



## Trashing “Junk Science”

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### I. INTRODUCTION

¶ 1 For almost a decade, the concept of “junk science” has been used to characterize, explain, and identify problems in the law-science relationship.<sup>1</sup> Use of the term “junk science” has become ubiquitous in discussions involving law and science. Despite widespread belief that the serious problems surrounding law-science interactions are substantially attributable to the existence of junk science, the concept remarkably remains largely unexamined. Yet, many commentators have based their analyses and proposed reforms upon a commitment to a model that posits junk science as distinguishable from, and repugnant to, “good science.” The main purpose of this paper is to question the utility, philosophical integrity, and empirical validity of this model.

¶ 2 Junk science is widely believed to be responsible for a range of problems with considerable social and economic implications, such as the inhibition of industrial, medical, and scientific innovation. It is regularly blamed for precipitating an ongoing legal “crisis” resulting in longer trials, more expensive litigation, and inconsistent, irrational judgments. The appearance of junk science in legal forums is used to suggest that there has been a failure of traditional “gatekeeping

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<sup>1</sup> See generally David E. Bernstein, *Junk Science in the United States and the Commonwealth*, 21 YALE J. INT’L L. 123, 124-25 (1996) (summarizing the history of the junk science concept); David E. Bernstein, *The Admissibility of Scientific Evidence After Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 15 CARDOZO L. REV. 2139 (1994); Sophia I. Gatowski et al., *The Diffusion of Scientific Evidence: A Comparative Analysis of Admissibility Standards in Australia, Canada, England, and the United States, and Their Impact on Social and Behavioral Sciences*, 4 EXP. EVID. 86 (1996); Stephen J. Odgers & James T. Richardson, *Keeping Bad Science Out of the Courtroom – Changes in American and Australian Expert Evidence Law*, 18 U. NEW S. WALES L.J. 108, 129 (1995).

processes” (such as *Frye*<sup>2</sup>) for the admissibility of expert evidence. Proposals for legal reforms to stem the perceived influx of junk science include more active scrutiny of the norms, methods, and accreditation of those purporting to give scientific evidence; stricter evidentiary admission standards; and tort/insurance law reform. The alleged prevalence of junk science is used as evidence for the failure of the judiciary and the public to comprehend science. In turn, this has led to a perceived need to improve judicial and public scientific literacy.<sup>3</sup> Thus, those subscribing to the junk science model believe that the very existence and appeal of “junk science” reflects broader difficulties facing modern science and society.

¶ 3 In contrast to these bleak images, we contend that the junk science model is a flexible, politically charged framework that should be rejected if a more sophisticated discussion of law-science interactions – indeed, science, technology, and society generally – is to be achieved.

## II. THE POLITICAL ORIGINS OF THE JUNK SCIENCE MODEL

¶ 4 The junk science model is located at the center of much contemporary legal discourse. The model plays a strategic, rhetorical role in the agendas of many who attempt to address the pervasive perception of an ongoing legal crisis. Junk science is a convenient scapegoat for deeper law-science conflicts because it plays on public fears of science and technology being out of control, while providing a rallying point for legal reform.

¶ 5 The term “junk science” seems to have emerged in the late 1980s and early 1990s. It received its initial impetus and articulation in the polemical works of Peter Huber of the Manhattan Institute, a conservative think-tank supported by various industry and insurance groups.<sup>4</sup> Huber has promoted the junk science model through a number of publications, including the popular *Galileo’s Revenge: Junk Science in the Courtroom*.<sup>5</sup>

¶ 6 The context in which both Huber and the Manhattan Institute came to exert substantial public influence is the so-called “litigation explosion” of the 1980s.<sup>6</sup> Before this, demands for tort reform and comprehensive access to reasonable health insurance had been featured in the popular media and legal discourse.<sup>7</sup> The emergence of mass toxic tort litigation in the 1980s, allegedly resulting in enormous pay-outs, subsequently produced loud complaints from industry and insurance

<sup>2</sup> *Frye v. United States*, 293 F. 1013 (D.C. Cir. 1923).

<sup>3</sup> See, e.g., Stephen Daniels, *The Question of Jury Competence and the Politics of Civil Justice Reform: Symbols, Rhetoric, and Agenda-Building* 52 LAW & CONTEMP. PROBS. 269 (1989); Gary Edmond & David Mercer, *Scientific Literacy and the Jury: Reconsidering Jury “Competence,”* 6 PUB. UNDERSTANDING OF SCI. 329 (1997); Ian Freckelton, *Expert Evidence and the Role of the Jury*, 12 AUSTL. BAR REV. 73 (1994); Neil Vidmar, *Are Juries Competent to Decide Liability in Tort Cases Involving Scientific/Medical Issues? Some Data from Medical Malpractice*, 43 EMORY L.J. 885 (1994).

<sup>4</sup> See Kenneth J. Chesebro, *Galileo’s Retort: Peter Huber’s Junk Scholarship*, 42 AM. U.L. REV. 1637, 1705-23 (1993) (detailing Huber’s emergence in the late 1980s and early 1990s); see also Dan Quayle, *Civil Justice Reform*, 41 AM. U.L. REV. 559, 565 (1992) (quoting Huber to argue that junk science is tarnishing the legal process).

<sup>5</sup> PETER W. HUBER, *GALILEO’S REVENGE: JUNK SCIENCE IN THE COURTROOM* (1991) [hereinafter *GALILEO’S REVENGE*]; see also PHANTOM RISK: SCIENTIFIC INFERENCE AND THE LAW (Kenneth R. Foster et al. eds., 1993); Foster, Bernstein & Huber, *Science and the Toxic Tort*, 261 SCIENCE 1509 (1993); Peter W. Huber, *Fact Versus Quack*, FORBES, July 4, 1994, at 132; Peter W. Huber, *Junk Science in the Courtroom*, 26 VAL. U. L. REV. 723 (1992); Peter W. Huber, *Medical Experts and the Ghost of Galileo*, 54 LAW & CONTEMP. PROBS. 119 (1991).

<sup>6</sup> See *GALILEO’S REVENGE*, *supra* note 5, at 181-82, for Huber’s description of the litigation explosion. For a history of the alleged phenomenon, see WALTER K. OLSON, *THE LITIGATION EXPLOSION: WHAT HAPPENED WHEN AMERICA UNLEASHED THE LAWSUIT* (1991).

<sup>7</sup> See Eliot Martin Blake, *Rumors of Crisis: Considering the Insurance Crisis and Tort Reform in an Information Vacuum*, 37 EMORY L.J. 401 (1988); James A. Henderson, Jr. & Theodore Eisenberg, *The Quiet Revolution in Products Liability: An Empirical Study of Legal Change*, 37 UCLA L. REV. 479 (1990); John A. Siliciano, *Mass Torts and the Rhetoric of Crisis*, 80 CORNELL L. REV. 990 (1995).

groups. Cases involving chemical manufacturers, pharmaceutical companies, and health care workers received the most attention, and industry argued that the profitability and viability of production and manufacturing in the United States were under serious attack.

¶ 7 Adding to, and indeed fueling, the public discussion of tort reform in the 1980s were a number of supposed insurance “crises.” The supply of insurance was purportedly jeopardized because of the faltering tort system.<sup>8</sup> Skepticism toward various forms of expertise emerged as a major public concern,<sup>9</sup> and the junk science model seems to have emerged as commentators sought a means to explain the “crises.”

¶ 8 To some, the “litigation explosion” – and its concomitant insurance “crisis” – represented a breakdown in the traditional, independent, pioneering ethos.<sup>10</sup> Not only had traditional American values been eroded, but the legal system itself had encouraged forms of “pathological” litigation. As politicians, lawyers, and industry looked to reform the “faltering” tort system, the junk science model emerged as a powerful, polemical tool. We do not suggest that the crises were entirely fictitious, but rather that they played a rhetorical role in an intensive political debate<sup>11</sup> over responsibility for the development and impact of potentially injurious products – including assessment of resultant harms and the apportionment of compensation.<sup>12</sup>

### III. PROBLEMS WITH THE JUNK SCIENCE MODEL

¶ 9 Despite the vagaries surrounding the junk science model, a number of key defining features and assumptions can be identified and examined. We will demonstrate that the model relies on the following: untenable images of efficacy, methods, norms, and motivations as hallmarks of “good science”; inadequate, and sometimes clearly erroneous, views drawn from the history of science; impoverished understandings of the public perception of risk and its influence on the construction of scientific knowledge; and naïve views of the relationship

<sup>8</sup> Cf. Robert F. Blomquist, *Emerging Themes and Dilemmas in American Toxic Tort Law, 1988-91: A Legal-Historical and Philosophical Exegesis*, 18 S. ILL. U. L.J. 1 (1993); Dan L. Burk, *When Scientists Act like Lawyers: The Problem of Adversary Science*, 33 JURIMETRICS J. 363, 369 (1993); Deborah Maliver, *Out of the Fryeing Pan and into Daubert: Trial Judges at the Gate Will Not Spell Relief for Plaintiffs*, 56 U. PITT. L. REV. 245, 248 (1994) (“In recent years, both courts and commentators have expressed concern that ‘junk science’ has pervaded civil litigation, particularly litigation involving toxic torts.”); Richard L. Marcus, *Evidence, Discovery Along the Litigation/Science Interface*, 57 BROOK. L. REV. 381 (1991); Deborah Young, *The Impact of Science and Technology on the Courts*, 43 EMORY L.J. 853, 854 (1994) (“For years courts have struggled with finding the proper balance between admitting novel scientific evidence and excluding ‘junk science.’”).

<sup>9</sup> See generally Joseph Sanders, *From Science to Evidence: The Testimony on Causation in the Bendectin Cases*, 46 STAN. L. REV. 1 (1993). Studies in the public understanding of science show nonetheless that the public still holds a generally positive image of science and expertise, a common feature being ambivalence towards the claims of specific experts rather than the rejection of expertise per se. See, e.g., ALAN IRWIN, *CITIZEN SCIENCE: A STUDY OF PEOPLE, EXPERTISE AND SUSTAINABLE DEVELOPMENT* (1995); SHEILA JASANOFF, *SCIENCE AT THE BAR: LAW, SCIENCE, AND TECHNOLOGY IN AMERICA* (1995); Mike Michael, *Lay Discourses of Science: Science-in-General, Science-in-Particular, and Self*, 17 SCI. TECH. & HUM. VALUES 313 (1992).

<sup>10</sup> Huber’s discourse reinforces related themes which appeared in popular discourse on technological risk. See, e.g., MARY DOUGLAS & AARON WILDAVSKY, *RISK AND CULTURE: AN ESSAY ON THE SELECTION OF TECHNICAL AND ENVIRONMENTAL DANGERS* (1982); William C. Clark, *Witches, Floods, and Wonder Drugs: Historical Perspectives on Risk Management*, in SOCIETAL RISK ASSESSMENT 287 (Richard C. Schwing & Walter A. Albers, Jr. eds., 1980). For critiques of these approaches see LANGDON WINNER, *THE WHALE AND THE REACTOR: A SEARCH FOR LIMITS IN AN AGE OF HIGH TECHNOLOGY* 138-54 (1986).

<sup>11</sup> See, e.g., JOSEPH R. GUSFIELD, *THE CULTURE OF PUBLIC PROBLEMS: DRINKING-DRIVING AND THE SYMBOLIC ORDER* (1981); Robert M. Hayden, *The Cultural Logic of a Political Crisis: Common Sense, Hegemony and the Great American Liability Insurance Famine of 1986*, 11 STUD. L. POL. & SOC’Y 95 (1991).

<sup>12</sup> See generally Robert F. Blomquist, *Science, Toxic Tort Law, and Expert Evidence: A Reaction to Peter Huber*, 44 ARK. L. REV. 629, 653 (1991); Henderson & Eisenberg, *supra* note 7, at 480 (describing state reaction to the perceived crisis brought on by extending liability).

between law and science. Many of these faulty assumptions result from the reluctance of junk science model proponents to draw upon research from the history, philosophy, and sociology of science. These fields specialize in examining the links between science, technology, and society, and provide theories to explain the nature of science and technology.

¶ 10 In this section, we will discuss the limitations of the junk science model, focusing particular attention on the simplistic, idealized, and frequently erroneous images of science employed by the model's proponents. We will also look at how these rhetorical flourishes help proponents mask their political commitments and agendas.

#### A. "Junk Science" is Vague

¶ 11 Despite its widespread use, the term "junk science" has neither a clear nor consistently applied meaning. Even its early promoter, Peter Huber, avoided providing a precise definition.<sup>13</sup> The flexibility of the term "junk science" is not surprising given the rhetorical work required of it. As our later discussion indicates, it is difficult to imagine a more precise and consistent definition being successfully used to address the diversity of scientific claims which its proponents hope to debunk. A large number of similarly pejorative, inflammatory, and imprecise synonyms for "junk science" are common features of discussions on law and science.<sup>14</sup> For some commentators, such labels are used interchangeably, whereas for others there are subtle, although perhaps indefensible, distinctions among them.<sup>15</sup>

¶ 12 Sometimes the term "junk science" is employed to describe knowledge claims which are not generally accepted or sufficiently mainstream,<sup>16</sup> though even on this

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<sup>13</sup> See GALILEO'S REVENGE, *supra* note 5, at 214 ("We . . . have no precise definition of 'junk science,' 'pseudoscience,' 'quack,' 'crank,' 'crackpot,' or 'snake oil.'"); see also Blomquist, *supra* note 12, at 635-37 ("law and sciosophy," "pathological science," "pseudo-scientist," "junk science," "critics, from the fringes of science and beyond," "unorthodox nostrums," "unrecognised techno-terrors," "the well-known Cheshire Cat phenomenon," "non-science," "bad scientists," "bad doctor," "quack cures," "quackcure movement," "extreme junk scientists," "modern crank," "the eccentric," "the true believers," "the proselytizers," "the missionaries," "reckless expertise," "charlatans," "the Aquarians," "the wanton witness," "the torrid testifier," "the licentious legal sidekick," "professional expert," "talking head," "the entrepreneurial experts," "defrocked professionals," "Darwinian Avenger," "hired gun," "willows in the wind consultants," "saxophones," "Mr. Professional Witness, U.S.A.," "farsiders," "typical paint-by-the-numbers presentation," "pseudo-scientific flim-flam," "repeat players," "fad terrors," "scam," "activist faith," and "witch hunters"). Compare this to Peter W. Sperlich, *The Liability of Junk and the Junk of Liability: Evidentiary Misdeeds in the Courts*, 75 JUDICATURE 273, 273 (1992) (book review) ("*Galileo's Revenge: Junk Science in the Courtroom* is an important publication. . . . The writing is graceful and free of jargon.").

