

# University of Pennsylvania Law Review

FOUNDED 1852

---

Formerly  
American Law Register

---

---

---

VOL. 152

MAY 2004

NO. 5

---

---

## ARTICLES

### CASE STUDIES, COUNTERFACTUALS, AND CAUSAL EXPLANATIONS

GREGORY MITCHELL<sup>†</sup>

*This Article critically examines the use of single-observation case studies to develop causal explanations for significant legal events, detailing the evidentiary and inferential problems inherent in this methodology. Most significant among these problems is that focus on a single case necessitates the use of counterfactual thought experiments to test causal hypotheses. The pitfalls of such thought experiments are illustrated through the discussion of the recent attempts to explain the collapse of Enron Corporation using this approach. Application of a set of normative criteria to the products of these Enron thought experiments reveals the suspect nature of causal explanations and policy prescriptions drawn from*

---

<sup>†</sup> Assistant Professor of Law, Florida State University College of Law. The author may be contacted by e-mail at [gmitchel@law.fsu.edu](mailto:gmitchel@law.fsu.edu) or by regular mail at Florida State University College of Law, 425 West Jefferson Street, Tallahassee, Florida, 32306-1601. I appreciate the helpful comments of Scott Baker, Steve Bank, Erica Beecher-Monas, Mitu Gulati, Chris Guthrie, Adam Hirsch, Kim Krawiec, Mae Kuykendall, Don Langevoort, John Monahan, Peter Oh, Jim Rossi, Mark Seidenfeld, Glen Staszewski, and Phil Tetlock on an earlier draft of this Article.

*single-observation case studies. The Article closes with a discussion of the tension in legal scholarship between political relevance and epistemic humility.*

The Enron matter will prove to be a very important event in the history of American shareholder capitalism.<sup>1</sup>

While many of the events are dramatic, their implications do not necessarily fall into the category of “news.”<sup>2</sup>

## INTRODUCTION

What should the legal system learn from the collapse of Enron Corporation?<sup>3</sup> Quite a bit, if the volume of scholarship is any guide. Within two years of Enron’s bankruptcy, legal scholars have offered a dizzying array of views on the meaning of Enron for the legal system’s regulation of corporations, securities and energy markets, gatekeepers and facilitators of these markets, taxes, bankruptcy, employee benefits, the environment, and even the mapping of the human genome.<sup>4</sup>

<sup>1</sup> Jeffrey N. Gordon, *What Enron Means for the Management and Control of the Modern Business Corporation: Some Initial Reflections*, 69 U. CHI. L. REV. 1233, 1249 (2002).

<sup>2</sup> William H. Beaver, *What Have We Learned from the Recent Corporate Scandals That We Did Not Already Know?*, 8 STAN. J.L. BUS. & FIN. 155, 156 (2002).

<sup>3</sup> Enron Corporation sought the protection of the bankruptcy court on December 2, 2001. Voluntary Petition for Enron Corporation at 9, *In re Enron Corp.*, 302 B.R. 455 (Bankr. S.D.N.Y. 2003) (No. 01-16034), available at <http://www.elaw4enron.com/download.asp?DocID=8&FileID=1012&FileName=0001.pdf>. At the time, Enron represented the largest company (in terms of assets) ever to declare bankruptcy within the United States. Within a year, however, an even larger company, WorldCom, Inc., filed for bankruptcy. See Rupert Cornwell, *WorldCom Pledges to Emerge from Chapter 11*, THE INDEPENDENT (London), July 23, 2002, at 17 (“The \$107bn of assets listed by WorldCom eclipses the previous record of \$63bn listed by Enron when it sought bankruptcy protection last December.”); see also BankruptcyData.com, *The Largest Bankruptcies 1980–Present*, at [http://www.bankruptcydata.com/Research/15\\_Largest.htm](http://www.bankruptcydata.com/Research/15_Largest.htm) (last visited Feb. 19, 2004) (listing the largest bankruptcy filings since 1980 by company name, filing date, and amount of total assets).

<sup>4</sup> Within a relatively short time period, law professors, law students, and practitioners have produced a staggering amount of scholarship on Enron. Indeed, as Professor Jonathan Lipson recently wrote, “[t]he Enron case has already spawned a cottage industry among legal academics.” Jonathan C. Lipson, *Directors’ Duties to Creditors: Power Imbalance and the Financially Distressed Corporation*, 50 UCLA L. REV. 1189, 1194 n.6 (2003). For a sampling of the diverse views being offered about the causes and implications of Enron’s demise, see Neil H. Aronson, *Preventing Future Enrons: Implementing the Sarbanes-Oxley Act of 2002*, 8 STAN. J.L. BUS. & FIN. 127 (2002); Douglas G. Baird & Robert K. Rasmussen, *Four (or Five) Easy Lessons from Enron*, 55 VAND. L. REV. 1787 (2002); Erica Beecher-Monas, *Corporate Governance in the Wake of Enron: An Examination of the Audit Committee Solution to Corporate Fraud*, 55 ADMIN. L. REV. 357 (2003); Margaret M. Blair, *Directors’ Duties in a Post-Enron World: Why Language Matters*, 38 WAKE FOREST L. REV. 885 (2003); William W. Bratton, *Enron and the Dark Side of Shareholder Value*,

Many of these ruminations on Enron offer interesting explanations of what went wrong, some suggest creative solutions for the future, and a few may even influence the legislators and judges who must deal with the political and legal fallout from recent corporate failures.<sup>5</sup> We may

---

76 TUL. L. REV. 1275 (2002); John C. Coffee, Jr., *Understanding Enron: "It's About the Gatekeepers, Stupid,"* 57 BUS. LAW. 1403 (2002); Roger C. Cramton, *Enron and the Corporate Lawyer: A Primer on Legal and Ethical Issues*, 58 BUS. LAW. 143 (2002); Lawrence A. Cunningham, *Sharing Accounting's Burden: Business Lawyers in Enron's Dark Shadows*, 57 BUS. LAW. 1421 (2002); Victor B. Flatt, *The Enron Story and Environmental Policy*, 33 ENVTL. L. REP. (Envtl. L. Inst.) 10,485 (July 2003); Victor Fleischer, *Enron's Dirty Little Secret: Waiting for the Other Shoe to Drop*, 94 TAX NOTES 1045 (2002); Louise W. Floyd, *Enron and One.Tel: Employee Entitlements After Employer Insolvency in the United States and Australia (Australian Renegades Championing the American Dream?)*, 56 SMU L. REV. 975 (2003); Lawrence J. Fox, *MDPs Done Gone: The Silver Lining in the Very Black Enron Cloud*, 44 ARIZ. L. REV. 547 (2002); Dennis R. Honabach, *Curbing Misuse of Genetic Information in Light of the Enron Debacle: A Comment on Dean Partlett's Proposal*, 42 WASHBURN L.J. 535 (2003); Robert R. Keatinge, *Multidimensional Practice in a World of Invincible Ignorance: MDP, MJP, and Ancillary Business After Enron*, 44 ARIZ. L. REV. 717 (2002); Kathryn J. Kennedy, *A Primer on the Taxation of Executive Deferred Compensation Plans*, 35 J. MARSHALL L. REV. 487 (2002); Mark Klock, *Two Possible Answers to the Enron Experience: Will It Be Regulation of Fortune Tellers or Rebirth of Secondary Liability?*, 28 J. CORP. L. 69 (2002); Donald C. Langevoort, *The Organizational Psychology of Hyper-Competition: Corporate Irresponsibility and the Lessons of Enron*, 70 GEO. WASH. L. REV. 968 (2002); Jonathan C. Lipson, *Enron, Asset Securitization and Bankruptcy Reform: Dead or Dormant?*, 11 J. BANKR. L. & PRAC. 101 (2002); Anthony J. Luppino, *Stopping the Enron End-Runs and Other Trick Plays: The Book-Tax Accounting Conformity Defense*, 2003 COLUM. BUS. L. REV. 35; Michael A. Perino, *Enron's Legislative Aftermath: Some Reflections on the Deterrence Aspects of the Sarbanes-Oxley Act of 2002*, 76 ST. JOHN'S L. REV. 671 (2002); Burnele V. Powell, *The Lesson of Enron for the Future of MDPs: Out of the Shadows and into the Sunlight*, 80 WASH. U. L.Q. 1291 (2002); Nancy B. Rapoport, *Enron, Titanic, and The Perfect Storm*, 71 FORDHAM L. REV. 1373 (2003); Deborah L. Rhode & Paul D. Paton, *Lawyers, Ethics, and Enron*, 8 STAN. J.L. BUS. & FIN. 9 (2002); Larry E. Ribstein, *International Implications of Sarbanes-Oxley: Raising the Rent on US Law*, 3 J. CORP. L. STUD. 299 (2003); Janis Sarra, *Rose-Colored Glasses, Opaque Financial Reporting, and Investor Blues: Enron as Con and the Vulnerability of Canadian Corporate Law*, 76 ST. JOHN'S L. REV. 715 (2002); Joel Seligman, *No One Can Serve Two Masters: Corporate and Securities Law After Enron*, 80 WASH. U. L.Q. 449 (2002); Steve Stein, *Taxes, Dividends, and Distortions*, POLY REV., June-July 2002, at 59; Norman Stein, *Three and Possibly Four Lessons About ERISA That We Should, but Probably Will Not, Learn from Enron*, 76 ST. JOHN'S L. REV. 855 (2002); Leo E. Strine, Jr., *Derivative Impact? Some Early Reflections on the Corporate Law Implications of the Enron Debacle*, 57 BUS. LAW. 1371 (2002); Cheryl L. Wade, *Comparisons Between Enron and Other Types of Corporate Misconduct: Compliance with Law and Ethical Decision Making as the Best Form of Public Relations*, 1 SEATTLE J. FOR SOC. JUST. 97 (2002); Hunter Carpenter, Comment, *Special-Purpose Entities: A Description of the Now Loathed Corporate Financing Tool*, 72 MISS. L.J. 1065 (2003); Bradley P. Rothman, Note, *401(k) Plans in the Wake of the Enron Debacle*, 54 FLA. L. REV. 921 (2002); Marissa P. Viccaro, Comment, *Can Regulation Fair Disclosure Survive the Aftermath of Enron?*, 40 DUQ. L. REV. 695 (2002); Edward J. McCaffery, *Remove a Major Incentive to Cheat*, WALL ST. J., July 9, 2002, at B2.

<sup>5</sup> For example, law professors seem to have successfully influenced the drafting of a provision of the Sarbanes-Oxley Act of 2002 § 307, 15 U.S.C.S. § 7245 (Law. Co-op.

ask, however, whether these Enron autopsies truly help us understand when business and regulatory failures are likely to occur and how we might prevent them in the future.<sup>6</sup>

---

Supp. 2003), which requires attorneys to report information about securities law violations. See Rachel McTague, *Law Professors Urge SEC to Require Lawyers to Disclose Securities Violations to Directors*, 34 Sec. Reg. & L. Rep. (BNA) No. 11, at 431 (Mar. 18, 2002) (discussing how some law professors believe that securities lawyers should be legally required to report clients' violations of securities laws); Rachel McTague, *SEC Required to Mandate That Lawyers Report Corporate Clients' Securities Offenses*, 34 Sec. Reg. & L. Rep. (BNA) No. 30, at 1227 (July 29, 2002) (describing the "sweeping new auditing and corporate governance legislation"); see also Cramton, *supra* note 4, at 179 n.147 (discussing the advocacy efforts of Professors Susan Koniak and Richard Painter prior to the enactment of the Corporate Reform Act); Larry E. Ribstein, *Bubble Laws*, 40 HOUS. L. REV. 77, 87 (2003) ("The [Sarbanes-Oxley] Act reflects efforts by three specific groups. First, there are the academic reformers who, since the 1930s, have called for increased federal regulation of corporate governance." (footnote omitted)). Furthermore, law professors testified to Congress about the implications of Enron for a variety of legal topics. See *The Legislative History of the Sarbanes-Oxley Act of 2002: Accounting Reform and Investor Protection Issues Raised by Enron and Other Public Companies: Hearings Before the Senate Comm. on Banking, Hous., & Urban Affairs*, 107th Cong. 534-50 (2002) (statement of John C. Coffee, Jr., Professor of Law, Colum. Univ. Sch. of Law) (reiterating previous testimony advocating for the creation of a new self-regulating organization for auditors), available at [http://banking.senate.gov/02\\_03hr/030502/coffee.htm](http://banking.senate.gov/02_03hr/030502/coffee.htm); *id.* at 550-53, 573-82 (statement of Joel Seligman, Dean and Professor of Law, Wash. Univ. Sch. of Law) (arguing for a new auditing self-regulatory organization), available at [http://banking.senate.gov/02\\_03hr/030502/seligman.htm](http://banking.senate.gov/02_03hr/030502/seligman.htm); *Accountability Issues: Lessons Learned from Enron's Fall: Hearing Before the Senate Comm. on the Judiciary*, 107th Cong. 46-47 (2002) (statement of Susan P. Koniak, Professor of Law, Boston Univ. Sch. of Law) (suggesting that the laws regarding suits against lawyers for fraud should be amended), available at [http://judiciary.senate.gov/print\\_testimony.cfm?id=149&wit\\_id=135](http://judiciary.senate.gov/print_testimony.cfm?id=149&wit_id=135); *id.* at 30-34 (statement of Nelson Lund, Professor of Law, Geo. Mason Univ. Sch. of Law) (commenting on the accountability issues that arose in connection with Enron's bankruptcy), available at [http://judiciary.senate.gov/print\\_testimony.cfm?id=149&wit\\_id=139](http://judiciary.senate.gov/print_testimony.cfm?id=149&wit_id=139); *The Fall of Enron: How Could It Have Happened?: Hearing Before the U.S. Senate Comm. on Governmental Affairs*, 107th Cong. 56-58, 93-102 (2002) (testimony and statement of John H. Langbein, Professor of Law, Yale Law Sch.) (proposing imposed diversification in defined contribution plans where employees bear the risk), available at [http://www.senate.gov/~gov\\_affairs/012402langbein.htm](http://www.senate.gov/~gov_affairs/012402langbein.htm); *id.* at 58-74, 103-34 (testimony of Frank Partnoy, Professor Law, San Diego Sch. of Law) (arguing that the derivatives market should be more regulated), available at [http://www.senate.gov/~gov\\_affairs/012402partnoy.htm](http://www.senate.gov/~gov_affairs/012402partnoy.htm); *The Enron Debacle and Gatekeeper Liability: Why Would the Gatekeepers Keep Silent?: Hearings Before the Senate Comm. on Commerce, Sci. & Transp.*, 107th Cong. 10 (2001) (testimony of John C. Coffee, Jr., Professor of Law, Colum. Univ. Sch. Law) (arguing for the creation of a new, self-regulating organization for auditors), available at <http://www.senate.gov/~commerce/hearings/121801Coffee.pdf>.

<sup>6</sup> Technically speaking, Enron Corporation is not yet dead; it remains in bankruptcy while it reorganizes and attempts to start again. However, the Enron known as the "innovative powerhouse on the verge of reshaping the world" surely is dead. Kurt Eichenwald & Diana B. Henriques, *Enron Buffed Image to a Shine Even as It Rotted from Within*, N.Y. TIMES, Feb. 10, 2002, at A1.

Accordingly, rather than offer yet another opinion on the legal lessons of Enron, this Article instead considers how we should go about learning these lessons. More specifically, this Article critically examines how causal stories are created to explain significant legal events such as Enron's demise and how these stories are used to generate policy prescriptions. In a causal story, which is a type of single-observation case study, the author constructs a story to explain why an event occurred and then uses this causal explanation to argue about why the law should or should not be reformed.<sup>7</sup> For instance, Professor Lynne Dallas composes a story about Enron's corporate climate that reveals a "culture [that] did not encourage and support ethical behavior."<sup>8</sup> She then assigns causal significance to this unethical corporate climate in order to support a regulatory agenda: "It is . . . clear that given Enron's climate it was only a matter of time before Enron would have imploded. Congress, the SEC, and other regulatory organizations are, therefore, justifiably directing attention to codes of ethics and ethical climates."<sup>9</sup>

While Professor Dallas and other legal scholars tell interesting and plausible stories to explain how the law failed with respect to Enron

---

<sup>7</sup> For a discussion of how legal scholars' causal stories about Enron fit within the case study research approach, see *infra* Part II.

<sup>8</sup> Lynne L. Dallas, *A Preliminary Inquiry into the Responsibility of Corporations and Their Directors and Officers for Corporate Climate: The Psychology of Enron's Demise*, 35 RUTGERS L.J. 1, 53 (2003). Professor Dallas constructed her story primarily from journalists' reports, *id.* at 45, and identified within Enron a host of cultural features said to encourage or permit unethical and even illegal behavior: (1) "The officers and employees of the company viewed laws and company rules as something to get around or change if they did not serve the company's purpose of making money." *Id.* (2) "Enron supported and encouraged unethical/illegal behavior by maintaining a reward system that was highly political and by failing to consider the manner in which employees booked profits." *Id.* at 46. (3) "The system assured that the powerful players would not be held accountable." *Id.* at 47. (4) "Unthinking loyalty, homogeneity, and the rejection of outsiders characterized the system." *Id.* at 49. (5) "The reward system, which ranked employees against each other and which offered substantial bonuses, did not encourage team work or caring among employees." *Id.* at 50. (6) "Enron attracted individuals who wanted to make a lot of money fast." *Id.* at 51.

<sup>9</sup> *Id.* at 64. To bolster the general import of her causal story, Professor Dallas also alludes to the significance of corporate culture in the WorldCom case and in the prevention of future failures. See *id.* ("By focusing on ethical climates, the organization can take steps to decrease the likelihood of unethical/illegal decisions that, as Enron, World Com and other corporations amply demonstrate, have devastating consequences for employees, shareholders, creditors, and the economy as a whole."). Furthermore, Professor Dallas offers a host of recommendations about possible reforms, including changes to New York Stock Exchange (NYSE) rules, the federal sentencing guidelines, and disclosure and fiduciary duty requirements. See *id.* at 58-63 (detailing such suggested regulatory changes).

and to support their views on reform, reader engagement and plausibility are poor guides to the validity of causal explanations.<sup>10</sup> In fact,

---

<sup>10</sup> Causal stories about Enron fit within the “causal relevance model of explanation.” W. H. Newton-Smith, *Explanation, in A COMPANION TO THE PHILOSOPHY OF SCIENCE* 127, 129 (W. H. Newton-Smith ed., 2000). “Reflections on the role of causation in explanation have given rise to the causal relevance model of explanation . . . . On this model explanations are not arguments. It is the causal features of the world, aspects of the causal mechanisms in the world, which explain.” *Id.* Causal explanations “draw attention to what in the world is in part at least causally responsible for what we want to explain,” *id.*, and causal explanations are “[p]robably the most fundamental form of explanation.” William F. Brewer et al., *Explanation in Scientists and Children*, 8 MINDS & MACHINES 119, 125 (1998). Moreover, others have stated that “[i]n all branches of social science, the identification of genuine causes is accorded a high priority because it is viewed as the basis for understanding social phenomena and building an explanatory science. Causal judgments are made to explain the occurrence of events, to understand why particular events occur.” Margaret Mooney Marini & Burton Singer, *Causality in the Social Sciences*, 18 SOC. METHODOLOGY 347, 347 (1988). Indeed, Jon Elster contends that causation plays an ultimate role in all explanation. See JON ELSTER, NUTS AND BOLTS FOR THE SOCIAL SCIENCES 3 (1989) (“To explain an event is to give an account of why it happened. Usually, and always ultimately, this takes the form of citing an earlier event as the cause of the event we want to explain, together with some account of the causal mechanism connecting the two events.”).

Some philosophical and psychological debate surrounds the nature of explanation, and the causal relevance model of explanation is not the sole account offered by philosophers or psychologists. See, e.g., Herbert A. Simon, *Discovering Explanations*, 8 MINDS & MACHINES 7, 9 (1998) (“In contrast to the general (although not universal) agreement that explanation is central to science, there has been much less agreement as to just what constitutes an explanation of empirical phenomena.”). For a sampling of different approaches to the topic of explanation, see ERNEST NAGEL, THE STRUCTURE OF SCIENCE: PROBLEMS IN THE LOGIC OF SCIENTIFIC EXPLANATION 15-28 (1961); Peter Achinstein, *The Pragmatic Character of Explanation*, 2 PSA: PROC. BIENNIAL MEETING PHIL. SCI. ASS’N 275 (1984); Michael Bradie, *Ontic Realism and Scientific Explanation*, 63 PHIL. SCI. S315 (1996); Clark Glymour, *Explanations, Tests, Unity and Necessity*, 14 NOÛS 31 (1980); Philip Kitcher, *Explanatory Unification*, 48 PHIL. SCI. 507 (1981); Peter Railton, *Probability, Explanation, and Information*, 48 SYNTHÈSE 233 (1981); Fritz Rohrlich, *Scientific Explanation: From Covering Law to Covering Theory*, 1 PSA: PROC. BIENNIAL MEETING PHIL. SCI. ASS’N 69 (1984); Bas C. van Fraassen, *Salmon on Explanation*, 82 J. PHIL. 639 (1985). Nevertheless, it appears that a great number of legal explanations involve, to some degree, causal explanations because causal claims are crucial to policy prescription, which is central to much academic legal scholarship. Indeed, Professor Edward Rubin identifies prescription as the central feature of standard legal scholarship, which he defines as “work which frames recommendations, or prescriptions, to legal decision-makers”:

In some cases the work critiques an existing judicial decision, statute, regulation, or constitution, and the recommendation is simply that the decision-maker should act differently. Less commonly, it merely offers a recommendation . . . . Most often, the work combines a critique of an existing decision with a prescription, . . . for a different approach. And sometimes it even suggests that the decision-makers are doing exactly the right thing. All this work, however, is characterized by its normative quality and the direct engagement of its recommendations with identifiable legal decision-makers.

the methodological problems associated with the causal story approach are so severe that many social scientists avoid this approach if at all possible. Social scientists would argue that causal stories provide, at best, innocently misleading portraits of the causes of behavior and, at worst, unavoidably partial stories biased by the writer's preexisting beliefs and values.<sup>11</sup>

---

Edward L. Rubin, *Law and the Methodology of Law*, 1997 WIS. L. REV. 521, 522.

<sup>11</sup> See, e.g., ROBYN M. DAWES, EVERYDAY IRRATIONALITY: HOW PSEUDO-SCIENTISTS, LUNATICS, AND THE REST OF US SYSTEMATICALLY FAIL TO THINK RATIONALLY 114 (2001) (noting that stories "are often *selected* to prove a point, rather than forming a basis of a statistical generalization . . . or a causal inference, and [i]t is the generalization or inference that leads to the selection of the story in the first place—with the results that the story provides absolutely *no* new information."); GARY KING ET AL., DESIGNING SOCIAL INQUIRY: SCIENTIFIC INFERENCE IN QUALITATIVE RESEARCH 211 (1994) ("In general, the single observation is not a useful technique for testing hypotheses or theories."); Tim Büthe, *Taking Temporality Seriously: Modeling History and the Use of Narratives as Evidence*, 96 AM. POL. SCI. REV. 481, 489 (2002) ("[D]ue to the limited truth claims of narratives, those who use historical narratives as empirical evidence for a causal explanation will probably fail to assess alternative explanations and, if they try, will fail to convince skeptics."); Shari Seidman Diamond, *Empirical Marine Life in Legal Waters: Clams, Dolphins, and Plankton*, 2002 U. ILL. L. REV. 803, 805 (noting social scientists' complaint that an anecdote is an unsatisfactory source of evidence not because it is nonempirical, but because it is a weak form of empirical evidence, and that "it typically is presented without information about how the particular instance described in the anecdote was selected, how accurately it is being described, and how representative it is of the population of occurrences the author is using it to illustrate"); Arend Lijphart, *The Comparable-Cases Strategy in Comparative Research*, 8 COMP. POL. STUD. 158, 160 (1975) ("Case studies . . . are intensive but uncontrolled examinations of single cases that cannot directly result in empirical generalizations and cannot even be used to test hypotheses."); see also Richard A. Epstein, *Does Literature Work as Social Science? The Case of George Orwell*, 73 U. COLO. L. REV. 987, 1002-10 (2002) (discussing problems with inferring general behavioral patterns and explanations from the personal experiences and preferences of the writer); Dean Keith Simonton, *Qualitative and Quantitative Analyses of Historical Data*, 54 ANN. REV. PSYCHOL. 617, 628 (2003) ("Many psychologists believe that quantitative analyses are far more scientific—more objective and rigorous—than qualitative analyses. Moreover, this belief enjoys some support in the empirical (quantitative) research on human information-processing capacities."). For a discussion of the use of a "one-shot case study" when "it is all that we have," see Donald T. Campbell, *Degrees of Freedom" and the Case Study*, 8 COMP. POL. STUD. 178, 179 (1975). Professor Donald Campbell, however, seems to have in mind not single-event studies, but rather, single-culture studies. *Id.* at 186-88. He also writes that "[w]ith a single observation at hand, it is impossible to separate the subjective and objective component" of the observation. *Id.* at 189 (citation omitted).

With respect to the ideological bias point from the text, consider the hypothesis offered by the political psychologist Philip Tetlock that, were we to substitute the failure of Enron with the failure of Amtrak, we likely would see a role reversal in causal storytelling. Instead of market skeptics drawing broad regulatory lessons from the fall of an erstwhile free market paragon, while fiscal conservatives downplay the regulatory lessons to be learned, we likely would see conservatives drawing broad, deregulatory lessons from the failure of a government-sponsored monopoly, while advocates of such

This Article analyzes treatments of the Enron matter using this causal story approach to illustrate (1) the evidence-selection and analytical problems that arise in the construction of causal stories; (2) the necessary reliance in this approach on thought experiments and counterfactual causal reasoning, modes of analysis highly susceptible to inferential bias and fallacy; and (3) the indeterminacy of causal accounts offered under this approach. Although these problems are substantial, the causal story approach does not need to be entirely rejected. A more constructive approach is to consider ways to improve the use of causal stories. To this end, this Article proposes a set of normative criteria that may be used to evaluate causal stories. Although the Enron causal stories often fail to satisfy one or more of these normative criteria and thus should be given little or no prescriptive weight as presently constructed, the criteria point to ways in which the stories may be improved.

The purpose of this Article is to draw attention to the unavoidable methodological and interpretive problems associated with the use of causal stories in the law and to argue that stringent normative criteria should guide the use of these modes of analysis. Causal stories about Enron serve as the focus of this discussion not because they represent particularly egregious examples of this genre, but because they provide good and prominent examples of the methodological hazards that arise employing this approach. Moreover, consumers of these stories may benefit from a discussion of ways to evaluate and adjudicate among conflicting causal accounts. Causal storytelling in the law is not unique to Enron, nor is the application of counterfactual thought experiments unique to causal storytelling.<sup>12</sup> The dangers of

---

government programs would downplay the deregulatory lessons to be learned. E-mail from Philip Tetlock, Lorraine Tyson Mitchell Chair II in Leadership and Communication, Haas School of Business, University of California, Berkeley, to Gregory Mitchell, Assistant Professor of Law, Florida State University College of Law (Nov. 28, 2002, 09:11 PST) (on file with author).

<sup>12</sup> See, e.g., Daniel A. Farber & Suzanna Sherry, *Telling Stories Out of School: An Essay on Legal Narratives*, 45 STAN. L. REV. 807, 808 (1993) ("Reliance on case studies and other narratives is hardly new to legal scholarship."). The terrorist attacks of September 11, 2001, for instance, have served as fodder for causal storytelling. See Kent Greenfield, *September 11th and the End of History for Corporate Law*, 76 TUL. L. REV. 1409, 1411-14 (2002) (analyzing the causal story that September 11th resulted from lax security at the airport). In one instance, Enron and the attacks of September 11th were combined to tell a legal story. Faith Stelman Kahn, *Bombing Markets, Subverting the Rule of Law: Enron, Financial Fraud, and September 11, 2001*, 76 TUL. L. REV. 1579 (2002). Other events, like the O.J. Simpson criminal trial, the Bill Clinton impeachment trial, and the case of *Bush v. Gore*, likewise provoked reflection on whether these cases are symptomatic of larger problems in the legal and political systems and, if so,

these methods extend beyond the creation of causal explanations for business failures.

Furthermore, the Enron stories do not serve as departure points because they necessarily get the story wrong. In fact, given the multiple, conflicting stories, some accounts surely get the story right. Indeed, implementing the reforms suggested by some of the Enron scholars might turn out well in the end. The simple point argued here is that these causal stories, as currently constituted, provide little justification for the reform of our laws or legal theories. While some causal stories are better than others, scrutiny of these stories reveals that even the best causal stories provide, at most, a starting point for more rigorous analysis of the causes of business and regulatory failures.

The Article proceeds by illustrating in Part I the range of causal stories used to explain Enron's collapse and to argue for and against particular legal reforms. These causal stories are then situated within the broader category of empirical approaches to causal inference to demonstrate that the stories present special problems when used to develop singular causal explanations for Enron's failure or general causal explanations for business failures.

Part II then details specific evidentiary and inferential problems with the Enron causal stories and promulgates a set of evaluative criteria that may be used to separate the better stories from the worse in future research. The Article concludes by considering reasons why legal scholars may choose the causal story approach over more rigorous forms of causal analysis and generalization.

---

whether there are possible cures. For discussions of the significance of these cases, see Peter Arenella, *O.J. Lessons*, 69 S. CAL. L. REV. 1233 (1996); Erwin Chemerinsky, *How Should We Think About Bush v. Gore*, 34 LOY. U. CHI. L.J. 1 (2002); Christopher B. Mueller, *O.J. Simpson and the Criminal Justice System on Trial*, 67 U. COLO. L. REV. 727 (1996); William J. Stuntz, *O.J. Simpson, Bill Clinton, and the Transsubstantive Fourth Amendment*, 114 HARV. L. REV. 842 (2001).

Prominent recent examples of counterfactual thought experiments about the law include the essays in WHAT BROWN V. BOARD OF EDUCATION SHOULD HAVE SAID: THE NATION'S TOP LEGAL EXPERTS REWRITE AMERICA'S LANDMARK CIVIL RIGHTS DECISION (Jack M. Balkin ed., 2001), and *Constitutional Commentary's* 1999 symposium, *The Sound of Legal Thunder: The Chaotic Consequences of Crushing Constitutional Butterflies*, in which legal scholars were asked to erase some development from American constitutional law and then "describe all the consequences, desirable or not, that flow from the stomping of the unlucky constitutional butterfly." Jim Chen, *Introduction: The Sound of Legal Thunder: The Chaotic Consequences of Crushing Constitutional Butterflies*, 16 CONST. COMMENT. 483, 484 (1999).

## I. TELLING ENRON STORIES

Many legal scholars mark the fall of Enron as a momentous event in our legal and economic history. They have described it in various ways, including as “an occasion for moral lament”;<sup>13</sup> evidence that “[w]hat is needed today, short of perhaps a wholesale change in modern capitalism, is a new SEC”;<sup>14</sup> proof of “the inability of government regulation and criminal law to prevent the widespread dislocations caused by corporate misconduct”;<sup>15</sup> and, most strikingly, along with the September 11th terrorist attacks, as “the product of unchecked, antidemocratic fanaticism” reflecting a “deep hostility to the norms and values embodied in the notion of the secular ‘rule of law.’”<sup>16</sup> Given the significance assigned to Enron, it is not surprising that many legal scholars would seek to understand the underlying reasons in hopes of preventing future collapses.<sup>17</sup>

<sup>13</sup> Norman W. Spaulding, *Where Were the Lawyers? Enron and Legal Ethics*, BOALT HALL TRANSCRIPT, Fall/Winter 2002-03, at 32.

<sup>14</sup> Michelle Chan-Fishel, *After Enron: How Accounting and SEC Reform Can Promote Corporate Accountability While Restoring Public Confidence*, 32 ENVTL. L. REP. (ENVTL. L. INST.) 10,965, 10,977 (Aug. 2002) (footnote omitted).

<sup>15</sup> Jill E. Fisch, *Taking Action Against Auctions: The Third Circuit Task Force Report*, 74 TEMP. L. REV. 813, 819 (2001).

