

Once we accept the messiness of the world we are trying to model, and the concomitant complexities and uncertainties in the behavioral evidence on rationality, legal decision theory may be able to move from its many vague *ceteris paribus* claims to sets of "restricted laws": "If a theorist believes that *in a certain domain* the interferences inadequately denoted by the implicit *ceteris paribus* clause are absent, he or she can regard the generalization *in that domain* as a restricted law."[77] Some recent work in legal decision theory seeks to do just this, by carefully examining the complex evidence on particular behavioral phenomena and considering the limits on transporting this evidence to legal settings.[78]

The first principle of legal decision theory should be to go wherever the data takes us, rather than always to reject the rationality assumption of law and economics or to assume that legal actors' irrational tendencies chronically lead to real world mistakes—as Professor Rachlinski persuasively argued recently.[79] If the data reveal that the rationality assumption better fits the evidence in a particular setting (as may be the case, for example, when DNA evidence is presented to juries using natural frequency and graphical formats rather than probability formats alone[80]), then we should favor

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swamp the (usually more subtle) variables that are the focus of the study. In order to maximize the possibility of detecting the effects of variables under study, researchers usually aim to produce cases that are ambiguous, that bring jurors near the midpoint of the scale of voting preferences. This insight from the experimenter's craft bespeaks a widely held, if sometimes unacknowledged, awareness of the power of case information." (footnote omitted)).

77. Hausman, *supra* note 55, at 310 (citation omitted). Elsewhere I discuss in detail how legal decision theory might make this move toward a more contextualized account of behavior. *See* Gregory Mitchell, *Mapping Evidence Law*, 2003 MICH. ST. DCL L. REV. (forthcoming).

78. *See* Chris Guthrie, *Prospect Theory, Risk Preference and the Law*, 97 NW. U. L. REV. 1115, 1156-62 (2003) (discussing limitations on the application of prospect theory research to the law); Korobkin, *supra* note 50, at 1293 ("As empirical research continues to deepen our understanding both about the contexts in which the endowment effect does and does not operate and the causes that drive the endowment effect in those contexts, endowment effect analysis will continue to become more precise, more conclusive, and more useful in the design of legal policy.").

79. *See* Rachlinski, *supra* note 36, at 1206-25 (discussing the adaptive nature of thought and behavior and how these adaptations cloud the case for paternalistic reforms within legal decision theory); *see also id.* at 1168 ("The principal lesson of cognitive psychology is not that people make mistakes. Rather, the lesson is that people develop complex, contextual strategies for making choices.")

80. *See, e.g.*, Samuel Lindsey et al., *Communicating Statistical DNA Evidence*, 43 JURIMETRICS J. 147, 160 (2003) ("Experts and nonexperts alike are easily confused by statistics expressed as probabilities. But this confusion diminishes when the same statistics are expressed as natural frequencies. We found that presenting the same statistical evidence as natural frequencies rather than conditional probabilities dramatically increased the proportion of correct statistical inferences by trained legal decisionmakers and influenced the verdicts in each case."); Dale A. Nance & Scott B. Morris, *An Empirical Assessment of Presentation Formats for Trace Evidence With a Relatively Large and Quantifiable Random Match Probability*, 42 JURIMETRICS J. 403, 437 & 448 app. B (2002) (finding that frequency formats led to less accurate assessments

the rationality assumption in that restricted setting. If legal decision theory overlooks those parts of the evidence that fail to justify paternalistic arguments and oversells those parts of the evidence that support arguments to convert the government into an irrationality monitor, then it will become a political movement rather than a scientific endeavor—and its lifespan will probably be quite short.[81]

III. CONCLUSION

To conclude, I return to my opening suggestion that perhaps Professor Prentice and I have fallen under the spell of biased assimilation processes in our treatments of psychological research on human rationality. If this is true, one should not assume that such a bias is necessarily a bad thing. Indeed, the philosopher of science Miriam Solomon argues that "cognitive bias and belief perseverance" may be "in fact conducive to scientific success."[82] This is so because cognitive and motivational biases in scientific reasoning may lead to a "distribution of research effort wherever differences in individual experience and prior belief arise."[83] Such a distribution of interests and effort may lead to the development of competing theories, the formulation of severe tests of theories with which one disagrees, and, ultimately, the separation of stronger from weaker theories.[84] In short, persistently holding onto our own idiosyncratic theories, even past

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of DNA evidence compared to a chart format in which the links between prior and posterior probabilities given a particular likelihood ratio are graphically presented). The DNA evidence example illustrates bow behavioral evidence on human capabilities and limitations may be combined to find those settings most conducive to rational judgment. A single-minded focus on finding irrational tendencies, which neglects finding the boundaries on these tendencies, would not lead to this adaptive approach.

81. In this respect, I certainly hope that Professor Prentice is correct in his observation that "[e]ven less than law and economics, legal decision theory seems to belong to no particular political camp." Prentice, *supra* note 4, at 1763.

82. Miriam Solomon, *Scientific Rationality and Human Reasoning*, 59 PHIL. SCI. 439, 443 (1992).

83. *Id.* at 452. Solomon is careful to note the speculative nature of this claim, which was formulated from a study of the revolution in geology caused by debate about continental drift theory. *See id.* at 444-52.

84. Professor Prentice makes a similar point: "Indeed, it is 'not so much the critical attitude that individual scientists have taken with respect to their own ideas that has given science its success . . . but more the fact that individual scientists have been highly motivated to demonstrate that hypotheses that are held by some other scientists are false.'" Prentice, *supra* note 4, at 1678 (quoting KEITH E. STANOVICH, HOW TO THINK STRAIGHT ABOUT PSYCHOLOGY 33 (6th ed. 2001)); *see also* Philip Kitcher, *The Division of Cognitive Labor*, 87 J. PHIL. 5, 8 (1990) ("Whereas it may be rational for each of the scientists to believe the theory that is better supported by the available evidence, it may not be rational for each of them to pursue that theory, and what the community cares about is the distribution of pursuit not the distribution of belief.").

some ideal point of rational belief, may, in the long run, be the best way to develop a coherent body of knowledge that can improve public policy.

So perhaps it is a good thing that I have not persuaded Professor Prentice to be more skeptical about the application of psychological research to the law and that Professor Prentice, despite his good arguments, has not yet persuaded me to be bolder in my applications of psychology to the law. Persistence in our views—even irrational persistence—may eventually lead to a brighter future for legal decision theory as competing arguments sort themselves out through further testing and debate.

War and American Constitutional Order

Mark E. Brandon 56 Vand. L. Rev. 1815 (2003)

This essay explores the relationship between armed conflict and constitutionalism in light of the experience of the United States. The relationship is fraught with tension, for while force may be necessary for creating or maintaining a constitutional order, it can also endanger the durability of constitutionalism in the order. Armed conflict has been such a persistent presence from the inception of the American nation that it is appropriate to characterize the United States as a "warrior state." The presence and persistence of militarism threaten to injure constitutionalist norms and institutions. There are five areas of concern in the American case: national ethos, rights, the operation of republican government, the allocation of institutional authority, and sovereignty. The essay concludes by offering a framework for further investigating the constitutional implications of armed conflict.

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