<sup>14</sup> See, e.g., Scott C. Andre, *Weird Science: Problems with the U.S. Supreme Court's New Evidentiary Standard for Expert Scientific Testimony and Oregon Case Law as a Possible Solution*, 73 OR. L. REV. 691 (1994); Bernstein, *Junk Science in the United States and the Commonwealth*, *supra* note 1, at 179 (referring to "speculative litigation," "speculative lawsuits," and "dubious science"); Bert Black, *A Unified Theory of Scientific Evidence*, 56 FORDHAM L. REV. 595, 669 (1988) (referring to testimony that is "patently unscientific"); Faigman, Porter & Saks, *Check Your Crystal Ball at the Courthouse Door, Please: Exploring the Past, Understanding the Present, and Worrying About the Future of Scientific Evidence*, 15 CARDOZO L. REV. 1799, 1816 (1994) (referring to "shoddy science"); Paul C. Gianelli, Daubert: *Interpreting the Federal Rules of Evidence*, 15 CARDOZO L. REV. 1999, 2021 (1994) (referring to "specious testimony").

<sup>15</sup> See, e.g., Faigman, *supra* note 14, at 1803 ("But science – good, bad, and pseudo – began long before 1923, as did the testimony of good, bad, and pseudoexperts.").

<sup>16</sup> See, e.g., G. Michael Fenner, *Evidence Review: The Past Year in the Eighth Circuit, plus Daubert*, 28 CREIGHTON L. REV. 611, 640 (1995) (quoting an *Omaha World Herald* editorial that defined junk science, in the courtroom context, as "testimony from expert witnesses who operate outside the mainstream of scientific thought"); Andre A. Moenssens, *Novel Scientific Evidence in Criminal Cases: Some Words of Caution*, 84 J. CRIM. L. & CRIMINOLOGY 1 (1993). For a discussion of the critical role played by "mainstream" science, see JASANOFF, *supra* note 9.

point there is disagreement.<sup>17</sup> Other commentators associate the term “junk science” with novelty, regardless of the legitimacy or ultimate vindication of the particular knowledge claim.<sup>18</sup> Some differentiate novel science that uses legitimate methods from that which reaches illegitimate and dubious conclusions.<sup>19</sup> Still other commentators focus upon the purported unreliability or non-experimental nature of “junk science.”<sup>20</sup>

¶ 13 Often, these deprecatory forms of “junk science” are contrasted with science “proper” or, more commonly, “good science.”<sup>21</sup> “Good science” is usually described as dependent upon qualities such as falsifiable hypotheses, replication, verification, peer-review and publication, general acceptance, consensus, communalism, universalism, organized skepticism, neutrality, experiment/empiricism, objectivity, dispassionate observation, naturalistic explanation, and use of the scientific method. A good example of this tendency is provided by Loewinger: “While there are innumerable specialized fields in science today, and while knowledge in one field does not necessarily transfer to another field, there are, nevertheless, general standards applicable to all fields of science that distinguish *genuine science* from pseudo-science and quack science.”<sup>22</sup>

¶ 14 Many commentators and courts combine these, and other supposed attributes of “good science,” in an attempt to capture the essence of science and demarcate it from non-scientific enterprises.<sup>23</sup> Yet, most of these writers fail to appreciate that the images are ideals. Few of these qualities are displayed consistently or

<sup>17</sup> See, e.g., Maliver, *supra* note 8, at 248 (“The *Frye* test was believed by many to guard against infiltration by ‘junk science’; Michael Rustad & Thomas Koenig, *The Supreme Court and Junk Social Science: Selective Distortion in Amicus Briefs*, 72 N.C. L. REV. 1060, 1082 (1994) (“Such evidence is not automatically ‘junk science’ simply because it may not yet be generally accepted.”). Advocates of the *Frye* test maintained that by requiring that a scientific technique or theory be subjected to peer review before gaining entrance into a courtroom, ‘junk’ would be weeded out by those who truly understood the science.”) (footnote omitted).

<sup>18</sup> See, e.g., Paul R. Rice, *Expert Testimony: A Debate Between Logic or Tradition Rather than Between Deference or Education*, 87 NW. U. L. REV. 1166, 1168 (1993) (“[E]ducation versus deference is the real substantive issue underlying the structure of expert testimony, the *Frye* controversy, and related proposals about ‘scientific consensus’ in novel, or junk, scientific areas.”); Young, *supra* note 8, at 854 (“For years courts have struggled with finding the proper balance between admitting novel scientific evidence and excluding ‘junk science.’”).

<sup>19</sup> Young, *supra* note 8, at 858 (“Galileos are apt to depend on novel methodology while junk scientists are likely to draw from valid methodology but reach ill-founded and dubious conclusions.”).

<sup>20</sup> See, e.g., Bert Black, Francisco J. Ayala & Carol Saffron-Brinks, *Science and the Law in the Wake of Daubert: A New Search for Scientific Knowledge*, 72 TEX. L. REV. 715, 761 (1994) (“Some scientists refer to this kind of work as pathological science, characterized by a fixation on effects that are difficult to detect, a readiness to disregard prevailing ideas and theories, and an unwillingness to conduct meaningful experimental testing.”); Alan W. Tamarelli, Jr., *Daubert v. Merrell Dow Pharmaceuticals: Pushing The Limits of Scientific Reliability – The Questionable Wisdom of Abandoning the Peer Review Standard for Admitting Expert Testimony*, 47 VAND. L. REV. 1175, 1178 n.15 (“‘Junk science’ is a pejorative term describing novel scientific testimony that has only a weak grounding in established and well-tested principles or methods. It refers to evidence that is held out to the trier of fact as scientific but lacks the element of reliability normally associated with that type of information. Courts and commentators have recognized the danger presented by making this testimony available to juries . . . but junk science nevertheless represents a growing problem in complex, modern litigation.”).

<sup>21</sup> See Clifton T. Hutchinson & Danny S. Ashby, *Daubert v. Merrell Dow Pharmaceuticals, Inc.: Redefining the Bases for Admissibility of Expert Scientific Testimony*, 15 CARDOZO L. REV. 1875, 1886 (1994); Miller, Rein & Bailey, *Daubert and the Need for Judicial Scientific Literacy*, 77 JUDICATURE 254, 260 (1994); Sean O’Connor, *The Supreme Court’s Philosophy of Science: Will the Real Karl Popper Please Stand Up?*, 35 JURIMETRICS J. 263, 276 (1995); James T. Richardson et al., *The Problems of Applying Daubert to Psychological Syndrome Evidence*, 79 JUDICATURE 10, 16 (1995).

<sup>22</sup> Lee Loewinger, *Science and Legal Rules of Evidence: A Review of Galileo’s Revenge: Junk Science in the Courtroom*, 32 JURIMETRICS J. 487, 500 (1992) (book review) (italics added).

<sup>23</sup> See Bernstein, *Junk Science in the United States and the Commonwealth*, *supra* note 1, at 173 (“[A]re juries able and willing to distinguish junk science from sound scientific evidence?”); Edward V. Di Lello, Note, *Fighting Fire with Firefighters: A Proposal for Expert Judges at the Trial Level*, 93 COLUM. L. REV. 473 (1993); Steven J. Grossman & Christopher K. Gagne, *Science and Scientific Evidence II*, 25 CONN. L. REV. 1053, 1065 (1993); James M. Wood & John E. Carne, *Daubert’s Lamppost: A Guide to the Admissibility of Scientific Evidence*, 2 J. PHARMACY & LAW 221, 224 (1993) (“Simultaneously the Court stripped from the junk scientist the ability to opine about the cause of a disease in a way that contradicts good science.”).

mechanically in what they describe as “good,” “genuine,” or “mainstream” science. We will have more to say on the norms of science, the scientific method, and falsification in our subsequent discussion. But for now, these ideal images of science can be thought of as constituting a grammar and vocabulary which make “junk science” discourse possible.

B. *Limits of Efficacy as the Demarcation Between “Real” and “Junk” Science*

¶ 15 One of the distinctions regularly employed to distinguish “real” or “good” science from “junk science” is that the former results in technically efficacious outcomes, whereas the latter does not. This means that while at times “junk science” may display the outward signs of “real” science, ultimately its inferiority will be revealed through its lack of practical utility. Huber parodies “junk science” through a comparison with the cargo cults of Polynesia:

During the war, they saw airplanes land with lots of good materials and they want the same thing to happen now. So they’ve arranged to make things like runways, to put fires along the sides of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head like headphones and bars of bamboo sticking out like antennas – he’s the controller – and they wait for the airplanes to land. They’re doing everything right. The form is perfect. It looks exactly the way it looked before. But it doesn’t work. No airplanes land.

“One finds many similar airports at the edge of the scientific community,” Feynman observes, “theories that don’t work, and science that isn’t science. . . . I call these things cargo cult science, because they follow all the apparent precepts and forms of scientific investigation, but they’re missing something essential, because the planes don’t land.”<sup>24</sup>

¶ 16 This rigid dichotomy oversimplifies the relationship between scientific knowledge and technical practices.<sup>25</sup> While it is undoubtedly true that many branches of modern science are strongly linked to technically useful bodies of knowledge, it is equally true that many branches of science are not. Numerous examples suggest that, at times, practical knowledge and technologies pre-dated later scientific formulation.<sup>26</sup> For example, the development of the steam engine occurred prior to, and in fact stimulated research into, theorization of energy conservation.<sup>27</sup> In the area of public health, statistical work by McKeown dates the most significant decline in infectious diseases – e.g., cholera, small pox, and typhoid

<sup>24</sup> GALILEO’S REVENGE, *supra* note 5, at 2-3.

Junk science is the mirror image of real science, with much of the same form but none of the same substance. There is the astronomer, on one hand, and the astrologist, on the other. The chemist is paired with the alchemist, the pharmacologist with the homoeopathist. . . . Further out on the surgical fringe are outright charlatans, well documented in the credulous pulp press, who claim to operate with rusty knives but no anesthesia, who prey on cancer patients so desperate they will believe a palmed chicken liver is really a human tumor.” *Id.* at 172.

<sup>25</sup> See, e.g., WIEBE E. BIJER ET AL., THE SOCIAL CONSTRUCTION OF TECHNOLOGICAL SYSTEMS: NEW DIRECTIONS IN THE SOCIOLOGY AND HISTORY OF TECHNOLOGY (1987); COLIN CHANT & JAMES MOORE, *Science and Technology: Problems of Interpretation*, in SCIENCE, TECHNOLOGY AND EVERYDAY LIFE 1870-1950 40 (Collin Chant ed., 1989); DAVID EDGE, THE SOCIAL SHAPING OF TECHNOLOGY (Working Paper No. 1, 1988); Barry Barnes, *The Science-Technology Relationship: A Model and a Query*, 12 SOC. STUD. OF SCI. 166 (1982).

<sup>26</sup> See, e.g., OTTO MAYR, *The Science-Technology Relationship*, in SCIENCE IN CONTEXT 155 (Barry Barnes & David Edge eds., 1982); JOHN M. ZIMAN, *Which Came First: Science or Technology?*, in THE FORCE OF KNOWLEDGE 8 (John M. Ziman ed., 1976).

<sup>27</sup> See THOMAS S. KUHN, *Energy Conservation as an Example of Simultaneous Discovery*, in THE ESSENTIAL TENSION: SELECTED STUDIES IN SCIENTIFIC TRADITION AND CHANGE 66 (Thomas S. Kuhn ed., 1977).

– as occurring *prior* to the widespread application of sulfur-based drugs and inoculation.<sup>28</sup> In addition, the basis for determining scientific and/or useful knowledge changes diachronically. Some current areas are adhered to because of their “methodological” alliance with other sciences – even while serious questions relating to their efficacy remain unanswered.<sup>29</sup> Any simple linkage between science and practice is further undermined when we consider the reworking and simplification of scientific knowledge as it moves from abstract theorizing into standardized forms sufficient to fulfill a technological function.

¶ 17 Thus, Jerome Ravetz asserts that knowledge which begins life in a pure research paper undergoes a subsequent process of “extension and standardization.”<sup>30</sup> Particular “facts” are standardized by smoothing away problematic particulars that give rise to obscurities or conceptual difficulties. The resultant “fact” is normally an assertion about classes of things or events that relate to the external world. This standardization of a fact is essential “if the fact is to be useful to those who lack the time, skill or inclination to master the elaborate theoretical context in which its sophisticated version is comprehensible.”<sup>31</sup> The process has two main features:

[First,] the content of a standardized fact may decay, almost without limit . . . the degree of sophistication and of faithfulness to its original which is necessary for its adequate performance of its function will depend very strongly on its use . . . [Second,] a version of a standardized fact which is good enough for one function can be quite inadequate for another; and since any standardized fact performs a variety of functions, it will naturally appear in a variety of versions.<sup>32</sup>

¶ 18 Ravetz’s model explains the first phase of transformation of a research finding to suit criteria of scientific usefulness. The process then continues, with increasing divergence, as established facts are radically redesigned to fit the field of practical application. Edwin Layton discusses the process further:

[T]he theory of structures is less abstract than physics, for example, in incorporating idealized versions of manmade devices. But in turn, structural design, which has become scientific in some respects, is much less abstract than structural theory. That is, the designer must take into account a more complex reality. The theorist may assume that the materials

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<sup>28</sup> See THE CULTURAL CRISIS OF MODERN MEDICINE 12 (John Ehrenreich ed., 1978); Michael Mulkey, *Knowledge and Utility: Implications for the Sociology of Knowledge*, 9 SOC. STUD. OF SCI. 63 (1979).