<sup>16</sup> Kahn, *supra* note 12, at 1599.

<sup>17</sup> Professor Faith Stevelman Kahn explains why so many legal scholars may be interested in the cause of Enron’s demise:

In performing the autopsy of Enron, WorldCom, and Arthur Andersen, it makes sense to consider what makes a body corporate powerful or weak, moral or corrupt, responsible or exploitative? Relatedly, what can be done through law reform to promote equality and meritocracy within corporate and securities law and elsewhere in economic regulation? These are not questions commonly addressed within academic corporate law, but they are—as American-style economic institutions and legal standards are becoming ever more globally influential—questions that could not be more important or timely.

*Id.* at 1638. It is important to note here that legal scholars cannot be faulted for seeking explanations of a significant legal event such as Enron’s failure. Indeed, the psychologist Alison Gopnik sees explanation as playing such a fundamental, formative role in the development of understanding of the world that she likens the drive to explain to the drive to procreate:

My hypothesis will be that explanation is to cognition as orgasm is to reproduction. It is the phenomenological mark of the fulfillment of an evolutionarily determined drive. From our phenomenological point of view, it may well seem to us that we construct and use theories in order to achieve explanation or have sex in order to achieve orgasm. From an evolutionary perspective, however, the relation is reversed[;] we experience orgasms and explanations to ensure that we make babies and theories.

Alison Gopnik, *Explanation as Orgasm*, 8 MINDS & MACHINES 101, 102 (1998). Thus, from the legal scholar’s viewpoint, explaining a significant event like Enron’s collapse

In this endeavor, legal scholars offer a wide range of explanations for Enron's demise and draw diverse and, at times, inconsistent, lessons from Enron. These Enron postmortems necessarily involve inferences about the causes of Enron's collapse. Any diagnosis of the conditions leading to the corporate failure or any prescription to prevent future failures requires a set of beliefs about the causes of past failures. Even broad forecasts about the historical significance of Enron require a set of beliefs about how past events will alter future events—that is, a set of beliefs about causal relations in the world. As the following examples illustrate, the stories being told about Enron vary in their specificity about the causal processes supposedly at work and the complexity of the causal story being told.

The most basic explanations for Enron assign greed or character a causal role: "It was the perceptions of greed that largely resulted in the demise of Enron and its tax advisors."<sup>18</sup> Professor Victor Flatt similarly sees an uncontrolled acquisitive drive as Enron's undoing:

In order to understand why Enron's actions and subsequent collapse were inevitable, we must focus on the basic primal principle of "taking" what is available. Property rights and accompanying moral codes must be seen as a derogation of what exists "naturally" before this time, and what exists in most animal species. That is, humans take whatever they can, in any way they can, without restraint, at least from noncooperating family or tribal units. This suggests that humans will work around any prohibition that is not clear, or will be tempted to take anything of value if the taking is not explicitly prohibited.<sup>19</sup>

Professor Nancy Rapoport settles on character as a key causal factor: "If we are to believe that there is a single root cause of the Enron mess (an arguable point at best in such a complicated situation), failure of character gets my nomination."<sup>20</sup> She then posits that both a lack of moral interdependence and the effects of cognitive dissonance as precursors to this lack of character ostensibly caused Enron's attorneys to turn a blind eye to the ongoing problems:

---

may be quite satisfying both professionally and intellectually, for the explanation may influence other academics as well as the legal system and may be the seed that gives birth to other ideas or even a broader theory. The question this Article addresses is not whether legal scholars should seek explanations for significant legal events, but rather, how they might best go about formulating them.

<sup>18</sup> Harold S. Peckron, *Watchdogs That Failed to Bark: Standards of Tax Review After Enron*, 5 FLA. TAX REV. 853, 913 (2002).

<sup>19</sup> Flatt, *supra* note 4, at 10,492.

<sup>20</sup> Rapoport, *supra* note 4, at 1379.

If the lawyers saw themselves as morally *independent* from Enron, rather than morally *interdependent*, then they might well have believed that it was Enron's job, not theirs, to ensure follow-through. A more complex explanation is that cognitive dissonance . . . prevented the lawyers from seeing some of these deals more clearly. My hunch is that both concepts—a mistaken belief in moral independence, rather than interdependence, and the effects of cognitive dissonance—played a part in any failures by the gatekeepers.

. . . .

. . . Enron's collapse wasn't due to a "perfect storm" of mere coincidence—the collapse was caused by humans and their hubris. We need to ensure that hubris doesn't blind us to the first rule of leadership: It's all about character.<sup>21</sup>

Professor Rapoport draws from her character-based Enron story a fairly simple solution that, if implemented, is unlikely to upset greatly the legal status quo: to encourage greater diligence and influence on the part of attorneys by reminding them that their professional livelihood and reputations are tied to the livelihood and reputations of their clients.<sup>22</sup> In contrast, Professor Flatt's Hobbesian view of

<sup>21</sup> *Id.* at 1384-85, 1394-95. Professor Rapoport notes that at least one in-house attorney did raise some questions about Enron's deals (Sherron Watkins apparently wrote a letter to CEO Kenneth Lay expressing concern about Enron's accounting practices), but Rapoport suggests that this attorney did not follow through sufficiently in the face of resistance by senior officers. *Id.* at 1380-82. Rapoport further maintains:

Watkins wasn't the lone voice questioning Enron's deals; others, including Enron Global Finance's General Counsel Jordan Mintz, were concerned about the structure and disclosure of the various deals. Apparently, [CFO Andrew] Fastow and [former CEO Jeffrey] Skilling didn't brook disagreement willingly. Those who objected often found themselves the subject of pressure, downright abuse, and exile.

*Id.* at 1383 (footnote omitted); *see also id.* at 1391 ("Lawyers *need* to behave as true counselors to their clients, rather than as hired guns who are just following orders. Society needs us to take on the role of the social conscience (or, if that sounds too darn highfalutin', the role of the grease that helps society run).").

<sup>22</sup> *See id.* at 1394 (describing a proposed new rule of professional responsibility that would make lawyers "morally accountable for their conduct as lawyers" (footnote omitted)). Professor Rapoport suggests that a new model rule of professional responsibility might encourage a greater sense of moral interdependence:

So how do we encourage lawyers to withstand peer pressure and client pressure, especially in those grey areas in which the lawyer gives advice akin to "it's an aggressive interpretation of the law" and the client chooses to use that aggressive interpretation, even at the risk of later litigation? . . .

Personally, I like Russ Pearce's idea that we create a new Model Rule 1.0. His Model Rule 1.0 would provide that "lawyers are morally accountable for their conduct as lawyers." That rule hits the question of moral interdependence head on, and it provides a powerful reminder that "just following orders" is the weakest of excuses.

corporate behavior would lead to a wholesale reconsideration of the reliance placed on self-regulation in the securities market and a reluctance to further deregulate energy markets.<sup>23</sup>

In perhaps the most influential analysis of Enron to date, Professor William Bratton reaches a similarly skeptical view of self-regulation and the power of market forces to constrain corporate misdeeds, through a different route than Professor Flatt.<sup>24</sup> Whereas Professor

*Id.* (footnotes omitted). While not arguing for changes in the law, Professor Rapoport does hope to upset the professional and educational status quo. She contends that improvements in legal ethics must start “from the top, with judges, partners, bar associations, and other senior lawyers all singing the same tune,” and not solely with the ineffectual preachings of law professors to students. *Id.* at 1391. Professor Harold Peckron likewise refrains from drawing intrusive lessons from Enron but writes that the example of Enron itself may deliver a useful wake-up call to accountants:

So if in the post-Enron planning world the typical tax advisor has become more circumspect and exercises greater due diligence in the review of proposed tax shelter transactions, can this be an entirely bad outcome? Perhaps now tax lawyers and accountants will consider anew how much they can lose and how much they can win.

Peckron, *supra* note 18, at 915-16.

<sup>23</sup> Flatt, *supra* note 4, at 10,495. Along these lines, Professor Flatt uses Enron to argue against market-based environmental policies that would involve significant self-regulation:

Self-regulation cannot be counted on, and if government regulation is too expensive or difficult, the savings of the “market” program may be illusory. The Enron story allows us to see what will happen in complex trading schemes and self-regulation in the face of opposing incentives. It is the story of human behavior itself. This lesson must be learned in formulating environmental policy. A large-scale market-based strategy, involving acid rain deposition, has worked, but it is the exception, not the rule. Enron is the rule. This is the legacy for environmental policy of the collapse of Enron.

*Id.*

<sup>24</sup> See Bratton, *supra* note 4, at 1337 (“Enron, then, reminds us that the monitoring model assures us of little.”); *id.* at 1340 (“But, then, if Enron teaches us anything, it is to question the reasonableness of reliance on any corporate monitor.”).

Several legal scholars rely on Professor Bratton’s article as a key source of information about the Enron affairs. See, e.g., Simon Deakin, *Squaring the Circle? Shareholder Value and Corporate Social Responsibility in the U.K.*, 70 GEO. WASH. L. REV. 976, 976 n.1 (2002) (referring to Professor Bratton’s article as an “exceptionally helpful account” of Enron’s fall); Theresa A. Gabaldon, *Corporate Conscience and the White Man’s Burden*, 70 GEO. WASH. L. REV. 944, 953 n.51 (2002) (referring the reader to the Bratton article for “an excellent discussion of the notorious Enron collapse”); Langevoort, *supra* note 4, at 973 n.14 (noting that the Bratton article served as one of the sources for his account of the Enron affair); Gregory A. Mark, *The Legal History of Corporate Scandal: Some Observations on the Ancestry and Significance of the Enron Era*, 35 CONN. L. REV. 1073, 1073 n.1 (2003) (referring the reader to the Bratton article for “a comprehensive account of what occurred inside Enron and a discussion of the merits of corporate governance reforms as a response to the corporation’s collapse”); David Millon, *Enron and the Dark Side of Worker Ownership*, 1 SEATTLE J. FOR SOC. JUST. 113, 113 n.2 (2002) (describing

Flatt tells a fairly simple causal story that invokes human nature to explain Enron, Professor Bratton's story is more complicated. He first reviews the apparent facts of Enron to evaluate "four causation stories"<sup>25</sup> that might explain Enron's collapse: (1) "Enron as [c]onventional [m]arket [r]eversal,"<sup>26</sup> (2) "Enron as [d]erivative [s]peculation [g]one [w]rong,"<sup>27</sup> (3) "Enron as a [d]en of [t]hieves,"<sup>28</sup> and (4) "Enron as a [b]ank [r]un."<sup>29</sup> Professor Bratton finds some truth in each of these stories but proposes an underlying causal

---

Bratton's article as "a thorough exposition of the problems at Enron and a highly insightful analysis of their policy and theoretical implications"); Marleen A. O'Connor, *The Enron Board: The Perils of Groupthink*, 71 U. CIN. L. REV. 1233, 1233 n.2 (2003) (directing the reader to the Bratton article for a "detailed history and examination of the causes of the fall of Enron"); MARGARET M. BLAIR, POST-ENRON REFLECTIONS ON COMPARATIVE CORPORATE GOVERNANCE 3 (Geo. Univ. Law Ctr., Working Paper No. 316663, 2002) (relying on the Bratton article for his description of the Enron climate as one in which "a group of grand masters of the U.S. system constructed increasingly complex and opaque transactions designed . . . to deceive them by moving the cups so quickly and cleverly that investors could no longer figure out which cup had the actual penny under it").

<sup>25</sup> Bratton, *supra* note 4, at 1286. Professor Bratton relies on media reports and editorials; the Powers Report, WILLIAM C. POWERS, JR., ET AL., REPORT OF INVESTIGATION BY THE SPECIAL INVESTIGATIVE COMMITTEE OF THE BOARD OF DIRECTORS OF ENRON CORP. (2002) [hereinafter POWERS REPORT], available at 2002 WL 198018; annual reports and other SEC filings by Enron; and testimony at congressional hearings to write his causation stories.

Note that the psychologist Robyn Dawes's description of the structure of a story accords with Bratton's notion of a "causation story": "The structure of a story is that it consists of a *single* sequence of events often linked with a set of *hypothesized* causal influences." DAWES, *supra* note 11, at 112.

<sup>26</sup> Bratton, *supra* note 4, at 1299. This story examines the role of "erroneous business judgment and bad luck on the part of managers." *Id.* at 1302. Professor Bratton asserts that "these causes were necessary but not sufficient for [Enron's] collapse, at least on the present state of the record." *Id.*

<sup>27</sup> *Id.* Professor Bratton argues that risky and undisclosed derivatives trading cannot explain why Enron failed when it did. "[D]erivatives trading very well may have brought Enron down in 2002 or thereafter. But in 2001, when Enron filed for bankruptcy, none of the foregoing was known to the financiers and related actors who determined Enron's fate. Strictly speaking, then, a malfunctioning derivatives operation did not bring Enron down." *Id.* at 1304.

<sup>28</sup> *Id.* at 1305. This story focuses primarily on the role of self-dealing by Enron insiders and potential fraud in the reporting of financial information for the personal enrichment of officers. *Id.* at 1305-20. Once such self-dealing and potential fraud were disclosed, Enron suffered a "credibility deficit" in the market that "in time could have brought down the firm. As to that we can only speculate, for independent reasons brought about Enron's collapse before the implications of its [Special Purpose Entity (SPE)] accounting could be assimilated fully." *Id.* at 1320.

<sup>29</sup> *Id.* This story focuses on the adverse effect of credit rating downgrades and creditor demands on Enron following key financial disclosures in 2001. *Id.* at 1320-25.

theme founded upon a tournament metaphor, a win-at-any-cost mentality that he contends informs all of these other subplots.<sup>30</sup>

Professor Bratton views this tournament mentality, which ostensibly led to a disastrous outcome in Enron when the other causal forces aligned themselves, as “inseparable from shareholder value maximization, for the big scores in the stock market come from firms run by entrepreneurs rather than by conventional managers. On the downside, however, it can lead to errant decision making.”<sup>31</sup>

Although Bratton eschews fundamental reforms to the securities market and corporate monitoring system, his understanding of Enron and its corporate culture leads him to three lessons that convey an overall message strongly in favor of stepped-up governmental oversight and greater skepticism about the powers of self-regulation and market discipline:

First, Enron collapsed the same way banks routinely collapsed in the days before deposit insurance. It did so because it had largely succeeded in realizing [Jeffrey] Skilling’s vision of becoming a financial institution. Huge financial institutions present special regulatory problems and are subject to special requirements. . . . Emerging financial institutions should be brought into the system in the ordinary course.

<sup>30</sup> Professor Bratton does not pick any particular story as the most compelling, but rather, states that “[a]ll four of the preceding stories figure into the final account of Enron’s collapse.” *Id.* at 1326. He does say, however, that “[w]e can pare down the account by coupling the crisis of confidence and the hidden \$4 billion of additional obligations as primary causes.” *Id.* He then supplements these stories with his own, primarily social-psychological account of hubris and motivations gone awry, to form one apparently encompassing causation story. *See id.* at 1332 (referring to his discussion of how the four causation stories form one story). He views Enron’s downfall as a result of this hubris and win-at-any-cost mentality:

Enron fell because it pursued winning to excess. At Enron, winning was everything and everything became a tournament.

. . . .  
 . . . To keep their victory lap going, Enron’s managers invented winning value numbers, crossing the line to fraud.

Enron’s managers, with a belief system biased toward winning, lost touch with both hard economic constraints and the rules of the game.

. . . .  
 As Enron’s leaders stepped across the line to fraud, their belief system trumped reality. So as to avoid confronting their own failures, they averted their eyes from the manifest implications of their own actions. They acted out the role of the tournament winner right up to the end.

*Id.* at 1329-32 (footnote omitted).

<sup>31</sup> *Id.* at 1331. Professor Bratton describes this mentality as a type of cognitive bias: “The optimistic entrepreneur labors under a cognitive bias, which underweights downside risk and overweights both the probability of upside gain and the entrepreneur’s own abilities and contributions.” *Id.*

Second, . . . [Enron] highlights the limits on what self-regulation and market incentives can achieve. Sovereign mandate and punishment remain the bedrock of capitalism.

Third, . . . we should treat with utmost skepticism actors who preach market discipline from positions of safety behind the shields of corporate entities.<sup>32</sup>

Professor Donald Langevoort builds on this story to suggest that “the social forces and selfish norms that emerge fairly naturally in highly competitive settings . . . dominate as behavioral influences over anything but high-powered legal controls.”<sup>33</sup> He sees Enron as representative of such highly competitive firms, where these strong, counterproductive behavioral tendencies may emerge.<sup>34</sup> Simply “tweaking” the law to improve corporate conduct will not prevent future Enrons.<sup>35</sup>

<sup>32</sup> *Id.* at 1360-61. As to lesson two, he writes that “if Enron teaches us anything, it is to question the reasonableness of reliance on any corporate monitor.” *Id.* at 1340. As to lesson three, Professor Bratton concludes that cases like Enron are unavoidable consequences of our choice of entrepreneurial system:

Enron shows that the incentive structure that motivates actors in our self-regulatory governance system generates much less powerful checks against abuse than many observers believed. This point does not by itself validate any particular regulatory corrective. The costs of any regulation can outweigh the compliance yield, particularly in a system committed to open a wide field for entrepreneurial risk taking. Such a system can no more break the iron law of risk and return than could Michael Milken and his junk bonds. If we seek high returns, we must discount for the risk that rationality and reputation will sometimes prove inadequate as constraints.

*Id.* at 1288. However, he does believe that the system could be improved with respect to the quality of gatekeeping provided in particular by auditors. In this respect, he advocates for a “complete separation of auditing from consultancy” for accounting firms. *Id.* at 1358; *see also id.* at 1356 (suggesting that the big accounting firms “be unbundled and auditing firms . . . perform only one function”).

<sup>33</sup> Langevoort, *supra* note 4, at 968.

<sup>34</sup> Professor Langevoort writes that the “paradigmatic examples” of highly competitive firms “are knowledge and service-based firms in markets with relatively low barriers to entry and high rewards for innovation. Enron clearly was one of these.” *Id.* He then explicates the possible behavioral process at work within Enron:

The [Enron] culture quickly identified itself as special and uniquely competent, believing that special skill rather than luck (or just being first) was responsible for the early victories. That self-definition then set a standard for how up-and-coming people acted out their roles: Enron was a place for winners. With this—and the stock market’s positive feedback—the company’s aspiration level rose.

This aspiration level required a high level of risk-taking by the firm, though like many egocentric people, key decision-makers were probably overconfident in their ability to manage these risks, and thus underestimated them. . . . Enron harshly penalized the laggards at the firm, which, on average, tends to lead to herding behavior (risk *aversion*). To counteract this, the

On the opposite side of the regulatory divide, Professor Larry Ribstein sees little reason for more governmental monitoring or new regulations, but good reason for greater reliance on the market:

Markets are capable of responding more quickly and precisely than regulation to corporate fraud, as long as regulation does not impede or mislead them. Although markets will remain imperfect, the potential for a market response, combined with the likely costs of regulation, make the case for additional regulation dubious.<sup>35</sup>

Indeed, Professor Ribstein argues that additional regulation might have the perverse effect of causing investors to ignore market signals and place unfounded faith in the regulators.<sup>37</sup>

Other scholars focus less on general flaws in human nature, the market, or the self-regulatory scheme of corporate and securities governance and more on fairly specific features of the legal landscape that supposedly enabled Enron's failure. Of particular interest under this approach has been the question of whether reliance on overly technical and specific accounting rules permitted Enron to hide its true financial condition from the market and regulators.<sup>38</sup>

company had to magnify the reward structure considerably for those who ended up as stellar performers—a winner-take-all kind of tournament.

Presumably this worked, and key people gradually placed more and more risky bets in a variety of fields, increasingly outside of the firm's core competence. And predictably, many of these turned bad. . . . In sum—and in previous work I've called this the "optimism-commitment whipsaw"—their overconfidence commits them to a high-risk strategy; once committed to it, they are trapped.

*Id.* at 973-74.

<sup>35</sup> See *id.* at 968 (noting with skepticism the suggestion that tweaking current law to reduce investor pressure on managers can change unhealthy corporate behavior).

<sup>36</sup> Larry E. Ribstein, *Market vs. Regulatory Responses to Corporate Fraud: A Critique of the Sarbanes-Oxley Act of 2002*, 28 J. CORP. L. 1, 61 (2002).

<sup>37</sup> *Id.* at 25. Professor Jerry Markham has similarly argued that SEC regulation itself was to blame for the Enron collapse by leading investors into a false sense of security:

Indeed, the Enron and telecom implosions that grew from the accounting fraud on [SEC Chairman Arthur Levitt's] watch were nurtured by the full disclosure environment he was so vigorously advocating and expanding. Investors were led to believe, falsely, that they were protected by, and assured of, full disclosure under the federal securities laws. They received no such thing. To the contrary, they were simply cheated by their government's totally unrealistic and false promises of such protection.

Jerry W. Markham, *Accountants Make Miserable Policeman: Rethinking the Federal Securities Laws*, 28 N.C. J. INT'L L. & COM. REG. 725, 765 (2003).

<sup>38</sup> See William W. Bratton, *Enron, Sarbanes-Oxley and Accounting: Rules Versus Principles Versus Rents*, 48 VILL. L. REV. 1023, 1040 (2003) ("Misleading treatments of off-balance sheet entities lie at the Enron scandal's core. Dissatisfaction with the

The most notable work in this area is Professor John Coffee's analysis, which assigns prime causal significance to the "rule-based" system of accounting and assigns greater blame to auditors and other gatekeepers than to Enron's board of directors: "None of the watchdogs that should have detected Enron's collapse—auditors, analysts or debt rating agencies—did so before the penultimate moment. This is the true common denominator in the Enron debacle, the collective failure of the gatekeepers."<sup>39</sup> Professor Coffee argues that the rule-based system of accounting "asks the gatekeeper to certify the issuer's compliance with an inventory of highly technical rules—without the auditor necessarily taking responsibility for the overall accuracy of the issuer's statement of its financial position."<sup>40</sup> Therefore, he argues that "Enron is more about gatekeeper failure than board failure."<sup>41</sup>

---

applicable rules and their form-over-substance approach has been widespread."). Professor Bratton himself does not agree with the emphasis being placed on the role of rules versus standards in Enron's collapse:

The story is accurate in one respect—the rules respecting accounting for transactions with SPEs are badly drafted and incomplete. But in all other respects the story is nonsense. Enron, in fact, did not follow the rules. Had it done so, the substance of all of its questionable dealings with SPEs would have been disclosed in its financial statements. It follows that the rules did not fail. This is a case less of regulatory arbitrage than of strategic evasion. The failure lay with actors at Enron and its auditor, Arthur Andersen.

*Id.* at 1041 (footnote omitted).

<sup>39</sup> Coffee, *supra* note 4, 1408-09. Professor Coffee defines gatekeepers as "reputational intermediaries who provide verification and certification services to investors." *Id.* at 1405. "Lawyers can also be gatekeepers when they lend their professional reputations to a transaction, but . . . the more typical role of lawyers serving public corporations is that of the transaction engineer, rather than the reputational intermediary." *Id.*

Professor Coffee considers two explanations for why the gatekeepers failed to prevent Enron's risky or illegal behavior: the "deterrence explanation" (or, rather, "the underdeterred gatekeeper" explanation) and the "irrational market story" (or the bursting market bubble story). *Id.* at 1409-13. He concludes that both the legal and market explanations hold some truth:

Reasonable persons can always disagree what reforms are desirable. But the starting point for an intelligent debate is the recognition that the two major, contemporary crises now facing the securities market (i.e., the collapse of Enron and the growing controversy over securities analysts, which began with the New York Attorney General's investigation into Merrill, Lynch) involve at bottom the same problem—both are crises motivated by the discovery by investors that reputational intermediaries upon whom they relied were conflicted and seemingly sold their interests short. Neither the law nor the market has yet solved either of these closely related problems.

*Id.* at 1419-20.

<sup>40</sup> *Id.* at 1416. Professor Coffee also moves from Enron to the legal profession at large: "[T]he same 'commodification' of professional services that reshaped the accounting profession has also impacted the legal profession. Thus, . . . as auditing firms

Though generally skeptical of the lessons being drawn from Enron,<sup>42</sup> Professor Coffee is willing to draw one prescription from his view that audit failure occurred: Auditors should be required to certify the compliance of financials with “a meaningful substantive standard,” rather than with “an inventory of highly technical rules” that may bear little relation to the “overall accuracy of the issuer’s statement of its financial position.”<sup>43</sup>

Professor William Widen also finds in Enron an “ethic of technical compliance” over substantive accuracy<sup>44</sup> and argues broadly for a move to standards over rules:

In crafting rules governing conduct and mandating disclosure, the better course is to employ general principles rather than to draft rules of a technical and complex nature. Compliance with technical rules has an insidious tendency to replace more general notions of right and wrong. In contrast, general rules and principles constantly challenge those seeking to comply to look to themselves for guidance when making difficult decisions rather than finding refuge in a technical safe harbor. If it is not possible for a person to “forget” what is right and wrong, we nevertheless would do well to enact regulatory structures that do not distract persons from this useful form of introspection.<sup>45</sup>

Others, however, doubt the causal role of this putative ethic of technical compliance. Professor Neil Aronson tells us, for instance, that,

---

evolved from offering a single professional service into a shopping center of professional services, they lost internal control. . . . [T]he same fate could face lawyers.” *Id.* at 1417-18.

<sup>41</sup> *Id.* at 1419. Professor Coffee supplements his comparison of the deterrence and irrational market stories with a consideration of the influence of four other factors that lead to gatekeeper failures: (a) “the increased incentive for short-term stock price maximization”; (b) “the absence of competition”; (c) “observability”; and (d) “principal/agent problems.” *Id.* at 1413-16. For his data, he relies on media reports; statements by present and former government officials, such as Alan Greenspan and Stanley Sporkin; the Powers Report, *supra* note 25; publicly filed documents; and congressional statements and testimony.

Professor David Millon shares the Coffee view that gatekeepers played an important causal role in Enron’s failure. See David Millon, *Who “Caused” the Enron Debacle?*, 60 WASH. & LEE L. REV. 309, 311 (2003) (“Enron’s auditor, the securities analysts who followed its fortunes, and the rating agencies that downgraded its creditworthiness all failed to discover and reveal the chicanery. . . . In this sense, these gatekeepers share the blame for the Enron debacle.”).

<sup>42</sup> See Coffee, *supra* note 4, at 1403 (“Major debacles of historical dimensions—and Enron is surely that—tend to produce an excess of explanations. In Enron’s case, the firm’s strange failure is becoming a virtual Rorschach test in which each commentator can see evidence confirming what he or she already believed.”).

<sup>43</sup> *Id.* at 1416-17.

<sup>44</sup> William H. Widen, *Enron at the Margin*, 58 BUS. LAW. 961, 965 (2003).

<sup>45</sup> *Id.* at 1002.

“[i]n the final analysis, few can deny that the demise of Enron and WorldCom was primarily caused by financial fraud rather than the inadequacy of audit principles.”<sup>46</sup> Professor William Beaver also doubts that a change to accounting principles over rules alone will do much good:

The form of the accounting standard along the bright line/concept-based dimension is unlikely to be the major determinant of the amount of discretion exercised. The basic issue, which is omitted from the “bright lines” versus “concept-based” discussion, is this: what are the incentives to exercise discretion in financial reporting? The way to alter discretion in financial reporting is to affect those incentives in a manner that increases the cost of opportunistic discretion.<sup>47</sup>

Yet, other scholars focus on the fiduciary duties of officers and directors as the weak point in the Enron structure, contrary to the argument that Enron was more a case of gatekeeper than board failure. Indeed, Professor Cheryl Wade offers an Enron story that counters not only Professor Coffee’s view, but also that offered by Professor Rapoport. She argues that blame lies with the managers, and that lawyers of greater character would not have mattered.<sup>48</sup> She states that proposals calling for greater guidance by attorneys in steering directors and managers toward ethical decision making are naive, since “[i]nvestigations of the Enron crisis revealed that managers failed to follow advice of counsel.”<sup>49</sup>

In Professor Wade’s view, a failure of communication between officers and directors, aided by insufficient incentives for officers to

<sup>46</sup> Aronson, *supra* note 4, at 137; *see also* Bratton, *supra* note 4, at 1354 (“If GAAP is not fundamentally flawed, then the solution to the Enron problem lies on the enforcement side, where we encounter some highly problematic institutional arrangements.”).

<sup>47</sup> Beaver, *supra* note 2, at 166; *see also* George Mundstock, *The Trouble With FASB*, 28 N.C. J. INT’L L. & COM. REG. 813, 838 (2003) (arguing that principles are merely mushy prose designed so that the auditor will take more responsibility, but in reality, they make matters much worse).

The “ethic of technical compliance” view offered by Professors Coffee and Widen and others seems to be carrying the day at the Securities and Exchange Commission (SEC). Notwithstanding concerns such as those of Professors Beaver and George Mundstock, the SEC recently recommended to the Financial Accounting Standards Board (FASB) that it “write more principles-based standards—standards that are ‘objectives-oriented’ and less based on rules.” Steve Burkholder, *SEC Staff Urges FASB to Write Principles-Based Accounting Standards*, CORP. ACCOUNTABILITY & FRAUD DAILY, July 28, 2003, available at <http://pubs.bna.com/ip/BNA/cad.nsf/is/A0A7B9A9C1>.

<sup>48</sup> Cheryl L. Wade, *Corporate Governance Failures and the Managerial Duty of Care*, 76 ST. JOHN’S L. REV. 767, 778 (2002).

<sup>49</sup> *Id.*

communicate matters to the board and for board members to ask the tough questions, holds more causal importance for Enron's collapse than the role of corporate counsel.<sup>50</sup> Thus, for Professor Wade, putting more bite into officers' and directors' duties of care holds greater promise than prescriptions focused, for instance, on lawyers' duties.<sup>51</sup>

As a final example, which also serves as a sign of just how much has already been written about Enron, Professor Frank Partnoy offers a "revisionist" view of Enron's unraveling:

[W]hat seems to be emerging as the "conventional story" of Enron, involving alleged fraud related to Special Purpose Entities (SPE), is incorrect. . . . Enron is largely a story about derivatives—financial instruments such as options, futures, and other contracts whose value is linked to some underlying financial instrument or index. . . . To the extent SPEs are relevant to understanding Enron, it is the derivatives transactions between Enron and the SPEs—not the SPEs themselves—that matter. Even more important were Enron's derivatives trades and transactions other than those involving the SPEs.<sup>52</sup>

This view, which assigns central importance to the opacity of many of Enron's derivatives transactions, leads Partnoy to argue for "a standards-based regime designed to capture more disclosure of financial contingencies related to derivatives."<sup>53</sup>

Several other causal stories could be described here, for Enron's collapse has provided a canvas onto which many complaints about the

<sup>50</sup> See, e.g., *id.* at 772 (stating that "[t]he one thing that seemed apparent almost immediately was that Enron's board was not adequately informed about the company's business affairs"); *id.* at 785 (calling for greater involvement and oversight of management's activities by the board and especially independent directors).

<sup>51</sup> *Id.* at 768-69. This is not to say that Professor Wade rejects the view that lawyers have a positive role to play in corporate governance, but she sees this role as difficult to fulfill at times. See *id.* at 780 ("Lawyers can help corporate managers respond more appropriately to inevitable allegations of wrongdoing. Corporate lawyers who give their clients advice about how to pursue ethically and socially responsible paths must carefully choose the most appropriate strategies for advising managers who often resist this kind of guidance.").

<sup>52</sup> Frank Partnoy, *A Revisionist View of Enron and the Sudden Death of "May,"* 48 VILL. L. REV. 1245, 1245-46 (2003) (footnotes omitted).

<sup>53</sup> *Id.* at 1262. Recall that one of Professor Bratton's "causation stories" to which he assigned some importance was the "Enron as derivatives speculation gone wrong" story. *Supra* text accompanying note 27. Professor Partnoy reviews the Enron matter and essentially concludes that Professor Bratton and others give this story too little weight. See Partnoy, *supra* note 52, at 1262 ("In sum, the story of Enron's collapse is not what at first appears. The firm was a highly-leveraged derivatives trading firm and it collapsed when its credit rating finally reflected that fact. The scholarly and regulatory response to Enron's collapse should reflect this understanding.").

current mix of law and economics can be painted. Or, as Professor Lawrence Cunningham puts it:

Enron's cacophonous commentators share the trait of the proverbial man with a hammer, to whom every problem looks like a nail. Advocates of stricter auditing standards see a system-wide breakdown in audit quality; devotees of corporate social responsibility cite Enron to support their cause. Those on the left use it to bolster their case that more regulation is needed, while those on the right point to insufficient market competition as the cause of the failure.<sup>54</sup>

Whichever causal story is chosen, the point of the storytelling is the same: to influence the debate about whether and how the law should be changed in light of the causal factors that allegedly gave rise to Enron and other recent corporate scandals.<sup>55</sup>

Within these stories, an important but hardly discussed transformation occurs at the point where the Enron story is tied to the larger policy debate. The scholar transforms her specific explanation for the isolated event into a general explanation for a class of potential

<sup>54</sup> Cunningham, *supra* note 4, at 1422 (footnote omitted); *see also* Robert Prentice, *Enron: A Brief Behavioral Autopsy*, 40 AM. BUS. L.J. 417, 417 (2003) ("People see what they want to see in the Enron (Global Crossing/WorldCom/etc.) corporate corruption scandal. Conservatives see a triumph of capitalism. Those of a more liberal persuasion see stark limitations of capitalism. Republicans blame the scandal on the Democrats. Democratic leaders, unsurprisingly, blame it on Republicans." (footnotes omitted)). Even the appropriate metaphor to describe Enron is in dispute. Professor Marianne Jennings likens Enron to *The Perfect Storm* scenario played out in Sebastian Junger's popular book of that title. *See* Marianne M. Jennings, *A Primer on Enron: Lessons from A Perfect Storm of Financial Reporting, Corporate Governance and Ethical Culture Failures*, 39 CAL. W. L. REV. 163, 167 (2003) ("As in *The Perfect Storm*, an Enron, with its full creative accounting scenario could not have occurred if just one of the three legs of the stool had functioned differently."). But Dean Rapoport tells us that the Enron disaster is really more akin to the Titanic disaster. *See* Rapoport, *supra* note 4, at 1375 ("Enron's demise was a synergistic combination of human errors and hubris: a 'Titanic' miscalculation, rather than a 'perfect storm.'").

<sup>55</sup> *See, e.g.*, Bratton, *supra* note 4, at 1281-82 (noting that Enron provides an attractive basis for argument between supporters of deregulation and those favoring greater regulation of the markets); Coffee, *supra* note 4, at 1416 (arguing that what one believes are the causal factors underlying Enron's demise shapes one's view on what steps, if any, should be taken to prevent future Enrons); Jennings, *supra* note 54, at 167 ("An examination of the confluence of these three separate storms provides insight into the types of reforms, regulatory and otherwise, that might prevent other unprecedented collapses or perhaps provide the means of intervention before the three components merge."); Partnoy, *supra* note 52, at 1280 ("The reasons for Enron's collapse should affect the normative conclusions of scholars . . .").

events, and it becomes a prediction for future problems. In other words, the authors switch from *singular* to *general* causal accounts.<sup>56</sup>

Thus we find that even Professor Coffee, who expresses caution about reading too much into Enron from a corporate governance perspective, nevertheless draws from it generalizations about the unreliability of gatekeepers. He cautions against the use of Enron “as an indication of any systematic governance failure” because of its unique organizational structure.<sup>57</sup> Enron’s distinctive governance precludes drawing broad conclusions as to boards in general. It “is an anecdote, an isolated data point that cannot yet fairly be deemed to amount to a trend.”<sup>58</sup> Despite this warning, Professor Coffee contends that the Enron disaster does, in fact, send a general message to the market that professional gatekeepers are not to be trusted “to filter, verify and assess complicated financial information.”<sup>59</sup>

Similarly, Professor Bratton draws from Enron a broad lesson about the inadequacy of market forces to deter “shabby” behavior:

That the firm with the seventh largest market capitalization and also the firm that preached market discipline the most loudly turned out to be the shabbiest of shops with the cooperation of outside directors, outside auditors, and institutional investors, highlights the limits on what self-regulation and market incentives can achieve.<sup>60</sup>

---

<sup>56</sup> See, e.g., John T. Addison et al., *Causation, Social Science and Sir John Hicks*, 36 OXFORD ECON. PAPERS 1, 2 (1984) (noting “the distinction between the causation of a particular single phenomenon individuated by unique spatial and temporal parameters and the causation of any particular instance of a given *kind* of event”); Jessica Wilson, *Causality*, in 1 THE PHILOSOPHY OF SCIENCE: AN ENCYCLOPEDIA (Sahotra Sarkar & Jessica Pfeifer eds., forthcoming Apr. 1, 2005) (manuscript at 2, on file with author) (“[C]ausal relations may be singular (*Socrates’s drinking hemlock caused Socrates’s death*) or general (*Drinking hemlock causes death*) . . .”), available at <http://www-personal.umich.edu/~jwils/Causality%20Entry.pdf>.

In recognition of the interest of others in drawing general understandings from the specific understanding of Enron, Professor Langevoort expressly delimits the situations for which he seeks to generalize from the Enron collapse. See Langevoort, *supra* note 4, at 968 (viewing Enron as a “new economy” company like the knowledge- and service-based firms that reward productivity and innovation, rather than as a monopolistic public utility or a firm with entrenched market power and high rates of free cash flow). Of course, the question remains of how representative the behaviors of Enron players are for officers and directors of other “new economy” firms.

<sup>57</sup> Coffee, *supra* note 4, at 1403.

<sup>58</sup> *Id.* at 1404.

<sup>59</sup> *Id.* at 1404-05.

<sup>60</sup> Bratton, *supra* note 4, at 1360. But see RAFAEL LA PORTA ET AL., WHAT WORKS IN SECURITIES LAW? 22 (Nat’l Bureau of Econ. Research, Working Paper No. 9882, 2003) (finding evidence that tough securities law enforcement and punishment do not affect the strength of a securities market as much as extensive disclosure requirements and

As further examples of the move from the singular to the general, Professor Widen reports that the “cultural problem revealed by Enron” is that “corporate and legal culture has lost all sense of right and wrong.”<sup>61</sup> Professor Flatt tells us that the “Enron story allows us to see what will happen in complex trading schemes and self-regulation in the face of opposing incentives. *It is the story of human behavior itself.*”<sup>62</sup>

In these assertions, the specific explanation becomes endowed with lawlike properties, and the causal relation posited for the Enron matter is presumed to hold in other corporate settings as well. This occurs with little or no demonstration of the applicability of this explanation for other events occurring under different circumstances. Although many scholars widely apply the conclusions they draw from Enron,<sup>63</sup> they do so with little more than bald assertions or limited anecdotal evidence to support their generalizations.<sup>64</sup>

procedures to facilitate investor loss recovery), available at <http://www.nber.org/papers/w9882>.

Another example of unsupported generalization is found in Professor Bratton's suggestion that Enron exhibited pathologies common to corporations today. See Bratton, *supra* note 4, at 1360 (“Every other critical detail [of Enron], including aggressive treatments, auditor capture, and the cognitive biases that facilitated the fatal step to fraud, implicates a well-known business pathology and a concomitant and well-worn regulatory discussion.” (footnote omitted)). This reference to “cognitive biases” leading to fraud, while fashionable, is particularly troubling because it subscribes to the growing but questionable view among many legal academics that cognitive processes are riddled with bias and error, and it assumes the easy identification of the operation of such biases. See Gregory Mitchell, *Taking Behavioralism Too Seriously? The Unwarranted Pessimism of the New Behavioral Analysis of Law*, 43 WM. & MARY L. REV. 1907 (2002) (discussing boundary conditions on psychological research into rationality that make application of the research to the law treacherous); 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) (using empirical evidence on individual and situational variability in rational behavior to show that the concept of rationality lies between perfect rationality and equal incompetence).

<sup>61</sup> Widen, *supra* note 44, at 962-63.

<sup>62</sup> Flatt, *supra* note 4, at 10,495 (emphasis added).

<sup>63</sup> Many of these broad claims resemble what Professor Paul Meehl calls “fireside inductions,” which he defines as “common-sense empirical generalizations about human behavior which we accept on the culture's authority plus introspection plus anecdotal evidence from ordinary life.” Paul E. Meehl, *Law and the Fireside Inductions: Some Reflections of a Clinical Psychologist*, in PAUL E. MEEHL: SELECTED PHILOSOPHICAL AND METHODOLOGICAL PAPERS 440, 440 (C. Anthony Anderson & Keith Gunderson eds., 1991). Professor Meehl calls for healthy skepticism regarding both fireside inductions and the empirical research that claims to contradict them. *Id.* at 468.

<sup>64</sup> See, e.g., Bratton, *supra* note 4, at 1283 (explaining that his article reviews the particulars of the Enron case and emphasizes the similarities between it and respectable firms); *id.* at 1287 (stating that similar Enron-like situations of auditor capture are “ubiquitous in America's corporate landscape”); Coffee, *supra* note 4, at 1407

For the scholar inclined to make policy recommendations, this inductive leap must occur because, while singular causal stories about specific events are of great interest to trial judges, juries, and the parties involved in a particular lawsuit, they are of little interest to the lawmaker, who enacts laws with broad behavioral implications beyond the specific case in mind.<sup>65</sup> Unless the causal explanation extends beyond Enron to provide a more general explanation of how certain behaviors and corporate and regulatory failures are related, then specific explanations for Enron provide little insight for lawmakers. Stated differently, if Enron is an aberration or the product of unique forces unlikely to be seen again, then why bother with “sweeping legal reforms”?<sup>66</sup> The focus should instead be placed on criminal

---

(acknowledging that his evidence of erosion of gatekeeper independence is substantially “anecdotal, but striking”).

Professor Coffee does offer, in support of some of his contentions, suggestive evidence drawn from data on earnings restatements and buy/sell recommendations. Furthermore, he notes that the claims he derives from this data are tentative, given that the data is subject to multiple interpretations. *Id.* at 1407-08; *see also id.* at 1412 n.41 (citing a study providing evidence of a relationship between earnings management and the purchase of nonauditing services from accounting firms).

Michael Saks states that the danger posed by reliance on anecdotal evidence is that it “permits only the loosest and weakest inferences about matters a field is trying to understand. Anecdotes do not permit one to determine either the frequency of occurrence of something or its causes and effects.” Michael J. Saks, *Do We Really Know Anything About the Behavior of the Tort Litigation System—and Why Not?*, 140 U. PA. L. REV. 1147, 1159 (1992).

<sup>65</sup> This push to go beyond the specific case and say something of general import is particularly well-illustrated in Professor Widen’s recent article. Most of his article focuses on how officers, directors, and lawyers for Enron might be held criminally liable, and hence, the article could be valuable to judges, regulators, prosecutors, and defense lawyers involved in the Enron litigation or similar cases. Widen, *supra* note 44, at 973-99. Professor Widen goes further, however, and labels Enron as evidence of a “crisis in capitalism,” offering only assertion and anecdotal references to support this view. *See id.* at 1001-02 (“The Enron case, more than other recent well-publicized cases of fraud, illustrates the general structure of a crisis in capitalism because of the direct involvement of the board of directors and lawyers in approval and structuring of criminal acts.”).

<sup>66</sup> *See* Lawrence A. Cunningham, *The Sarbanes-Oxley Yawn: Heavy Rhetoric, Light Reform (and It Just Might Work)*, 35 CONN. L. REV. 915, 917 (2003) (discussing how the president, the SEC Chairman, and other “participants and observers” of the Sarbanes-Oxley Act frequently referred to is as “sweeping reform”). Professor Cunningham questions just how “sweeping” the legal reforms so far put in place really have been, despite public rhetoric to that effect. *See* Lawrence A. Cunningham, *The Sarbanes-Oxley Yawn: Heavy Rhetoric, Light Reform (and It Just Might Work)*, 35 CONN. L. REV. 915, 987 (2003) (noting that, with the exception of the funding of the auditing oversight board and accounting standard-setters, the Sarbanes-Oxley Act is “not major reform, but patches and codifications and further study”). *But see* Joseph A. Grundfest, *Punctuated*

punishment, civil liability, and reparations for the players in the Enron case alone.

The claim here is not that legislation is never directed at specific cases or problems, or that holdings in specific cases do not at times have broad behavioral implications. Certainly some laws are designed to redress specific issues or serve special interests, and court decisions may affect persons beyond the immediate parties. Indeed, the need to “just do something” in response to Enron—either to try to restore faith in the markets or for political reasons—may well have been a motivating factor in the reforms. This assertion is merely that lawmakers would most likely be unmoved by specific explanations for Enron that included a caveat stating that “this story has nothing of significance to say about the securities market in general or about any other corporation.” However, to the extent that, for political purposes, lawmakers need to take some action in response to Enron, legal scholars provide the putative empirical or analytical cover needed. These general explanations, derived from the unique causes of Enron, provide the hook upon which to hang legislative hats, regardless of their validity.

We see, then, that legal scholars who draw prescriptive lessons from Enron really tell two stories in response to the Enron matter. First, a singular causal story is offered to explain the specific events surrounding Enron. Second, a more general causal story about market failures, corporate misconduct, regulatory shortcomings, or professional malpractice is drawn from the specific story. These stories raise questions of internal and external validity.<sup>67</sup> In creating these

---

*Equilibria in the Evolution of United States Securities Regulation*, 8 STAN. J.L. BUS. & FIN. 1, 2 (2002) (arguing that the scope of the Sarbanes-Oxley Act is “sweeping and dramatic”).

<sup>67</sup> See, e.g., JOHN BREWER & ALBERT HUNTER, MULTIMETHOD RESEAT: A SYNTHESIS OF STYLES 158 (1989) (“Research to generate and test causal hypotheses is usually judged in terms of two standards: internal and external validity.”).

Professors Thomas Cook and Donald Campbell define internal validity as “the approximate validity with which we infer that a relationship between two variables is causal or that the absence of a relationship implies the absence of cause.” THOMAS D. COOK & DONALD T. CAMPBELL, QUASI-EXPERIMENTATION: DESIGN & ANALYSIS ISSUES FOR FIELD SETTINGS 37 (1979); see also JOHN J. SHAUGHNESSY & EUGENE B. ZECHMEISTER, RESEARCH METHODS IN PSYCHOLOGY 25 (1985) (defining internal validity as arising when independent variables are not “allowed to co-vary simultaneously,” making it “possible to determine which variable is responsible for any served difference in performance”); John C. Coffee, Jr., *Law and Regulatory Competition: Can They Co-exist?*, 80 TEX. L. REV. 1729, 1729 (2002) (referring to “the problem of multi-collinearity that usually confounds efforts to infer causation from correlation” (footnote omitted)).

Cook and Campbell define external validity as “the approximate validity with which we can infer that the presumed causal relationship can be generalized to and

stories, to what extent have the authors considered, and appropriately eliminated, alternative credible explanations for Enron (i.e., is the singular causal story internally valid)? Furthermore, to what extent are the authors' extrapolations from Enron to other cases or circumstances warranted (i.e., is the general causal story externally valid)? Invalidity in either respect undercuts the prescriptive force of the explanations drawn from Enron.

## II. SINGLE-OBSERVATION CASE STUDIES AND THEIR LIMITATIONS

Wittingly or unwittingly, when scholars write causal stories about Enron, they engage in a form of single-observation case study research.<sup>68</sup> "The case study is a research strategy which focuses on understanding the dynamics present within single settings."<sup>69</sup> In

---

across alternate measures of the cause and effect and across different types of persons, settings, and times." COOK & CAMPBELL, *supra*, at 37; see also SHAUGHNESSY & ZECHMEISTER, *supra*, at 25 ("The internal validity of a study must be distinguished from the study's *external validity*. External validity involves the extent to which research results can be generalized to different populations, settings, and conditions.").

<sup>68</sup> While most of the Enron scholars do not explicitly label their works as case studies, a few do. See Pamela H. Bucy, *Private Justice*, 76 S. CAL. L. REV. 1, 8 (2002) ("[T]he following [c]ase [s]tudy [of Enron] shows in concrete terms how private justice could have made a difference in one instance of massive wrongdoing."); Floyd, *supra* note 4, at 984-99 (presenting a case study of Enron for comparison with the collapse of an Australian company and arguing that American law should be reformed); O'Connor, *supra* note 24, at 1238 (using "social psychology to build a case study for how the Enron Board may have been affected by a significant impediment to group deliberation called 'grouphink'").

I employ the term "causal story," rather than "case study," because Professor Bratton, in probably the most prominent case study of Enron to date, specifically refers to the creation of "causation stories," Bratton, *supra* note 4, at 1286, and because many of the discussions of Enron provide a narrative causal account of the undoing of Enron. In addition, to the extent any legal scholars sought to use their accounts of Enron more for political than truly empirical purposes, such analyses of Enron would seem to fit well within Deborah Stone's definition of "causal stories" that are used for political influence purposes. Deborah A. Stone, *Causal Stories and the Formation of Policy Agendas*, 104 POL. SCI. Q. 281 (1989). I return later to Professor Stone's notion of causal stories in relation to the Enron stories. *Infra* note 215 and accompanying text.

<sup>69</sup> Kathleen M. Eisenhardt, *Building Theories from Case Study Research*, 14 ACAD. MGMT. REV. 532, 534 (1989) (citation omitted). Other scholars have noted, however, that the term "case study" lacks a standard usage. Martyn Hammersley & Roger Gomm, *Introduction to CASE STUDY METHOD: KEY ISSUES, KEY TEXTS 1*, 1 (Roger Gomm et al. eds., 2000). But Professor Kathleen Eisenhardt's definition captures the case study emphasis on understanding behavior and events in context. See, e.g., PERVEZ GHOURI & KJELL GRØNHAUG, *RESEARCH METHODS IN BUSINESS STUDIES: A PRACTICAL GUIDE 171* (2d ed. 2002) ("In business studies, case study research is particularly useful when the phenomenon under investigation is difficult to study outside its natural setting and also when the concepts and variables under study are difficult to quantify."). The

*single-observation* case studies (i.e., research employing a sample size of one, or  $N = 1$ ), there is no opportunity to develop a range of data points on the dependent variable or to study several instances of the phenomenon to be explained. Instead, one outcome or event, such as the collapse of Enron, serves as the focal point.<sup>70</sup>

---

leading expert on the case study methodology, Robert Yin, gives a definition similar to that of Professor Eisenhardt. See Robert K. Yin, *The Case Study Crisis: Some Answers*, 26 ADMIN. SCI. Q. 58, 59 (1981) (“As a research strategy, *the distinguishing characteristic of the case study is that it attempts to examine*: (a) a contemporary phenomenon in its real-life context, especially when (b) the boundaries between phenomenon and context are not clearly evident.”). Kin further elaborates on the case study methodology in writing:

The case study inquiry

- copes with the technically distinctive situation in which there will be many more variables of interest than data points, and as one result
- relies on multiple sources of evidence, with data needing to converge in a triangulating fashion, and as another result
- benefits from the prior development of theoretical propositions to guide data collection and analysis.

ROBERT K. YIN, CASE STUDY RESEARCH: DESIGN AND METHODS 13-14 (3d ed. 2003) (emphasis omitted). For a historical discussion of the development and uses of the case study approach, see Jennifer Platt, “Case Study” in *American Methodological Thought*, 40 CURRENT SOC. 17 (1992).

<sup>70</sup> Salient examples of single-observation case studies outside the law can be found within MBA programs and history scholarship. See, e.g., VIRTUAL HISTORY (Niall Ferguson ed., 1997) (providing case studies describing the circumstances that would have resulted if nine different historical events had occurred differently); Yuen Foong Khong, *Confronting Hitler and Its Consequences*, in COUNTERFACTUAL THOUGHT EXPERIMENTS IN WORLD POLITICS: LOGICAL, METHODOLOGICAL, AND PSYCHOLOGICAL PERSPECTIVES 95, 95 (Philip E. Tetlock & Aaron Belkin eds., 1996) (analyzing the single event of British Prime Minister Neville Chamberlain’s appeasement of Hitler at Munich to determine its causal effects on World War II); Donald C. Langevoort, *Teaching Problem Solving: An Academic’s Perspective*, BUS. LAW TODAY, July-Aug. 1999, at 33, 37 (noting the emphasis in MBA programs on “richly detailed case studies”). In fact, professors at the University of Virginia’s Darden Graduate School of Business Administration have already developed a case study on Enron for use in classes. Darden Sch. of Bus. Admin., Univ. of Va., *Enron; 1986–2001*, <http://it.darden.virginia.edu/preview/enron> (last visited Mar. 4, 2004). For a debate on the merit of using case studies for teaching purposes within business schools, compare Chris Argyris, *Some Limitations of the Case Method: Experiences in a Management Development Program*, 5 ACAD. MGMT. REV. 291, 291 (1980) (arguing that “the case method of instruction may unintentionally reinforce individual and organizational forces against double-loop learning,” a sophisticated type of problem solving that is usually the responsibility of managers and policymakers), with Michael A. Berger, *In Defense of the Case Method: A Reply to Argyris*, 8 ACAD. MGMT. REV. 329 (1983) (responding to Argyris’s criticism of the case method by pointing out the methodological and conceptual flaws of Argyris’s study and emphasizing the positive features of the case method).

Single-observation case studies should be distinguished from comparative case studies (or multiple-case designs), in which two or more cases are compared, often for the purpose of isolating differences in potential causal variables or testing a theory

Single-observation case studies can provide rich descriptions of an event and, like any good story, can personalize and highlight the wrongs suffered in particular settings.<sup>71</sup> If the Enron scholars offer up their stories simply as cautionary tales to illustrate what could go wrong in certain corporate settings, then the stories would be subject to scrutiny only for their descriptive accuracy and narrative persuasiveness.<sup>72</sup> The Enron stories, however, go beyond mere description and actually prescribe ways to avoid similar outcomes in the future.<sup>73</sup> Once the Enron scholars offer causal explanations or generalize from Enron to other cases, we should ask not only whether they accurately describe the facts of the Enron story, but also whether they employ sound means of causal and inductive reasoning. If not, then the

with contrasting cases. See Martyn Hammersley et al., *Case Study and Theory, in CASE STUDY METHOD*, *supra* note 69, at 234, 239 (“Comparative method requires that data be available from more than one case, perhaps from a substantial number, such that the effects of various candidate causal factors can be controlled or assessed.”). For a useful discussion of the limits of comparative case studies, albeit in the context of rational deterrence theory in political science, see Christopher H. Achen & Duncan Snidal, *Rational Deterrence Theory and Comparative Case Studies*, 41 *WORLD POL.* 143 (1989).

<sup>71</sup> See, e.g., Deborah L. Rhode, *Legal Scholarship*, 115 *HARV. L. REV.* 1327, 1345-46 (2002) (“Narratives often provide a level of eloquence, passion, and immediacy that conventional forms of scholarship rarely duplicate. Vivid, personalized accounts are likely to be especially memorable and especially effective in evoking empathic responses.” (footnotes omitted)). Many anecdotes found within the law have a personal experience element and are used to demonstrate the supposed effects of some event, policy, or practice without focusing on underlying causes. See e.g., Marc Galanter, *Real World Torts: An Antidote to Anecdote*, 55 *MD. L. REV.* 1093, 1098 (1996) (noting the use of anecdotal “atrocious stories” to support arguments for tort reform); Douglas A. Hedin, *On Losing*, 26 *AM. J. TRIAL ADVOC.* 107, 109 n.9 (2002) (noting the use of personal experience anecdotes to support employment discrimination claims); see also David A. Hyman, *Do Good Stories Make for Good Policy?*, 25 *J. HEALTH POL. POL’Y & L.* 1149, 1149 (2000) (discussing the use of anecdotes that have an atrocity-story quality to them to influence health care legislation); Michael Rustad, *In Defense of Punitive Damages in Products Liability: Testing Tort Anecdotes with Empirical Data*, 78 *IOWA L. REV.* 1, 15 (1992) (arguing that the outcry against punitive damage awards is based on out-of-context anecdotes and misleading statistics and that any legislative reform of the practice must have a firm empirical foundation). For an argument that the vicarious experience permitted by single-case studies is one of the prime benefits of this research approach, see Robert Donmoyer, *Generalizability and the Single-Case Study, in CASE STUDY METHOD, supra* note 69, at 45, 60-65.

<sup>72</sup> That is, how accurately does the story relate to the underlying facts and does the story achieve the desired effect? In some cases, of course, the goals of accuracy and persuasion may be at odds. For a discussion of litigation as a competition between competing narratives, using the Microsoft antitrust case as a case study, see Joshua A. Newberg, *The Narrative Construction of Antitrust*, 12 *S. CAL. INTERDISC. L.J.* 181 (2003). Professor Newberg suggests that the government sacrificed accuracy and rigor for persuasiveness in its case against Microsoft. *Id.* at 216.

<sup>73</sup> *Supra* Part I.

stories should only be used cautiously for explanatory, predictive, or prescriptive purposes.

As it turns out, for the reasons discussed in the succeeding sections, causal stories drawn from single-observation case studies inevitably lead to causal conclusions of suspect internal and external validity. Accordingly, causal stories typically offer little guidance to causal connections in the world.<sup>74</sup> “Unfortunately, good stories are so compelling to us when we take the role of psychologist or social analyst that we do not realize that at best they constitute just a starting point for analysis.”<sup>75</sup> By examining the flaws with the causal story approach, however, we may move beyond this commonly flawed starting point to perform better analyses of significant legal events like Enron’s collapse.

### A. Sources of Internal Invalidity

#### 1. The Problem of Extreme Restriction of Range on the Dependent Variable (and Why Enron Stories Remain Problematic Even When Characterized as “Crucial Case” Studies)

The validity of one’s causal inferences depends, initially, on the nature and quality of the evidence gathered to test one’s causal hypotheses.<sup>76</sup> If evidence is poorly selected to test a hypothesis, then

<sup>74</sup> To be clear, the claim is not that single-observation case studies are useless. Such studies may be valuable first steps toward theory development, explanation, and prediction, but they should not be the only step. In general, we should consider whether case studies are being used to the best of their advantages. See, e.g., William F. Dukes, *N = 1*, 64 PSYCHOL. BULL. 74, 78 (1965) (“Problem-centered research on only one subject may, by clarifying questions, defining variables, and indicating approaches, make substantial contributions to the study of behavior. Besides answering a specific question, it may . . . provide important groundwork for the theorists.”). For a discussion of the practical value of case studies, which also acknowledges many of the limitations of the case study approach, see John S. Odell, *Case Study Methods in International Political Economy*, 2 INT’L STUD. PERSP. 161 (2001); Kristin Shrader-Frechette & Earl D. McCoy, *Applied Ecology and the Logic of Case Studies*, 61 PHIL. SCI. 228 (1994). See also ELSTER, *supra* note 10, at 7 (noting that causal explanations are distinct from storytelling because the former accounts for what happened, while the latter speculates what might have happened). Professor Elster notes that storytelling could “suggest new, parsimonious explanations.” *Id.* He cautions, however, that storytelling can be harmful if the speculation is mistaken for explanation. *Id.* at 8.

<sup>75</sup> DAWES, *supra* note 11, at 138. For an example of the lasting and misleading effects that “good” but false stories may have on the legal system, see Kevin M. Clermont & Theodore Eisenberg, *Litigation Realities*, 88 CORNELL L. REV. 119, 146-47 (2002).

<sup>76</sup> For instance, Professor Peter Achinstein emphasizes the importance of evidence-selection procedures over whether the evidence is being used for explanatory or predictive purposes (i.e., whether the test is backward- or forward-looking) when

even the most elegant test will be worthless. Thus, for example, a technically competent regression analysis of a large data set intended to test the relationship between judicial ideology and case outcomes provides at best limited information about this relationship if the ideology variable is poorly operationalized or unreliably coded for use in the regression analysis.<sup>77</sup>

When legal scholars select only the Enron case as evidence of why corporate calamities occur, they make it difficult, maybe even impossible, to conduct any serious test of a causal hypothesis about business failure or to construct a valid causal explanation for Enron. This shortcoming arises from what has been called the “Fundamental Problem of Causal Inference”—that a causal inference can only be drawn from observing the value of a dependent (or response) variable in the presence and absence of an independent (or putatively causal

evaluating the strength of evidence and the stringency of a test of a hypothesis. Peter Achinstein, *Explanation v. Prediction: Which Carries More Weight?*, 2 PSA: PROC. BIENNIAL MEETING PHIL. SCI. ASS'N 156, 161 (1994); see also Eisenhardt, *supra* note 69, at 536-37 (“Selection of cases is an important aspect of building theory from case studies. . . . [S]election of an appropriate population [of case studies] controls extraneous variation and helps to define the limits for generalizing the findings.”); Marini & Singer, *supra* note 10, at 348 (“Regardless of the research approach taken, the degree of belief in a causal hypothesis depends on the strength of evidence available to support it.”).

A reader of an earlier draft of this Article raised the question of whether this Article itself falls prey to such evidential problems (i.e., has the evidence been poorly selected to make the argument that case studies raise serious methodological problems?). The answer is “no” because this Article does not seek to test causal hypotheses or describe problems beyond those associated with single-observation case studies used for causal analysis and generalization purposes. In other words, the goal of the Article is to heighten awareness of the methodological dangers associated with the use of single-observation case studies, as found in several legal scholars’ causal stories about Enron, rather than to address all legal discussions of Enron itself or to address all purposes for which case studies may be used. Cf. Jennifer Platt, *Cases of Cases . . . of Cases*, in *WHAT IS A CASE? EXPLORING THE FOUNDATIONS OF SOCIAL INQUIRY* 21, 49 (Charles C. Ragin & Howard S. Becker eds., 1992) (using cases to demonstrate how authors use cases, not to draw substantive conclusions about the topic).

<sup>77</sup> Compare Lee Epstein & Gary King, *The Rules of Inference*, 69 U. CHI. L. REV. 1, 83-99 (2002) (discussing the importance of using reliable measurement and coding techniques of facially valid measures of a variable and criticizing the variable of judicial ideology as an example), with Richard L. Revesz, *A Defense of Empirical Legal Scholarship*, 69 U. CHI. L. REV. 169, 180-83 (2002) (responding to Lee Epstein and Gary King regarding the operationalization of judicial ideology). On the relation between reliability and validity, Professor Christopher Slobogin properly notes that, “[i]f reliability is low, validity is suspect as well, because a lack of agreement between two raters means that at least one of them is wrong.” Christopher Slobogin, *Doubts About Daubert: Psychiatric Anecdotes as a Case Study*, 57 WASH. & LEE L. REV. 919, 921 (2000).

or explanatory) variable.<sup>78</sup> Because a dependent variable cannot simultaneously hold two values, it is logically impossible to draw a causal inference from a single observation. Scientists usually resolve the Fundamental Problem of Causal Inference using scientific (i.e., experimental) or statistical (i.e., quantitative) solutions in which either multiple measurements of the same unit are made over time or multiple measurements of different units are aggregated.<sup>79</sup> These

---

<sup>78</sup> The statistician Professor Paul Holland noted that causal inference is frustrated by the inherent fact of observational life, which he termed the "Fundamental Problem of Causal Inference." Paul W. Holland, *Statistics and Causal Inference*, 81 J. AM. STAT. ASS'N 945, 947 (1986). He defined the problem as the impossibility "to observe the value of  $Y_i(u)$  and  $Y_c(u)$  on the same unit and, therefore, it is impossible to observe the effect of  $t$  on  $u$ ." *Id.* The notations refer to the value of a variable,  $u$ , in the presence of a causal factor,  $Y_i$ , and, in the absence of it, the control condition,  $Y_c$ .

This fundamental inferential problem is why the number of observations must be greater than the number of explanatory variables studied or why there must be positive "degrees of freedom" in any test of a cause-effect relationship. See James D. Fearon, *Counterfactuals and Hypothesis Testing in Political Science*, 43 WORLD POL. 169, 172 n.6 (1991) (defining degrees of freedom as "the number of cases minus the number of explanatory variables minus one").