McKeown examined the cause of the decline in deaths from a group of diseases whose disappearance as major killers accounts for the bulk of the decline in overall death rate in England since the early nineteenth century (tuberculosis, scarlet fever, typhoid, typhus, cholera, diarrhea and dysentery, and smallpox). He concluded that the reasons for their disappearance as a major cause of death were, in order of importance: first, improvements in the standard of living (*e.g.*, nutrition, housing); second, improvements in control of the environment (*e.g.*, water supply and other sanitary services); and only *third*, personal medical care. John Powles and A.L. Cochrane have summarized further evidence that the death rates for a number of major noninfectious diseases (*e.g.*, cancer, heart disease) have not responded to modern medical approaches. Echoing the contentions of some feminists who charged that modern medicine had been overrated, these studies suggested that modern medical care was and is, at best, much less effective at reducing morbidity and mortality than the doctors have claimed and most people have believed. *Id.*

Compare GALILEO’S REVENGE, *supra* note 5, at 196 (“The story of cholera is the story of how medicine was transformed from black art to science, from a pseudoscience of the individual to a science of groups.”).

<sup>29</sup> EVELLEEN RICHARDS, VITAMIN C AND CANCER: MEDICINE OR POLITICS? (1991).

<sup>30</sup> JEROME RAVETZ, SCIENTIFIC KNOWLEDGE AND ITS SOCIAL PROBLEMS 199 (1973).

<sup>31</sup> See *generally id.*

<sup>32</sup> *Id.* at 202.

are uniform, but the designer must be aware of nonuniformities in his materials and make due allowances for them.<sup>33</sup>

¶ 19 This insight suggests a progressive transformation of the meaning of concepts and facts as they pass through different social and technical orientations. In practical terms, for example, they must pass through the mediations of the scientific community where they are manipulated to conform with established knowledge, and undergo subtle modifications to suit particular scientific and technical sub-disciplines.<sup>34</sup> Such standardization processes can mean that the scientific knowledge used in a practical application may have quite a different identity – in theoretical terms – from its original formulation.

¶ 20 Finally, another reason to reject simple dichotomies centered on efficacy is that technologists selectively choose from a variety of scientific theories when trying to suit the practical purposes at hand. A good example of this is the continuing popularity of Ptolemaic and Tyconic astronomical models for use in navigation.<sup>35</sup> Mulkey asserts that:

[R]eformulation of theories in practical contexts, use of only part of a theory, low accuracy requirements and the complexity of practical situations show that ‘infinitely many possible rival theories’ can yield results which are identical in practical terms and that it is undoubtedly possible for a false theory or a partly false theory to be practically effective.<sup>36</sup>

¶ 21 Overall, those studying the detailed linkages between science and technology indicate that the relationship between knowledge and utility/efficacy is complex. False theories can be practically effective while there can be significant shifts in the meanings of concepts as they move from theoretical to practical contexts. Efficacy, therefore, cannot be used as a simple means of distinguishing “good” from “junk” science.

### C. *Limits of Motivation/Intention as the Demarcation Between “Good” and “Junk” Science*

¶ 22 The huge financial stakes often involved in litigation and regulation are frequently seen as a possible reason why the normal values of scientific work get subverted. This subversion is often portrayed as a characteristic of junk science. Influenced by lawyers, relaxed evidence admission standards, and huge incentives, the claim is that scientists find it hard to resist the temptation to produce junk science.<sup>37</sup>

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<sup>33</sup> BRUNO LATOUR, *SCIENCE IN ACTION: HOW TO FOLLOW SCIENTISTS AND ENGINEERS THROUGH SOCIETY* (1987); Edwin Layton, *Conditions of Technological Development*, in *SCIENCE, TECHNOLOGY AND SOCIETY: A CROSS-DISCIPLINARY PERSPECTIVE* 197, 210 (Ina S. Spiegel-Rosing & Derek de Solla Price eds., 1977); see also BIKER et al., *supra* note 25; Mulkey, *supra* note 28;.

<sup>34</sup> Stephen Hilgartner, *The Dominant View of Popularization: Conceptual Problems, Political Uses*, 20 *SOC. STUD. OF SCI.* 519, 520-531 (1990).

<sup>35</sup> THOMAS S. KUHN, *THE COPERNICAN REVOLUTION: PLANETARY ASTRONOMY IN THE HISTORY OF WESTERN THOUGHT* (1957). Alternative knowledge systems to Western science can, of course, generate practical outcomes. This has been well attested by studies of Micronesian navigation, the pyramids, medieval cathedrals and indigenous pharmacopoeia. See DAVID TURNBULL, *MAPPING THE WORLD IN THE MIND: AN INVESTIGATION OF THE UNWRITTEN KNOWLEDGE OF THE MICRONESIAN NAVIGATORS* (1991); David Turnbull, *The Ad Hoc Collective Work of Building Gothic Cathedrals with Templates, String, and Geometry*, 18 *SCI. TECH. AND HUM. VALUES* 315 (1993); Helen Watson-Verran & David Turnbull, *Science and Other Indigenous Knowledge Systems*, in *HANDBOOK OF SCIENCE AND TECHNOLOGY STUDIES* 115 (Sheila Jasanoff, Gerald E. Markle, James C. Petersen & Trevor Pinch eds., 1995).

<sup>36</sup> Mulkey, *supra* note 28, at 75; Derek J. de Solla Price, *Is Technology Historically Independent of Science?*, 6 *TECH. AND CULTURE*, 553 (1965).

<sup>37</sup> Bernstein, *Junk Science in the United States and the Commonwealth*, *supra* note 1, at 181 (“The most effective way the United States could significantly reduce litigation based on junk science would be to restrict the financial (footnote continued)

¶ 23 The notion that the motivation of scientists can assist in distinguishing junk science from good science is simplistic when we empirically analyze the behavior of scientists and the culture of science. Unlike the impression conveyed by junk science model proponents, in actuality scientists frequently find themselves in a competitive environment where strong emotional commitment to their views and sensitivity to finance and funding are essential to career progression – even academic and institutional survival. There are numerous well documented cases of the competitiveness and financial worldliness of scientists.<sup>38</sup> In important new areas of science, such as biotechnology, microelectronics, and biomedical research, financial and research considerations have become inseparable.<sup>39</sup> Some junk science model proponents would have us believe that factors like financial opportunism are hallmarks of junk science, but in reality such factors provide powerful motors for many fields of contemporary science.

¶ 24 The attack on the motivations of “junk scientists” is also highly asymmetrical. It largely ignores counter-examples such as where industry-sponsored scientists participate in mass tort litigation.<sup>40</sup> In the debate over the health risks of electromagnetic fields (EMF), investigative journalist Paul Brodeur has consistently cited the existence of research undertaken primarily to quell public concern and dampen legal challenges to power authorities. Brodeur writes that during the 1980s there was a “stable” of scientists with questionable intentions aiding the Washington-based legal firm Crowell and Moring by supplying research reports and finely-honed testimony derived to support power authorities.<sup>41</sup>

#### D. *The Problem of “Scientific Norms”*

¶ 25 A prominent feature of the junk science model is the supposed existence of “scientific norms,” which represent accepted methods, standards, and practices. While courts and critics might invoke such criteria, scientific norms do not form a code of conduct or attitudinal orientation which can simply be adduced to demarcate the appropriateness of particular scientific practices. There have been a number of attempts to formulate the ideal standards of conduct (norms) and institutional imperatives of science. Robert Merton provided the most famous formulation of these imperatives, categorizing them under the four headings of

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incentives to bring such cases . . .”); GALILEO’S REVENGE, *supra* note 5, at 3 (“Junk science is impelled through our courts by a mix of opportunity and incentive. ‘Let-it-all-in’ legal theory creates the opportunity. The incentive is money: the prospect that the Midas-like touch of a credulous jury will now and again transform scientific dust into gold. Ironically, the law’s tolerance for pseudoscientific speculation has been rationalized in the name of science itself. The open-minded traditions of science demand that every claim be taken seriously, or at least that’s what many judges have reasoned.”).

<sup>38</sup> Alison Motluk, *Millionaires’ Row*, NEW SCIENTIST, Sept. 14, 1996 at 28 (“I don’t do things that won’t make me money,” “That’s all crap,” he says. ‘It’s the money that allows it all to happen.’”, “As soon as you become reasonably financially successful, people insist you can’t be a good scientist.” (Dr. Chris Evans), “In the first week he told me to build a physics department. In the second, he asked me to get on with setting up a business.” ([Professor] Desmond Smith)). See also JAMES D. WATSON, THE DOUBLE HELIX: A PERSONAL ACCOUNT OF THE DISCOVERY OF THE STRUCTURE OF DNA (1968); Edward Yoxen, *Speaking Out About Competition*, in EXPOSITORY SCIENCE: FORMS AND FUNCTIONS OF POPULARIZATION 163, 178 (Terry Shinn & Richard Whitley eds., 1985) (“I remarked that some former colleagues say *The Double Helix* exaggerates the competitiveness of science. Watson’s manner changed. He bit the words out sharply. ‘I probably understated it – It is the *dominant motive* (original emphasis) in science. It starts at the beginning; if you push first, you become a professor first; your future depends on some indication that you can do something by yourself. It’s that simple. Competitiveness is very, very dominant. The chief emotion in the field. The second is you have to prove to yourself that you can do it – that’s the same thing. You’ve got to keep doing it; you can’t just – once.”).

<sup>39</sup> See RANDAL ALBURY, THE POLITICS OF OBJECTIVITY (1983); Pierre Bourdieu, *The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason*, SOC. SCI. INFO. Jun. 1975, at 19; Hilgartner, *supra* note 34.

<sup>40</sup> See Chesebro, *supra* note 4, at 1652, 1677.

<sup>41</sup> PAUL BRODEUR, CURRENTS OF DEATH (1989).

communalism, universalism, disinterestedness, and organized skepticism.<sup>42</sup> Much of the relevant law-science literature is framed in terms similar to Merton's well known formulation.<sup>43</sup> Mulkay later formulated them in less specialized terms:

The norms of science are seen as prescribing that scientists should be detached, uncommitted, impersonal, self-critical, and open-minded in their attempts to gather and interpret objective evidence about the natural world. It is assumed that considerable conformity to these norms is maintained; and the institutionalization of these norms is seen as accounting for that rapid accumulation of reliable knowledge which has been the unique achievement of the modern scientific community.<sup>44</sup>

¶ 26 While commonly cited, however, the idea of norms has been questioned as a description of actual scientific behavior. Numerous empirical studies of the conduct of scientists have failed to show that adherence to such norms is a necessary feature of doing scientific work. A widely quoted study by Ivan Mitroff found that the ideal norms of scientific conduct were matched by the existence of counter-norms.<sup>45</sup> For example, while scientists sometimes stressed the value of emotional neutrality, they also claimed the need for the occasional strong, even unreasonable commitment, to scientific ideas. This attitude was seen to be psychologically significant given the disappointments, frustrations, and intellectually taxing nature of scientific work.<sup>46</sup> Further, the ideals of universalism and judging claims on impersonal grounds were matched by scientists considering it perfectly normal to assess knowledge claims on personal criteria such as the experience, education, affiliations, status, reliability, and skills of a particular researcher.<sup>47</sup>

¶ 27 It has been argued that such "gate-keeping" processes are a pragmatic response to the vast quantity of modern scientific research output and the pressures of maintaining research momentum in the typically competitive environment of modern scientific research.<sup>48</sup> Further examples, identified by Mitroff, of the pragmatic flexibility in the application of the "norms" of science include the balancing of ideals of the open communication of scientific results with the use of

<sup>42</sup> See ROBERT K. MERTON, *THE SOCIOLOGY OF SCIENCE* (1973) (originally formulated in 1942).

<sup>43</sup> See Committee on the Conduct of Science, National Academy of Sciences, *On Being a Scientist* 1 (1989); GALILEO'S REVENGE, *supra* note 5, at 196-97, 209; Black, *supra* note 14; Black et al., *supra* note 20, at 777, 779; David L. Faigman, *Mapping the Labyrinth of Scientific Evidence*, 46 HASTINGS L.J. 555, 572 (1995); Hutchinson & Ashby, *supra* note 21, at 1900, 1904; Peter H. Schuck, *Multi-Cultural Redux: Science, Law, and Politics*, 11 YALE L. & POL'Y REV. 1, 16, 18, 24, 31, 33 (1993); John F. Baughman, *Galileo's Revenge: Junk Science in the Courtroom*, 90 MICH. L. REV. 1614, 1615-19 (1992) (book review). There does seem to be some evidence for the recent development of more sophisticated official approaches within the 'scientific community' to the issues of ethics and norms of practice. See discussion in FRANCIS L. MACRINA, *SCIENTIFIC INTEGRITY: AN INTRODUCTORY TEXT WITH CASES* (1995).

<sup>44</sup> MICHAEL MULKAY, *SCIENCE AND THE SOCIOLOGY OF KNOWLEDGE* 64 (1979).