Professors King, Robert Keohane, and Sidney Verba argue strongly against studies of single cases that do not include any effort to compare the case to other cases or to break the case down into subparts that may allow intracase comparisons for causal testing purposes. See KING ET AL., *supra* note 11, at 211 (concluding that single observation case studies are "not a useful technique for testing hypotheses or theories," unless they are "part of a research program" and can be compared with other single observations).

<sup>79</sup> Professor Holland describes the scientific and statistical solutions to the Fundamental Problem of Causal Inference as follows:

There are two general solutions to the Fundamental Problem, which for the sake of convenience I will label the *scientific solution* and the *statistical solution*.

The scientific solution is to exploit various homogeneity or invariance assumptions. For example, by studying the behavior of a piece of laboratory equipment carefully a scientist may come to believe that the value of  $Y_c(u)$  measured at an earlier time is equal to the value of  $Y_c(u)$  for the current experiment. All he needs to do now is to expose  $u$  to  $t$  and measure  $Y_i(u)$  and he has overcome the Fundamental Problem of Causal Inference. Note, however, that this hypothetical scientist has made an untestable homogeneity assumption. By careful work he may convince himself and others that this assumption is right, but he can never be absolutely certain . . .

The statistical solution is different and makes use of the population  $U$  in a typically statistical way. . . . The important point is that the statistical solution replaces the impossible-to-observe causal effect of  $t$  on a specific unit with the possible-to-estimate *average* causal effect of  $t$  over a population of units.

Holland, *supra* note 78, at 947; see also Marini & Singer, *supra* note 10, at 367 (noting that "covariation may be exhibited either cross-sectionally [across the population] or longitudinally [based on a specific member of the group]"). An illustration from the field of psychology may help. When psychological researchers use within-subjects research designs, in which multiple subjects participate in each of the experimental conditions and then averaged measures on a dependent variable are compared across

solutions, however, are not available to the researcher performing a case study involving a sample of only one or a few cases.

In performing a case study of Enron, the researcher who eschews experimental and quantitative approaches to causal hypothesis testing is left with only two ways to test her causal hypotheses: (1) by comparing the Enron case to similar real cases that resulted in like and unlike outcomes in order to try to isolate common or differential causal factors across the cases (the structured, focused comparison approach to case studies uses this method<sup>80</sup>) or (2) by engaging in a counterfactual thought experiment in which one imagines whether changes in background conditions would have led to other outcomes.<sup>81</sup> Each of these alternative approaches presents problems.

conditions, the researcher is taking advantage of both scientific and statistical solutions to the causal inference problem. Measurements on the same unit are taken over time across different treatment conditions, but these measurements are taken from a cross-section of a population (combining a weak form of the scientific solution's invariance assumption with the statistical solution's estimates of average causal effects in a population).

<sup>80</sup> See, e.g., Achen & Snidal, *supra* note 70, at 146-50 (providing a critical analysis of comparative case studies, particularly the structured, focused comparison approach). For another example of a systematic approach to causation using case studies, see Larry J. Griffin, *Narrative, Event-Structure Analysis, and Causal Interpretation in Historical Sociology*, 98 AM. J. SOC. 1094, 1105-08 (1993). Professor Arend Lijphart calls this method the "comparable-cases strategy," and explains that it directs the researcher to select cases "in such a way as to maximize the variance of the independent variables and to minimize the variance of the control variables." Lijphart, *supra* note 11, at 163-64 (emphasis omitted). The comparable-cases strategy falls within the category of multiple case designs and is synonymous with the comparative case study method. See *supra* note 70 and accompanying text (distinguishing single-observation case studies from comparative case studies).

<sup>81</sup> See Fearon, *supra* note 78, at 171 (describing a counterfactual case as hypothesizing whether, absent the causal factor, the same event would have occurred); see also Neal J. Roese & James M. Olson, *Counterfactual Thinking: A Critical Overview*, in WHAT MIGHT HAVE BEEN: THE SOCIAL PSYCHOLOGY OF COUNTERFACTUAL THINKING 1, 12 (Neal J. Roese & James M. Olson eds., 1995) (arguing that causal conclusions can be derived from a "counterfactual simulation in one's head" just as from a comparison of two parallel factual occurrences).

David Collier argues that the small-sample researcher might also usefully employ a Millian "method of elimination," which can exclude causal factors if they are consistently not present when a given outcome occurs." David Collier, *Translating Quantitative Methods for Qualitative Researchers: The Case of Selection Bias*, 89 AM. POL. SCI. REV. 461, 464 (1995) (citation omitted). This method of elimination, like the actual case comparison approach discussed by James Fearon, presumes a systematic comparison of some small sample of cases. Moreover, as Professor Stanley Lieberson points out, there are a host of analytic problems with using the Millian eliminative approach to causation. Stanley Lieberson, *More on the Uneasy Case for Using Mill-Type Methods in Small-N Comparative Studies*, 72 SOC. FORCES 1225, 1225-34 (1994).

The obvious problems of comparing Enron to other real cases (the comparative case study approach) are (1) the difficulty of finding comparable cases that differ in only a few relevant but discernible respects, and (2) the difficulty of knowing with any certainty whether all of the causally relevant facts are known about the cases studied.<sup>82</sup> The obvious problem with the counterfactual approach is that one's counterfactual comparisons and conclusions will always remain highly debatable because, given the absence of a reality metric, no one can be right or wrong in a counterfactual world.<sup>83</sup> The less obvious problems

One other option when studying a single case is to conduct intracase comparisons over time to try to discern causal relations (e.g., to make repeated measurements on the same experimental subject under various experimental conditions). Like an eliminative induction approach, however, an intracase approach requires more than one observation of the dependent variable of interest. See DAVID H. BARLOW & MICHEL HERSEN, *SINGLE CASE EXPERIMENTAL DESIGNS: STRATEGIES FOR STUDYING BEHAVIOR CHANGE* 37 (2d ed. 1984) ("The basis of this search for sources of variability is repeated measurement of the dependent variable or problem behavior.")

<sup>82</sup> See, e.g., Lijphart, *supra* note 11, at 163 (noting that the comparative method is challenging because it is not easy to identify comparable cases and, even if its possible, the cases are likely to have similar confounding background variables, which should be controlled or operative).

I am not aware of any legal scholars who have employed a systematic case study approach in which Enron is carefully compared to similar other cases for causal analysis purposes. In a few instances, scholars have made reference to Enron in light of other cases, but in a very informal or unsystematic way, without any sustained effort to compare and contrast possible causal forces in the different cases. For instance, Professor Partnoy briefly compares Enron to the Long-Term Capital Management debacle, but his analysis did not take the form of a structured, focused comparison. *The Fall of Enron: How Could It Have Happened?: Hearing Before the U.S. Senate Comm. on Governmental Affairs*, 107th Cong. 1-2 (2002) (testimony of Frank Partnoy, Professor of Law, San Diego Sch. of Law). Professors Cunningham and Millon also compare Enron to several corporate scandals, but similarly do not engage in systematic or focused and structured case comparisons. See Cunningham, *supra* note 4, at 1423-30 (laying out a brief history of corporate scandals from the 1960s through the 1990s); Millon, *supra* note 41, at 310-11 (mentioning other scandals that occurred following Enron). Professor Louise Floyd does conduct a comparative case study involving Enron and an Australian company as part of a comparative law analysis, but her primary purpose is not to identify the cause(s) of Enron's collapse. See Floyd, *supra* note 4, at 984-99 (discussing the similarities and differences between Enron and an Australian company to argue for legal reforms in American law).

<sup>83</sup> If one employs a correspondence theory of truth, then by definition in a counterfactual world (i.e., a "contrary to fact" world), there is no "there" to which propositions can correspond. See, e.g., ALVIN I. GOLDMAN, *KNOWLEDGE IN A SOCIAL WORLD* 59 (1999) ("[T]he root idea of the correspondence approach is that truth involves a relation to reality. I would add another ingredient, namely, that items are candidates for truth only if they (purport to) describe reality.")

This is not to say necessarily that all counterfactual assertions are equally debatable or equally useful in theory development. "The utility of a particular explanation . . . depends on the plausibility of its implied counterfactual. If the counterfactual

with the counterfactual approach involve a host of specific analytical deficiencies that are taken up below.<sup>84</sup>

By choosing a method that confines itself to a qualitative review of historical evidence, the Enron scholars reject the scientific and statistical solutions to the Fundamental Problem of Causal Inference. By selecting for study only a single case of corporate failure—thus neglecting cases of corporate success that may be factually similar in many respects (e.g., that may involve the same gatekeeper organizations; may have had the same mix of independent and insider board members; may have had equally mendacious or reputable officers; or may have operated under the regulation of the same SEC officials in the same markets at the same times)—the Enron scholars likewise reject the *comparative* case study approach, which requires a systematic comparison of at least two actual cases. Therefore, the Enron scholars who conduct single-observation case studies are left with only counterfactual thought experiments to reach causal conclusions.<sup>85</sup> The comparisons and manipulations necessary to reach

---

world necessarily posited by an explanation is not plausible, the explanation should not have been advanced.” Griffin, *supra* note 80, at 1101 n.5 (citations omitted). For discussions of what makes some counterfactual comparisons “better” than others, see the essays collected in COUNTERFACTUAL THOUGHT EXPERIMENTS IN WORLD POLITICS, *supra* note 70. See also Fearon, *supra* note 78, at 189-94 (mentioning problems that arise when using counterfactuals to analyze a situation); Robert N. Strassfeld, *If . . . : Counterfactuals in the Law*, 60 GEO. WASH. L. REV. 339, 397-402 (1992) (addressing ways to focus counterfactual inquiries about causation in litigation settings). This topic of evaluating alternative counterfactual comparisons is discussed further in Part II.C.

<sup>84</sup> *Infra* Part II.A.2 (describing specific problems in counterfactual thought experiments).

<sup>85</sup> To make this causal inference problem more concrete, imagine entering a hotel room in which the only light fixture in the room is unilluminated, and there is an electrical panel on the entryway wall that has two switches in the “off” position. How might one draw inferences about which electrical switch controls the light in the room? An experimental approach would favor systematically manipulating the two switches while controlling other factors in the room (and, of course, on entering a strange room, most people take this experimental approach to find the proper switch). A quantitative approach would favor sampling all similar rooms in the hotel to obtain data on the association between the light actually being off/on and the off/on positions of switches 1 and 2 or, alternatively, passively taking measurements from the single room over time to examine the association. On the other hand, a small-*N* comparative case study approach might favor, for example, going into a few of the rooms on the same floor to try to determine from these other rooms whether switch 1 or switch 2 seems to control the light (i.e., one would look for similarly designed rooms with the light on and the two switches in different positions that might allow one to eliminate one of the switches as the likely causal candidate in one’s own room).

In a single-observation case study, none of these conventional empirical approaches to causal inference is possible. And note that, even among these empirical approaches, some will lead to more dependable causal inferences than others. See, e.g.,

causal conclusions must occur mentally, rather than through some explicit, replicable process in which the effects of hypothesized causal variables on dependent variables are assessed using experimental testing, quantitative analysis of actual cases, or qualitative comparisons of real cases.

If we return to our Enron stories, we see that this is indeed how the scholars go about inferring causality. They compare the real world in which Enron failed to some counterfactual world in which Enron did not fail due to some hypothetical change in a causal candidate. "Ifs" and "should haves" typically signal the approach of a counterfactual thought experiment in the Enron stories.

---

Mark Klock, *Finding Random Coincidences While Searching for the Holy Writ of Truth: Specification Searches in Law and Public Policy or Cum Hoc Ergo Propter Hoc?*, 2001 WIS. L. REV. 1007, 1036 ("Even if one had Olympian knowledge that there were no missing variables causing both of the associated variables to move together, a statistical relationship still does not prove causality."). Since only a single passive observation of the relationship of the light's status to the position of the two switches is made in such a case study, one cannot manipulate the experiment, nor can one examine other similar rooms or return at different points in time to the same room to observe the positions of the switches in relation to the status of the light. Therefore, the only method of causal inference that remains available in this case is to use counterfactual thought experiments in which the two switches are systematically turned on and off in one's mind, and the effect of these manipulations on the light is imagined. These thought experiments might introduce other extrinsic knowledge about light switches from other situations one has encountered as well as other observable evidence in the room that might suggest which light switch is the better causal candidate. In the end, however, there is no way to be sure that the counterfactual thought experiment leads to the correct conclusion about which switch controls the light.

One should note that in a case such as the above example, where the likely cause-effect relation will be direct, simple, and noncontroversial, a historical approach might also yield very good information about which switch likely controls the light. For instance, perhaps a review of the architectural plans and wiring diagrams would help answer the question, or an interview of the hotel management would provide insight into which switch causes the light to turn on. As the causal relations become more complex, causal candidates proliferate, and the historical record becomes more incomplete or debatable. Eventually, the historical approach becomes as indeterminate and impotent as the counterfactual approach, and the historian who seeks causal explanations will actually have to resort to counterfactual reasoning to try to choose between possible causal candidates for historical events. See, e.g., James D. Fearon, *Causes and Counterfactuals in Social Science: Exploring an Analogy Between Cellular Automata and Historical Processes*, in COUNTERFACTUAL THOUGHT EXPERIMENTS IN WORLD POLITICS, *supra* note 70, at 39, 40 (acknowledging the need for using a counterfactual approach to determine the causal explanation for a historical event); Richard Ned Lebow, *What's So Different About a Counterfactual?*, 52 WORLD POL. 550, 553 (2000) (noting that in analyzing historical events, the difference between counterfactuals and facts is fairly small because historians often must infer the actors' motives, since historical documents rarely establish causation and actors' statements and recollections are viewed with skepticism). Problems with the historical record in a complex, recent case such as Enron are discussed below. *Infra* Part II.A.3.

Professor Coffee argues, for instance, that none of the corporate watchdogs that should have detected problems at Enron did so in a timely fashion.<sup>86</sup> Implicit in this conclusion is a comparison of the real world to an alternative, counterfactual world: *If* one of these watchdogs had barked, *then* shareholders and others would have been spared the bite from Enron's collapse. Through this counterfactual thought process, gatekeeper failure is assigned prime causal significance in Professor Coffee's analysis.<sup>87</sup>

Professor David Millon likewise assigns causal responsibility to the gatekeepers through a more explicit counterfactual route:

*If* the gatekeepers had done their work properly, it seems quite likely that Enron would not have flown as high as it did and would have experienced a much swifter return to earth. Even under a stricter proximate cause standard, it seems reasonable to conclude that the gatekeepers' failures contributed significantly to Enron's ability to get away with its fraudulent activities for as long as it did.<sup>88</sup>

Professor Bratton uses an implicit counterfactual comparison to assign causal force to the actions of Enron's officers:

[T]he principals saw themselves as players in a tournament. Their job was not just to make money, but to make the most money—to be the superstar firm. For a superstar firm, success did not mean merely doing better than the next firm. It meant destroying the next firm and much of industrial organization along with it and always delivering good numbers. *This single-minded pursuit of first-place competitive victory caused Enron's managers to destroy their firm.*<sup>89</sup>

---

<sup>86</sup> Coffee, *supra* note 4, at 1408.

<sup>87</sup> Evidence of Professor Coffee's confidence in his own counterfactual-based conclusion is found in the subtitle to his article: *It's About the Gatekeepers, Stupid*. Coffee, *supra* note 4, at 1403.

<sup>88</sup> Millon, *supra* note 41, at 327 (emphasis added). A few scholars, like Professor Millon, make the counterfactual comparisons used in their thought experiments fairly explicit, but in most cases the counterfactual comparison remains implicit. Professor Stephen Cohen provides another example from the former camp, as he is quite explicit in imagining an alternative world in which the Supreme Court issues different opinions relevant to accounting fraud and how these differences might have affected behavior in the Enron case. See Stephen B. Cohen, *Even Before Enron: Bank Regulators, the Income Tax, the S&L Crisis, and Deceptive Accounting at the Supreme Court*, 5 GREEN BAG 2D 387, 392 (2002) (noting that, even as a bystander, the Court had a responsibility in earlier cases to point out "dishonesty and dereliction" in financial accounting practices, which may have influenced the subsequent behavior of Enron's managers and accountants).

<sup>89</sup> Bratton, *supra* note 4, at 1286-87 (emphasis added).

Thus, a counterfactual thought experiment tells us that without this single-minded pursuit of victory, the managers' conduct would not have caused the firm to fail.

After assigning primary blame to those Enron executives aiming to be superstars, Bratton allocates secondary responsibility to Arthur Andersen LLP, again through a counterfactual:

Secondary blame attaches to Enron's auditors, who manifestly *should have* refused to give a favorable opinion on Enron's financials. Here arise the case's strong regulatory implications. It is clear that Enron had captured its auditor, denuding the relationship of its necessary adversary aspect. Similar situations of capture are ubiquitous in America's corporate landscape.<sup>90</sup>

Here, Bratton imagines another counterfactual world in which Enron would not have collapsed because Arthur Andersen was not captured by Enron and refused to sign off on Enron's financials. This counterfactual leads Bratton to assign proximate cause and blame to Andersen's actions. Notice, as well, the importance of Bratton's unsupported generalization about client capture of auditors—that such capture is “ubiquitous in America's corporate landscape”—and the assumption of correctness in his version of the facts. In short, his telling of the story makes Arthur Andersen's blame “manifestly” clear. A

---

<sup>90</sup> *Id.* at 1287 (emphasis added). Bratton also imagines the effect of different disclosure standards:

[T]he monitoring process can lose its way under stacks of technical reports. Accordingly, the process mandate needs to confront moments at which the managers and directors make critical judgments and force them to disclose not only the result and its justification but the competing variables and counterfactuals. By implication, *had* Enron been forced to this higher level of disclosure it *would have* followed less aggressive accounting policies. Or, alternatively, much of the accounting would have stayed the same, but Enron would have been forced to make additional pro forma disclosures, which would have revealed all the debt or showed how earnings might have been lower if all of the SPE's were consolidated.

*Id.* at 1336 (emphasis added). Professor Janis Sarra engages in similar counterfactual reasoning when she argues that “[i]f Enron directors had been required to disclose the estimated social and economic costs of the risky SPE transactions to investors, the transactions would never have been approved. The directors would have known that investor confidence would plummet and capital would flee the corporation.” Sarra, *supra* note 4, at 744; see also Jonathan R. Macey, *A Pox on Both Your Houses: Enron, Sarbanes-Oxley and the Debate Concerning the Relative Efficacy of Mandatory Versus Enabling Rules*, 81 WASH. U. L.Q. 329, 334 (2003) (“Unfortunately, one of the object lessons in the Enron collapse is that every component in this complex corporate governance infrastructure is fundamentally broken. If but one of these components had worked properly, Enron would not have been able to deceive the investing public in the way that it did.”).

counterfactual analysis of our own reveals that if auditor capture is not ubiquitous *or* if Bratton has some of his crucial facts wrong, then Bratton's "strong regulatory implications" disappear.

The causal inference process in the Enron stories, thus, generally takes the following counterfactual form: *If* \_\_\_\_\_ [insert here one or more of your preferred scapegoat(s)—e.g., Arthur Andersen, Vinson & Elkins, the Audit and Compliance Committee of the Enron Board, the SEC, analysts, the laws put in place by Congress, etc.] had been tougher or more exacting, *then* the collapse of Enron would have been avoided.<sup>91</sup> In these thought experiments, the supposedly known

<sup>91</sup> This counterfactual causal reasoning process is on the surface quite similar to the "but for" test of causation found within tort and criminal law. See David Hamer, 'Chance Would Be a Fine Thing': *Proof of Causation and Quantum in an Unpredictable World*, 23 MELB. U. L. REV. 557, 567-71 (1999) (pointing out the parallel reasoning between the "but for" test of causation and the counterfactual analysis through two tort cases). There may be significant differences, however, between trying mentally to undo a relatively tight sequence of events in assessing "but for" causation in a litigation setting and trying mentally to undo a complex and lengthy sequence of events leading up to the collapse of a corporation such as Enron, which played controversial and innovative roles in so many fields. As Robert Strassfeld explained:

The factfinder need not confront a limitless array of antecedents; the negligence inquiry (or the definition of the defect in a strict products liability case) defines the tortious element of the past that needs to be removed in framing the antecedent.

We similarly bound the set of legally relevant consequents. . . . Our screens of legal relevance make the counterfactual inquiry more manageable, but our choice of antecedent inevitably raises questions regarding relevant conditions or cotenability.

Strassfeld, *supra* note 83, at 398-99 (footnotes omitted). Furthermore, in criminal and civil litigation, there is no choice but to engage in a single-observation case study (except perhaps in mass tort or class action cases), and it appears that jurors often do create causal stories to explain a case. See, e.g., Shari Seidman Diamond & Neil Vidmar, *Jury Room Ruminations on Forbidden Topics*, 87 VA. L. REV. 1857, 1861-62 (2001) ("Consistent with the story model, jurors do not simply record and store the evidence for later use as they receive it. Rather, they actively select and organize the trial evidence to construct a story about what happened."); see also Nancy Pennington & Reid Hastie, *The Story Model for Juror Decision Making*, in *INSIDE THE JUROR: THE PSYCHOLOGY OF JUROR DECISION MAKING* 192, 192-213 (Reid Hastie ed., 1993) (describing and presenting empirical research to support a model of story construction on the part of juries in criminal trials). For a discussion of why the law accepts necessarily incomplete causal stories in the litigation context, see Stephen J. Morse, *The Moral Metaphysics of Causation and Results*, 88 CAL. L. REV. 879, 894 (2000). In positive lawmaking settings, however, where the law acts in a forward-looking way and legislates for more than a single case, the legal system is not confined to retrospective case studies to draw causal inferences. Nevertheless, as David Hyman discusses, anecdotes and narratives often carry significant weight in legislative debates, despite their ability to mislead. See David A. Hyman, *Lies, Damned Lies, and Narratives*, 73 IND. L.J. 797, 802 (1998) ("Courts have historically embraced anecdotal evidence . . . although there have recently been some encouraging signs of increased skepticism. Legislatures—populated by lawyers and exceedingly

facts of Enron are compared to an imagined set of facts in which the value of the hypothesized causal variable (the antecedent) is changed from its real-world value, and the outcome (the consequent) in this altered world then is imagined to determine whether a change in the hypothesized causal variable would have led to a different outcome. If a different outcome would have resulted, the hypothesized variable is assigned causal force.<sup>92</sup>

It is no way around this reliance on counterfactual causal reasoning to contend that the Enron case serves as a “critical” empirical test of some theory or causal hypothesis—for example, a hypothesis that market forces will ensure adequate corporate compliance with legal norms to prevent massive fraud or major failure.<sup>93</sup> A “critical” or “crucial”

attuned to public pressure—are enthusiasts of anecdotal evidence. Members of the executive branch, regardless of political affiliation, share the same sentiments.” (footnotes omitted).

<sup>92</sup> This description presents a simplified and stylized account of a counterfactual thought experiment. The precise nature of the psychological processes used in causal inference is the subject of much study and debate. See generally Patricia W. Cheng, *From Covariation to Causation: A Causal Power Theory*, 104 PSYCHOL. REV. 367 (1997) (discussing various psychological approaches to causal inference). In the context of legal storytelling, a counterfactual or “but for” description that can be used to justify a causal conclusion may be necessary to make the story compelling. This holds true even in cases when the author reaches the causal inference in an alternative way, whether it be through a mental simulation of events, through covariation detection paired with induction, and/or through application of a preexisting causal schema or understanding of cause-effect relations to a new set of facts. In any case, some sort of mental comparison or matching process is likely to occur. Indeed, cognitive scientist Mark Turner goes so far as to say that “there seems to be *no form of causal inference in the social sciences* that does not depend upon counterfactual reasoning.” MARK TURNER, COGNITIVE DIMENSIONS OF SOCIAL SCIENCE 78 (2001); see also Lebow, *supra* note 85, at 558 (“Counterfactuals are an essential ingredient of scholarship.”). Causal inference using a “mental simulation heuristic” most closely approximates the prototypical image of a counterfactual thought experiment:

To test whether event *A* caused event *B*, we may undo *A* in our mind, and observe whether *B* still occurs in the simulation. Simulation can also be used to test whether *A* markedly increased the propensity of *B*, perhaps even made *B* inevitable. We suggest that a test of causality by simulation is involved in examples such as “You know very well that they would have quarreled even if she had not mentioned his mother.”

Daniel Kahneman & Amos Tversky, *The Simulation Heuristic*, in JUDGMENT UNDER UNCERTAINTY: HEURISTICS AND BIASES 201, 202-03 (Daniel Kahneman et al. eds., 1982).

<sup>93</sup> Professor Bratton, for instance, makes statements suggesting that Enron should be seen as a critical failure for advocates of free markets and reduced governmental regulation (i.e., that Enron provides strong negative evidence against this view), but he is also careful to note that Enron does not necessarily point the way to a fraud-free environment (i.e., Enron does not provide positive evidence in support of any particular new regulation). Bratton, *supra* note 4, at 1281-82. Bratton divides Enron

case study seeks to find “a case that *must closely fit* a theory if one is to have confidence in the theory’s validity, or, conversely, *must not fit* equally well any rule contrary to that proposed.”<sup>94</sup> In this view, the outcome in the crucial case must be as predicted, or rather postdicted, by the theory or the theory fails. Viewed as a crucial test case of a theory about the power of the market to encourage proper disclosure and discourage fraud, the outcome in Enron might serve to test a range of other possible theories about the causes of Enron’s collapse or, at least, to falsify or refute this particular causal theory. This analysis proceeds without recourse to true experiments or counterfactual thought experiments in which the causal relation of independent and dependent variables is subjected to actual or mental testing, or to statistical analyses drawing on a number of real cases to assess the association between independent and dependent variables.

The crucial case study approach cannot perform satisfactorily even this limited negation function, however, for two reasons: a fundamental analytical shortcoming and the lack of clear standards for separating crucial from noncrucial cases. First, the analytical problem is that a crucial case study cannot falsify a probabilistic theory of causation, as a moment’s reflection on the definition of probabilistic causality should reveal. A probabilistic theory of causation assigns positive causal relevance to an event when the presence of that event increases

---

scholars into two camps: one supporting deregulation, viewing the scandal as manageable by existing regulation, the other supporting increased government oversight of the market, viewing Enron as “an especially attractive basis” for reform proposals. *Id.* He identifies with the second camp. *Id.* Although he recognizes that the potential benefits of a regulation can be outweighed by its costs, Bratton maintains *that the grand narrative of the Enron scandal is a story about the failure of self-regulation.* *Id.* at 1288. He writes:

That the firm with the seventh largest market capitalization and also the firm that preached market discipline the most loudly turned out to be the shabbiest of shops with the cooperation of outside directors, outside auditors, and institutional investors, highlights the limits on what self regulation and market incentives can achieve.

*Id.* at 1360.

<sup>94</sup> Harry Eckstein, *Case Study and Theory in Political Science*, in *CASE STUDY METHOD*, *supra* note 69, at 119, 148. Eckstein provides an alternative definition of crucial case studies that “focus[es] inquiry on ‘most-likely’ or ‘least-likely’ cases—cases that ought, or ought not, to invalidate or confirm theories, if any cases can be expected to do so.” *Id.* at 149. What Eckstein calls “crucial” case studies, Yin calls “critical” case studies. See YIN, *supra* note 69, at 40 (“One rationale for a single case is when it represents the *critical case* in testing a well-formulated theory (again, note the analogy to the critical experiment).”).

the probability of another event.<sup>95</sup> Put simply, in a probabilistic causal theory, one event does not always have to cause another event in a determinate manner, but rather, the first event only has to increase the likelihood of the second event's occurrence. (In general causation terms, an event of type *C* increases the probability of an event of type *E*.) As should be apparent, then, a single-observation case study, which utilizes only one measurement point on the dependent variable, cannot disprove a probabilistic causal relationship. This is because we cannot know whether a nonpairing of putative cause and effect in this one instance is evidence of no causal relationship or is just an instance of the conditions not being right for the first event to provide sufficient force or effect to cause the second event.<sup>96</sup>

---

<sup>95</sup> Probabilistic approaches to causation differ in their particulars, and the simplified version presented in the text is closest to a statistical-relevance approach, as opposed to a view that would treat only high probability relations as causal. See, e.g., Peter Menzies, *Probabilistic Causation and Causal Processes: A Critique of Lewis*, 56 PHIL. SCI. 642, 642-43 (1989) ("[P]robabilistic theories of causation do not require that a cause confer a probability of 1, or indeed even a high probability, on the occurrence of the effect."); David Papineau, *Probabilities and Causes*, 82 J. PHIL. 57, 57 (1985) ("On the standard view, the connection between probabilities and causal explanation is simple: one event explains another just in case the former gives the latter a high probability. The higher the probability, the better the explanation." (footnote omitted)); *id.* at 59 ("An alternative to the standard view is the view that the explanation of one event by another does not require that the former give the latter a *high* probability, but simply that it make it *more* probable than it would otherwise have been." (footnote omitted)); Wesley Salmon, *Introduction to STATISTICAL EXPLANATION AND STATISTICAL RELEVANCE* 3, 11 (1971) ("An explanation is an assembly of facts statistically relevant to the explanandum, regardless of the degree of probability that results." (emphasis omitted)); Michael Strevens, *Do Large Probabilities Explain Better?*, 67 PHIL. SCI. 366, 366 (2000) ("[In] the last twenty-five years[,] . . . [we have seen a] move from accounts where only high probabilities explain to accounts where all probabilities can explain . . .").

<sup>96</sup> Charles Kegley states that:

[A] case study does *not* permit the analyst to conduct a meaningful test of a general theory, because the case under examination might comprise an exception to the general pattern that the theory seeks to explicate. A single exception or deviant case does not disconfirm a general theory, it only diminishes the extent to which its predictive and explanatory power is potent.

Charles W. Kegley, Jr., *How Did the Cold War Die? Principles for an Autopsy*, 38 MERSHON INT'L STUD. REV. 11, 32 (1994); see also Stanley Lieberson, *Small N's and Big Conclusions: An Examination of the Reasoning in Comparative Case Studies Based on a Small Number of Cases*, in CASE STUDY METHOD, *supra* note 69, at 208, 211 ("[E]xcept for probabilistic situations which approach 1 or 0 (in other words are almost deterministic), studies based on a small number of cases have difficulty in evaluating probabilistic theories." (emphasis omitted)). Stanley Lieberson further notes:

The implications of this are seen all the time in social research. In practice, for example, it is very difficult to reject a major theory because it appears not to operate in some specific setting. One is wary of concluding that Max Weber was wrong because of a single deviation in some inadequately understood time

Therefore, unless one takes the untenable position that advocates of deregulated (or less regulated) markets posit a deterministic causal relationship between market forces and self-compliance with legal norms (i.e., unless one contends that market forces must *always* prevent shabby behavior for the theory of self-regulated securities disclosure to have any validity), the crucial case study cannot truly falsify this theory. Stated more generally, to believe that a single case can falsify a causal theory, the advocate of the crucial case study approach must assume that causal relations are deterministic, but such an assumption cannot be maintained credibly for many, if not most or all, phenomena studied within the social sciences.<sup>97</sup>

---

or place. In the same fashion, we would view an accident caused by a sober driver as failing to disprove the notion that drinking causes automobile accidents.

*Id.* at 212; see also James Lee Ray & Bruce Russett, *The Future as Arbiter of Theoretical Controversies: Predictions, Explanations and the End of the Cold War*, 26 BRIT. J. POL. SCI. 441, 466 (1996) ("Since social science is necessarily a probabilistic rather than a deterministic exercise, the failure to produce an accurate forecast about one particular event is not sufficient to discredit any theory in any field . . .").

<sup>97</sup> See Lieberson, *supra* note 96, at 212 ("[T]he formal procedures used in the small-*N* comparative, historical and organizational analyses under consideration here are all deterministic in their conception. Indeed, small-*N* studies cannot operate effectively under probabilistic assumptions, because then they would require much larger *N*'s to have any meaningful results.").

Probably the dominant view within social science is that causal theories about social phenomena have a probabilistic character, or that the best we can achieve are probabilistic causal explanations. Cook and Campbell provide an example of this dominant view:

[I]n the social sciences the causal explanations we will be dealing with will be molar and contingently causal rather than ultimately micromediational and inevitable. We are also reconciled to the belief that, where any one of the contingent conditions is not as assumed, manipulating the cause may not produce the effect. Given the difficulties in conceptualizing and testing all the relevant contingent conditions, many genuine effects will appear to occur sporadically. Only later will the conditions become clear under which the cause more frequently leads to the effect. As a result, the evidence supporting molar causal laws will usually be probabilistic; it is probably the case that the more molar the causal assertion and the longer and more unspecified the assumed micromediational causal chain, the more fallible the causal law and the more probabilistic its supporting evidence.

COOK & CAMPBELL, *supra* note 65, at 33; see also Stanley Lieberson, *Einstein, Renoir, and Greeley: Some Thoughts About Evidence in Sociology: 1991 Presidential Address*, 57 AM. SOC. REV. 1, 7 (1992) ("Since there is such a wide array of conditions affecting an outcome, it is naive to think that a correct theory will predict or even explain the outcome in any given circumstance."); Michael E. Sobel, *Causal Inference in the Social Sciences*, 95 J. AM. STAT. ASS'N 647, 648 (2000) ("[T]he relationships that scientists study rarely hold in every instance. Thus it seems reasonable to entertain notions of causation that allow us

Second, the question of whether a particular case truly provides a "critical" test of a theory will usually remain debatable because there is no easy or objective way to determine criticality:

[I]n a crucial case it must be extremely difficult, or clearly petulant, to dismiss any finding contrary to theory as simply "deviant" (due to change, or the operation of unconsidered factors, or whatever "deviance" might refer to other than the fact of deviation from theory *per se*) and equally difficult to hold that any finding confirming theory might just as well express quite different regularities. One says difficult and petulant because claims of deviance and the operation of other regularities can always be made. The question is therefore not whether they are made but how farfetched or perverse the reasons for them (if any) are.<sup>98</sup>

Enron itself illustrates the difficulty of finding that incontrovertible critical test case of a theory. As Professor Bratton notes, even while the advocates of greater regulation herald Enron as evidence of market failure, the advocates of deregulated markets point to Enron's comeuppance as an example of the market's genius.<sup>99</sup>

In short, a single-observation case study cannot tell us that a probabilistic causal theory failed some crucial test, nor can we even be sure

to speak of causation in individual instances and/or that feature so-called 'probabilistic causation.'").

The probabilistic nature of causal relations in the phenomena studied by social scientists is one reason why Elster favors the search for causal mechanisms, rather than the causal laws of behavior:

It is sometimes said that the opposite of a profound truth is another profound truth. The social sciences offer a number of illustrations of this profound truth. They can isolate tendencies, propensities and mechanisms and show that they have implications for behavior that are often surprising and counterintuitive. What they are more rarely able to do is to state necessary and sufficient conditions under which the various mechanisms are switched on. This is another reason for emphasizing mechanisms rather than laws. Laws by their nature are general and do not suffer exceptions. One cannot have a law to the effect that "if *p*, then sometimes *q*." Mechanisms, by contrast, make no claim to generality. When we have identified a mechanism whereby *p* leads to *q*, knowledge has progressed because we have added a new item to our repertoire of ways in which things happen.

ELSTER, *supra* note 10, at 9-10 (footnotes omitted).

<sup>98</sup> Eckstein, *supra* note 94, at 148. Another way to understand this problem is to ask how we can know whether a case really provides a severe test of a hard-core assumption of a theory or, instead, an auxiliary assumption that must accompany the theory in order to operationalize the theory and make it potentially falsifiable.

<sup>99</sup> See Bratton, *supra* note 4, at 1281-82 (noting that deregulation supporters present Enron's collapse as evidence of free market success while advocates of greater market regulation view Enron as further evidence of the need for increased restrictions); see also *supra* note 54 and accompanying text (exhibiting that Enron can be interpreted in many different ways).

that the case studied provides a crucial test of any theory. In the end, any causal statements made on the basis of a single-observation case study will depend more on the researcher's supposition and counterfactual reasoning than on empirical observation and experimental or statistical testing.

## 2. Biases, Fallacies, and Dead Ends in Counterfactual Thought Experiments

At first blush, the use of counterfactual reasoning to analyze Enron may not strike one as particularly troubling because the causal conclusions of many Enron scholars seem obviously right: if the gatekeepers, directors, or officers of Enron had acted *sufficiently* differently for some reason, then surely a different outcome would have occurred.<sup>100</sup> But are all of these counterfactual worlds reconcilable, and if not, why select one particular counterfactual world over another?<sup>101</sup> Moreover, what do these counterfactual arguments really tell us about the exact cause(s) of Enron's collapse and the specific conditions or legal policies that could likely have led to a different outcome? Finally, is

---

<sup>100</sup> For more on these and other Enron stories, see *supra* Part I.

<sup>101</sup> Innumerable alternative counterfactual worlds can be constructed. *Supra* Part I. Indeed, simply changing the timing of events could lead to drastically different outcomes. For example, Bratton acknowledges that, if market conditions had differed, then very different results may have transpired for Enron, see Bratton, *supra* note 4, at 1301-05 (detailing and analyzing the conditions leading to Enron's downfall), perhaps leading to an earlier collapse before some of the apparent fraud occurred or perhaps even leading to no collapse at all. Or if a law firm other than Vinson & Elkins had been retained to investigate the Watkins letter, see *supra* note 21 (explaining Watkins's role within Rapoport's causal story), perhaps the outcome would have been different. Or if Enron and other energy companies had not successfully lobbied for regulatory changes that allowed the considerable expansion of business, perhaps the controversy would have been avoided. Or if the Private Securities Litigation Reform Act of 1995, Pub. L. No. 104-67, 109 Stat. 737 (codified as amended in scattered sections of 15 U.S.C.), had not passed, perhaps risk assessments would have been different. Or perhaps most simply, if only a few persons with different levels of integrity, greed, and/or fear had been in senior management or on the board—if there had been people with greater "strength of character" in charge, to put it in Dean Rapoport's terms, Rapoport, *supra* note 4, at 1380; see also *supra* note 20 and accompanying text (relaying Rapoport's belief that character flaws caused Enron's collapse)—then perhaps Enron would not have become the poster child for corporate misconduct.

For a prospective counterfactual about Enron's legacy that seems downright heretical in comparison to much of the writing by legal scholars about Enron, consider an editorial by two business school professors, Samuel Bodily & Robert Bruner, *What Enron Did Right*, WALL ST. J., Nov. 19, 2001, at A20 (opining that, despite the legal and moral questions raised, "Enron has created an enormous legacy of good ideas that have enduring value").

facial plausibility even a useful guide for determining the validity of causal conclusions drawn from retrospective counterfactuals?<sup>102</sup>

In fact, facial plausibility is not a particularly useful guide because, in hindsight, it is easy to find numerous explanations to fit the evidence, and it is therefore easy to create a counterfactual world in which Enron's collapse would supposedly not occur.<sup>103</sup> The legal scholar constructing a causal story for Enron is free to marshal whatever evidence will make her counterfactual thought experiment compelling—that

<sup>102</sup> I use the term “retrospective counterfactual” to reference thought experiments in which the past is mentally altered to reach a causal conclusion or to test a causal hypothesis and to distinguish these kinds of thought experiments from conditional forecasts about the future or prospective counterfactuals in which alternative futures are imagined and outcomes are predicted. See, e.g., Philip E. Tetlock, *Theory-Driven Reasoning About Plausible Pasts and Probable Futures in World Politics*, in *HEURISTICS AND BIASES: THE PSYCHOLOGY OF INTUITIVE JUDGMENT* 749 (Thomas Gilovich et al. eds., 2002) (discussing conditional forecasts of the future and retrospective reasoning about counterfactual worlds). I refer to predictions about future worlds as “prospective counterfactuals.”

<sup>103</sup> See, e.g., Herbert A. Simon, *Prediction and Hindsight as Confirmatory Evidence*, 22 *PHIL. SCI.* 227, 229 (1955) (“It is always easier to find *ad hoc* hypotheses—those with many degrees of freedom—to fit a body of evidence, than to find simple or parsimonious hypotheses to fit the same evidence. . . . Hence the confidence to be attached to any one of these [ad hoc hypotheses] is low.”); Philip E. Tetlock & Richard Ned Lebow, *Poking Counterfactual Holes in Covering Laws: Cognitive Styles and Historical Reasoning*, 95 *AM. POL. SCI. REV.* 829, 833 (2001) (“It is easy to extract lessons from history that reinforce our ideological or theoretical stance, and it is difficult to avoid the temptation of selection bias and theoretically self-serving reasoning in defense of that stance.” (citations omitted)).

This problem is sometimes referred to as the “Cleopatra’s Nose Problem”: Would the course of history have changed dramatically if Cleopatra’s nose had been unattractive, causing Antony not to fall for her? See Fearon, *supra* note 78, at 190 (claiming that the Cleopatra’s Nose Problem represents the following concern: If the statement “[i]f A had not occurred, [then] B would not have occurred” were true, then would one be committed to saying that A caused B? (emphasis added)). The problem is used to point out the difficulty of selecting, on a principled basis, from among the enormous number of mutations to history that could lead to an alternative outcome on the dependent variable in a counterfactual thought experiment.

The “goodness of fit” between the imagined antecedent and the consequent serves as a particularly weak measure of internal validity in thought experiments because no quantitative measure of degree of fit is possible in this qualitative approach to causal inference. In quantitative tests of theories, as in regression models and most experiments, a statistical assessment of fit is possible using measures such as percentage of variance explained—although goodness of fit remains a very weak test of a theory even when such statistical measures are used. See Seth Roberts & Harold Pashler, *How Persuasive Is a Good Fit? A Comment on Theory Testing*, 107 *PSYCHOL. REV.* 358, 361 (2000) (“[S]howing that a theory fits data is not enough. . . . Because of the flexibility of many theories, the variability of measurements, and the simplicity of most psychological data functions, it is often quite possible that the theory could fit any plausible outcome to within the precision of the data.”).

will, in effect, confirm her preferred causal story.<sup>104</sup> Causal stories thus risk becoming “intuition pumps,” rather than fair-minded efforts to evaluate causal hypotheses.<sup>105</sup>

Once freed from the constraints imposed by real-world experiments or quantitative analyses of real data, the legal scholar acting as thought experimenter is left to her own psychological devices in imagining supportive evidence and causal connections as needed to confirm her “pet theory.”<sup>106</sup> Therefore, from a psychological perspective,

<sup>104</sup> “The problem is that there is a many-many relationship between antecedents and consequences in the course of human life. As we retrospect, in contrast, we can create many-one relationships.” Robyn M. Dawes, *Prediction of the Future Versus an Understanding of the Past: A Basic Asymmetry*, 106 AM. J. PSYCHOL. 1, 16 (1993). “Our common attributional procedures . . . create many-one relationships between antecedents and consequence through selectively choosing antecedents once we have observed the consequence.” *Id.* at 16-17.

<sup>105</sup> Daniel Dennett coined the phrase “intuition pump,” referring to thought experiments designed more to suggest particular conclusions or insights than to rigorously test propositions:

A popular strategy in philosophy is to construct a certain sort of thought experiment I call an *intuition pump*. Such thought experiments (unlike Galileo’s or Einstein’s, for instance) are *not* supposed to clothe strict arguments that prove conclusions from premises. Rather, their point is to entrain a family of imaginative reflections in the reader that ultimately yields not a formal conclusion but a dictate of “intuition.” Intuition pumps are cunningly designed to focus the reader’s attention on “the important” features, and to deflect the reader from bogging down in hard-to-follow details. There is nothing wrong with this in principle. Indeed one of philosophy’s highest callings is finding ways of helping people see the forest and not just the trees. But intuition pumps are often abused, though seldom deliberately.

DANIEL C. DENNETT, *ELBOW ROOM: THE VARIETIES OF FREE WILL WORTH WANTING* 12 (1984) (citation omitted).

A story may hold intuitive appeal regardless of the logical or empirical validity of the story’s propositions or conclusions. Gopnik and Eric Schwitzgebel offer a definition of intuition that emphasizes how mysterious an intuition’s origins may seem:

We will call any judgment an *intuitive judgment*, or more briefly an *intuition*, just in case that judgment is not made on the basis of some kind of explicit reasoning process that a person can consciously observe. Intuitions are judgments that grow, rather, out of an underground process, of whatever kind, that cannot be directly observed.

Alison Gopnik & Eric Schwitzgebel, *Whose Concepts Are They, Anyway? The Role of Philosophical Intuition in Empirical Psychology*, in *RETHINKING INTUITION: THE PSYCHOLOGY OF INTUITION AND ITS ROLE IN PHILOSOPHICAL INQUIRY* 75, 77 (Michael R. DePaul & William Ramsey eds., 1998).

<sup>106</sup> See ROY A. SORENSEN, *THOUGHT EXPERIMENTS* 263 (1992) (warning of “pet theory bias” in thought experiments). Roy Sorensen simultaneously warns that

[a]ll theoreticians must guard against question-begging and wishful interpretations of their data. What makes the problem especially acute for thought experiment is the greater pliability of the data. . . . In the case of thought experiment . . . there is less external constraint on interpretation. We only have

a particular concern with thought experiments is the potential biasing influence of preexisting theories, values, and beliefs; what one hopes to find may strongly influence what one ultimately finds.<sup>107</sup> Indeed, rules of experimentation are designed to guard against such confirmation and assimilation biases by forcing the experimenter to formulate and state testable hypotheses, to subject these hypotheses to tests that present at least a risk of falsification, to apply generally accepted standards for evaluating the evidence to determine whether falsification has occurred, and to report these results publicly so that others may scrutinize the study for confirmation bias or other biasing influences. In counterfactual thought experiments, the risk of confirmation and assimilation bias increases because most of these safeguards disappear: (1) empirical falsification is, of course, not possible (because, by definition, the propositions are contrary to fact), and hence all one needs to do is generate some confirmatory evidence; (2) the mental testing process cannot be directly observed and replicated; (3) often the counterfactual theorist will fail to state clearly her hypotheses and the counterfactual comparisons used to test them; (4) the historical record that serves as the starting point for the counterfactual analysis may be disputed and open to competing interpretations; and (5) no set of normative criteria is generally accepted for evaluating counterfactual thought experiments (though a possible set of such criteria can be assembled, as discussed below).<sup>108</sup>

---

an intangible imaginary scenario. Interpretation can wander, because it is no longer tethered to a public data base.

*Id.*

<sup>107</sup> See, e.g., JONATHAN BARON, THINKING AND DECIDING 195 (3d ed. 2000) ("People tend not to look for evidence against what they favor, and, when they find it anyway, they tend to ignore it. David Perkins has named these two characteristics 'myside bias.'" (quoting D. N. Perkins et al., Learning to Reason (1986) (unpublished manuscript))). A paper by Charles Lord and his colleagues provides an additional example:

[T]here 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.

Charles 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).

<sup>108</sup> See *infra* Part II.C (outlining normative criteria that would help determine the most sound causal stories).

Similar concerns led Philip Tetlock and Aaron Belkin, in a review of counterfactual thought experiments used in political science scholarship, to state that “[p]erhaps the most lethal threat to the validity of counterfactual thought experiments comes . . . from theory-driven thinking.”<sup>109</sup> The desire to confirm one’s own theory and refute opposing theories may lead to shifting burdens of proof: “People often succumb to the temptation of applying strong tests to dissonant arguments and weak tests to consonant ones—a temptation that may be especially pronounced when the arguments invoke possible worlds that no one can ever enter and that can never be decisively disconfirmed.”<sup>110</sup> Thus, we should not be surprised that Professor Bratton finds cause for regulatory concern from Enron after he announces that he is “predisposed to draw regulatory inferences from business disasters.”<sup>111</sup> Nor should we be surprised that Professor Coffee sees

---

<sup>109</sup> Philip E. Tetlock & Aaron Belkin, *Counterfactual Thought Experiments in World Politics: Logical, Methodological, and Psychological Perspectives*, in COUNTERFACTUAL THOUGHT EXPERIMENTS IN WORLD POLITICS, *supra* note 70, at 3, 34. Matthew Crawford and Sean McCrea provide an experimental demonstration of how preexisting attitudes may bias counterfactual thoughts:

The results of the studies supported our contention that attitudes can bias counterfactual thought. Attitudes toward social issues biased both the *content* of generated counterfactuals (Study 1) and the *number* of counterfactuals generated (Study 2) in response to the scenarios. Together with the work reported by Tetlock . . . and Visser, the studies provide strong support for the notion that counterfactual reasoning is influenced by attitudes, and that counterfactual generation, as well as judged plausibility, can be theory-based.

Matthew T. Crawford & Sean M. McCrea, *When Mutations Meet Motivations: Attitude Biases in Counterfactual Thought*, 40 J. EXPERIMENTAL SOC. PSYCHOL. 65, 72 (2004) (citing Philip E. Tetlock, *Close-Call Counterfactuals and Belief-System Defenses: I Was Not Almost Wrong but I Was Almost Right*, 75 J. PERSONALITY & SOC. PSYCHOL. 639 (1998); Philip E. Tetlock & Penny S. Visser, *Thinking About Russia: Plausible Pasts and Probable Futures*, 39 BRIT. J. SOC. PSYCHOL. 173 (2000)).

<sup>110</sup> Tetlock & Belkin, *supra* note 109, at 34. Dawes refers to this problem as one of “pseudodiagnosticity”:

The main problem here is that hypotheses are not compared; instead, single hypotheses are evaluated in terms of the degree to which evidence is “consistent with” them; in addition, evidence is often *sought* in terms of its consistency with . . . “favorite hypotheses”—rather than in terms of its ability to distinguish *between* hypotheses.

Robyn M. Dawes, *Behavioral Decision Making and Judgment*, in 1 THE HANDBOOK OF SOCIAL PSYCHOLOGY 497, 533 (Daniel T. Gilbert et al. eds., 4th ed. 1998); *see also* DAWES, *supra* note 11, at 114 (“[S]tories . . . are often *selected* to prove a point, rather than forming a basis of a statistical generalization . . . or causal inference. It is the generalization or inference that leads to the selection of the story in the first place—with the results that the story provides absolutely *no* new information.”).

<sup>111</sup> Bratton, *supra* note 4, at 1282. Bratton may well be right that supporters of de-regulation are predisposed, on the other hand, *not* to draw regulatory inferences from

gatekeepers as the primary problem with Enron, given his prior emphasis on the importance of gatekeepers in securities markets,<sup>112</sup> or that Professor Partnoy focuses on the role of derivatives regulation, given his prior emphasis on the importance of this topic.<sup>113</sup>

Bratton's admitted tendency to see importance in business *disasters* highlights another concern of psychologists with regard to retrospective counterfactuals: the overweighting of vivid, anecdotal evidence relative to drab, statistical, or actuarial data.<sup>114</sup> One of the better-documented findings in psychology is that, while actuarial data itself may not be a particularly good predictor of behavior,<sup>115</sup> it generally leads to better predictions than clinical or anecdotal data.<sup>116</sup> This overemphasis on vivid data seems particularly strong when the outcome of interest is negative: "Perhaps the primary 'engine' for counterfactual thinking is a negative outcome. Indeed, negative outcomes command more intense and effortful attention in a variety of ways than do positive outcomes."<sup>117</sup> Thus, salient, spectacularly bad events

business disasters, *id.* at 1281, which would support the point that one should be wary of theory-driven biases in counterfactual thought experiments concerning charged political and legal matters. Bratton asserts that

[o]n one side stand supporters of deregulation, many of whom once touted Enron and now find it more than a little embarrassing. Its collapse, they tell us, should be taken as an exemplar of free market success. If Enron was a house of cards, it was free market actors who blew it down, with a free market administration keeping its hands off.

*Id.* (footnote omitted).

<sup>112</sup> See, e.g., John C. Coffee, Jr., *Brave New World?: The Impact(s) of the Internet on Modern Securities Regulation*, 52 BUS. LAW. 1195, 1232 (1997) ("The recurrent themes in this Article have involved gatekeepers and intermediaries. The gatekeeper has played a critical role in the history of securities regulation.").

<sup>113</sup> See, e.g., Frank Partnoy, *Why Markets Crash and What Law Can Do About It*, 61 U. PITT. L. REV. 741, 807-17 (2000) (discussing the role of derivatives investments and other factors in the downturn in Asian markets during the latter half of the 1990s); Frank Partnoy, *Playing Roulette with the Global Economy*, N.Y. TIMES, Sept. 30, 1998, at A17 ("[I]f our markets are going to crash, the most likely spark will be hedge funds and derivatives . . .").

<sup>114</sup> This greater attention to salient evidence may lead to illusory causation: "Illusory causation occurs when people ascribe unwarranted causality to a stimulus simply because it is more noticeable or salient than other available stimuli." G. Daniel Laster et al., *Illusory Causation: Why It Occurs*, 13 PSYCHOL. SCI. 299, 299 (2002).

<sup>115</sup> See Dawes, *supra* note 110, at 528 ("Another problem that arises is that both predictions [from clinical and statistical evidence] tend to be rather low.").

<sup>116</sup> See *id.* at 527-29 (reviewing the professional literature that demonstrates the superiority of prediction based on statistical, rather than clinical, evidence).

<sup>117</sup> James M. Olson et al., *Psychological Biases in Counterfactual Thought Experiments*, in COUNTERFACTUAL THOUGHT EXPERIMENTS IN WORLD POLITICS, *supra* note 70, at 296, 299 (citation omitted).

such as Enron's collapse may assume much greater importance in the mind of a counterfactual theorist than statistical evidence about the infrequency of corporate collapses or the prevalence of corporate fraud, even if the salient events are unrepresentative of the category of behavior or outcomes in question.<sup>118</sup> A corporation operating in the same economic and legal environment as Enron, but committing no apparent illegalities and only providing investors with "adequate" returns, prompts few to imagine counterfactual scenarios in which these adequate returns become pitiful. Yet, if counterfactual thought experiments are going to approximate true experiments in their approach to hypothesis testing, then both negative and positive retrospective counterfactuals should be run.<sup>119</sup>

Further, retrospective review of the events and circumstances leading up to Enron's bankruptcy bestows a sort of omniscience that may also bias our considerations of how the bankruptcy could have been avoided. Though it may appear quite obvious, given what we now know—that Enron's board of directors had sufficient reason to inquire further into many of Enron's related-party transactions involving the chief financial officer and other officers (i.e., it seems obvious that

---

<sup>118</sup> This greater attention to negative as opposed to positive events seems indicative of a general tendency for bad events to have greater psychological impact than good events. See Roy F. Baumeister et al., *Bad Is Stronger than Good*, 5 REV. GEN. PSYCHOL. 323, 323-24 (2001) ("In general, and apart from a few carefully crafted exceptions, negative information receives more processing and contributes more strongly to the final impression than does positive information. Learning something bad about a new acquaintance carries more weight than learning something good, by and large."); *id.* at 355 (describing "the greater impact of bad than good" as "extremely pervasive").

<sup>119</sup> The Enron scholars should imagine, therefore, how their proposed legal reforms would impact the behavior and output not only of companies that have failed, but also of companies that have succeeded in the present factual world. This suggestion is not as far-fetched as it may initially sound. In fact, political scientists and economists often employ computer simulations of counterfactual worlds to examine (i.e., to imagine in a more systematic way) how changes in political and market conditions might affect world security and firm performance or otherwise play out over time. See, e.g., Leonard A. Coad & Cornelis Van De Panne, *Computer Simulation for Supply-Demand Interaction*, 29 CAN. J. ECON. S308, S308-12 (1996) (using computer simulation to model the interaction of individual agents); Henry G. Grabowski & John M. Vernon, *Pioneers, Imitators, and Generics—A Simulation Model of Schumpeterian Competition*, 102 Q.J. ECON. 491, 491-516 (1987) (using computer simulation to study competition in "R&D-oriented industries"); V. M. Sergeev et al., *Interdependence in a Crisis Situation: A Cognitive Approach to Modeling the Caribbean Crisis*, 34 J. CONFLICT RESOL. 179, 192-96 (1990) (using computer simulation to explore alternative scenarios in the Cuban Missile Crisis); Richard J. Stoll, *System and State in International Politics: A Computer Simulation of Balancing in an Anarchic World*, 31 INT'L STUD. Q. 387, 392-402 (1987) (using computer simulation to explore the consequences of states acting solely in their own self-interests).

the board *should have* asked more and better questions)—what we now know may be quite different from what Enron’s board then knew or was even capable of knowing. Likewise, what we now know may be quite different from what Enron’s accountants, attorneys, and analysts knew. The danger here, for the legal scholar imagining a world in which Enron avoids bankruptcy, is what Roy Sorensen calls “[t]he [b]lindspot [f]allacy”:<sup>120</sup>

There is a difference between what the characters within a thought experiment can know and what the audience can know. Generally, people on the outside of the story have the advantage of being able to know everything on the storyteller’s word. The characters within the hypothetical cannot rely on this stipulative source. Indeed, sometimes the depicted situation will be epistemically inaccessible to the character.<sup>121</sup>

Thus, when we endow our counterfactual world with the information that is presently known, we may well ignore the many inevitable or hard-to-overcome “blindspots” that confronted the actual players in the real-world drama, but not the hypothetical players in an alternative drama.<sup>122</sup>

Just as the lack of constraints on imagination may bias our counterfactual thought experiments in that one’s predispositions and beliefs may determine the features of the other worlds imagined, the limits on our imagination may also degrade our mental simulations of causality. Consider the problem of chance events.<sup>123</sup> Just as chance

<sup>120</sup> SORENSEN, *supra* note 106, at 273.

<sup>121</sup> *Id.*

<sup>122</sup> This “curse of knowledge” in thought experiments, which may lead to a disregard of blindspots that likely arose in a historical sequence of events, is akin to the “curse of knowledge” associated with the “hindsight bias,” which may make it difficult for persons with outcome knowledge to recreate or simulate the judgments of persons without outcome knowledge. See, e.g., Colin Camerer et al., *The Curse of Knowledge in Economic Settings: An Experimental Analysis*, 97 J. POL. ECON. 1232, 1247 (1989) (“The curse of knowledge suggests that informed subjects will be unable to ignore the information they have that the uninformed subjects lack, causing a bias in their predictions.”).

<sup>123</sup> By “chance events” I mean events having extremely small probabilities of occurrence such that it becomes difficult to have great faith in any particular outcome’s occurrence. The intended meaning incorporates the colloquial sense of unanticipated, random, accidental, or very-hard-to-predict events/outcomes. My intended meaning is not quite that espoused by Jerome Manis and Bernard Meltzer because I need not take as strong a position as they do on the absence of cause and regularity: “*Chance*, as we use it here, refers to events that possess the following overlapping features: 1) absence of cause, 2) absence of predictability, and 3) absence of regularity in the sequence of the action and its antecedent conditions.” Jerome G. Manis & Bernard N. Meltzer, *Chance in Human Affairs*, 12 SOC. THEORY 45, 45 (1994). Chance events might also include events for which all possible outcomes are equally probable,

plays a role in the factual world, it surely should play a role in counterfactual worlds, but chance events and their effects, by definition, cannot be predicted in counterfactual worlds.<sup>124</sup> The only way to know what random events will occur is to experience their effects. Attempts at imagining random events that might have changed the course of Enron's history lead us down a multitude of avenues that meet in an analytical dead end because we cannot imagine all possible chance events and we lack the means to evaluate the probability of those truly chance events that we do imagine.<sup>125</sup> Thus, chance as a causal force must remain an inestimable source of variance in our counterfactual thought experiments.

Of course, one could dismiss this limitation on thought experiments, claiming that real-world experiments cannot control or account for all chance events either. This claim is true, although the experimentalist can obtain some estimates of error variance and can seek to minimize the influence of chance through random assignment to experimental conditions.<sup>126</sup> However, once over this hurdle, proponents

as in the flipping of a fair, two-sided coin. However, this type of chance event, with finite and well-defined but uncertain outcomes, presents fewer problems for the counterfactual thought experimenter, as a mental simulation containing each of the possible outcomes could be run.

<sup>124</sup> For interesting discussions concerning the role of chance events in social and historical theory, see David S. Landes, *What Room for Accident in History?: Explaining Big Changes by Small Events*, 47 *ECON. HIST. REV.* 637, 637-54 (1994); Manis & Meltzer, *supra* note 123, at 52-54.

<sup>125</sup> Professor Steven Ramirez suggests that former Enron CEO Kenneth Lay's ties to President George W. Bush may have contributed to a feeling of being "above the law," which perhaps enabled a culture of greed to grow at Enron. See Steven A. Ramirez, *Fear and Social Capitalism: The Law and Macroeconomics of Investor Confidence*, 42 *WASHBURN L.J.* 31, 67 (2002) ("No doubt this culture of greed was driven . . . by a feeling of being above the law and insulated from . . . legal attack. For example, former Enron CEO Ken Lay was a top donor to the Bush campaign, and he has yet to be indicted as this article goes to press."). So if we imagine a world in which enough of the hanging chads on ballots in the 2000 presidential election in Florida just *by chance* fell off and enough of the Florida voters supposedly confused by the butterfly ballot just *by chance* voted for Al Gore, then perhaps Gore would have become president and Enron would not have gone bankrupt. Or imagine a world in which Andrew Fastow experiences a close call with death and undergoes an ethical change of heart. When we begin intentionally imagining chance events, then we return to the problem of our limitless imagination, for any number of chance events can be posited that would have altered the outcome in Enron—that is, we encounter again the Cleopatra's Nose Problem, *supra* note 103. But we also confront the problem that, if the chance event can be imagined, then it is not sufficiently a chance event in our counterfactual world.

<sup>126</sup> Elliot Aronson, Timothy Wilson, and Marilyn Brewer provide the standard (idealized) view of randomization's benefits:

Random assignment is the great equalizer: as long as the sample size is sufficiently large, researchers can be relatively certain that differences in the

of counterfactual thought experiments confront a more serious analytical indeterminacy resulting from a severe problem of underdetermination of hypotheses by the data: "if data cannot unequivocally pick out hypothesis *H* over alternatives, then the hypotheses are underdetermined by evidence."<sup>127</sup>

While some researchers such as Willard Van Orman Quine assert that this methodological underdetermination problem also exists to some extent in experimental approaches to causation (the practical bite of this assertion remaining debated with regard to the empirical sciences),<sup>128</sup> the problem is particularly severe in counterfactual thought experiments because the data in thought experiments are so weak and controvertible given their imaginary status and because there is no generally agreed upon (and applied) set of standards for evaluating the quality of evidence generated by thought experiments.<sup>129</sup> This lack of clear norms for evaluating thought experiment data leaves only weak norms, such as patent absurdity (the obverse of facial plausibility) or

personalities or backgrounds of their participants are distributed evenly across conditions. Any differences that are observed, then, are likely to be due to the independent variable encountered in the experiment . . . .

Elliot Aronson et al., *Experimentation in Social Psychology*, in 1 THE HANDBOOK OF SOCIAL PSYCHOLOGY, *supra* note 110, at 99, 104. For discussions regarding error measurement, see Charles M. Judd & Gary H. McClelland, *Measurement*, in 1 THE HANDBOOK OF SOCIAL PSYCHOLOGY, *supra* note 110, at 180, 203-06; David A. Kenny et al., *Data Analysis in Social Psychology*, in 1 THE HANDBOOK OF SOCIAL PSYCHOLOGY, *supra* note 110, at 233, 262.

<sup>127</sup> DEBORAH G. MAYO, *ERROR AND THE GROWTH OF EXPERIMENTAL KNOWLEDGE* 174 (1996).

<sup>128</sup> For a helpful discussion of Quine's underdetermination thesis and other versions of this thesis, see Larry Laudan, *Demystifying Underdetermination*, in 14 MINNESOTA STUDIES IN THE PHILOSOPHY OF SCIENCE: SCIENTIFIC THEORIES 267 (C. Wade Savage ed., 1990). Larry Laudan concludes on a cautiously optimistic note about the status of science:

None of this involves a denial (a) that theory choice is always deductively underdetermined . . . or (b) that the nonuniqueness thesis may be correct. But one may grant all that and still conclude from the foregoing that no one has yet shown that established forms of underdetermination do anything to undermine scientific methodology as a venture, in either its normative or its descriptive aspect. The relativist critique of epistemology and methodology, insofar as it is based on arguments from underdetermination, has produced much heat but no light whatever.

*Id.* at 291-92.