<sup>45</sup> See IVAN MITROFF, *THE SUBJECTIVE SIDE OF SCIENCE: A PHILOSOPHICAL INQUIRY INTO THE PSYCHOLOGY OF THE APOLLO MOON SCIENTISTS* 219 (1974). Much of our discussion of Mitroff has been drawn from Mulkay, *supra* note 44. See also Barry Barnes & Robert Dolby, *The Scientific Ethos: A Deviant Viewpoint*, 11 ARCHIVES, EUROPEENNES DE SOCIOLOGIE, 3 (1970); Michael Mulkay, *Norms and Ideology in Science*, 15 SOC. SCI. INFO. 637 (1976).

<sup>46</sup> The theme of the importance of commitment in science is also apparent in scientific biography, for instance the case of Johannes Kepler as outlined in ARTHUR KOESTLER, *THE SLEEPWALKERS: A HISTORY OF MAN'S CHANGING VISION OF THE UNIVERSE* (1959). This point has also been noted by ALBURY, *supra* note 39, and in scientific autobiographies such as WATSON, *supra* note 38.

<sup>47</sup> Note that these are characteristics which are often seen to be criteria used by a 'scientifically illiterate' jury to come to decisions based on scientific evidence and usually employed to criticize the institution or current organization of the jury. Consider Bernstein, *supra* note 1, at 174; M. Klein, *After Daubert: Going Forward with Lessons from the Past*, 15 CARDOZO L. REV. 2219, 2223 (1994). Cf. BRIAN WYNNE, *RATIONALITY AND RITUAL: THE WINDSCALE INQUIRY AND NUCLEAR DECISIONS IN BRITAIN* 133 (1982).

<sup>48</sup> See ALBURY, *supra* note 39.

secrecy. The latter was often justified by scientists to avoid priority disputes, check results without jeopardizing priority, and avoid criticism of preliminary results which could dampen enthusiasm for further research.<sup>49</sup> Mitroff's empirical work suggests that norms and institutional imperatives often associated with science do not constitute a set of rules or consistent way of demarcating "good" from "bad" science. Rather, to quote Mulkay, they can be seen as part of "a repertoire or vocabulary which scientists can use flexibly to categorize professional actions differently in various social contexts."<sup>50</sup>

#### E. *The Problem of "Scientific Method"*

¶ 28 Another common assumption adopted by law-science commentators and courts is that scientists are in possession of a simple, identifiable, universal scientific method which guides activity and can be employed in practical contexts to distinguish "good science" from "junk science."<sup>51</sup> Faigman, for example, writes:

Although the scientific method continues to attract its share of detractors and skeptics, the significant advances in science and technology in the twentieth century illustrate the power of that method. . . . A substantial level of sophistication in the scientific method will be necessary if judges are ever going to integrate science successfully into their legal decisions.<sup>52</sup>

¶ 29 The notion that there is a simple, identifiable, universal scientific method used in some kind of standard way by scientists to distinguish science from non-science is difficult to support on any kind of empirical basis. One of the factors which illustrates the implausibility of this contention is the sheer diversity of activities which can be placed beneath the umbrella of modern science. Given such diversity, various branches of scientific knowledge rely, to different degrees, upon observational practices, experimental tests, mathematical proofs, and so on. At anything other than the most ideal, unrealistic, and abstract level, empirical studies suggest it is better to talk of *scientific methods* or *heuristics* – plural – where only particular subsets of these apply to any one scientific specialization. For instance, in some branches of industrial chemistry, test situations can be established where there are strong linkages between theory, practice, and phenomena. In contrast, other areas of science rely upon situations intrinsically difficult to test. These situations, then, may rely on statistical methods, new, sensitive measuring devices, and phenomena not easily modeled in the laboratory. The latter is true in many areas of atmospheric physics, ecology, and epidemiology.<sup>53</sup> Thus, even the sorts of concepts often taken as key ingredients of scientific method – standards of proof, models, acceptable error rates, and observation – vary substantially from one branch of science to the next.

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<sup>49</sup> MULKAY, *supra* note 44 at 67, 70-71.

<sup>50</sup> *Id.* at 71.

<sup>51</sup> See *Daubert v. Merrell Dow Pharm., Inc.*, 509 U.S. 579 (1993) ("But, in order to qualify as 'scientific knowledge,' an inference or assertion must be derived by *the scientific method.*") (emphasis added); GALILEO'S REVENGE, *supra* note 5, at 210 ("All science – all *real* science – contains what Karl Popper called "stopping rules." Statements of scientific fact are statements that could be systematically shown to be false (if they are false) after some finite, circumscribed inquiry."); Brian Stuart Koukoutchos, *Solomon Meets Galileo (And Isn't Quite Sure What to Do with Him)*, 15 CARDOZO L. REV. 2237, 2243 (1994); Arvin Maskin, *The Impact of Daubert on the Admissibility of Scientific Evidence: The Supreme Court Catches up with a Decade of Jurisprudence*, 15 CARDOZO L. REV. 1929, 1942 (1994).

<sup>52</sup> Faigman, *supra* note 43, at 560, 579.

<sup>53</sup> This problem has been discussed at some length by STEVEN YEARLEY, *THE GREEN CASE: A SOCIOLOGY OF ENVIRONMENTAL ISSUES, ARGUMENTS AND POLITICS* (1991).

¶ 30 But beyond this, there is more than twenty-five years of work in the history, philosophy, and sociology of science<sup>54</sup> investigating how the judgments made by scientists are not determined by any kind of over-arching scientific method.<sup>55</sup> A significant theme in these studies is the important role played by the socialization of scientists working in a “paradigm” or research tradition. Rather than working according to a universal scientific method, scientists make judgments according to standards of measurement, ways of reporting and evaluating results, ideal problem solutions, and particular types of experimental practices. While some of these practices are specified in ideal terms in textbooks, they are more often the components of craft, or tacit knowledge, over which there is a negotiated consensus for given times and places during settled periods of science. This “consensus” is not fixed and, because it is built on tacit knowledge, does not constitute any kind of simple, reducible algorithm against which “good” or “junk” science can be evaluated. Judgments as to what constitutes “good” versus “junk” science are sometimes made, but they are social judgments open to dispute and negotiation, and are affected by considerations such as the status of relevant scientists, their research backgrounds, and both their narrow career interests and responses to broader social pressures.<sup>56</sup>

¶ 31 The flexible and tacit nature of definitions of science within a particular paradigm or research tradition dictates against simple notions of a single, identifiable scientific method. If we further consider the numerous research traditions and paradigms making up the rich diversity of modern “science,” each with its own standard of scientific adequacy, the notion that there exists an operationally viable “scientific method” useful across them all, and applicable by nonscientists such as lawyers and judges, looks decidedly fragile.<sup>57</sup>

¶ 32 In their references to method, many legal commentators – including judges – cite Karl Popper’s doctrine of falsification as an important ingredient in understanding scientific method.<sup>58</sup> But, falsification has been subjected to a number of criticisms.<sup>59</sup> Philosophers criticize falsification because to falsify a theory conclusively we must rely upon observations which are, as Popper noted, fallible and

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<sup>54</sup> KUHN, *supra* note 35, *passim*. It is important to note that while Kuhn’s work has been extremely significant in re-shaping the way in which many issues in the history and philosophy of science (HPS) and the sociology of scientific knowledge (SSK) are addressed, a number of his more specific ideas such as paradigm and incommensurability have been significantly modified. Kuhn represents an important starting point to a tradition rather than providing all the tools used by practitioners of HPS/SSK. BARRY BARNES, T.S. KUHN AND SOCIAL SCIENCE (1982). “This view is contrary to currently influential belief about advances in scientific thinking and the role of the individual thinker.” *Id.* See also THOMAS KUHN, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS (1970).

<sup>55</sup> Examples of case studies include: THE POLITICS AND RHETORIC OF SCIENTIFIC METHOD: HISTORICAL STUDIES (John A. Schuster & Richard R. Yeo eds., 1986); HARRY M. COLLINS & TREVOR PINCH, THE GOLEM: WHAT EVERYONE SHOULD KNOW ABOUT SCIENCE (1993).

<sup>56</sup> See NIGEL GILBERT & MICHAEL MULKAY, OPENING PANDORA’S BOX (1984); Thomas F. Gieryn, *Boundaries of Science*, in HANDBOOK OF SCIENCE AND TECHNOLOGY STUDIES 393, 403-04 (Sheila Jasanoff et al. eds., 1995). See also Harry Collins & Trevor Pinch, *Construction of the Paranormal: Nothing Unscientific Is Happening in ON THE MARGINS OF SCIENCE: THE SOCIAL CONSTRUCTION OF REJECTED KNOWLEDGE*, 27 SOC. REV. MONOGRAPH 237 (Roy Wallis ed., 1979).

<sup>57</sup> See generally BARRY BARNES, ABOUT SCIENCE (1985); HARRY M. COLLINS, CHANGING ORDER: REPLICATION AND INDUCTION IN SCIENTIFIC PRACTICE (1985); RAVETZ, *supra* note 30.

<sup>58</sup> See, e.g., *Daubert v. Merrell Dow Pharm., Inc.*, 509 U.S. 579 (1993); STEVEN GOLDBERG, CULTURE CLASH: LAW AND SCIENCE IN AMERICA 8 (1994); Michael D. Green, *Expert Witnesses and Sufficiency of Evidence in Toxic Substances Litigation: The Legacy of Agent Orange and Bendectin Litigation*, 86 NW. U. L. REV. 643 (1992); Baughman, *supra* note 43, at 1616; Klein, *supra* note 47, at 2230; Schuck, *supra* note 43, at 4, 16, 18.

<sup>59</sup> See Gary Edmond & David Mercer, *What Judges Should Know About Falsificationism*, 5 Exp. Evid. 29 (1997); O’Connor, *supra* note 21; Adina Schwartz, *A “Dogma of Empiricism” Revisited: Daubert v. Merrell Dow Pharmaceuticals, Inc. and the Need to Resurrect the Philosophical Insight of Frye v. United States*, 10 HARV. J.L. & TECH. 149 (1997).

open to revision.<sup>60</sup> A further critique emphasizes Popper's extremely naïve view of testing, which fails to account for the uncertainties and complexities of real world test situations. Thus, while it may appear that a theory is being disproved by a negative test result or failed prediction, it is always possible that part of the test situation itself might be the source of problems and not the theory.<sup>61</sup>

¶ 33 A number of case studies in the history, philosophy, and sociology of science illustrate that when testing and replication are exposed to close empirical and philosophical scrutiny they are revealed as complex processes relying on tacit judgments and social negotiation.<sup>62</sup> The "facts" of a test situation "do not speak for themselves" and require a context and considerable interpretation.<sup>63</sup>

¶ 34 An example of this is the contemporary debate surrounding the health and safety of EMF.<sup>64</sup> The parties to the dispute are highly scientifically polarized. What is for one side a convincing experimental test enhancing the plausibility of links between EMFs and ill health is, to the other side, an unconvincing experimental error or artifact. One central experiment involves the movement of calcium ions across the cell membrane of brain cells in tissue cultures exposed to weak EMFs. This effect is observed to occur with exposure to field levels putatively too weak to cause heating or other direct energy effect on the cells. Further, the effect appears to be "information related," i.e., responsiveness seems to be dependent on wave form and frequency rather than the strength of field.<sup>65</sup> The experiment is important because, if accepted, it challenges the scientific rationale on which most EMF health and safety regulations have been framed – that the only relevant biological effects of EMF are those linked to direct energy transfer to living things such as electrocution or heating.<sup>66</sup>

¶ 35 Supporters of this scientific rationale reject the relevance of the calcium movement studies on numerous grounds. Some argue that the effect defies the laws of physics, and, in the absence of a plausible alternative physical explanation, must be the result of some yet-to-be-identified failure in experimental control (magnite contamination of tissue cultures being one suspect).<sup>67</sup> Alternatively, the argument is that the researchers who identified the effect are guilty of self-deception, a plausible charge given the emotionally charged nature of the debate and the subtle measurements involved. Still others have argued that the effect may exist, but it is impossible to extrapolate from the laboratory to the "real" world. They argue that the experiment provides no evidence for the movement of "biologically relevant calcium."<sup>68</sup>

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<sup>60</sup> ALAN CHALMERS, *WHAT IS THIS THING CALLED SCIENCE?* 60 (1982).

<sup>61</sup> *See Id.* at 61; *see also* ALBURY, *supra* note 39; DAVID OLDROYD, *THE ARCH OF KNOWLEDGE* (1986); Imre Lakatos, *Falsification and the Methodology of Scientific Research Programmes*, in *CRITICISM AND THE GROWTH OF KNOWLEDGE* (Imre Lakatos & Alan Musgrave eds., 1970).

<sup>62</sup> *See* Harry M. Collins, *The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics*, 9 *SOC.* 205 (1975); YEARLEY, *supra* note 53; Collins & Pinch, *supra* note 56.

<sup>63</sup> JOHN A. SCHUSTER, *AN INTRODUCTION TO THE HISTORY AND SOCIAL STUDIES OF SCIENCE* (1995).

<sup>64</sup> *See* David W. Mercer, *The NIEMR/EMF Controversy: The Social Construction of Scientific Knowledge and Science Policy in the 'Gibbs' Powerline Inquiry 1990/91* (1993) (unpublished Ph.D. dissertation, University of Wollongong (Australia) (on file with author); *see also* Foster et al., *Science and the Toxic Tort*, *supra* note 5; LES DALTON, *RADIATION EXPOSURES* (1991).

<sup>65</sup> *See* W. Ross Adey, *The Energy Around Us*, *THE SCIENCES.*, Jan.-Feb. 1986, at 52.

<sup>66</sup> *See* DALTON, *supra* note 64, at 8-9.

<sup>67</sup> *See* WILLIAM BENNETT, *HEALTH AND LOW-FREQUENCY ELECTROMAGNETIC FIELDS* 12-16, 136-39 (1994).