<sup>129</sup> See *supra* notes 103-05 and accompanying text (addressing the difficulty of definitively attributing particular results to specific causes in thought experiments). For suggestions about what more "severe" normative criteria should look like, see *infra* Part II.C.

narrative ridiculousness, to do the work of data evaluation and hypothesis selection.

This methodological underdetermination leads to causal overdetermination (in an epistemological sense), meaning that counterfactual thought experiments may be very good for nominating causal candidates but very bad for selecting among these candidates. An unsurprising consequence of this analytical deficiency, therefore, is the proliferation of competing causal accounts for any phenomenon of even passing historical interest, as we see in the scholarship on Enron. Another consequence is the practice that the political scientist James Fearon calls “loading up of explanatory factors”: “The researcher lists several causes for the phenomenon being explained, all of which were present in the cases where the phenomenon occurred. In formal terms, the researcher has a multicollinearity problem.”<sup>130</sup> Lacking any good means to choose among competing causal candidates, the scholar resorts to combining all of the causal candidates into one overarching causal story.

We see this “loading up” practice occurring in some of the Enron stories. For instance, to his credit, Professor Bratton sought to test four alternative causal stories that might explain Enron, but his indeterminate, single-case method predictably found evidence to support all of these causal stories and resulted in multiple causation stories.<sup>131</sup> By loading up on these explanatory factors, Bratton turns a negative analytical trait into an attractive portrait of complexity and nuance. Rather than focus on the inability of the chosen mode of analysis to distinguish causal relationships from spurious relationships (i.e., to choose among confounded causal candidates), Bratton provides us with multiple causation stories that are read by a receptive audience as “a thorough exposition of the problems at Enron and a highly insightful analysis of their policy and theoretical implications.”<sup>132</sup>

---

<sup>130</sup> Fearon, *supra* note 78, at 186.

<sup>131</sup> See *supra* note 30 and accompanying text (recognizing that Bratton’s conclusion rests on a theme applicable to all four subplots). Of the four causation stories Bratton first tells, he is willing to settle on two primary causes, to which he then adds a corporate culture cause. See Bratton, *supra* note 4, at 1326 (“We can pare down the account by coupling the crisis of confidence and the hidden \$4 billion of obligations as primary causes.”); *id.* at 1332 (adding the corporate culture/“Machiavellian” mentality to the causal story mix).

<sup>132</sup> Millon, *supra* note 24, at 124 n.2. Professor Coffee provides another example of loading up on explanatory factors. See Coffee, *supra* note 4, at 1403 (noting the “excess of explanations” apparent in many “debacles of historical dimensions”); *supra* text accompanying note 59 (quoting Coffee’s statements about Enron’s anecdotal nature yet potential for generalized significance).

Rather than view this loading up of explanatory factors as an analytical weakness, one may see this practice as an acceptable move toward conjunctural explanation. "Conjunctural explanation emphasizes that a particular combination of structural causes and events, in a particular time and place, may create unique outcomes that will not necessarily be repeated in other contexts."<sup>133</sup> From this perspective, the explication of multiple explanatory factors would be understood as a statement of the unique conjunction of forces leading to Enron.<sup>134</sup>

---

Another way to understand the methodological underdetermination problem and its resultant "loading up of explanatory factors" is in terms of a single case study's inability to separate necessary causal conditions from sufficient causal conditions. As W. S. Robinson showed many years ago, studying only those cases in which the phenomenon of interest occurred (as will always be the situation in a single-observation case study) "gives only the necessary and not the sufficient conditions for the phenomenon to be explained." W. S. Robinson, *The Logical Structure of Analytic Induction*, 16 AM. SOC. REV. 812, 814 (1951), reprinted in CASE STUDY METHOD, *supra* note 69, at 187, 190. Absent the consideration of cases in which the phenomenon of interest did occur and cases in which the phenomenon did not occur (e.g., public companies audited by Arthur Andersen that did fail and that did not fail under similar market conditions), it is impossible to separate sufficient from necessary causal conditions. In a single case of corporate failure, all of the conditions leading to the failure appear to be necessary to that failure, and only empirical study of additional cases can reveal which of these factors are sufficient, but not necessary, causal conditions.

<sup>133</sup> Jeffery M. Paige, *Conjuncture, Comparison, and Conditional Theory in Macrosocial Inquiry*, 105 AM. J. SOC. 781, 782 (1999). Conjunctural explanation is similar to the notion that "causes are often disjunctions of conjunctions," Marini & Singer, *supra* note 10, at 349, which leads to probabilistic, rather than deterministic, views of causation in the social sciences. "Because the traditional notions of necessity and sufficiency in causation pertain to complex scenarios, often involving a disjunction of conjunctions that we rarely, if ever, know fully, our elliptical understanding of these scenarios results in the observation of probabilistic regularities between identifiable 'causes' and their effects." *Id.* at 356; cf. Howard S. Becker, *Cases, Causes, Conjunctions, Stories, and Imagery*, in WHAT IS A CASE? EXPLORING THE FOUNDATIONS OF SOCIAL INQUIRY, *supra* note 76, at 205, 208 ("Another approach . . . suggests . . . that causes are effective when they operate in concert. Variable  $X_1$  has an effect, but only if variables  $X_2$  and  $X_3$  and  $X_4$  are also present. In their absence,  $X_1$  might as well have stayed home." (italics added)). This "disjunction of conjunctions" view of causation flows from J. L. Mackie's notion of "INUS conditions" in causal relations:

The central idea in Mackie's conception of causation is that a cause of an event is neither a necessary nor a sufficient condition of that event, although it is a condition of a sort closely related to it. Briefly, a cause is often "an *insufficient* but *necessary* part of a condition which is itself unnecessary but sufficient for the result"; Mackie calls a condition of this kind an "INUS condition."

Jaegwon Kim, *Causes and Events: Mackie on Causation*, 68 J. PHIL. 426, 427 (1971) (quoting J. L. Mackie, *Causes and Conditions*, 2 AM. PHIL. Q. 245, 245 (1965)).

<sup>134</sup> Historians often favor such conjunctural explanations over attempts to find a few powerful causes for events or developments:

There is also the trap of monism—the search for the one explanatory factor. This is a particular and persistent temptation for economists, who worship

However, this position would seem untenable to scholars who want to portray Enron as symptomatic of larger problems.<sup>155</sup> Those who raise general alarm or arguments for reform based on a banner case can hardly concede that the outcome in the banner case arises from a confluence of historical forces unlikely to recur.<sup>156</sup>

The methodological underdetermination problem is exacerbated by what may be called the thought experiment's "bias against complexity": the counterfactual thought experiment seems best suited to examining simple and direct causal relationships (in which the independent variable takes on only two values and has a main effect on the dependent variable), rather than probabilistic, contingent, and complex causal relationships (in which the independent variable may take on a range of values and may interact with other causal variables).<sup>157</sup> By way of illustration, consider how Professor Stephen Cohen approaches a counterfactual involving the Supreme Court and Enron. Instead of imagining exactly how Supreme Court precedent would have needed to differ in order to prevent the collapse of Enron, as

what they call parsimony and like to remind us that one good reason is enough. But it has to be good. Historians tend to be suspicious of simplicity; they see it less in events and developments than in the eye of the beholder. Large processes of historical change are not likely to hinge on single causes; many pieces have to come together. Hence, the presence or absence of particular features in other places should not lead us to expect or preclude parallel and simultaneous processes of development.

Landes, *supra* note 124, at 653; see also John Lewis Gaddis, *History, Theory, and Common Ground*, INT'L SECURITY, Summer 1997, at 75, 80 ("Historians know . . . that every concept is embedded in a context. We doubt that even the most rigorous definitions fix phenomena in quite the manner that amber freezes flies.").

<sup>155</sup> See *supra* notes 56-66 and accompanying text (discussing the uniqueness of Enron's organizational structure and the difficulty such uniqueness causes those who attempt to glean general insights from the Enron situation).

<sup>156</sup> Consider on this point the statements of Professor Joshua Ronen:

Sifting through all these causes can be a nightmare to any diagnostician. It may be tempting to suggest that all have contributed to the situation in varying degrees, but such a sweeping conclusion offers no constructive policy remedies. Effective crisis resolution requires sharp distinctions: which of the implicated circumstances are truly harmful, and which are benign, possibly even salubrious when considered in isolation? Among the potentially baneful conditions, which can be cured by reform and which are resistant to legislative or regulatory intervention? Finally, considering those ills that are susceptible to effective treatment, who would be the savior, government or the free market?

Joshua Ronen, *Post-Enron Reform: Financial Statement Insurance, and GAAP Re-visited*, 8 STAN. J.L. BUS. & FIN. 39, 40 (2002).

<sup>157</sup> According to Sorensen, this "[b]ias against complexity" means that "[p]rocesses that turn on high degrees of complexity and detail get left out" of thought experiments. SORENSEN, *supra* note 106, at 266.

well as the multiple factors that might interact with a change in precedent to affect behavior, Cohen simply imagines that a switch from an uncritical to a critical tone in a key Supreme Court opinion involving deceptive accounting might have mattered greatly:

We can even imagine what might have happened if the Court had criticized the Federal Reserve for condoning deceptive accounting in its earlier opinion in *Frank Lyon*. The [Federal Home Loan Bank Board] might have been less eager a few years later to encourage the deceptive accounting that covered up the S&L crisis. Even Enron's managers and accountants, two decades later, might not have made such egregiously false financial claims.<sup>138</sup>

Here the counterfactual comparison switches the valence of a single historical feature nominated as a causal candidate to imagine how this switch might have affected the outcome. There is no effort to consider the complex ways in which other historical conditions might have changed had the Supreme Court written this opinion differently, nor to consider what specific changes in precedent would have been necessary to deter Enron's managers and accountants.<sup>139</sup> This preference for tests of gross, either/or types of causal relationships is understandable because more fine-tuned analyses of hypothesized causal antecedents would require the running of many different mental simulations. Each of these simulations would then require fine-grained alterations in the historical record, and this process would likely end in a series of complicated contingency statements, rather than in a good story that provides a definite, but overly deterministic, causal explanation.<sup>140</sup>

---

<sup>138</sup> Cohen, *supra* note 88, at 392.

<sup>139</sup> Thus, in addition to dealing with the contemplated precedent changes only in generalities, Cohen fails to address the "interconnectedness" problem presented by his counterfactual:

Scholars not infrequently assume that one aspect of the past can be changed and everything else kept constant. . . .

"Surgical" counterfactuals are no more realistic than surgical air strikes. Causes are interdependent and have important interaction effects. Even minimal rewrites of history may alter the context in such a way as to render the consequent moot or to undercut the chain of events or logic leading to it.

Lebow, *supra* note 85, at 575.

Cohen also fails to consider second-order counterfactuals that may flow from his first-order counterfactual: "The problem of prediction is further complicated by the fact that the clock of history does not stop if and when the hypothesized consequent is reached. Subsequent developments can return history to the course from which the antecedent was intended to divert it." *Id.* at 576.

<sup>140</sup> This process might also lead to much less certainty on the part of the legal scholars themselves with respect to the results of their counterfactual thought experiments.

As with other Enron stories, Cohen's argument seems facially plausible; a change in Supreme Court case law certainly could have transformed history in many ways. It could even have altered the outcome in Enron. But if we are willing to mutate history to this extent in assigning some degree of causal force to Supreme Court precedent in the specific case of Enron, then the limits on which counterfactual mutations fall out of bounds seem close to nonexistent. This parsing of a complex past into digestible causal units that our imagination can handle results in a good story that may have little connection to a plausible, alternative world. "The limitation of the story to a single sequence and the essentially ad hoc nature of causal attributions call into question the whole procedure of using stories as evidence, and of thinking that they establish causality or patterns of reasons."<sup>141</sup>

In sum, the plausibility of a legal scholar's causal story about Enron fails as anything other than a weak measure of the validity of the causal explanation being offered because plausibility may arise independently of the empirical or logical validity of the causal claim.<sup>142</sup> The histories of the natural and social sciences are littered with examples of intuitively appealing but invalid hypotheses and theories,<sup>143</sup>

---

Tetlock and Richard Ned Lebow, in a study of experts from the history and political science fields, show that asking these experts to imagine multiple ways in which history may have occurred (to consider counter-counterfactuals) and to unpack antecedent events into their specific component parts (to acknowledge the multiple contingencies in the string leading to the consequent) can affect an expert's confidence in assigning an outcome to a general causal theory. See Tetlock & Lebow, *supra* note 103, at 830 (explaining that experimental studies were designed to test the premise that "the mental processes of imagining specific counterfactual scenarios can induce us to change our mind and become more circumspect about the power of our favorite causal generalizations to delimit the range of historical possibilities"); *id.* at 839 (observing that experimental results are consistent with the theory that, "under [the] unpacking [manipulation], observers shift from a theory-driven, covering-law mode of thinking to a more idiographic, case-by-case mode").

<sup>141</sup> DAWES, *supra* note 11, at 113.

<sup>142</sup> See Robyn M. Dawes, *A Message from Psychologists to Economists: Mere Predictability Doesn't Matter Like It Should (Without a Good Story Appended to It)*, 39 J. ECON. BEHAV. & ORG. 29, 31-32 (1999) ("[A] story, which provides causal information, creates a cohesion among the elements of the inference that can otherwise be lacking, with the result that some of these elements are underutilized or even ignored completely."). For discussions regarding the psychological factors that may contribute to faulty intuitions, see Eldar Shafir, *Philosophical Intuitions and Cognitive Mechanisms*, in RETHINKING INTUITION, *supra* note 105, at 59, 71-73; Edward J. Wisniewski, *The Psychology of Intuition*, in RETHINKING INTUITION, *supra* note 105, at 45, 54-55.

<sup>143</sup> Consider the following statements by J. D. Trout:

Some of our favorite stories in the history of science, such as Kekule's famous "Eureka" episode, feature dramatic journeys to compelling explanations. In these scenarios, the explanation "felt right." This sense of understanding is

which is why many natural and social scientists reject intuition and thought experiments as authoritative guides to learning about the world.<sup>144</sup> Free of the discipline inspired by public experimentation and public demonstration of results, the thought experimenter may find it hard even to trust herself to conduct a rigorous analysis, as the Nobel Laureate Vernon Smith suggested recently when explaining his attraction to experimental, as opposed to wholly theoretical, economics:

Doing experimental economics has changed the way I think about economics. There are many reasons for this, but one of the most prominent is that designing and conducting experiments forces you to think through the process rules and procedures of an institution. Few, like Einstein, can perform detailed and imaginative mental experiments.<sup>145</sup>

---

causally potent. It can be so comforting that explanation stops when this feeling is experienced. I will argue that the psychological sense of understanding is just a kind of confidence, abetted by hindsight, of intellectual satisfaction that a question has been adequately answered. Thus this sense of satisfaction is confidence that one enjoys an accurate description of the underlying causal factors sufficient (under the circumstances) to bring about the phenomenon we are examining. But confidence is, notoriously, not an indicator of truth.

....

... In order to accord explanation the epistemic role it seems to play in successful theory selection in contemporary science, we must abandon our sentimental attachment to the comforting sense of understanding, or, at least, abandon the idea that this sense is a valid cue of truth.

J. D. Trout, *Scientific Explanation and the Sense of Understanding*, 69 PHIL. SCI. 212, 213-14, 230 (2002) (footnotes omitted).

<sup>144</sup> See, e.g., Wisniewski, *supra* note 142, at 58 ("It is clear that researchers must be very careful about relying on their intuitions in formulating theories of thought and behavior. Experimental methods are absolutely essential for determining the validity of such intuitions."). Even some philosophers reject such means of knowing. For instance, Robert Cummins observes:

Philosophical intuition is epistemologically useless, since it can be calibrated only when it is not needed. Once we are in a position to identify artifacts and errors in intuition, philosophy no longer has any use for it. Moreover, the most plausible account of the origins of philosophical intuitions is that they derive from tacit theories that are very likely to be inaccurate.

Robert Cummins, *Reflection on Reflective Equilibrium*, in RETHINKING INTUITION, *supra* note 105, at 113, 125.

For an excellent consideration of the pros and cons of both scientific experiments and philosophical thought experiments, see SORENSEN, *supra* note 106.

<sup>145</sup> Vernon L. Smith, Constructivist and Ecological Rationality in Economics, Prize Lecture (Dec. 8, 2002), available at <http://www.nobel.se/economics/laureates/2002/smith-lecture.pdf>.

### 3. Problems in the Construction of the Factual Component of Causal Stories

The counterfactual approach to causation must assume a particular factual world against which the counterfactual comparison takes place, namely, one in which a change in the valence or value of a proposed causal antecedent could reasonably have led to a different outcome. For example, an argument that accounting *standards*, rather than technical accounting *rules*, would have led to a different result in Enron (i.e., the ethic of technical compliance argument) presumes that, in reality, Enron complied with accounting rules but that these rules failed to constrain or reveal the company's deceptions. If Enron did not in fact comply with existing accounting rules, then it becomes hard to argue that a change from rules to standards would make any positive difference in the outcome.<sup>146</sup> Or if Enron's lawyers did in fact counsel Enron against certain courses of action that were nevertheless taken, then counterfactual arguments positing that better legal advice might have averted economic disaster become less convincing. Therefore, the accuracy of the factual baseline against which counterfactual comparisons are made may be crucial to the validity of counterfactual causal claims.<sup>147</sup>

---

<sup>146</sup> For an argument that Enron did not in fact comply with then-existing accounting rules, see Anthony H. Catanach, Jr. & Shelley Rhoades-Catanach, *Enron: A Financial Reporting Failure?*, 48 VILL. L. REV. 1057 (2003). According to the analysis presented by Anthony Catanach and Shelley Rhoades-Catanach, little proof exists that an inadequacy of accounting principles led to Enron's collapse:

While the implications of Enron's accounting errors for its financial position are clear, conclusions regarding the adequacy of existing accounting standards are less apparent. In each of the cases discussed above, Enron violated existing financial reporting standards and SEC reporting regulations. Following the collapse of Enron, many argued that inadequate accounting principles were at fault. Yet, Enron's financial statements did not conform to existing accounting standards, suggesting that the standards themselves were *not* at fault. While the recent focus on financial reporting requirements may bring about needed changes and improvements in the quality of financial information provided to investors, current standards should not be blamed for Enron's failure.

*Id.* at 1074 (footnote omitted).

<sup>147</sup> Of course, to the extent that one relies on the Enron stories for descriptions of background conditions without regard to the validity of the causal conclusions made about Enron, factual accuracy matters as well. In other words, factual accuracy is important whether the Enron stories are used for descriptive or explanatory purposes.

In constructing the factual component of their causal stories, the Enron scholars rely heavily on journalists' reports<sup>148</sup> and the report of the Special Investigative Committee of the Board of Directors of Enron Corp. (Powers Report).<sup>149</sup> Although the means for selecting particular sources is not typically revealed in any detailed way,<sup>150</sup> the news sources relied on appear to represent a convenient sample<sup>151</sup> of stories

---

<sup>148</sup> See, e.g., Bratton, *supra* note 4, *passim* (citing numerous newspaper and magazine stories as well as editorials for the Enron facts); Dallas, *supra* note 8, at 45 (reporting that "[a] review of journalist accounts of Enron" was used to develop a description of the ethical climate at Enron); Robert W. Gordon, *Portrait of a Profession in Paralysis*, 54 STAN. L. REV. 1427, 1436-37 nn.18-21 (2002) (employing various newspaper stories as supporting authorities); Lipson, *supra* note 4, at 114 nn.58 & 61 (referencing newspaper accounts); Nancy B. Rapoport, *Multidisciplinary Practice After In re Enron: Should the Debate on MDP Change at All?*, 65 TEX. B.J. 446, 446 & 447 n.2 (2002) (citing Web site collections of news accounts regarding Enron); Viccaro, *supra* note 4, *passim* (relying on numerous newspaper, magazine, and television stories, by both reporters and pundits, as sources of information).

<sup>149</sup> POWERS REPORT, *supra* note 25. This report is often referred to as the "Powers Report" because the dean of the University of Texas School of Law, William C. Powers, Jr., served as the chair of the special committee. Examples of scholarly reliance on the Powers Report include Bratton, *supra* note 4, *passim*; Gordon, *supra* note 148, at 1438 n.22; Kahn, *supra* note 12, at 1608; Lipson, *supra* note 4, at 102; Millon, *supra* note 41, at 316-19 nn.26-40.

<sup>150</sup> In a few instances, the scholars do report that they relied primarily on Professor Bratton's retelling of the story. See *supra* note 24 (listing scholars who explicitly rely on Bratton's narrative).

<sup>151</sup> The apparently nonrandom selection of sources raises the possibility of evidence selection bias, unless a deliberate, nonrandom strategy was employed to guard against such bias. This selection problem may be common within the social science arena. In historical sociology, for example,

[t]he primary methodological problem is . . . not . . . the difficulty of finding necessary information. The more daunting question is how to choose sources of data without permitting correspondence between the categories and implicit theoretical postulates used in the chosen sources to ensure positive answers to the questions being asked about the data. For social scientists this type of problem—unintended contamination of observations due to the analyst's disposition to corroborate hypotheses—is not an unfamiliar problem.

Ian S. Lustick, *History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias*, 90 AM. POL. SCI. REV. 605, 608 (1996).

The danger here is that the researcher, left to her own nonrandom selection of data, will be theory-driven in her selection of evidence, much like the supposed sharpshooter who shoots first and then draws a target around the bullet hole. See J.M. Balkin, *Too Good to Be True: The Positive Economic Theory of Law*, 87 COLUM. L. REV. 1447, 1461 (1987) (analogizing the action of drawing a target around a bullet hole to authors' behavior in "trying to get to a certain conclusion . . . by constructing an economic model that guarantees that [conclusion]" (reviewing WILLIAM M. LANDES & RICHARD A. POSNER, *THE ECONOMIC STRUCTURE OF TORT LAW* (1987))).

from preferred publications such as the *New York Times* and the *Wall Street Journal*.<sup>152</sup>

This repeated reliance on a few common sources increases the risk of error cascades in which “erroneous claims perpetuate themselves through repetition.”<sup>153</sup> If advocates of particular viewpoints have a strong interest in perpetuating a particular version of the Enron facts, then biased factual accounts may appear in the work of even disinterested or open-minded scholars through such error cascades.<sup>154</sup>

<sup>152</sup> Reports by Kurt Eichenwald and Diana Henriques of the *New York Times* seem particularly popular citations. See, e.g., Timothy P. Duane, *Regulation's Rationale: Learning from the California Energy Crisis*, 19 YALE J. ON REG. 471, 472 n.1 (2002) (“For a detailed account of Enron’s rise and fall, see Kurt Eichenwald, *Audacious Climb to Success Ended in a Dizzying Plunge*, N.Y. TIMES, Jan. 13, 2002, at A1.”); Powell, *supra* note 4, at 1293 n.13 (describing a story by Eichenwald and Henriques as a “helpful historical account of the secret machinations that brought about Enron”); Marisa Rogoway, *Recent Developments, Proposed Reforms to the Regulation of 401(k) Plans in the Wake of the Enron Disaster*, 6 J. SMALL & EMERGING BUS. L. 423, 424 n.1 (2002) (“For a thorough treatment of Enron’s last year in business, see Kurt Eichenwald & Diana B. Hendriques [sic], *Enron Buffed Image to a Shine Even as it Rotted from Within*, N.Y. TIMES, Feb. 10, 2002, at 1.”).

<sup>153</sup> Duane, *supra* note 152, at 537; cf. Cass R. Sunstein, *Foreword: On Academic Fads and Fashions*, 99 MICH. L. REV. 1251, 1264 (2001) (“[I]n law and [elsewhere], ideas may spread and prosper, not because they are good, but because dozens, hundreds, or even thousands of imperfectly informed people have fortified the very signals by which they have been influenced. . . . [L]ongevity, even for bad ideas, is hardly out of the question.”).

<sup>154</sup> In addition to a possible ideological or results-oriented bias in stories about Enron, such reporting may be affected by what Sendhil Mullainathan and Andrei Shleifer refer to as a “spin” bias, which reflects a newspaper’s goal of condensing stories while making them attractive to readers. SENDHIL MULLAINATHAN & ANDREI SHLEIFER, *MEDIA BIAS 2* (Harv. Inst. of Econ. Research, Discussion Paper No. 1981, 2002), available at <http://post.economics.harvard.edu/hier/2002papers/2002list.html>. These authors distinguish between two kinds of media bias:

In this paper, we draw a sharp analytical distinction between . . . two kinds of biased reporting. We call the traditional left or right bias ideology, and the less traditional bias—one based on the need to tell a story—spin. We see ideology as coming directly from the preferences of either editors or reporters. For example, left wing newspapers may simply prefer to report news one way. We see spin, on the other hand, as coming from a newspaper’s attempt to tell a simple and memorable story. The act of simplification [sic] leads to bias since some information is necessarily discarded.

*Id.* When media competition and a lack of diversity of ideological viewpoints on a story exist, we may see a greater bias toward spinning a story for its effect on readers:

For news that have significant right-left ideological dimensions, such as presidential elections, competitive media are likely to produce accurate reporting, on average. On the other hand, for news that do not have such a dimension, such as cases involving law enforcement or many aspects of foreign policy, spin rather than ideological diversity is likely to shape competitive reporting.

Even if intentional distortions are absent from the reporting on the Enron affair, some factual errors should still be expected given the necessary abstraction and interpretation accompanying historical records.

All sources used to construct a narrative—oral histories, ethnographies, newspaper accounts, official and personal documents, and secondary accounts—are abstractions from a myriad of facts. They are necessarily selective and possibly erroneous owing to the limited information contained even in the primary sources, to faulty recall, and even to deliberate prevarication. Accounts, in brief, are constructions of events rather than necessarily truthful accounts of what really happened. Moreover, narratives formulated explicitly by the investigator from any source, for the purpose of further analysis . . . are doubly constructed.<sup>155</sup>

Therefore, when dealing with any historical account of an event, but particularly one involving as many complex legal and business matters as the Enron affair, errors in the reconstruction and reporting of the event should be expected.<sup>156</sup> Professor Timothy Duane notes, for

For such stories, competition does not eliminate the media bias, and there is no good reason to expect accuracy in media.

*Id.* at 21. The cautionary comments of Simon Hug regarding reliance on secondary sources complement the conclusions of Mullainathan and Shleifer:

Selection by a third party occurs when a researcher relies on secondary sources. Often such reliance on secondary sources is unavoidable, as field research is practically impossible. Given that the “creators” or “authors” of the secondary sources almost by definition select what they wish to report, considerable biases can result. . . . Unfortunately, it is . . . well known that media are subject to selectivity, choosing only particular stories to report. Research clearly shows that newspapers disproportionately report violent and large events. Consequently, using data based on newspaper reports can lead to serious selection biases.

Simon Hug, *Selection Bias in Comparative Research: The Case of Incomplete Data Sets*, 11 POL. ANALYSIS 255, 258 (2003) (footnote and citations omitted).

For discussions about distortions of facts concerning the civil justice system in general, high profile tort cases—particularly the McDonald’s coffee case—and the frequency and size of punitive damage awards, see Daniel S. Bailis & Robert J. MacCoun, *Estimating Liability Risks with the Media as Your Guide: A Content Analysis of Media Coverage of Tort Litigation*, 20 LAW & HUM. BEHAV. 419 (1996); Theodore Eisenberg et al., *Reconciling Experimental Incoherence with Real-World Coherence in Punitive Damages*, 54 STAN. L. REV. 1239 (2002); Marc Galanter, *An Oil Strike in Hell: Contemporary Legends About the Civil Justice System*, 40 ARIZ. L. REV. 717 (1998); Michael McCann et al., *Java Jive: Genealogy of a Juridical Icon*, 56 U. MIAMI L. REV. 113 (2002); Jennifer K. Robbenolt, *Determining Punitive Damages: Empirical Insights and Implications for Reform*, 50 BUFF. L. REV. 103, 159-99 (2002).

<sup>155</sup> Griffin, *supra* note 80, at 1128.

<sup>156</sup> Ideally, the case study researcher follows evidence-gathering rules designed to obtain a diverse sample of perspectives from a variety of sources, and this strategy reveals converging evidence. As argued by Professor Yin:

example, that news stories about the California energy crisis—which some contend Enron had a hand in creating—regularly contained factual errors.<sup>157</sup> Professor Partnoy warns more broadly that the importance of Enron's derivatives trading has not been fully appreciated by legal scholars, leaving existing accounts of Enron's collapse incomplete.<sup>158</sup>

---

The use of multiple sources of evidence in case studies allows an investigator to address a broader range of historical, attitudinal, and behavioral issues. However, the most important advantage presented by using multiple sources of evidence is the development of *converging lines of inquiry*. . . . Thus, any finding . . . is likely to be much more convincing and accurate if it is based on several different sources of information, following a corroboratory mode.

YIN, *supra* note 69, at 98; *see also* Lustick, *supra* note 151, at 615-16 (discussing ways of dealing with the problem that historical accounts cannot be viewpoint or theory neutral when using historical sources to construct a background narrative).

<sup>157</sup> Professor Duane noted in particular:

I found four to five factual errors per story in some of the major California newspapers when rolling blackouts first hit in January 2001, although that number dropped to just one to two errors per story over the next few months. Some of those errors—especially the notion that California's restructuring effort caused California not to build any new generating capacity in the 1990s—continue to persist today. . . . One of the fundamental premises of the public policy debate—that California had not added any new generating capacity in the 1990s—was therefore simply wrong. Yet, public misperception of the relationship between supply and demand drove both the policy discourse and the selected course of action.

Duane, *supra* note 152, at 537 (footnote omitted).

<sup>158</sup> Professor Partnoy contends that a greater understanding of Enron's financial instruments will lead to a causal story assigning centrality to derivatives:

[T]he key to understanding Enron's collapse is to reframe this discussion in terms of the complexity of the financial instruments—derivatives and off-balance sheet transactions—that drove Enron's major businesses. Unfortunately, even after intense media scrutiny, congressional hearings and other government investigations, most of the firm's derivatives dealings remain unpenetrated. Even after Enron's bankruptcy, the firm's own officials were unable to grasp enough detail to issue an annual report in 2002; even with the help of a new team of accountants from PricewaterhouseCoopers, they simply could not add up the firm's assets and liabilities. This Article's claim is that those details are central. If scholars are to understand the implications of Enron's collapse, they must begin by revisiting the conventional story about Enron.

Partnoy, *supra* note 52, at 1248 (footnotes omitted). He further explains:

The reasons for Enron's collapse should affect the normative conclusions of scholars, and the standard account of these reasons is incomplete. At its core, Enron was a derivatives trading firm; it made billions trading derivatives, but it lost billions on virtually everything else it did. Enron used its expertise in derivatives to hide these losses. For most people, the fact that Enron had transformed itself from an energy company into a derivatives trading firm is a surprise, although there were many clues buried in its financial statements.

Reluctance to treat the Powers Report as the final word on Enron's transactions seems particularly appropriate given the conditions under which the report was prepared. It was drafted quickly, with the assistance of counsel, in the midst of litigation, under intense political scrutiny, and by a committee that included a member of the Enron board supposedly on watch when the trouble began.<sup>159</sup> The introduction to the Powers Report itself clearly expresses the report's limited purpose and acknowledges a basic incompleteness:

We were not asked, and we have not attempted, to investigate the causes of Enron's bankruptcy or the numerous business judgments and external factors that contributed [to] it. Many questions currently part of public discussion—such as questions relating to Enron's international business and commercial electricity ventures, broadband communications activities, transactions in Enron securities by insiders, or management of employee 401(k) plans—are beyond the scope of the authority we were given by the Board.

There were some practical limitations on the information available to the Committee in preparing this Report. We had no power to compel third parties to submit to interviews, produce documents, or otherwise provide information. Certain former Enron employees who (we were told) played substantial roles in one or more of the transactions under investigation—including Fastow, Michael J. Kopper, and Ben F. Glisan, Jr.—declined to be interviewed either entirely or with respect to most issues. We have had only limited access to certain workpapers of Arthur Andersen LLP[,] . . . Enron's outside auditors, and no access to materials in the possession of the Fastow partnerships or their limited partners. Information from these sources could affect our conclusions.<sup>160</sup>

---

*Id.* at 1280; *see also id.* at 1263 (“[M]any market participants have fundamentally misunderstood Enron's collapse, even more than a year after the company's bankruptcy . . .”).