<sup>68</sup> *See* ELECTRICITY COMMISSION OF NSW (ELCOM), *SUBMISSION TO INQUIRY INTO COMMUNITY NEEDS AND HIGH VOLTAGE TRANSMISSION LINE DEVELOPMENT* (1990).

¶ 36 On the other side, proponents of the calcium movement experiment argue that the deconstruction of their experimental work is politically motivated. The enormous economic implications of stricter EMF regulation means that critics find it easier to get their work funded and published.<sup>69</sup> Proponents also argue that the narrow specialization of biophysics means that many of their critics simply do not have the appropriate training to understand the sophisticated bioelectromagnetic processes involved.<sup>70</sup> All these claims and counter claims are also set against debates about the status of various researchers.<sup>71</sup>

¶ 37 The EMF debate indicates how unhelpful Popper's abstract theorizing is to living scientific debates. The calcium movement experiment is unable to unilaterally determine the issues at stake. The complex test situation is prone to deconstruction and differing assessments of its significance, due to webs of subsidiary hypotheses and the competing theoretical vantage points of its antagonists. Moreover, a Popperian gloss can be used by either side to give their claims authority. For the proponents of the calcium movement experiment, traditional "energy" viewpoints of EMF have been falsified and a new branch of science opened. For the more dismissive opponents, the hypothesis is not framed in sufficiently clear physical terms (according to their understanding of the laws of physics) to be properly falsifiable, and is therefore unscientific.

¶ 38 As a philosophy of method, falsificationism glosses over important dynamics involved in the development of scientific knowledge. Many historians and philosophers of science have long noted that every area of scientific knowledge has its own deeply embedded basic sets of assumptions that are not open to falsification. Such baseline "metaphysics" involve implicit theories of causality, standards of proof, laws, models, and so on.<sup>72</sup>

¶ 39 While judgments about what counts as science in practice are tacit and flexible, scientists still utilize the term "scientific method" in some contexts. It is used as part of a flexible rhetoric of justification.<sup>73</sup> Two important areas where this occurs are in scientific controversies and boundary disputes<sup>74</sup> between "fringe" and "orthodox" science.<sup>75</sup> In the context of scientific controversies, the normally "taken for granted" social negotiation and resolution of interpretive flexibility become more "visible." Harry Collins has described the virtue of investigating controversies in terms of the metaphor of looking at the construction of ships in bottles:

[I]t is only by examining scientific controversies while they are in progress that the mechanism by which ships (scientific findings) get in to bottles (validity) can be understood. If this process is not seen in operation it may

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<sup>69</sup> See BRODEUR, *supra* note 41.

<sup>70</sup> See IAN MACMILLAN, ELECTROMAGNETIC FIELDS, ELECTRIC POWER AND PUBLIC HEALTH (1987); *Science Scope – EPA: Physicists Unwelcome on EMF Panel*, 251 SCI. 863 (David Hamilton ed., 1991).

<sup>71</sup> See Mercer, *supra* note 64.

<sup>72</sup> See EDWIN A. BURTT, THE METAPHYSICAL FOUNDATIONS OF MODERN SCIENCE (1932); see also KUHN, *supra* note 35; ALEXANDER KOYRÉ, METAPHYSICS AND MEASUREMENT: ESSAYS IN THE SCIENTIFIC REVOLUTION (1968).

<sup>73</sup> Compare Schuck, *supra* note 43, at 31 ("Rhetorical strategies that would be professionally unacceptable in science and often in law – explicit appeals to sentiment, ideology, or interest – are standard tactics in politics, where the 'mobilization of bias' is both normal and normative.")

<sup>74</sup> See Gieryn, *supra* note 56.

<sup>75</sup> It is important to remember that these labels are flexible, dynamic and open to negotiation.

be thought that ships were always in the bottles, and that all scientists did find them ready assembled, as it were.<sup>76</sup>

¶ 40 A number of rhetorical patterns using ideals of the scientific method have been observed operating in scientific controversies. Most important, are the various ways scientists have used “flexible evaluative repertoires;” that is, the use of flexible vocabularies for describing their own work relative to their opponents according to different social contexts and various social interests.<sup>77</sup> For example, in evaluation of the rhetoric used by scientists in a controversy in bio-chemistry, Mulkay observed a consistent pattern of a dual conception of what constituted a scientific “fact.”

This strategy . . . seems to be related to the dual conception of scientific fact which has appeared in every letter so far. The interpretative conception of “fact” is used in criticizing one’s opponent. The interpretative basis of the latter’s view is made visible and emphasized as the author formulates the inconsistencies, uncertainties, and mistakes perpetrated by his opponent. It is always possible for the author to find such errors because the opponent’s claims are inevitably assessed in relation to the authors’ different conception of the facts and their scientific meaning. In contrast, when formulating his own views, each author minimizes the interpretative work apparently involved. As a result, each author’s position comes to appear in the text of each separate letter as indistinguishable from the observable realities of the biochemical world.<sup>78</sup>

¶ 41 In basic terms, a tendency has been observed for scientists, in the setting of controversy, to deploy rhetoric to suggest how their scientific findings are isomorphic to nature, constituted by the application of “appropriate” scientific practices (the so-called “constitutive forum”). Rival scientific work, on the other hand, can be explained as the by-product of social contingencies (the so-called “contingent forum”).<sup>79</sup>

¶ 42 The use of scientific method as part of a flexible evaluative repertoire is observed to follow similar patterns in debates between “orthodox” and “fringe” sciences.<sup>80</sup> In a study of para-psychology and its relationship to orthodox science, researchers noted that the manner by which para-psychology “scientific claims” were rejected by orthodox science did not follow the pattern of the imposition of some kind of clear demarcation criteria. While para-psychologists placed considerable effort into framing their claims via experimental methods, ideas of proof, and mimicking communication processes and the institutional trappings of orthodox science (e.g. peer review), their claims were still largely rejected by orthodox scientists on a variety of grounds spanning the contingent and constitutive forums.<sup>81</sup> For example, some scientists rejected the claims of para-psychologists as

<sup>76</sup> Collins, *supra* note 62, at 45.

<sup>77</sup> See Michael Mulkay & Nigel Gilbert, *Accounting for Error: How Scientists Construct their Social World when they Account for Correct and Incorrect Belief*, 16 *SOCIOLOGY* 165 (1982); *Warranting Scientific Belief*, 12 *SOC. STUD. OF SCI.* 383 (1982).

<sup>78</sup> MICHAEL MULKAY, *THE WORLD AND THE WORD* 43 (1985).

<sup>79</sup> Collins & Pinch, *supra* note 56, at 237-70; MULKAY, *supra* note 44.

<sup>80</sup> We are not attempting to make a judgment about what should or should not be regarded as legitimate science. Rather these are social actors’ categories.

<sup>81</sup> While still largely rejected there was some evidence that taking on the institutional trappings of science did provide some kind of enhancement of the scientific status, even if not decisive. Note that legal commentators are generally dismissive of what they describe as ‘junk’ and ‘pseudo sciences’ without clearly demonstrating faults in their underlying methodologies. See, e.g., Faigman et al., *supra* note 14, at 1801 (“The Court has served notice that experts should trade in their crystal balls for electron microscopes.”); Koukoutchos, *supra* note 51, at 2244 (“And adherence to the consensus on methods and fundamental principles is all that is required to eliminate risk of confusing science with alchemy, astrology, or creationism.”).

uninteresting examples of empty correlations unworthy of further research. In other contexts, empirical work of para-psychologists was rejected on the basis that the theory underlying the work was unconvincing. Finally, even when some results of experimental work were accepted as conforming to existing standards in probability theory, it was argued that such results should be inadmissible on the basis that given the wider validity of probability theory in recognized “scientific contexts” the positive results should be interpreted as representing the by-product of experimental error, fraud, or self-deception.<sup>82</sup> Ideas of scientific method, then, used in demarcating orthodox science from fringe, “non-science,” are not strongly influenced by mechanical appeals to doctrines of scientific method. Rather, they come into play when the objects of discourse are seen as intuitively implausible and unacceptable – given the tacit judgments of the relevant fields of science. In some contexts even the “relevant” field of science will also be open to challenge.<sup>83</sup>

¶ 43 A final point that mitigates against a universal scientific method is provided by evidence that scientists rarely reflect on scientific method – in philosophical terms – in their day-to-day work. Barnes, for instance, notes surveys showing a lack of formal philosophical literacy amongst working scientists,<sup>84</sup> and Mulkay and Gilbert note the inconsistent meanings attached to the philosophies of science in those instances when scientists emphasized the importance of scientific method.<sup>85</sup> As Yearley ironically notes, classes on Popper and Lakatos and how to apply the scientific method are much more common in economics, sociology, and psychology than they are in chemistry.<sup>86</sup>

¶ 44 Overall, the above discussion shows that norms and scientific method are better understood as part of the professional rhetoric of science; operating as a vocabulary used to describe, in simplified terms, the ideal workings of science, rather than as rules or explicit guides to demarcate “good” from “bad” science.<sup>87</sup> This rhetoric is most often deployed in contexts where the symbolic authority of science, more generally, is at stake: scientific controversies, educational contexts, legal contexts, and science “popularizations.”<sup>88</sup> The closer the empirical focus on the actual workings of science, and the more current and uncertain the area of science examined, the more difficult it is to identify simple ideal models of methods and norms.

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<sup>82</sup> MULKAY, *supra* note 44, at 113-118.

<sup>83</sup> See TREVOR PINCH & HARRY COLLINS, *FRAMES OF MEANING: THE SOCIAL CONSTRUCTION OF EXTRAORDINARY SCIENCE* (1982); DAVID HESS, *SCIENCE IN THE NEW AGE: THE PARANORMAL, ITS DEFENDERS AND DEBUNKERS, AND AMERICAN CULTURE* (1993).

<sup>84</sup> BARNES, *supra* note 57. This does not discount the contribution to the philosophy of science by eminent scientific figures such as Hiesenberg and Mach. Compare OLDROYD, *supra* note 61.

<sup>85</sup> Michael Mulkay & Nigel Gilbert, *Putting Philosophy to Work: Karl Popper's Influence on Scientific Practice*, 11 *PHIL. OF THE SOC. SCI.* 389 (1981). See, e.g., PETER MEDAWAR, *ADVICE TO A YOUNG SCIENTIST* (1979).

<sup>86</sup> Steven Yearley, *Understanding Science from the Perspective of the Sociology of Scientific Knowledge – An Overview*, 3 *PUB. UNDERSTANDING OF SCI.* 245, 249 (1994). For similar observations see CHALMERS, *supra* note 60. The popularity of Popper with law students at the London School of Economics was given added support through a discussion with Dr. A. Taylor. It is ironic then that commentators have suggested that “judges must achieve at least a basic level of literacy” when this so-called “literacy” in its philosophical guise is not part of conventional scientific training. See Miller et al., *supra* note 21, at 254.

<sup>87</sup> See Part III(D), *supra*.

<sup>88</sup> It is worth noting that a relatively small number of elite scientists dominate public discourse on science and possess a disproportionate influence over packaging the images of science presented to the public. See, e.g., STUART BLUME, *TOWARDS A POLITICAL SOCIOLOGY OF SCIENCE* (1974); RAE GOODELL, *THE VISIBLE SCIENTISTS* (1977); DOROTHY NELKIN, *SELLING SCIENCE: HOW THE PRESS COVERS SCIENCE AND TECHNOLOGY* (rev. ed. 1995); Rae Goodell, *The Role of the Mass Media in Scientific Controversy*, in *SCIENTIFIC CONTROVERSIES: CASE STUDIES IN THE RESOLUTION AND CLOSURE OF DISPUTES IN SCIENCE AND TECHNOLOGY* 585 (H. Tristram Engelhardt & Arthur L. Caplan eds., 1987).

F. *Is “Junk Science” a Product of Legal Distortion?*

¶ 45 One of the central themes of the junk science model is that junk science is promoted, encouraged, and perhaps even caused, by the legal distortion of the “normal” processes of science.<sup>89</sup> Images of legal distortion rely upon an excessively simplistic image of the law-science relationship that can be criticized on a number of grounds.

1. *Do Legal Systems Amplify Pre-Existing Scientific Disagreements?*

¶ 46 One explanation for the emergence of junk science is that it is created by legal pressures that also produce artificial scientific disagreements. The suggestion is that without legal-political pressures the scientific community would be able to weed out deviant junk science claims. While proponents of the junk science model acknowledge that science lacks absolute certainty, they usually contend that various mechanisms – stopping rules, proper method, peer review, and consensus formation<sup>90</sup> – enable modern science to avoid the intransigent conflicts that mark the legal arena (a domain “infected” by “junk science” claims).

¶ 47 While the legal-political contexts involved in mass tort litigation can promote and shape scientific disagreements, proponents go too far in implying that such disagreements are uncommon outside of legal contexts. “Disagreements” in science do not require explanations that rely on theories of intrinsic “epistemological distortion” caused by the entry of social factors into the “epistemologically pristine” domain of science. Rather than exemplify instances of “epistemological distortion” (i.e. junk science), scientific disagreements occur for a variety of reasons. Two examples are “internal” pressures within sub-cultures of scientists from shifts in “research traditions” and “paradigms,”<sup>91</sup> and competition between scientists for funding and social authority.<sup>92</sup> The latter often occurs within and across various branches of science in response to social demands for authoritative explanations of natural phenomena – particularly in areas where there is a lack of settled knowledge, or well defined boundaries of relevant expertise.<sup>93</sup> From this perspective, scientific disagreement should be analyzed as part of the diversity of processes of science, and not as something to be explained away as epistemologically atypical cases of junk science.