<sup>159</sup> The report itself states:

One member of the Special Investigative Committee, Herbert Winokur, Jr., was a member of the Board of Directors and the Finance Committee during the relevant period. The portions of the Report describing and evaluating actions of the Board and its Committees are solely the views of the other two members of the Committee, Dean William C. Powers, Jr. of the University of Texas School of Law and Raymond S. Troubh.

POWERS REPORT, *supra* note 25, at 6 n.1. Perhaps this partition approach was successful and Winokur had no improper or self-interested influence on the tone, emphases, or conclusions of the Powers Report, but given the ultimate governing authority of the board, one wonders how the special committee could have neatly partitioned aspects of the investigation with possible adverse implications for Winokur from those parts of the investigation without such implications.

<sup>160</sup> *Id.* at 2.

The concern here is not that the Powers Report is too hard or soft on any officers, directors, or outside professionals providing services to Enron. Rather, the concern is with the use of the report as an authoritative and unbiased account of the transactions that it does address (and sometimes as the only source on certain transactions), instead of as one data point or as an example of how corporations respond to a calamitous event giving rise to considerable potential liability for the officers and directors.

#### 4. Summary of Concerns About Internal Invalidity

Serious problems exist with the evidence relied on by the Enron scholars and the inferences these scholars have drawn from it. Because the evidence chosen consists of only one case and involves a single outcome, it is not possible to use experimental, quantitative, or comparative case study approaches to discern potential causes of this outcome. Instead, the Enron scholars must rely on counterfactual thought experiments in developing their causal explanations for Enron's collapse. Counterfactual thought experiments, however, suffer from a serious methodological underdetermination problem, as well as a variety of other inferential shortcomings. In addition, many of the Enron scholars have not approached the historical data as skeptical consumers who question the accuracy and completeness of their sources. Instead, the Enron scholars have treated news stories and the Powers Report as authoritative factual sources and seem to have expended little independent effort to confirm the sources' accuracy or find converging evidence. Taken together, these factors raise serious doubts about the internal validity of the causal stories being told about Enron.

##### B. *External Invalidity: The Problem of Generalizing from Small Samples*

The selection of a single case for study drastically limits not only the internal validity, but also the external validity of the Enron stories (i.e., the validity of inductive inferences drawn from Enron to explain or predict outcomes in other cases). Because there is simply no way to make a valid *statistical* generalization from a single case, a single-observation

case study can never possess any demonstrated external validity.<sup>161</sup> In short, “[a] single case does not a tendency make.”<sup>162</sup>

A single-observation case study permits only *analytical* generalization.<sup>163</sup> Analytical generalization is synonymous with theoretical inference

<sup>161</sup> Professor Janet Ward Schofield summarizes the problem well:

Practically speaking, no matter what one’s philosophical stance on the importance of generalizability, it is clear that numerous characteristics that typify the qualitative approach are not consistent with achieving external validity as it has generally been conceptualized. For example, the traditional focus on single-case studies in qualitative research is obviously inconsistent with the requirements of statistical sampling procedures, which are usually seen as fundamental to generalizing from the data gathered in a study to some larger population. This fact is often cited as a major weakness of the case study approach.

Janet Ward Schofield, *Increasing the Generalizability of Qualitative Research*, in CASE STUDY METHOD, *supra* note 69, at 69, 70; *see also* Epstein & King, *supra* note 77, at 110 (“[R]andom selection is the only selection mechanism in large-*n* studies that automatically guarantees the absence of selection bias. . . . When appropriately applied, random selection prevents bias except by chance, and a large *n* means that the chance is exceptionally small.”).

<sup>162</sup> *In re Synthroid Mktg. Litig.*, 201 F. Supp. 2d 861, 881 (N.D. Ill. 2002).

<sup>163</sup> *See* YIN, *supra* note 69, at 10 (“[C]ase studies, like experiments, are generalizable to theoretical propositions and not to populations or universes. In this sense, the case study . . . does not represent a ‘sample,’ and . . . your goal will be to expand and generalize theories (analytic generalization) and not to enumerate frequencies (statistical generalization).”); *id.* at 37 (“Survey research relies on *statistical* generalization, whereas case studies (as with experiments) rely on *analytical* generalization. In analytical generalization, the investigator is striving to generalize a particular set of results to some broader theory.”); Shrader-Frechette & McCoy, *supra* note 74, at 239 (“[T]he single case study and the single experiment face the problem that both can be generalizable to theoretical propositions but not to populations or universes.”).

The terms “analytical generalization” and “statistical generalization” are synonyms for the terms “theoretical inference” and “empirical generalization,” respectively:

There seem to us to be just two effective strategies for drawing conclusions from some smaller set of cases to a larger set. Furthermore, these are the ones used by experimental and survey researchers. . . .

Theoretical inference involves reaching conclusions about what always happens, or what happens with a given degree of probability, in a certain type of theoretically defined situation. . . . So, the aim in research directed towards drawing conclusions on the basis of theoretical inference is to identify a set of relationships among variables that are universal, in the sense of occurring everywhere that specified conditions hold, other things being equal (that is, wherever there are not countervailing or overdetermining factors). . . .

By contrast, survey researchers typically rely on what we shall call empirical generalization in order to produce general findings. This involves drawing inferences about features of a larger but finite population of cases from the study of a sample drawn from that population. At its simplest, this amounts to reaching conclusions about the *distribution* of particular features within a population.

and means only that the researcher distills from her single case study a causal relationship that she hypothesizes will hold, all other things being equal, for a larger class of cases. Professor Langevoort provides a good example of an analytical generalization in describing the class of cases to which his causal theory about Enron might generalize.<sup>164</sup> The better analytical generalizations, like Langevoort's, explicitly identify the cases to which a causal theory should and should not apply.<sup>165</sup>

Even strong advocates of case study research admit that an analytical generalization, standing alone, proves little about the reach of a case study's findings.<sup>166</sup> In moving from an analytical generalization to a statistical or empirical generalization, additional evidence must be gathered to show that the causal story being told about Enron applies to other corporate settings as well. Unfortunately, within the Enron stories, we find too much reliance on *ipse dixit*, and too few examples of additional data that might support generalizations from the Enron case.<sup>167</sup>

For instance, when a scholar with the well-earned academic stature of Professor Coffee tells the reader that "experience over the 1990s suggests that professional gatekeepers do acquiesce in managerial fraud, even though the apparent reputational losses seem to dwarf the gains to be made from the individual client,"<sup>168</sup> the reader is expected simply to accept Professor Coffee's claim given his expertise and reputation in the field. Coffee offers no proof to back up his

Roger Gomm et al., *Case Study and Generalization*, in *CASE STUDY METHOD*, *supra* note 69, at 98, 102-03.

<sup>164</sup> Professor Langevoort limits the application of his causal theory about Enron to highly competitive firms in the "new economy," and he supplies a more detailed definition of such firms. See Langevoort, *supra* note 4, at 968 (maintaining that firms of the "new economy" . . . require[] a high rate of creative productivity from a large number of key managers and employees").

<sup>165</sup> See J. Clyde Mitchell, *Case and Situation Analysis*, in *CASE STUDY METHOD*, *supra* note 69, at 165, 182 ("All cases are necessarily contextualized and generalizations made from case studies must therefore be qualified . . . It is incumbent on the observer to provide readers with a minimal account of the context to enable them to judge for themselves the validity of treating other things as equal in that instance.").

<sup>166</sup> See YIN, *supra* note 69, at 37 ("The [analytical] generalization is not automatic, however. A theory must be tested by replicating the findings in a second or even a third [case], where the theory has specified that the same results should occur. . . . [T]he results might [then] be accepted as providing strong support for the theory . . .").

<sup>167</sup> See *supra* notes 63-64 and accompanying text (juxtaposing the danger in drawing general conclusions from anecdotal evidence with the prevalence of this reasoning in establishing Enron causal stories).

<sup>168</sup> Coffee, *supra* note 4, at 1405.

claim other than a citation to another article by another authoritative source, which itself also provides no specific empirical data to support the generalization made by Coffee.<sup>169</sup> Likewise, we are expected to accept on faith Professor Bratton's lesson that, "if Enron teaches us anything, it is to question the reasonableness of reliance on any corporate monitor,"<sup>170</sup> even though Bratton advances this assertion after analyzing

---

<sup>169</sup> *Id.* at 1405 & n.10 (citing Robert A. Prentice, *The Case of the Irrational Auditor: A Behavioral Insight into Securities Fraud Litigation*, 95 NW. U. L. REV. 133, 219 (2000)). While Professor Coffee may have good data to support his claim of gatekeeper acquiescence in managerial fraud, this particular citation to Professor Robert Prentice's article and other citations within Coffee's article do not provide it.

Consider as a counterpoint to Professor Coffee's suggestion of significant acquiescence statements by Daniel Dooley, a senior partner with PricewaterhouseCoopers LLP, at a law school forum on Enron:

On average, there has never been more than 2.9% of the registrants in the market alleged to have committed a financial fraud over the last ten years. On average, it is 220 per year. You can run the math. There are roughly 14,900 registered companies. That includes about 10,500 domestic and the rest of them are foreign that are on the U.S. exchanges.

....

... So every year, roughly 220 to, in a top year, it was about 325, and that year was the year they were clearing out their backlog at Milberg Weiss's firm, so right before the 1995 Private Securities Litigation Reform Act ("PSLRA") format.

So what gives here? I know that Bill Lerach is not cutting anybody a break with respect to bringing litigation. So if, in fact, fraud is rife, how come fraud only seems to pop up over a ten-year period of time with a frequency of less than three percent? Ninety-seven percent of the people who file Form 10-Ks and Form 10-Qs and Form 8-Ks and press releases either are getting away with murder or they are not committing fraud.

Panel Discussion, *Enron: What Went Wrong?*, 8 FORDHAM J. CORP. & FIN. L. S1, S13 (2002) (statement of Daniel Dooley, Partner, PricewaterhouseCoopers LLP) (footnote omitted); see also PRICEWATERHOUSECOOPERS LLP, 2001 SECURITIES LITIGATION STUDY 3 (2002) ("While the percentage of cases alleging accounting violations continues to grow each year, the number of actual accounting cases has declined for the fourth year in a row. The 100 accounting cases filed in 2001 are the fewest since 1997."), available at [http://www.pwcglobal.com/gx/cfr/investigations/pwc\\_securitieslitigationstudy\\_2001.pdf](http://www.pwcglobal.com/gx/cfr/investigations/pwc_securitieslitigationstudy_2001.pdf); Aronson, *supra* note 4, at 135 ("In fact, one can find few instances of independent auditors colluding with companies to defraud investors. Instead, auditors were usually defrauded in almost all instances."). These statements are not particularly dispositive because (1) the number of reported cases of alleged fraud does not equal the actual number of fraud cases, (2) the costs associated with the fraud are important to know independent of the frequency data, and (3) Dooley, of course, has some self-interest in underestimating the magnitude of the fraud concern. Nevertheless, these statements show that informed persons disagree about the seriousness and prevalence of gatekeeper acquiescence in fraud. In the end, the point is that hard data on the frequency and costs of accounting fraud need to be advanced in support of the arguments.

<sup>170</sup> Bratton, *supra* note 4, at 1340. It is worth reiterating, however, that Bratton is skeptical of basing fundamental corporate and securities law reforms on Enron. See *supra* note 38 and accompanying text (explaining Bratton's view that Enron's collapse

only a single corporate monitoring failure without any attempt to determine the ratio of corporate monitoring failures to successes. Skepticism about this claim increases when we consider that evidence of monitoring failures is likely to have much greater public availability and salience than evidence of monitoring successes, regardless of the relative frequency of failures and successes. Indeed, one may venture a guess that every business day numerous unpublicized instances occur in which the mere presence of corporate monitors serves to positively constrain the behavior of corporate officers (but this hypothesis awaits empirical proof, as do the statements of Professors Coffee and Bratton and others generalizing from Enron's example).

The scholar extending her Enron story to other cases may sincerely believe that the causal circumstances of the Enron case hold in other cases and may even possess some good evidence to support her contentions. However, sincere belief absent any public display of empirical proof amounts to little more than an appeal to the authoritative status of the scholar.<sup>171</sup> "In the absence of data, anyone's word may be advanced as a law of behavior."<sup>172</sup>

### C. *Criteria for Evaluating Causal Stories Employing Thought Experiments*

One reasonable response to the single-observation case study approach might be to reject it altogether, given its dependence on counterfactual causal reasoning and its inability to generalize knowledge about a single case to a class of cases.<sup>173</sup> Another response might be to

should not be blamed on the failure of the rules, but rather on the fact that Enron strategically evaded the rules).

<sup>171</sup> A respectable argument can be made for deference to the epistemic authority of the expert under certain conditions. See, e.g., Robert Pierson, *The Epistemic Authority of Expertise*, 1 PSA: PROC. BIENNIAL MEETING PHIL. SCI. ASS'N 398, 403 (1994) ("[W]hen an expert is concerned only with claims that result from the control and manipulation of her discipline's defining set of variables, then there is no rational room for extra-systematic or lay evaluation of those claims."). However, in the face of conflicting expert opinions, and absent objective measures of trustworthiness and expertise, it seems that an examination of the empirical and logical foundations of these opinions would be a sensible place to start in adjudicating among the different findings with regard to both Enron and its wider consequences. Furthermore, as the discussion of the validity of the Enron stories suggests, legal expertise does not guarantee expertise at causal inference or empirical generalization.

<sup>172</sup> Baruch Fischhoff, *Can Any Statements About Human Behavior Be Empirically Validated?*, 4 BEHAV. & BRAIN SCI. 336, 337 (1981).

<sup>173</sup> See, e.g., KING ET AL., *supra* note 11, at 130 ("The cases of extreme selection bias—where there is by design no variation on the dependent variable—are easy to deal with: avoid them! We will not learn about causal effects from them."); YIN, *supra* note 69, at 10 ("A second common concern about case studies is that they provide little

accept that single-observation case studies have been, and are likely to remain, a common research strategy within historical, legal, and scientific research and to ask how such studies might best be used to advance our understanding of significant legal events such as the collapse of Enron.<sup>174</sup>

If one takes the latter position, then one must develop a good way of determining which counterfactual thought experiments stand out as being better than others. From a learning standpoint, the primary problem with causal stories generated through counterfactual means is that they offer no obvious way to choose which stories are superior. Critics are left to judge the relative plausibility or absurdity of the stories. Thus, there is little way to tell which stories provide sound bases for learning about causal relations in the world. This unsatisfactory situation arises because single-observation case studies employing thought experiments do not permit the usual types of "severe tests" of causal hypotheses that conventional empirical research permits, such as tests of increasing precision or stringency or confrontations of opposing theories using controlled and observable experiments.<sup>175</sup>

basis for scientific generalization."); Lebow, *supra* note 85, at 550-51 ("[F]or most members of our profession counterfactual arguments appear to have no scientific standing. They are flights of fancy, fun over a beer or two in the faculty club, but not the stuff of serious research.").

<sup>174</sup> See, e.g., Lebow, *supra* note 85, at 558 ("Counterfactuals are an essential ingredient of scholarship. They help determine the research questions we deem important and the answers we find to them. They are also necessary to evaluate the political, economic, and moral benefits of real-world outcomes. These evaluations in turn help drive future research.").

<sup>175</sup> Deborah Mayo offers one account of "severe tests" of hypotheses, MAYO, *supra* note 127, at 178-83, and explains the logic behind the use of severe tests:

Although a single inquiry involves a network of models, an overall logic of experimental inference emerges: data  $e$  indicate the correctness of hypothesis  $H$ , to the extent that  $H$  passes a severe test with  $e$ . All the tasks of the interconnected models are directed toward substantiating this piece of reasoning. To remind us, hypothesis  $H$  passes a severe test with  $e$  if  $e$  fits  $H$ , and the test procedure had a high probability of producing a result that accords *less well* with  $H$  than  $e$  does, if  $H$  were false or incorrect.

*Id.* at 445. She further explains:

The thrust of the "other hypothesis" objection is this: the fact that data fit hypothesis  $H$  fails to count (or to count much) in favor of  $H$  because the data also fit other, possibly infinitely many, rival hypotheses to  $H$ . The above characterization of severe tests suggests how this objection is avoidable: mere fitting is not enough! If hypotheses that fit the data equally well were equally well supported (or in some way credited) by the data, then this objection would have considerable weight. But the very *raison d'être* of the severity demand is to show that this is not so.

*Id.* at 187.

Nevertheless, a set of normative criteria more severe than facial plausibility or patent absurdity may be specified to test the products of counterfactual thought experiments and thus help us to separate the stronger from the weaker causal stories.<sup>176</sup> These criteria not only may serve a sorting function among existing stories, but also may guide future research in a more positive direction.

### 1. Transparency

First and foremost is the transparency of the evidence selection, causal inference, and generalization process.<sup>177</sup> The more particularity with which a theory or hypothesis is stated, the more testable it becomes: "A theory is said to be testable to the extent that it can be confirmed if it is correct, and rejected if it is wrong. The more a theory prescribes certain events and prohibits others, the more testable it should be."<sup>178</sup> Implicit in this notion of testability is that an explicit

<sup>176</sup> Excellent starting points in the task of specifying normative criteria for causal thought experiments are offered by Tetlock and Belkin, who extracted from the social science and philosophical literature on counterfactuals a list of six normative criteria for counterfactual arguments. See Tetlock & Belkin, *supra* note 109, at 18 (listing the criteria as "[c]larity," "[l]ogical consistency," "[h]istorical consistency," "[t]heoretical consistency," "[s]tatistical consistency," and "[p]rojectability"). Lebow continues in this direction by building on Tetlock and Belkin's work to propose eight normative criteria. See Lebow, *supra* note 85, at 581-85 (delineating the following criteria: "[c]larity," "[l]ogical consistency," an avoidance of "[e]nabling counterfactuals [that] . . . undercut the antecedent," "[h]istorical consistency," "[t]heoretical consistency," an avoidance of "the conjunction fallacy," a recognition of "the interconnectedness of causes and outcomes," and a consideration of "second-order counterfactuals"). As Tetlock and Belkin note, "the quest for a one-size-fits-all epistemology is quixotic." Tetlock & Belkin, *supra* note 109, at 16. Accordingly, the following discussion both expands on and simplifies in some respects the criteria proposed by Tetlock, Belkin, Lebow, and other theorists in an attempt to provide guidance for evaluating counterfactuals in legal scholarship.

<sup>177</sup> Tetlock and Belkin suggest a similar but less encompassing "clarity" component, which requires the counterfactual experimenter to "[s]pecify and circumscribe the independent and dependent variables (the hypothesized antecedent and consequent)." Tetlock & Belkin, *supra* note 109, at 18. Lebow concurs:

All causal arguments should define as unambiguously as possible what is to be explained (the consequent in counterfactual arguments), what accounts for this outcome (the antecedent), and the principle(s) linking the two. Good counterfactuals should also specify the conditions that would have to be present for the counterfactual to occur.

Lebow, *supra* note 85, at 581.

<sup>178</sup> Brown Grier, *Prediction, Explanation, and Testability as Criteria for Judging Statistical Theories*, 42 PHIL. SCI. 373, 376 (1975); see also David Gooding, *The Procedural Turn; or, Why Do Thought Experiments Work?*, in 15 MINNESOTA STUDIES IN THE PHILOSOPHY OF SCIENCE: COGNITIVE MODELS OF SCIENCE 45, 70 (Ronald N. Giere ed., 1992) ("The demonstrative power of thought experiment depends on creating situations in which

statement of the essential details entailed by a theory should be made so that these details become susceptible to evaluation for their truth-value.<sup>179</sup>

With specific regard to Enron, the transparency norm directs attention to three particular features of counterfactuals that should be clearly specified in order to validate the thought experiment: (1) the precise changes in the law, regulatory environment, or market conditions that supposedly would have averted the disaster; (2) the mechanism linking the antecedent to the consequent (e.g., if increased criminal sanctions or heightened enforcement of criminal laws is proposed as the antecedent, then the theorist should specify that a deterrent effect is the proposed mechanism linking the antecedent to the consequent of greater compliance with the law so that the proposal may be evaluated in light of existing research on deterrence); and (3) the details of the counterfactual world as it is presumed to exist after history has been rewritten to alter the result in Enron, with particular consideration given to how proposed changes in the law or market would likely affect other aspects of this new

---

most moves or manipulations are wholly transparent but in which certain moves are impracticable, just as real-world experiments encounter anomalies and recalitrances.”).

<sup>179</sup> See, e.g., Mario Bunge, *The Weight of Simplicity in the Construction and Assaying of Scientific Theories*, 28 PHIL. SCI. 120, 126 (1961) (“The basic predicates of a scientific theory need not be observable or measurable in a direct way (few of them are). Only, they must be open to public scrutiny by the method of science . . .”). More precisely, we need propositions entailed by the details, which are also entailed by the theory, for it is the truth or falsity of these propositions that is tested. As Alvin Goldman explains:

Beliefs are commonly said to be true or false, and so are assertions. But, strictly, it is not *acts* of assertion, or *states* of belief, that are true or false. It is the *contents* of these acts or states. Since we are taking propositions to be the contents of beliefs and assertions, they have the role of bearers of truth or falsity. What is primarily true or false is a proposition. A belief qualifies as true only derivatively: when its content is a true proposition.

ALVIN I. GOLDMAN, *EPISTEMOLOGY AND COGNITION* 17 (1986).

Jonathan Bennett discusses the difficulties that arise in designating those parts of an event that are “essential” to a counterfactual analysis. See Jonathan Bennett, *Event Causation: The Counterfactual Analysis*, 1 PHIL. PERSPS. 367, 369-73 (1987) (reviewing literature and a related example to illustrate the difficulties in establishing what is essential). Suffice it to say here that the counterfactual analyst should at least specify those features of the counterfactual world that must be altered and those that must not be altered in order for the proposed causal relationship to hold (i.e., to provide the means for others to test the empirical, logical, and intuitive appeal of the counterfactual analysis). Specifying these essential features may be quite difficult, but proves necessary nonetheless.

world.<sup>180</sup> Absent an explicit statement of the causal chain proposed to alter the outcome in the Enron case, the thought experiment should be rejected on grounds that it is not susceptible to replication or serious evaluation.<sup>181</sup>

## 2. Counterfactuality of the Proposed Antecedent

Once the terms of the thought experiment are made explicit and specific, we are no longer confined to facial plausibility or intuitive appeal in judging the validity of the experiment's results. As an initial matter, this transparency leads to a simple check on the validity of the thought experiment in the form of asking whether the proposed *counterfactual* antecedent truly is counterfactual (i.e., does the value assigned to the proposed antecedent (causal) variable in the alternative world really contradict the facts as they existed in the known world?). If not, then this antecedent should not be assigned causal force in our hypothetical world because the value given to the antecedent in the hypothetical world existed in the actual world without causal efficacy. For instance, if Enron's lawyers did advise Enron management not to

---

<sup>180</sup> If the transparency demand is difficult to meet, and it may be very difficult to meet in some cases, then that difficulty should be seen as negative feedback about the viability of one's argument. For example, Professor Widen hypothesizes that a broad move from more complex rules to general principles would challenge individuals and organizations to make difficult decisions based on value judgments and ethics, rather than on mechanical compliance with technicalities. Widen, *supra* note 44, at 1002. To assess the likely causal efficacy of this change, however, we need much more detail about the antecedents and the psychological mechanism(s) that will supposedly trigger this greater caution and law-abidingness. We also need greater detail about the legal changes proposed so that we can evaluate both the feasibility of such changes and the ramifications of these changes in an alternative world.

Similarly, Professor Coffee submits that some "substantive reform of substantive accounting principles" is necessary, and he likewise looks to the government for help on grounds that "this shift cannot come simply through private action." Coffee, *supra* note 4, at 1417. The help sought is not direct legislation, but rather, the legislative creation of "a neutral and independent body to promulgate substantive accounting rules." *Id.* at 1417 n.57. As to the shape of these substantive accounting reforms that could have lessened the likelihood of or averted the Enron debacle, "reasonable persons can disagree as to the best means of improving the quality of the financial standards with which the auditor measures compliance." *Id.* at 1417. So again, few specifics of the proposed reform are offered, and concomitantly, little can be said about the likely effects of these reforms.

<sup>181</sup> In addition, if the results of a thought experiment are generalized to other cases, then the basis for such extrapolation should be revealed, whether that basis is simply an analytical generalization or is an inductive inference based on evidence indicating the similarity of the Enron case to other cases. If the basis is the latter, then the sources of this evidence should also be identified because the revelation will again provide validity and the opportunity for repeat application of the extrapolation.

pursue certain courses of action, and yet management ignored that advice, then this information would bring into question a proposed counterfactual antecedent that assigns causal force to the role of lawyers' intervention in the prevention of Enron's demise.<sup>182</sup> This check on the counterfactuality of the antecedent encourages care in the gathering of facts to construct one's causal story. Furthermore, it illustrates the importance of precision in specifying the antecedent, for it may be that advocates of the causal role of lawyers have in mind more active participation than that supposedly exhibited by Enron's lawyers or a different type of advice than that offered by Enron's lawyers. Assuming such intended differences, these advocates' causal theory may retain some viability if they note the antecedent with proper specificity.<sup>183</sup>

### 3. Consideration of Competing Hypotheses

One of the primary problems with single-observation case studies is their weak ability to separate true causal relations from spurious relations (the methodological underdetermination problem).<sup>184</sup> We gain confidence in a particular causal explanation to the extent that alternative competing explanations for an event have been obviously eliminated. Conversely, if a theorist ignores the role of serious competing hypotheses and focuses only on confirming her pet theory, then we should place little confidence in the results of this theorist's experiment. Thus, good experiments anticipate variables that may confound experimental results and either control these variables or

---

<sup>182</sup> See *supra* notes 49-51 and accompanying text (relaying Professor Wade's opinion that the legal advice sought in Enron matters little as managers failed to follow it anyway).

<sup>183</sup> Thus, if Professor Wade is right about the facts and Enron's lawyers did counsel against key actions, then Dean Rapoport's counterfactual world in which lawyers of greater character made a difference remains a possibility. But the particular kind of intervention envisioned needs to be specified and would need to go beyond a simple recommendation for attorneys to provide more active intervention. Dean Rapoport suggests that she has in mind a more persistent course of action than attorneys just advising against a course of action, but the level of additional intervention or persistence by the lawyers that might have altered the outcome is not clear. See *supra* note 21 (addressing Rapoport's call for greater character among lawyers).

<sup>184</sup> See *supra* notes 127-30 and accompanying text (exploring the methodological underdetermination problem).

test their causal influence as compared to the influence of the theoretical variable of interest.<sup>185</sup>

Likewise, the stronger thought experiments and resulting causal stories explicitly consider competing causal hypotheses and either explain why one hypothesis is more credible than another or explain why multiple hypotheses remain credible.<sup>186</sup> Just as our faith in the validity of a causal story increases as competing causal hypotheses are eliminated, our faith in the validity of a causal story should decrease to the extent that competing causal hypotheses are ignored. Particularly problematic for many of the Enron stories in this respect is Professor Partnoy's "revisionist" story, in which he contends that many of the Enron stories pay too little attention to the role of derivatives in the Enron chronology.<sup>187</sup> If Professor Partnoy is correct, then many of the existing stories omit an important causal factor from their explanations and perhaps attribute causation to spurious causal factors.

#### 4. Theoretical and Statistical Reasonableness of the Proposed Causal Chain

If the purpose of a counterfactual thought experiment is to find the best causal explanation of an event, not simply a *plausible* causal explanation, then we need some way to evaluate the probability that a particular antecedent could lead to the consequent in a counterfactual world.<sup>188</sup> One solution is to evaluate the probabilistic relation

<sup>185</sup> James Hampton explains the problem of confounding variables:

We want to arrange that the . . . conditions are identical in all respects except for the critical factor that we are testing—the value of the independent variable. Where some other factor also differs systematically between the conditions, then we call this a *confounding variable* or factor, and it undermines our ability to interpret the results of the experiment. In effect we cannot tell which of the two variables was responsible for any observed difference in the dependent variable.

James Hampton, *The Between-Subjects Experiment*, in *LABORATORY PSYCHOLOGY: A BEGINNER'S GUIDE* 15, 26 (Julia Nunn ed., 1998).

<sup>186</sup> Professors Bratton and Coffee, for instance, consider competing explanations for Enron, although they ultimately conclude that a multitude of factors contributed to the outcome. See *supra* notes 25-30 and accompanying text (discussing Bratton's consideration of multiple causal hypotheses); *supra* notes 39-43 and accompanying text (discussing Coffee's consideration of legal versus market explanations for Enron's fall).

<sup>187</sup> See *supra* note 158 and accompanying text (discussing Partnoy's emphasis on the causal centrality of derivatives).

<sup>188</sup> That is, we must find some principled way to overcome the Cleopatra's Nose Problem, *supra* note 103, the dilemma of so many plausible causes that each has a small or indeterminable probability of being the true cause in any particular setting.

between the proposed antecedent and the consequent in other, real-world cases and then import this probability into our evaluation of the thought experiment to the extent that it is theoretically reasonable to do so. On this view, "the expectation of 'what would have happened' [in our counterfactual world] must be a 'reasonable' one based on a supportable statistical argument."<sup>189</sup>

While Robyn Dawes, the proponent of this view, contends that "counterfactual inferences are normatively justified if and only if they are embedded as instances in generally valid statistical relationships,"<sup>190</sup> one need not commit to the strong form of this argument in order to see its value as an evaluative tool.<sup>191</sup> The more theoretically or statistically justifiable the propositions in a thought experiment, the more defensible the conclusions drawn from the experiment. Thus, if legal scholars state their counterfactual propositions in terms that allow us to assess the fit of these propositions with known event probabilities or behavioral regularities, then we are given some means beyond facial plausibility to assess the retrospective counterfactual. If afforded these means in the Enron case, we could assess the justifiability of the scholars' backward induction from evidence in other real cases.<sup>192</sup>

---

<sup>189</sup> Robyn M. Dawes, *Counterfactual Inferences as Instances of Statistical Inferences*, in COUNTERFACTUAL THOUGHT EXPERIMENTS IN WORLD POLITICS, *supra* note 70, at 301, 305.

<sup>190</sup> *Id.* at 306; *see also id.* at 307 ("[C]ounterfactual inferences are normatively justified only when they are embedded in a broader experimental or statistical context and justified in terms of *expectation*, which may be applied to an individual instance.").

<sup>191</sup> Tetlock and Belkin endorse a weaker version:

One need not, of course, accept the radical epistemological argument of Dawes to agree with the more moderate mainstream view that canons of sound statistical reasoning should constrain our judgments of counterfactuals and, indeed, that we should be alert to the psychological fact that people are flawed intuitive statisticians who fall prey to various biases in detecting and using covariation data.

Tetlock & Belkin, *supra* note 109, at 29 (citation omitted).

<sup>192</sup> This argument is similar to that requiring the scholar to make explicit any covering laws (or covering theories, in Professor Fritz Rohrlich's modification) or foundational assumptions from which counterfactual propositions have been deduced or generated. Consider Professor Rohrlich's comments made in the context of scientific claims generally, rather than of counterfactual claims specifically:

In its barest outlines, science seeks to identify the furniture of the world, its properties, and the way it functions. Correspondingly, scientific explanation must first identify this furniture of the world, i.e. it must first specify the perception within which the explanation is to take place: its cognitive level and its ontology. (Some philosophers speak of context dependence.) It requires the choice of a scientific theory and a suitable model within that theory. Only then can the explanation proceed to deduce from first principles the way

This criterion serves a constructive function as well because it forces the theorist to consider whether a causal relation would likely appear under the circumstances of the Enron case in light of existing empirical evidence. A positive example in this regard is Professor Langevoort's behavioral explanation for Enron, which draws on organizational and psychological research about the interaction of personality, cognition, and organizational culture to argue that the Enron culture fostered a "Machiavellian" type of senior management that was prone to extremely risky and unwise courses of action under certain high-stress conditions.<sup>193</sup> A byproduct of this attention to existing social scientific evidence and the conditions under which particular behavioral phenomena may occur is that Professor Langevoort limits the reach of his causal explanation for Enron to other highly competitive firms in similar markets.<sup>194</sup> Thus, greater care about the internal validity of an argument may lead to greater care in the generalization of an argument.