¶ 48 Following this framework, we should also avoid “explaining away,” as an *a priori* epistemological problem, the presence of scientific disagreement in legal contexts. In some instances, a legal setting may be drawing on pre-existing scientific disagreement, yet, in others there may be special features of the legal setting itself which are contributing to the disagreement in question. Scientific disagreements in legal settings should be empirically investigated with consideration of the particulars of the scientific knowledge claims in question, the specific features of the legal

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<sup>89</sup> See, e.g., GALILEO’S REVENGE, *supra* note 5, at 2 (“Maverick scientists shunned by their reputable colleagues have been embraced by lawyers.”); *id.* at 3 (“Junk science is impelled through our courts by a mix of opportunity and incentive. ‘Let-it-all-in’ legal theory creates the opportunity. The incentive is money . . . .”); Marc Galanter, *Predators and Parasites: Lawyer-Bashing and Civil Justice*, 28 GA. L. REV. 633, 652 (1994).

<sup>90</sup> See GALILEO’S REVENGE, *supra* note 5.

<sup>91</sup> See, e.g., CAROL A. G. JONES, EXPERT WITNESSES: SCIENCE, MEDICINE AND THE PRACTICE OF LAW 270 (1994); WYNNE, *supra* note 47, at 135; Brian Wynne, *Establishing the Rules of Laws: Constructing Expert Authority*, in EXPERT EVIDENCE: INTERPRETING SCIENCE IN THE LAW 38 (Roger Smith & Brian Wynne, eds., 1989).

<sup>92</sup> See, e.g., ALBURY, *supra* note 39; MULKAY, *supra* note 44, at 113-118.

<sup>93</sup> See, e.g., YEARLEY, *supra* note 53; Gieryn, *supra* note 56.

setting in question, and the specific way science and law have been brought together. This does not mean that it is impossible to make generalizations about science, law, and their relationship, but that such generalizations should not be built on the basis of *a priori* epistemological categories such as junk science.

## 2. *Deconstruction Rather Than Distortion*

¶ 49 Proceeding from the contextual view of the law-science relationship outlined above, writers such as Wynne,<sup>94</sup> Jasanoff,<sup>95</sup> and Yearley<sup>96</sup> provide a more nuanced image of law-science interactions than the simplistic junk science model. Rather than emphasizing legal distortion, such writers focus on the “legal deconstruction” of science. The slippage between ideal images of science and the messy realities of scientific practice provides a particularly fertile source for the legal “deconstruction” of science – especially in adversarial settings. In such contexts, the work of scientists and their knowledge claims can be measured against standards of conduct and proof provided by ideal images of scientific norms and method. By juxtaposing these idealized images against revelations of the more craft-based nature of scientific work, as well as the socially contingent status of scientific knowledge claims, an interpretative space is created for the deconstruction of scientific authority: “Scientists are constantly at risk of being hoist by their own positivist petard.”<sup>97</sup>

¶ 50 A widely quoted example of these processes can be found in the work of Oteri, Weinberg, and Pinales,<sup>98</sup> on the cross-examination of chemists in drug cases, outlining the specific way an expert’s authority can be thrown into doubt. They note that the lawyer may: challenge whether or not the qualifications of the chemist neatly match the practical issue at stake;<sup>99</sup> highlight the variations between the methods used in various drug tests;<sup>100</sup> or introduce evidence whether the chemist relied on hearsay from other researchers rather than personally testing the specific substance at hand.<sup>101</sup> Furthermore, some tests may be performed which have a strong empirical background, but an absence of deeper theoretical basis for the underlying processes involved. Such tests may be widely accepted by convention, even though they rely on numerous taken-for-granted assumptions. Additional considerations might be that the tests are not the most accurate, but rather have been chosen because they are cheaper, quicker, or easier to perform.

¶ 51 Scientific experts pitted against one another in a legally mediated environment, where scientific knowledge claims are frequently exposed to intense skepticism, are hardly in a position to acknowledge the more informal processes involved in the construction of scientific knowledge. Ironically, maintaining their authority in such

<sup>94</sup> WYNNE, *supra* note 47, at 120-137; Wynne, *supra* note 91.

<sup>95</sup> Sheila Jasanoff, *What Judges Should Know About the Sociology of Science*, 32 JURIMETRICS J. 345 (1992); JASANOFF, *supra* note 9.

<sup>96</sup> YEARLEY, *supra* note 53, at 140-143.

<sup>97</sup> JONES, *supra* note 91, at 270.

<sup>98</sup> J.S. OTERI, M.G. WEINBERG & M.S. PINALES, *Cross Examination of Chemists, in DRUGS CASES, IN SCIENCE IN CONTEXT: READINGS IN THE HISTORY AND PHILOSOPHY OF SCIENCE* 250 (Barry Barnes & David Edge eds., 1982).

<sup>99</sup> *Id.* at 251.

<sup>100</sup> *Id.* at 253.

<sup>101</sup> See, e.g., MARK ARONSON & JILL HUNTER, LITIGATION: EVIDENCE & PROCEDURE 971 (1995) (“[w]orking with hearsay is one of the hallmarks of most areas of expertise.”). See also JONES, *supra* note 91, at 106-10; WYNNE, *supra* note 47, at 132 (“scientific knowledge is diffused by social transmission via established authority patterns rather than by independent testing.”).

settings is one of the very things that leaves their scientific work open to deconstruction. Wynne develops these themes to critique the various ways artificial images of rationality foreclose a better understanding of the law-science relationship. He posits that one of the reasons the more craft and tacit-based knowledges of science are not openly acknowledged in legal contexts is that the legal system boosts its own social authority by nurturing a self-image of legal practice similar to the ideal image of science.<sup>102</sup> The ideal self-images of legal thought and practice emphasize the possibility that the legal system can transcend political and personal biases to ensure the optimal rational outcomes in conflict resolution, given the constraints of formal law, via the objective discovery of facts and impersonal application of rules.<sup>103</sup> This image has notable similarities to that of defining science according to its possession of behavioral “norms” and the application of the scientific method. Recognition that legal forms of knowledge and assessment, like science, rely on various tacit and contingent judgments could weaken legal claims for social authority. It is, in a sense, structurally difficult for each body of practice/discourse to acknowledge the more localized features relevant to the framing and negotiation of both scientific and legal knowledge.

¶ 52 In sociological terms much of this tension in the law-science relationship involves the process described by Gieryn as “boundary work” – the processes involved when subcultures attempt to establish claims about the scope, extent, and application of their expertise and preferred professional image to outsiders.<sup>104</sup> It is not surprising that in contexts where law and science are brought together there is little recognition of the contingent nature of the application of rules, or the divergence between images of rationality and the actual processes involved in the construction of knowledge.

¶ 53 Such unremitting skepticism is not the only way in which the legal context provides a restrictive framework for scientists. In addition, scientists are often called upon to answer problems that do not neatly mesh with any pre-defined body of scientific expertise, work with unfamiliar time constraints, and accept that their knowledge claims will be “reconstituted” into legally tractable terms.<sup>105</sup>

¶ 54 On this latter point it is worth noting that scientists have not always been willing to accept the re-working of their knowledge into legally tractable terms. For example, many scientists were unwilling to participate in, or accept the authority of, “science courts.” Originally proposed in 1976 by a White House Task Force, these courts were designed to work through three phases of problem solving: first, identify significant questions of science and technology associated with the issue in

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<sup>102</sup> WYNNE, *supra* note 47, at 129.

<sup>103</sup> This image has been open to criticism from the time of the American Realists and openly explored in the writings by more contemporary critical scholars. A sample includes: Alan D. Freeman, *Truth and Mystification in Legal Scholarship*, 90 YALE L.J. 1229 (1981); Robert Gordon, *Critical Legal Histories*, 36 STAN. L. REV. 57, 125 (1984); Mark G. Kelman, *Trashing* 36 STAN. L. REV. 293 (1984); Vicki Quade, *Are Lawyers Really Necessary?: Barrister Interview with Duncan Kennedy*, BARRISTER, Fall, 1987, at 10; Joseph Singer, *The Player and the Cards: Nihilism and Legal Theory*, 94 YALE L.J. 1 (1984); Jeffrey A. Standen, *Critical Legal Studies as an Anti-Positivist Phenomenon*, 72 VA. L. REV. 983 (1986); Mark Tushnet, *Legal Scholarship: Its Causes and Cures*, 90 YALE L.J. 1205 (1981).

<sup>104</sup> See Gieryn, *supra* note 56; see also Jasanoff, *supra* note 95. See generally Shana M. Solomon & Edward J. Hackett, *Setting Boundaries Between Science and Law: Lessons from Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 21 SCI. TECH. & HUM. VALUES 131 (1996) (using the *Daubert* case to illustrate how courts determine judicial validity).

<sup>105</sup> See, e.g., JONES, *supra* note 91, at 194-223 (examining the work of forensic pathologists and forensic scientists to show how they influence and are influenced by the conviction process). See generally LAW AND SCIENCE IN COLLABORATION: RESOLVING REGULATORY ISSUES OF SCIENCE AND TECHNOLOGY (J. D. Nyhart & Milton M. Carrow eds., 1983) (presenting case studies designed to suggest a regulatory framework for practical scientific and technological decision making); Sanders, *supra* note 9, at 3 (describing tractability in terms of “translation”).

question, leaving ethical/political questions for later consideration; second, establish an adversary proceeding, presided over by scientist-judges, where scientific experts would testify under the cross-examination of science advocates; and third, adjudicate the scientific facts pertaining to a disputed technical question.<sup>106</sup>

¶ 55 A leading proponent of science courts, Alan Mazur, attempted to set up a science court/adversary proceeding in the wake of the New York Public Service Powerline Inquiry.<sup>107</sup> Mazur contacted two leading proponents of linkages between health problems and EMF, Marino and Becker, and a number of experts backing the power utilities. He asked the various proponents to put forward their key claims as clearly as possible, receiving early assent from Marino and Becker. Acting as referee, Mazur recommended ways that Becker and Marino could modify their claims to free them from ambiguous, unfalsifiable assertions and “value judgments.” While this was eventually done to Mazur’s satisfaction, the experts for the power utilities ultimately scuttled the project by refusing to co-operate with him – arguing that any response on their part gave their opponents’ claims false credibility.<sup>108</sup>

¶ 56 Yet, some authors disagree with the idea that the legal deconstruction of scientific knowledge is detrimental. Sheila Jasanoff, for instance, highlights the important role litigation plays in improving the technologies involved in DNA typing.<sup>109</sup> She argues that the prevalence of litigation in areas involving risk and new technologies provides an important source for civic education and public participation in the development of science and technology.<sup>110</sup> The important role of the legal deconstruction of scientific and technical claims will be discussed in more detail later in our section on junk science and the public perception of risk.<sup>111</sup>

¶ 57 The notion that law-science tensions arise simply from the proclivity of legal contexts to provide epistemologically “impure” forums ripe for the emergence of “junk science,” fails to capture the intricacies involved. The junk science model diverts attention from the real tensions presented by the differences among instrumental objectives, sub-cultural values, cognitive processes, and decision-making settings of various branches of science and the law. Rather than encourage

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<sup>106</sup> See Barry Caspar & Paul Wellstone, *Science Court on Trial in Minnesota*, in SCIENCE IN CONTEXT 282-83 (Barry Barnes & David Edge eds., 1982); see also Arthur Kantrowitz, *Democracy and Technology*, in SCIENCE, TECHNOLOGY AND THE HUMAN PROSPECT 199 (Chauncy Starr & Philip C. Ritterbush eds., 1979) (explaining his proposal of a science court, its procedures, and grounds for its implementation); Roger D. Masters & Arthur R. Kantrowitz, *Scientific Adversary Procedures: The SDI Experiments at Dartmouth*, in TECHNOLOGY AND POLITICS (Michael E. Kraft & Norman J. Vig eds., 1988) (evaluating the concept of Scientific Adversary Procedures, an alternative to the original proposal of a science court, as applied to the debate over the Strategic Defense Initiative (“Star Wars”)).

<sup>107</sup> See ALLAN MAZUR, *THE DYNAMICS OF TECHNICAL CONTROVERSY* 34-42 (1981); Allan Mazur, Andrew A. Marino & Robert O. Becker, *Separating Factual Disputes from Value Disputes in Controversies over Technology*, 1 TECH. IN SOC. 229, 233-34 (1979).

<sup>108</sup> See MAZUR, *supra* note 107, at 41-42. In a similar vein science court proposals have received criticism for assuming that the use of court-like procedures would be able to separate scientific facts from social preconceptions. One problem is that for a scientist to gain sufficient scientific authority to pronounce in an authoritative way on a matter of scientific controversy, such a scientist is normally already a participant in the controversy in question. Selecting “scientist-judges” or “experts” who possess scientific authority but are not simultaneously embroiled in the proceedings is difficult. Further, selection of scientist-judges without prior involvement may well lead to inconclusive, non-authoritative conclusions. See Mercer, *supra* note 64, at 295-312, 361-369. Alternatively a “decisive” but not scientifically authoritative conclusion may appear to be a technocratic imposition. See K. GUILD NICHOLS, ORGANIZATION FOR ECONOMIC CO-OPERATION AND DEVELOPMENT, *TECHNOLOGY ON TRIAL: PUBLIC PARTICIPATION IN DECISION-MAKING RELATED TO SCIENCE AND TECHNOLOGY* 98 (1979); see also David Mercer, *Understanding Scientific/Technical Controversy*, Science and Technology Policy Research Group, Occasional Paper No. 1 (November, 1996).

<sup>109</sup> JASANOFF, *supra* note 9, at 206, 208, 211; Jasanoff, *supra* note 95, at 349-53. For a discussion of the limits of Jasanoff’s account of civic education, see Gary Edmond & David Mercer, *Manifest Destiny: Law and Science in America*, 10 METASCIENCE 40, 52-57 (1996).

<sup>110</sup> See generally JASANOFF, *supra* note 9; Jasanoff, *supra* note 95.

<sup>111</sup> See Part III(H), *infra*.

detailed empirical policy development to improve the production of scientific and legal knowledges, the junk science model merely encourages “witch-hunting.”

### 3. *Law-Science Hybrids*

¶ 58 Another problem with the junk science model’s claim that legal-political contexts distort<sup>112</sup> science is that it glosses over the significant number of areas where science and law have been brought together in “hybrid” forms.<sup>113</sup> Such hybrids often experience difficulty locating and legitimating their knowledge claims in terms of the traditional professional rhetorics of law and science. Examples of hybrids include: forensic science, patent law, environmental regulation, and insanity laws. With increasing demands on governments to formulate authoritative public policy, certain branches of science and law have evolved together in close relationships.<sup>114</sup> Smith and Wynne describe this trend as follows: “At the same time as science’s role in legal processes expands, legal procedural models also begin to enter science . . . because science’s own rather informal procedural mechanisms are found inadequate for reaching authoritative truths in contentious policy issues.”<sup>115</sup>

¶ 59 This integration of science and law often operates more deeply than merely the specific settings of given legal proceedings. In fact the very constitution of some types of scientific knowledge can be shown to be shaped by the demands of legal/quasi-legal settings.<sup>116</sup> Smith and Wynne note that this integration appears at its most obvious when we consider fields of knowledge such as forensic pathology:

[I]t is not only the court room interaction that socially shapes knowledges: the institutional integration of a particular expert profession into the legal process already achieves this. Indeed, for forensic science and pathology, the legal process itself has created their particular type of professional interaction and expert knowledge. The social integration of forensic expertise with the law is such that forensic experts have learnt to reconcile themselves to the regular adversarial skepticism of legal processes, while maintaining the normal consensual discourses of scientific expertise. Whereas other disciplines may manage this by defining the court-room[sic]

<sup>112</sup> GALILEO’S REVENGE, *supra* note 5, at 19 (“You get a professor who earns \$60,000 a year and give him the opportunity to make a couple of hundred dollars in his spare time and he will jump at the chance . . . . They are like a bunch of hookers in June.”) (quoting Dennis Roberts).

<sup>113</sup> See, e.g., JASANOFF, *supra* note 9, at 1-24, 50-52 (providing background on the intersection of law and science and the increasing trend of law in inspiring innovative scientific studies and techniques); Alberto Cambrosio, Peter Keating & Michael MacKenzie, *Scientific Practice in the Courtroom: The Construction of Sociotechnical Identities in a Biochemical Patent Dispute*, 37 SOC. PROBS. 275 (1990) (analyzing expert testimony from patent litigation to show the interplay of social, economic and philosophical concerns with science in the courtroom); Roger Smith, *Forensic Pathology, Scientific Expertise, and the Criminal Law*, in *Expert Evidence: INTERPRETING SCIENCE IN THE LAW* 56 (Roger Smith & Brian Wynne, eds., 1989) (“[F]orensic experts form distinct occupational groups by virtue of combining scientific expertise with expertise in satisfying the requirements of legal or administrative settings.”) (footnote omitted). Cf. Christopher Arup, *Introduction*, 10(2) LAW IN CONTEXT 5 (1992) (“[W]e run the risk of reifying both law and science if we predicate a direct relationship between the two. . . . [L]aw and science only interact as part of the processes of the society at large and therefore in conjunction with such other social phenomena as gender and economy.”).

<sup>114</sup> Yaron Ezrahi, *The Authority of Science in Politics*, in *SCIENCE AND VALUES: PATTERNS OF TRADITION AND CHANGE* (Arnold Thackray & Everett Mendelsohn, eds., 1974).

<sup>115</sup> See *EXPERT EVIDENCE: INTERPRETING SCIENCE IN THE LAW 2* (Roger Smith & Brian Wynne eds., 1989); see also Sheila Jasanoff, *Beyond Epistemology: Relativism and Engagement in the Politics of Science*, 26 SOC. STUD. OF SCI. 393, 397 (1996) (employing the term “co-production” to capture the intricacies involved with knowledge construction and compatible situated social orders: “A full-blown political analysis of science and technology seeks to illuminate the ‘co-production’ of scientific and social order – that is, the production of mutually supporting forms of knowledge and forms of life – with all the details and specificity that such a project entails.”).

<sup>116</sup> See Jasanoff, *supra* note 115, at 403-09 (exploring how the *Daubert* court resolved controversies over the admissibility of scientific evidence at trial).

interaction as “unscientific,” this is not so easily available to forensic experts, because the courtroom is their ultimate professional arena.<sup>117</sup>

¶ 60 The development of hybrids reinforces our earlier contention that understanding law-science interactions requires a more finely nuanced empirical concern for the intricate ways science and law are brought together. It is far too easy to claim that hybrids are inadequate on the basis of exposing their genesis in social, economic, or technical needs, and comparing this to artificial, ideal images of science as an activity totally insulated from social contexts. The difficulties in distinguishing “good” from “junk” science, on the basis of the contexts in which the knowledge is developed, has been highlighted by the problems encountered in Bendectin birth defect litigation<sup>118</sup> following *Daubert v. Merrell Dow Pharmaceuticals, Inc.*<sup>119</sup> On remand from the Supreme Court, the Ninth Circuit asserted that forms of science created for litigation are inferior to other forms of scientific inquiry:

One very significant fact to be considered is whether the experts are proposing to testify about matters growing naturally out of research they have conducted independent of litigation, or whether they have developed their opinions expressly for purposes of testifying . . . in determining whether proposed expert testimony amounts to good science, we may not ignore the fact that a scientist’s normal workplace is the lab or the field, not the courtroom or the lawyer’s office.<sup>120</sup>

¶ 61 Yet, such a distinction may be practically unworkable due to the large number of institutionalized law-science hybrids. For instance, the court listed exceptions to its own rule – such as areas of forensic science – without any clear explanation as to why these forms of litigation-generated knowledge should be exempted.<sup>121</sup> We anticipate that the list of exemptions will grow (without attendant explanation), and is likely to be a source of political contention.

#### G. “Support” For the Junk Science Model From the History of Science

¶ 62 Junk science model proponents often draw upon the history of science to support their various contentions. Yet, much of this is self-serving and reflects little knowledge of history or science.<sup>122</sup> Peter Huber’s work is perhaps the best example of this, as he draws support for many of his ideas about junk science from the history of science.<sup>123</sup> However, in the examples Huber provides there is little

<sup>117</sup> EXPERT EVIDENCE: INTERPRETING SCIENCE IN THE LAW, *supra* note 115, at 15.

<sup>118</sup> Bendectin is an anti-nausea medication that was given to pregnant women during the 1960s. Plaintiffs in several law suits alleged that it caused serious birth defects. *See generally* Green, *supra* note 58.

<sup>119</sup> 509 U.S. 579 (1993).

<sup>120</sup> *Daubert v. Merrell Dow Pharm., Inc.*, 43 F.3d 1311, 1317 (9th Cir. 1995). *See also* *Daubert v. Merrell Dow Pharm., Inc.*, 951 F.2d 1128, 1131 n.3 (9th Cir. 1991) (arguing that scientific studies prepared in anticipation of litigation are not subject to the standard peer review process that scientific studies normally go through and are therefore less reliable), *vacated*, *Daubert v. Merrell Dow Pharm., Inc.*, 509 U.S. 579 (1993).

<sup>121</sup> This was duly noted by the Ninth Circuit Court of Appeals,

There are, of course, exceptions. Fingerprint analysis, voice recognition, DNA fingerprinting and a variety of other scientific endeavors closely tied to law enforcement may indeed have the courtroom as a principal theatre of operations. . . . As to such disciplines, the fact that the expert has developed an expertise principally for the purposes of litigation will obviously not be a substantial consideration.

43 F.3d 1311, 1317 n.5 (citation omitted).

<sup>122</sup> In fact, much of the “history” relied upon are popular myths with no basis in historical scholarship, as we shall see in the case of Peter Huber’s use of Galileo’s “martyrdom” for science.

<sup>123</sup> For example, Huber argues that the rise of refereed medical journals in 19th Century England was instrumental in moving the practice of medicine away from “bedside secrets” towards the investigation of  
(footnote continued)

evidence of understanding the context in which the scientists in question operated. Historical episodes function as vehicles to carry forward his polemical message, leaving him clearly guilty of an inaccurate, “whiggish” history of science.<sup>124</sup> Chesebro describes this problem well when he notes that:

[I]t may merely be a reflection that at the time these claims were made, even the best of scientists were not certain what could or could not cause cancer. In ridiculing yesterday’s court for having permitted yesterday’s scientists to testify about a theory that today’s scientists regard as false, Huber proves himself to be the classic Monday-morning quarterback, heaping scorn on yesterday’s scientists (and lawyers and judges) for not knowing then what everyone knows now.<sup>125</sup>

¶ 63 A more detailed knowledge of the history of science reveals that it is common for scientists to have strong commitments to their views during the early phases of new research. Evaluated in the context of their own time and place, the theories of Mendel, Kepler, Darwin, Newton, Galileo, and Copernicus relied upon leaps of faith and observations at the threshold of theoretical plausibility, together with the deliberate omission of counter arguments. But any of these efforts would qualify as “pathological science” in Huber’s schema.

¶ 64 Given the title of Huber’s book, it is certainly worth rescuing Galileo from his whig historical abuse in the junk science debate.<sup>126</sup> Galileo is used consistently throughout the law-science literature, and in particular by Huber and his main critic Chesebro, as an exemplar of the good scientist with radical new views who is suppressed by political authority. Of course, the title of Huber’s book – *Galileo’s Revenge* – alludes to the excessive leniency of modern courts toward the admission of novel scientific evidence for fear of suppressing a current-day Galileo.

¶ 65 But, there is a veritable Galileo industry in the history and philosophy of science which has investigated the complexities and intricacies of his conflict with the Church. The bulk of modern scholarship, however, portrays Galileo as a devout Catholic, ambitious natural philosopher, and cavalier political provocateur. Far from a simple showdown between knowledge and authority, it is now apparent that Galileo embroiled himself in theological debates clearly beyond the normally accepted concerns of a natural philosopher.<sup>127</sup> Indeed, Galileo ignored the

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statistical regularity, and then observes that “all modern science has similar origins.” Huber, *Junk Science*, *supra* note 5, at 739.

<sup>124</sup> Whig history was an approach to “history” captured succinctly by historian Herbert Butterfield as a study of:

[t]he past with reference to the present . . . [T]hrough this system of immediate reference to the present day, historical personages can easily and irresistibly be classed into men who furthered progress and men who tried to hinder it . . . [T]he whig historian stands on the summit of the twentieth century and organises his scheme of history from the point of view of his own day.

A. Rupert Hall, *On Whiggism*, 21 HIST. SCI. 45, 46 (1983), citing HERBERT BUTTERFIELD, *THE WHIG INTERPRETATION OF HISTORY* 11-13 (1931).

<sup>125</sup> Chesebro, *supra* note 4, at 1663.

<sup>126</sup> Examples of history of science with “Whiggish” overtones include: Black et al., *supra* note 20, at 779-80 (“His [Galileo’s] problem, however, was not with other scientists, and his treatment argues not for abandoning scientific standards but for separating the scientific process from both church and state control.”); Alan W. Tamarelli, *Daubert v. Merrell Dow Pharmaceuticals: Pushing the Limits of Scientific Reliability – The Questionable Wisdom of Abandoning the Peer Review Standard for Admitting Expert Testimony*, 47 VAND. L. REV. 1175, 1202 (1994) (“After all, some have argued that even the views of Einstein and Galileo were not immediately accepted, and that Copernicus was adjudged a heretic.”). Yet Copernicus’ major work, *de Revolutionibus* was not placed on the Catholic Index of prohibited books until 1616, 73 years after his death in 1543. See generally Owen Gingrich, *From Copernicus to Kepler: Heliocentrism as Model and as Reality*, 177 PROC. AM. PHIL. SOC’Y 513 (1973) (discussing Copernicanism). See also COLLINS & PINCH, *supra* note 55, at 27 (discussing relativity).

<sup>127</sup> “Natural philosopher” is the term for those who practiced one of the areas, along with medicine and mathematics, that might be considered the precursor of modern science. See David C. Lindberg, *Science and the* (footnote continued)

strongest scientific objections to his claims<sup>128</sup> and relied on observations and experiments at the threshold of detectability,<sup>129</sup> obtained through instruments, such as the telescope, which were new and poorly understood. Further, Galileo went beyond the academic conventions of his time (and ours) by avoiding the audience of his scientific peers in favor of “going directly to the public” and publishing in vernacular Italian rather than academic Latin.<sup>130</sup> Galileo’s activities clearly satisfy some of Huber’s main criteria for junk science:

[They] rely heavily on testimonials and anecdotes as evidence that their remedy is safe and effective . . . . [They] don the mantle of science while at the same time traducing the reputable scientists of their day. . . . [They] cite examples of physicians and scientists of the past who were forced to fight the rigid dogma of their day. . . . [T]hey do not use regular channels of communication, such as journals, for reporting scientific information, but rely instead on the mass media and word of mouth.<sup>131</sup>

. . .

The ultimate test of a scientist’s competence is her ability to publish in peer-reviewed journals. The ultimate test of her scientific integrity is her readiness to publish and be damned. *That* is the one real lesson judges should have learned from Galileo.<sup>132</sup>

¶ 66

The use of heroic figures from the history of science is not limited to Galileo. In a similar manner, Huber employs Newton’s embrace of alchemy as an example of a tendency among many great scientists to, sooner or later, embark on some foolish frolic. Huber fails to acknowledge the substantial historical discourse on Newton which posits that alchemy was not entirely an aberration in Newton’s scientific work, but was in many ways constitutive of it.<sup>133</sup> It is also worth noting that one of

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*Early Church*, in GOD AND NATURE: HISTORICAL ESSAYS ON THE ENCOUNTER BETWEEN CHRISTIANITY AND SCIENCE 19, 21 (David C. Lindberg & Ronald L. Numbers eds., 1986).