##### 5. Cotenability and Counterfactual Minimalism

Professor Dawes's argument for statistical justification of counterfactual propositions suggests another criterion that may be used to evaluate retrospective counterfactuals: the cotenability of the counterfactual antecedent with other features likely to exist in the imagined world and with features already existing in the real world.<sup>195</sup> The cotenability criterion requires assessing whether the features of the

---

things are and the way things function on that level. *And since every scientific theory has its validity limits and every model its idealizations, these validity limits and idealizations must be indicated in order for the questioner to decide on its credibility.*

Rohrlich, *supra* note 10, at 76 (emphasis added). Thus, if the legal scholar relies on an assumption of perfect rationality to supply contrary-to-fact propositions or to fill in any gaps in the counterfactual world, the role of this assumption should be transparent so that it and its application can be subjected to scrutiny. *Cf.* Tetlock & Belkin, *supra* note 109, at 27 ("To prevent competing schools of thought from simply inventing counterfactuals of convenience, we need reality constraints. Counterfactuals must not only fit existing historical and statistical data[,] . . . they must stimulate testable predictions that hold up reasonably well against new data . . .").

<sup>193</sup> See Langevoort, *supra* note 4, at 968-73 (providing background for this theory); *id.* at 973-75 (speculating about this theory's applicability to Enron).

<sup>194</sup> *Id.* at 968; see also *supra* note 56 (describing Professor Langevoort's delimitation of the situations for which Enron can be generalized).

<sup>195</sup> See Fearon, *supra* note 78, at 193 (building on the notion of cotenability as developed by Nelson Goodman).

alternative world are logically and historically consistent.<sup>196</sup> Accordingly, the cotenability criterion directs attention (1) to whether the scholar has changed only those parts of the alternative world that suit the scholar's needs without considering whether these changes require additional changes in the alternative world<sup>197</sup> and (2) to whether the scholar has proposed illogical or historically impossible changes for the alternative world. If the counterfactual antecedent requires an impossible alteration in history (e.g., the possibility of time travel or the establishment of a communist system of government in America during the recent past), then the counterfactual should be absolutely rejected on lack-of-cotenability grounds.<sup>198</sup>

The cotenability criterion suggests the related criterion of counterfactual minimalism: "The fewer the changes from the actual world required by a counterfactual supposition, the easier it will be to draw

---

<sup>196</sup> Lebow breaks logical consistency and historical consistency into separate normative criteria. He argues for cotenability or logical consistency on these grounds: "Every counterfactual is a shorthand statement of a more complex argument that generally requires a set of connecting conditions or principles. The hypothetical antecedent should not undercut any of the principles linking it to the consequent." Lebow, *supra* note 85, at 582. He argues for historical consistency on these grounds: "A minimal rewrite that makes only one alteration in reality may not qualify as a plausible-world counterfactual if the counterfactual is unrealistic or if numerous subsequent counterfactual steps are necessary to reach the hypothesized consequent." *Id.* at 583.

I call Professor Lebow's historical consistency rule the "counterfactual minimalism" rule, which states that the alternative world must resemble the real world as much as possible. Given the similar focus of these criteria—a comparison of the counterfactual to the factual world—and given that the cotenability criterion constrains what minimal rewrites are credible, I combine the cotenability and counterfactual minimalism criteria here. Tetlock and Belkin label what I call "counterfactual minimalism" the "minimal-rewrite-of-history" rule. Tetlock & Belkin, *supra* note 109, at 23; *see also id.* at 23-25 (attributing the rule to previous scholars and describing its application).

<sup>197</sup> Professor Fearon writes:

It is not appropriate to criticize a counterfactual argument by saying that the antecedent could not have occurred. Rather, we need an explicit argument saying that if the antecedent had been the case, other changes would be required in the counterfactual scenario that would have affected the outcome in a different way.

Fearon, *supra* note 78, at 193.

<sup>198</sup> In other words, "miracle counterfactuals" should not be allowed. Lebow endorses the use of such miracle counterfactuals for some purposes, particularly to evaluate settled theories. *See* Lebow, *supra* note 85, at 566 ("The value of miracle counterfactuals derives not from their realism but from the analytical utility of considering alternative worlds."). However, where the goal of the counterfactual is to identify possibly true conditions under which Enron would not have failed, only "plausible world counterfactuals" should be accepted.

and support causal inferences, and the more defensible they will be."<sup>199</sup> The closer the counterfactual world is to the real world, the greater the chance that the two worlds will be cotenable and the lesser the chance that the theorist will overlook some consequence that follows from a minimalist rewrite of history. The closer the two worlds, the fewer the mental simulations required by the thought experiment and, thus, the more psychologically manageable, transparent, and replicable the experiment. The minimalism rule also provides an additional solution to the Cleopatra's Nose Problem:<sup>200</sup> all other things being equal, always choose a causal chain that requires less counterfactual changes over a causal chain that requires more counterfactual changes.

The cotenability and counterfactual minimalism criteria give rise to serious concerns about the use of counterfactual reasoning in a case such as Enron, which arose in the context of a complex political, economic, and legal system.<sup>201</sup> Compared to a relatively simple counterfactual claim such as, "I would not have been the winning bidder had I not raised my hand near the end of the auction," which entails few historical alterations to imagine (indeed, it may entail only the

<sup>199</sup> Fearon, *supra* note 78, at 193-94. Geoffrey Hawthorn expresses a similar view:

All possibilities for a world . . . whether they are suggested by our explanations or by contrasts and comparisons with what we want to explain, should . . . start from a world as it otherwise was. They should not require us to unwind the past. And the consequences we draw from these alternatives should initially fit with the other undisturbed runnings-on in that world.

GEOFFREY HAWTHORN, *PLAUSIBLE WORLDS: POSSIBILITY AND UNDERSTANDING IN HISTORY AND THE SOCIAL SCIENCES* 158 (1991).

The philosopher David Lewis's similarity standard for counterfactuals is also relevant here:

On [Lewis's] account, a counterfactual is true just in case the consequent is true at worlds closest (most similar) to the actual world . . . [T]he crucial thing is to keep the past, prior to the time of the occurrence of the actual (cause) event which is counterfactually absent, fixed (while minimizing violation of the relevant laws).

Wilson, *supra* note 56 (manuscript at 22) (citing David Lewis, *Counterfactual Dependence and Time's Arrow*, 13 *NOÛS* 455 (1979)).

<sup>200</sup> *Supra* note 103 and accompanying text.

<sup>201</sup> Under Robert Jervis's definition, a system exists

when elements or units are interconnected so that the system has emergent properties—i.e., its characteristics and behavior cannot be inferred from the characteristics and behavior of the units taken individually—and when changes in one unit or the relationship between any two of them produce ramifying alterations in other units or relationships.

Robert Jervis, *Counterfactuals, Causation, and Complexity*, in *COUNTERFACTUAL THOUGHT EXPERIMENTS IN WORLD POLITICS*, *supra* note 70, at 309, 309.

one plausible change of raising the hand at an earlier time in the auction),<sup>202</sup> proposed changes that undo Enron may be much more extensive in their historical effects or in the other simultaneous changes necessary to enable the new hypothetical world in which Enron's collapse is averted. As the complexity of the system encompassing the target event increases, one's ability to imagine all of the changes necessary to undo the event, and then to imagine all of the ramifications of these changes, decreases greatly.<sup>203</sup>

Consider counterfactual arguments about Enron along the cotenability/minimalism continuum to see the problems that arise when theorists ascribe causation to any forces that also exist beyond the Enron setting. Near the strong cotenability end of the continuum, we find arguments that require simple changes in personnel such as, "if David Duncan had not been the lead Arthur Andersen accountant for

<sup>202</sup> This example is adapted from Bennett, *supra* note 179, at 369. Even for this simple counterfactual antecedent to have causal force and to avoid cotenability problems, we must assume at least one other bidder who will bid a price acceptable to the seller after my earlier bid, and we must assume that my change in behavior is possible or makes sense in the context of the situation (i.e., my later bid was not compelled by other situational features).

<sup>203</sup> As Jarvis remarks:

[C]ounterfactuals are both useful and tricky when we deal with a system. They can help us think through the connections we believe to be at work, but cannot be employed to help us imagine a world that is like our own in all ways except for one. A change will inevitably have many effects; often the change itself is only possible if other factors change as well . . . . The use of counterfactuals to test propositions or guide action can be designed to help us trace consequences, but the complex interconnections involved are likely to make the exercise a difficult one.

Jarvis, *supra* note 201, at 316.

Professor Susan Bandes noted, for instance, the tremendous difficulty that she encountered in mentally undoing the events associated with the Supreme Court's decision in *Terry v. Ohio*, 392 U.S. 1 (1968):

It's a tough call whether to obliterate *Terry*. The question can't be whether *Terry* was correct when decided, because there is no way, from our current vantage point, to ignore more than thirty years of evidence about how it has worked in practice. The rules of this game don't deprive us of our historical knowledge . . . so the question must be: in light of what we know now, would we have been better off without the *Terry* decision? The question is complicated by the fact that during the time we were accruing evidence about the effects of stop and frisk, we were also gaining a less linear, more sophisticated understanding of the laws of cause and effect.

Susan Bandes, *Terry v. Ohio in Hindsight: The Perils of Predicting the Past*, 16 CONST. COMMENT. 491, 491-92 (1999); see also Grundfest, *supra* note 66, at 3 ("In a system as complex as the securities markets, it is highly improbable that any single cause, such as deregulation, can carry the weight of [Bill] Lerach's argument [about the cause of Enron].").

Enron, then Enron's financial fraud would have been averted or lessened."<sup>204</sup> Near the weak cotenability end, we find arguments that require systemic changes such as, "if we had a standards-based set of legal norms, rather than a rules-based set of norms, then Enron would not have occurred."<sup>205</sup> While each of these arguments may have some causal plausibility, the latter argument is surely less compelling given its weak cotenability (i.e., given the many other changes in the world needed to enable a wholesale switch to a standards-based system and given the many hard-to-anticipate effects that may arise from these first-order changes).

Even counterfactuals that involve changes somewhere in the middle of the cotenability/minimalism spectrum present difficulties in the complex causal environment surrounding the Enron scenario. For instance, Professor Cohen's suggestion that changes in Supreme Court case law on deceptive accounting might have altered the result in Enron<sup>206</sup> or Professor Wade's suggestion that modifications to officer and director fiduciary duties might have done likewise<sup>207</sup> raises the question of what chain of other events these historical changes would have set in motion. Most important is the second-order counterfactual problem: in light of the lobbying power of accounting firms and large public companies, would these first-order changes have led to second-order changes that would have effectively undone the first-order reforms?<sup>208</sup> That is, what is the potential for a work-around of these more moderate reforms to legal and economic history by determined actors who favored the previous legal and economic environment?

The cotenability and minimalism criteria ultimately present a paradox for the legal scholar seeking to draw broad causal lessons from Enron using counterfactual analysis. The stronger the cotenability of a counterfactual antecedent (because the historical mutations required are less drastic), the weaker the counterfactual argument for

<sup>204</sup> Professor Bratton, for instance, proposes an argument that places blame on the Arthur Andersen accountants. See *supra* note 90 (reviewing Bratton's blame assessments).

<sup>205</sup> Professor Widen, for instance, submits this very argument. See *supra* text accompanying notes 44-45 (reviewing Widen's causal story).

<sup>206</sup> See *supra* note 138 and accompanying text (describing Cohen's speculation that the Enron scandal might have been prevented by Supreme Court criticism of the Federal Reserve).

<sup>207</sup> See *supra* notes 49-51 and accompanying text (describing Wade's argument that weak corporate governance rules caused the Enron scandal).

<sup>208</sup> See *supra* note 139 (discussing the problem of second-order counterfactuals).

legal reform (because the need for wholesale reform concomitantly lessens).<sup>209</sup> Legal scholars favoring reforms to the systems of corporate governance and securities regulation are likely to reject personnel changes as sufficient counterfactual antecedents (i.e., these scholars are probably unwilling to explain Enron in terms of the unique personalities involved). Yet more extensive counterfactual antecedents such as industry-wide accounting changes or market changes may not be cotenable with other immutable historical facts. Indeed, at some point the alternative world imagined will differ so much that Enron itself will cease to exist. Although positing substantial changes might be one sure way to avoid Enron's failure in a counterfactual world, it is not a plausible route to explaining why the Enron that did exist in our world failed.<sup>210</sup>

## 6. Projectibility

Finally, a theory drawn from a retrospective counterfactual should generate valid predictions for other cases. This requirement is drawn from Nelson Goodman's "theory of projection"<sup>211</sup> and can be seen as

<sup>209</sup> Cf. Fearon, *supra* note 78, at 193 ("I expect that in practice, the cotenability requirement will be more plausibly satisfied for small causes, such as specific policy decisions, than for big causes, such as nationalism, imperialism, or a cult of the offensive.").

<sup>210</sup> Actually, if one believes that Enron is the unavoidable product of a capitalist market system regardless of the extent to which private and governmental monitoring of corporations occurs within this system, then one might employ a counterfactual in which the U.S. switches to a government-controlled economic system to imagine whether this change would undo Enron. Of course, such a counterfactual lacks cotenability and falls into the category of "miracle counterfactuals." See *supra* note 198 (contending that miracle counterfactuals should not be accepted if the goal is to identify true conditions that would have prevented Enron's failure).

<sup>211</sup> See NELSON GOODMAN, FACT, FICTION, AND FORECAST 87–120 (1955) (presenting his theory of projection); *id.* at 57 ("[T]he problem of making the projection from manifest to non-manifest cases is . . . not very different from the problem of going from known to unknown or from past to future cases. The problem of dispositions looks suspiciously like one of the philosopher's oldest friends and enemies: the problem of induction."); *id.* at 90 ("Actual projection involves the overt, explicit formulation and adoption of the hypothesis—the actual prediction of the outcome of the examination of further cases."); see also Bütche, *supra* note 11, at 489 ("[T]he ability of a model to withstand the difficult test of application to different occurrences of the *explanandum* without ad hoc alterations makes more plausible that it has captured the central, generalizable dynamics rather than unique elements of a particular case."); Tetlock & Belkin, *supra* note 109, at 30 (noting that, from Goodman's perspective, "whether the generalization is bounded or unbounded by moderator variables and whether the generalization is deterministic or probabilistic, it is subject to the same acid test of scientific legitimacy: namely, its projectability or its ability to predict what will happen in new, hitherto unobserved cases").

the complement of the cotenability requirement, for a counterfactual world that is more cotenable with the actual world should lead to more valid predictions in the real world and other plausible counterfactual worlds. The projectibility requirement serves two useful purposes: (1) it forces the researcher to expose her theory to additional tests of internal validity, and (2) it helps to identify the boundary conditions on, or external validity limits of, the theory. Even more fundamentally, if the researcher cannot state her theory in such a way as to make it testable in other settings, either using other counterfactual thought experiments, real experiments, or comparative or quantitative analyses of other real cases, then the theory should be rejected as impermissibly vague. Thus, the projectibility requirement brings us full circle to the initial requirement that a legal scholar state her theory in as explicit and testable a form as possible.

### 7. Summary of Potential Normative Criteria for Causal Stories

Overall, then, in addition to facial plausibility, the quality of an argument dependent on a counterfactual thought experiment should be assessed along at least six dimensions: (1) the transparency with which the evidence-selection, causal inference, and generalization processes are described; (2) the “counterfactuality” of the proposed causal antecedent; (3) the degree to which the favored causal hypothesis has survived confrontation with competing hypotheses; (4) the theoretical and statistical reasonableness of the counterfactual propositions in light of known event probabilities and behavioral evidence; (5) the cotenability and counterfactual minimalism of the propositions in the thought experiment, with particular regard to the complexity of the system in which the counterfactual mutations occur; and (6) the projectibility of the thought experiment’s results.

Considering the Enron stories along these dimensions reveals important ways that scholars could improve their analyses. Perhaps most troubling is the lack of transparency and specificity in many of the causal stories, both because this requirement is relatively easy to satisfy and because failure to satisfy this requirement makes it difficult to engage in any serious evaluation of the story’s merit along the other dimensions. In counterfactual analysis, much of the necessary information is private and not capable of independent discovery by the reader, such as the precise rewrites of history undertaken in the thought experiment and the evidence-selection procedures employed in gathering information for the factual component in the causal story. For example, were some news accounts of Enron rejected and,

if so, why? Was any effort made to gather facts from diverse sources? What fact-checking occurred, if any? Moreover, it is the obligation of the scholar, not the reader, to find and produce evidence to support the statistical reasonableness of a proposed causal antecedent and any generalizations from Enron to other cases. The very acts of writing down the details behind the creation of the causal story and disciplining oneself to make public one's evidence in support of inferences contained in the causal story are likely to lead to improvements in the story.

Perhaps most problematic for scholars who use counterfactual reasoning from Enron to support their arguments for legal or market reform is the weakness of these arguments when measured on the cotenability/counterfactual minimalism dimension. Any Enron story used to justify a systemic reform, whether it be one as extensive as the broad move from legal rules to standards favored by Professor Widen<sup>212</sup> or a more meager reform such as the requirement that accounting firms not be allowed to provide both auditing and consulting services to a client favored by Professor Bratton,<sup>213</sup> suffers on this dimension. This weakness proves so pervasive because any such reform in a system as complex as the one in which Enron was embedded is likely to have so many unanticipated and unintended effects that it becomes difficult to follow the causal chain from the reform to the prevention of Enron and other business failures.

### CONCLUSION

The use of the single-observation case study to develop causal explanations for single events and for classes of events poses several challenges to the researcher. First, because this method does not allow conventional empirical tests of causal hypotheses using experimental, quantitative, or qualitative data, the researcher must construct a counterfactual thought experiment to test her causal hypotheses. Second, the researcher who uses such counterfactual thought experiments must be vigilant so as to avoid the biases and fallacies that may intrude on mental simulations. Third, even the best counterfactual thought experiments will likely prove useful only in the testing of simple,

---

<sup>212</sup> See *supra* notes 44-45 and accompanying text (presenting Professor Widen's notion that rules governing conduct and disclosure are better formulated as general principles, rather than as technical rules, to avoid obviating the sense of right and wrong); *supra* note 180 (same).

<sup>213</sup> See *supra* note 32 (conveying Professor Bratton's argument for the prohibition of coexistent auditing and consulting functions).

rather than complex, causal theories. Fourth, assuming that the thought experiment leads the researcher to a credible result, this result can be used only for analytical generalization and not as a basis for empirical generalization. Therefore, additional empirical research will always be necessary to determine whether the explanation generated for the single case has application to other cases.

Single-observation case studies pose challenges to the consumer of such research as well. The most pressing problem for readers of causal stories drawn from these case studies is that many of these stories, on the surface, appear compelling—even when the stories reach opposing conclusions. When Professor Coffee explains why it is that Enron's external gatekeepers, not Enron's board, failed, the reader comes away agreeing that Enron really is "about the gatekeepers, stupid[!]"<sup>214</sup> and that better regulation of the gatekeepers is in order. Yet, on the contrary, when Professor Ribstein explains that the market has not been allowed to do its work and that *less* regulation is imperative, the reader comes away inspired by the power of markets. Pick up Professor Partnoy's story about the underregulated derivatives market, however, and one's enthusiasm for the market quickly fades. The same sense of enlightenment follows the reading of Professor Bratton's story about the role of corporate culture and accounting conflicts, the reading of Professor Langevoort's story about the interaction of personality and organizational pressures in highly competitive firms, and the reading of many other Enron stories—despite their different causal conclusions.

This causal indeterminacy results from the lack of stringent, normative standards for judging the validity of such stories. Unlike traditional empirical research, in which the reports of experimental and quantitative research follow established normative standards for judging validity (such as the transparent and detailed reporting of information about experimental design, rules for the sampling and selection of data, and the use of statistical tests to separate random from nonrandom effects), there is no clear set of normative standards for judging the validity of counterfactual thought experiments. This lack of guidance leaves the consumer with only facial plausibility or intuitive appeal to judge the products of thought experiments. Yet, as a reading of the Enron stories will quickly reveal, facial plausibility amounts to only the weakest measure of validity.

---

<sup>214</sup> See *supra* note 87 (quoting Professor Coffee's conclusion).

To help overcome this causal indeterminacy problem, this Article proposes a set of normative criteria that may be used to separate the better counterfactual arguments about causation from the weaker arguments. The primary goal in proposing such normative criteria is not to argue that these particular criteria are the best measures of validity, but rather, to motivate consideration of what constitutes a good thought experiment for causal analysis purposes. Consideration of this issue seems particularly important because, if the normative standards advanced in this Article are appropriate measures of a good thought experiment, then the causal stories being told about Enron suffer from serious defects.

It is important to recognize that this Article's criticisms directed at single-observation case studies and the telling of causal stories treat this body of scholarship as serious empirical legal research that should be scrutinized for its ability to lead to the growth of knowledge about causal relations in the world. If we are unwilling to evaluate causal stories for their analytical rigor and truth-value, then either we are implicitly dismissing this research as meaningless or we are implicitly endorsing a view that the tendentious use of "empirical" legal scholarship is inevitable or acceptable. In the latter case, causal stories in legal scholarship become synonymous with what Professor Deborah Stone calls causal stories for political-agenda-setting purposes:

In politics, causal theories are neither right nor wrong, nor are they mutually exclusive. They are ideas about causation, and policy politics involves strategically portraying issues so that they fit one causal idea or another. The different sides in an issue act as if they are trying to find the "true" cause, but they are always struggling to influence which idea is selected to guide policy. Political conflicts over causal stories are, therefore, more than empirical claims about sequences of events. They are fights about the possibility of control and the assignment of responsibility.<sup>215</sup>

From this perspective, legal scholars interested in regulatory reform must package Enron's collapse in a way that makes it amenable

---

<sup>215</sup> Stone, *supra* note 68, at 283. Stone goes on to state that "the competition over causal theories in problem definition is bounded not only by the usual political conditions that constrain agenda setting, *but also by law and science*, two social institutions that are each in their own fashion charged with arbitrating disputes about causal theories." *Id.* at 299-300 (emphasis added). What Stone has in mind with respect to the law is the use of litigation to parse causal claims, rather than legal scholars' seeking to influence public policy. See *id.* at 294-95 (discussing how "the rules of the game in law are crucial determinants of the political success of causal theories, even theories with the stamp of approval of science").

to, and serious enough for, governmental action. This task can best be accomplished by associating Enron with the causal ideas of market failure and deregulation run amuck. Opponents of governmental action then put forth their competing causal stories of overregulation or isolated occurrences of fraud to counteract the calls for legal or market reform.<sup>216</sup> There is nothing troublesome about using Enron stories in this way if one sees history as consisting of multiple, equally valid stories, or if one sees value-driven empirical analyses as completely inevitable or as legitimate means to an end.

The tendentious use of causal stories is troublesome, however, if one believes that there are better and worse ways of learning about causal relations in the world and that false causal stories may be misleading to lawmakers.<sup>217</sup> This critical view does not require that empirical legal researchers achieve perfection in their research methods. It simply requires that they strive for the greatest rigor possible in light of the constraints on their resources and methodological training *and*

<sup>216</sup> See, e.g., Edward D. Herlihy et al., *Improving the Clarity of Disclosures in the Post-Enron Environment*, CORP. BOARD MEMBER MAG. (Sept. 2002) (“We should all be mindful of the fact [that] a few bad practices by [isolated] companies does not mean that the current reporting system is fundamentally flawed or that investors have been deprived of meaningful information in many instances.”), at [http://www.boardmember.com/network/index.pl?section=1097&article\\_id=11112&show=article](http://www.boardmember.com/network/index.pl?section=1097&article_id=11112&show=article); cf. Burton G. Malkiel, *The Market Can Police Itself*, WALL ST. J., June 28, 2002, at A12 (“But in the final analysis, the immediate punishment doled out by the market itself will be the most powerful deterrent to future corporate wrongdoing. And eventually the stock market will react to the increase in economic activity instead of the growth in corporate scandals.”). Former Secretary of the Treasury Paul O’Neill’s statement that Enron’s failure evidenced “the genius of capitalism” is probably the most prominent example of an opposing view being offered by pro-market advocates. *Enron’s Fall Shows ‘Genius of Capitalism,’* BBC NEWS (Jan. 14, 2002) (quoting Secretary O’Neill), at <http://news.bbc.co.uk/1/hi/business/1759033.stm>.

<sup>217</sup> Consider these comments by Professor Beaver:

At this stage, there has been a great deal of rhetoric and outrage but relatively little analysis. There has been pressure for rapid responses in the absence of fully understanding the causes of the problems and how they are linked to structural defects in the financial reporting-corporate governance environment. Without these links, it is possible that, in spite of an increase in legislation and regulation, the same problems will reappear.

Beaver, *supra* note 2, at 168; see also Steven L. Schwarcz, *Enron and the Use and Abuse of Special Purpose Entities in Corporate Structures*, 70 U. CIN. L. REV. 1309, 1318 (2002) (“Ultimately, the greatest danger of the Enron debacle is our possible overreaction, and consequent over-regulation.”); Strine, *supra* note 4, at 1401 (“Enron and corporations like it are dangerous not only for the obvious harm they cause to their own constituencies and confidence in the capital markets in general, but also because they generate the potential for overreaction by policymakers, to the overall detriment of our economic well-being.”).

that they not overstate the completeness, accuracy, or reach of their empirical claims.<sup>218</sup>

Perhaps the most important step toward achieving greater rigor and appropriate humility in empirical legal scholarship is to reconsider the emphasis placed on prescription within legal scholarship in general. If legal scholarship's "basic nature is to structure its statements as recommendations to [legal] decision-makers,"<sup>219</sup> then the Enron scholars were simply following the norm to discuss the importance and possible legal ramifications of their analyses of the Enron matter. Operating under such a norm, the best route to publication is not to express the epistemic and prescriptive humility that should accompany causal stories, but rather, to portray Enron as the very "story of human behavior itself."<sup>220</sup>

In keeping with legal scholarship's demand for bold claims, this Article ends with a prospective counterfactual of its own: an attitude that places timeliness and stated importance over accuracy and humility will continue to dominate empirical legal scholarship indefinitely,

---

<sup>218</sup> I do not mean to suggest that none of the Enron scholars expressed reservations about their stories. Some did qualify parts of their stories and articulated caution about their causal conclusions. For instance, Professor Bratton notes that evidence of events at Enron was incomplete at the time of his article's publication. See, e.g., Bratton, *supra* note 4, at 1302 (qualifying his claim as based "on the present state of the record"); *id.* at 1360 n.302 (qualifying his claim as based on "present public knowledge"). And Dean Rapoport forthrightly indicated the likelihood of multiple causes of Enron before selecting lack of character as a key cause. See *supra* text accompanying note 20 (quoting Rapoport's statement expressing doubt that Enron can be traced to a "single root cause"). Others expressed reservations as well. See, e.g., Coffee, *supra* note 4, at 1408 n.28 (observing that "the empirical evidence is limited" regarding the quality of recommendations by independent analysts as opposed to the recommendations by analysts associated with an issuer's underwriters).

<sup>219</sup> Edward L. Rubin, *The Practice and Discourse of Legal Scholarship*, 86 MICH. L. REV. 1835, 1851 (1988); see also Edward L. Rubin, *Passing Through the Door: Social Movement Literature and Legal Scholarship*, 150 U. PA. L. REV. 1, 2-3 (2001) ("In the case of legal scholarship, the reasons [for the emphasis that it adopts] are its essentially prescriptive stance and, more importantly, its unity of discourse with the judiciary, which creates a mentality that tends to assimilate the style of legal analysis to arguments before a court."); *supra* note 10 and accompanying text (describing legal scholarship's purpose and methodology as providing courts with prescriptions for legal issues).

<sup>220</sup> See *supra* text accompanying note 62 (quoting Professor Flatt's memorable line); see also *supra* note 65 (pinpointing the error of generalizing from specific behavior apparent in Professor Widen's work); cf. Suzanna Sherry, *Too Clever by Half: The Problem with Novelty in Constitutional Law*, 95 NW. U. L. REV. 921, 930 (2001) ("In short, the quickest route to publication—and thence to fame and fortune—for legal academics is to be dazzlingly clever and propose some completely novel thesis."). For a contrary view, positing that mainstream legal scholarship is (or at least previously was) "dreadfully responsible in tone" (if not in its substance and effects on the legal system), see Pierre Schlag, *The Brilliant, the Curious, and the Wrong*, 39 STAN. L. REV. 917, 927 (1987).

given the incentive structure within the discipline. The present material benefits of performing rigorous empirical research appear to be no greater than performing less rigorous research, while the cost of doing rigorous empirical research—both in terms of monetary expense and in terms of the amount of time necessary to bring an article to publication—may be high. Yet the professional benefits of doing less rigorous empirical work, such as causal storytelling, may be substantial—in the form of good, quick journal placements and a possible influence on courts, agencies, and legislators—while the professional cost of telling such stories is likely to remain low.<sup>221</sup> As someone

---

<sup>221</sup> Professor David Bryden offered a similar assessment of empirical legal research more than a decade ago:

Now of course empirical research has its limitations, and they are much more severe than some enthusiasts acknowledge. But those limitations, it seems to me, are not the main reason why most of us shy away from it. The main reason, I believe, is an accurate calculation of self-interest. As one who used to do empirical research, let me explain some of the problems. I found that it usually took me at least a year of hard work to come up with a research plan and obtain the requisite financial aid. One must spend considerable time dickering with foundation officials about details (“you should study five states, not just two”) over which conventional legal scholars have absolute control. It often takes another year or two to do the research and write an article analyzing the results.

... [F]ew law professors know enough about empirical research methods or social and behavioral science theory to conduct methodologically complex projects. So even if enough money were available, and time were not a problem, most of us would hesitate to venture into this alien territory, no matter how many critics of legal scholarship urge us to do so, unless we perceived unique rewards at the top of the empirical mountain.

David P. Bryden, *Scholarship About Scholarship*, 63 U. COLO. L. REV. 641, 645-46 (1992); see also William M. Landes, *The Empirical Side of Law & Economics*, 70 U. CHI. L. REV. 167, 168 (2003) (“In the academic marketplace, scholars select research projects based on a comparison of returns and costs. It follows, therefore, that law and economics scholars are more likely to choose theoretical projects because they hold out the prospect of greater rewards and lower costs than empirical projects.”). I suggest that the incentive structure remains largely as it was when Bryden wrote, and I predict that it will likely remain the same for a good time to come. However, there is some potential counterevidence on the horizon in the christening of the new *Journal of Empirical Legal Studies* by Cornell Law School. Blackwell Publ'g, *New Journal from Cornell Law School!: Journal of Empirical Legal Studies*, at <http://www.blackwellpublishing.com/journal.asp?ref=1740-1453> (last visited Mar. 18, 2004). The emergence of empirical research programs at a few law schools supplies further counterevidence. See, e.g., Harv. Law Sch., *Program on Empirical Legal Studies*, at <http://www.law.harvard.edu/programs/pels/> (last modified Oct. 13, 2000) (providing information about the Program on Empirical Legal Studies at Harvard Law School); Nw. Univ., *Center for Legal Studies*, at <http://www.northwestern.edu/legalstudies/> (last visited Mar. 18, 2004) (providing information about the Center for Legal Studies at Northwestern University); UCLA Sch. of Law, *The Empirical Research Group*, at <http://www1.law.ucla.edu/~erg/> (last visited Mar. 18, 2004) (providing information about the Empirical Research Group at the UCLA School of

concerned about the empirical validity of legal scholars' claims and the influence these claims may have, I can only hope that my counterfactual reasoning on this point turns out to be no better than much of the counterfactual reasoning about Enron.

---

Law); USC Law Sch. & Cal. Inst. of Tech., *Center for the Study of Law and Politics*, at <http://lawweb.usc.edu/cslp/> (last visited Mar. 18, 2004) (providing information about the Center for the Study of Law and Politics, a joint venture between the University of Southern California Law School, the California Institute of Technology, and the Initiative and Referendum Institute).