<sup>128</sup> Embodied in the “mainstream” contemporaneous work of fellow natural philosopher, Tycho Brahe, these included observations of the parallaxes of Mars and the sun inconsistent with Copernican systems (though later, Brahe appears to have contradicted his own studies on this point) and imperfect predictions of planetary movements based on the then-current Copernican models. A. Blair, *Tycho Brahe’s Critique of Copernicus and the Copernican System*, 51 J. HIST. IDEAS 355, 365-368 (1990).

<sup>129</sup> Huber emphasizes that junk science often depends on experiments at the threshold of detectability, with claims emerging from data that are selectively incomplete because wishful researchers unconsciously discard enough “bad” data to “find” a pattern in the remaining data. GALILEO’S REVENGE, *supra* note 5, at 27. See generally CHALMERS, *supra* note 60; I. BERNARD COHEN, THE BIRTH OF THE NEW PHYSICS (1961); BRIAN EASLEA, WITCH-HUNTING, MAGIC AND THE NEW PHILOSOPHY: AN INTRODUCTION TO DEBATES OF THE SCIENTIFIC REVOLUTION 1450-1750 (1980); PAUL FEYERABEND, AGAINST METHOD (1975); KOESTLER, *supra* note 46; THE POLITICS AND RHETORIC OF SCIENTIFIC METHOD: HISTORICAL STUDIES, *supra* note 55.

<sup>130</sup> FEYERABEND, *supra* note 129, at 141.

<sup>131</sup> GALILEO’S REVENGE, *supra* note 5, at 33, citing Ellis, *Clinical Ecology: Myth and Reality*, BUFFALO PHYSICIAN, Feb. 1986, at 28. See also Jean Dietz Moss, *The Rhetoric of Proof in Galileo’s Writings on the Copernican System*, in THE GALILEO AFFAIR: A MEETING OF FAITH AND SCIENCE 41 (G.V. Coyne et al. eds., 1985) (demonstrating Galileo’s extensive use of rhetorical artifice in his formal and informal defenses of Copernican theory).

<sup>132</sup> GALILEO’S REVENGE, *supra* note 5, at 209. Huber’s critic Chesebro, *supra* note 4, at 1726, makes similar misuse of the Galileo historiography (“Galileo would attribute the prominence of the book and its author to clever public relations, not merit, and would denigrate it as junk scholarship in search of “junk science.”).

<sup>133</sup> See BURTT, *supra* note 72, at 207; EASLEA, *supra* note 129, at 171; Brian P. Copenhaver, *Natural Magic, Hermeticism, and Occultism in Early Modern Science*, in REAPPRAISALS OF THE SCIENTIFIC REVOLUTION 261 (David C. Lindberg & Robert S. Westman eds., 1990); Alan Gabbey, *Newton and Natural Philosophy*, in COMPANION TO THE HISTORY OF MODERN SCIENCE 243 (Robert C. Olby & Geoffrey Cantor eds. 1990); Marie Boas Hall, *Newton’s Voyage in the Strange Seas of Alchemy*, in REASON, EXPERIMENT, AND MYSTICISM IN THE SCIENTIFIC REVOLUTION 239 (M.L. Righini Bonelli & William R. Shea eds., 1975); Gary Hatfield, *Metaphysics and the New Science*, in REAPPRAISALS OF THE SCIENTIFIC REVOLUTION 93 (David C. Lindberg & Robert S. Westman eds., (footnote continued)

Newton's central ideas – universal gravitation – was subject to considerable criticism in its own time for introducing occult forces into the nascent mechanical philosophy which was trying to liberate itself from such forces. Using history of science in the selective manner that Huber does, Newton's famous statement "hypothesis non fingo"<sup>134</sup> and his reluctance to specify the exact causes, in mechanical terms, to many of the processes he observed,<sup>135</sup> conforms with the image of evasiveness indicative of junk science.<sup>136</sup>

¶ 67 Huber's whig history is not exclusive to the remote past, but also extends to his understanding of relatively recent history. It is not surprising to discover that he embarks upon some extremely selective accounts of the recent history of a variety of harmful substances. Huber uses the history of asbestos to argue that interventions by the legal system have failed to contribute to science and regulation:

Lawyers are good at finding scientists who just happen to be at the cutting edge of things, and society benefits accordingly. Or so the lawyers maintain. History records otherwise. The pathbreaking scientific work on asbestos was conducted in the early 1960s, and was accepted soon thereafter in mainstream scientific publications and symposia. But the lawsuit that launched asbestos litigation was not filed until October 1969, and four more years would pass before appeals were resolved.<sup>137</sup>

¶ 68 Such an account obscures the more complex history involved. In particular, it fails to acknowledge the complex interactions between scientific identification of a hazard, regulation, and litigation. In fact, scientific concerns over asbestos can be traced back to the turn of the century. In the 1920s, United States insurance companies refused to sell life insurance policies to asbestos workers. In 1933, the British government introduced legislation to reduce occupational exposure to asbestos dust, and asbestosis was made a compensable disease. Even though the British safety standards were published in the *American Journal of Industrial Hygiene*, the U.S. asbestos industry opposed attempts to introduce similar regulation into the United States.

¶ 69 In the 1930s, Dr. Anthony Lanza of the Metropolitan Life Insurance Company undertook research suggesting that 53% of randomly assessed asbestos workers in the U.S. suffered from asbestosis. This material was delivered to Johns-Manville, the largest asbestos producer in the United States, where the text of the report was altered to minimize suggestion of the level of risk. This marked a long history – up to the 1970s – of the United States' asbestos industry commissioning and suppressing scientific research into asbestos. It was partly through the investigative work of lawyer Carl Asch that the longstanding industry suppression of asbestosis risk was disclosed.<sup>138</sup>

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1990); Simon Schaffer, *Newtonianism*, in COMPANION TO THE HISTORY OF MODERN SCIENCE 610 (Robert C. Olby & Geoffrey Cantor eds., 1990); Richard S. Westfall, *The Role of Alchemy in Newton's Career*, in REASON, EXPERIMENT, AND MYSTICISM IN THE SCIENTIFIC REVOLUTION 189, 189-232 (M.L. Righini Bonelli & William R. Shea eds., 1975).

<sup>134</sup> "I do not frame [make] hypotheses."

<sup>135</sup> Such as the behavior of light or the action at a distance of planetary bodies.

<sup>136</sup> Other figures from the history of science chosen by Huber as examples of "junk science" also have more complex contextual histories. See, for instance, more sophisticated discussions of "failed science" such as Blondlot in DAVID BLOOR, KNOWLEDGE AND SOCIAL IMAGERY 20-39 (1976), and Thomas F. Gieryn, *The Ballad of Pons and Fleischman: Experiment and Narrative in the (Un)making of Cold Fusion*, in THE SOCIAL DIMENSIONS OF SCIENCE 217 (Ernan McMullin, ed., 1992); see also COLLINS & PINCH, *supra* note 55, at 57-78; Bruce V. Lewenstein, *Cold Fusion and Hot History*, 7 (2nd Series) OSIRIS 135 (1992).

<sup>137</sup> GALILEO'S REVENGE, *supra* note 5, at 172.

<sup>138</sup> See NICHOLAS HILDYARD, COVER UP: THE FACTS THEY DON'T WANT YOU TO KNOW 177-190 (1981); JOCK McCULLOCK, ASBESTOS: ITS HUMAN COST 36-69 (1986).

¶ 70 It is beyond the scope of our paper to provide a more detailed account of the history of asbestos regulation, but even this brief historical sketch shows the limitations of Huber's polemical approach. Asbestos, and its ultimate regulation, reveals a matrix of science, politics, and litigation. To separate these factors and convey the impression that the history of asbestos is best understood by some single act of scientific achievement isolated from law and politics is highly inaccurate. Huber's omission of the broader social context in which the scientific research was embedded is self-serving; a more detailed history would reveal the need for industry to be pursued through the legal system to accept scientific research that should influence its practices. The history of asbestos reveals the lack of regulatory use of scientific research when it is not linked to litigation or political decision-making. Huber's emphasis on isolated scientific achievement is used to reinforce his mythological image of the existence of science possessing political authority, divorced from any linkages to broader political and social contexts.

¶ 71 Huber's whiggish use of the history of science is representative of much of the law-science literature.<sup>139</sup> The history of science is used in these settings as a vehicle to pursue whatever argument is currently favored. A surprising anti-intellectualism is revealed in this tendency, as many of the more detailed histories of scientists such as Newton and Galileo – and even contemporary scientific debates and developments – are widely available. At its worst, this anti-intellectual tendency is displayed in the by-passing of scholarly histories of science for encyclopedias and other fora designed for non academic purposes.<sup>140</sup>

H. *"Support" For the Junk Science Model From the Naïve View of the Public Perception of Risk*

¶ 72 Another subtext attributable to the junk science model is that the hysteria or, as Huber describes it, "the paranoia plebiscite," of irrational fears regarding the risk posed by new technologies and drugs has been an important contributor to, and is itself stimulated by, junk science. Much toxic tort litigation is portrayed as relying upon very weak science and irrational public perceptions of risk:<sup>141</sup> "But even so

<sup>139</sup> See, e.g., GALILEO'S REVENGE, *supra* note 5, at 9, 24, 194, 217 (discussing the European witchcraze, Blondlot, Darwin, Mendel/Lysenko); Francisco J. Ayala & Bert Black, *Science and the Courts*, 81 AM. SCI. 230, 234-38 (1993) (employing historical examples such as Kekule, Mendel, Lysenko, Koch, Watson & Crick); Black et al., *supra* note 20, at 766 ("Countless examples from the history of science demonstrate how the model of science we have sketched out actually works in practice."); Edward J. Imwinkelried, *The Next Step after Daubert: Developing a Similarly Epistemological Approach to Ensuring the Reliability of Nonscientific Expert Testimony*, 15 CARDOZO L. REV. 2271, 2275-2276 (1994) (tracing common law restrictions on opinion testimony to Lockean epistemology); Koukoutchos, *supra* note 51, at 2251 ("The remedy for Lamarckism is Mendelian genetics; the answer to Velikovsky is Newton (and Charles Lyell)."); Richardson et al., *supra* note 21, at 12-13 (using bloodletting and Freudian theory as historical examples of the efficacy of falsification). Compare Parascandola, *Philosophy in the Laboratory: The Debate Over Evidence of E.J. Steele's Lamarckian Hypothesis*, 26 STUD. HIST. PHIL. SCI. 469 (1995). See also BARRY BARNES, DAVID BLOOR & JOHN HENRY, *SCIENTIFIC KNOWLEDGE: A SOCIOLOGICAL ANALYSIS* 94-102 (1996).

<sup>140</sup> See Black et al., *supra* note 20, at 774, 780 (citing such sources as the Cambridge Encyclopedia of Human Evolution and the Dictionary of Scientific Biography); Imwinkelried, *supra* note 139, at 2276-77 ("However, Newton systematized the experimental technique and helped standardize the use of such terminology as 'hypothesis.' His work became 'the model of scientific' research. Lockean epistemology and Newtonian Science supplied the template for the Daubert decision.")

<sup>141</sup> HUBER, *Junk Science*, *supra* note 5, at 754 ("Junk science's one very real power is to stir up fear."). See also GALILEO'S REVENGE, *supra* note 5, at 139 ("In due course, chemical toxins, electromagnetic fields, microwaves, and trace contaminants in foods would all be elevated to new legal respectability by similar appeals to fear rather than facts."); *id.* at 134 ("Groundless fears, in short, would be controlled by the courts if shared by the teeming masses. The fear is irrational, granted. At war with science, if you insist. But 'reasonable' nonetheless. How can this be? Only if science does not have the last word on reason. Which is to say, only if views of 'the people generally,' with all their prejudices, superstitions, and inchoate dreads, count even more."); Burk, *supra* note 8, at 374 ("Many of the problems of the current system, especially that of 'junk' science, arise from the inability of a scientifically illiterate judiciary to screen out opinions that are unaccepted by the scientific (footnote continued)

interpreted, federal antidiscrimination law covers a narrow range of acts directed against specific individuals. Countless other opportunities for junk-fear litigation against hospitals and hospices, factories, products, and power plants, remain at hand.”<sup>142</sup>

¶ 73 Commentators frequently make comparisons between, on the one hand, fringe “junk risks,” their accompanying “junk science,” and opportunistic legal supporters, and, on the other hand, the “real” risk of living in modern society – which they see as obviously demonstrable and scientifically proven. “Real” risks such as smoking and sexual lifestyle choices are portrayed as those that should generate concern. In disparaging popular risk perceptions as irrational, these commentators engage in a common form of discourse in the literature on risk research mainly promoted by those who are supporters of industry self-monitoring and weak regulation.

¶ 74 The down-playing of popular perceptions of risk has nevertheless been open to considerable criticism. Writers such as Perrow, Nelkin, and Wynne have pointed out that it is simplistic to describe public perceptions of risk as irrational.<sup>143</sup> They argue that such perceptions play an important role in the democratic political process. Thus, it is important to transcend a technocratic approach when evaluating the rationality of public risk perceptions. Important factors to consider include: the trustworthiness of the institutions involved in imposing and assessing risks; the amount of control over the risk, for instance, whether the risk is voluntarily assumed or imposed; the alternatives to the technology which is perceived to be imposing a risk; the distribution of attendant risks and benefits across society; and the reversibility of the risk, under the ambit of concerns about ecologically sustainable development and environmental ethics.<sup>144</sup>

¶ 75 Much of the risk discourse artificially simplifies risk to a question of supposedly “factual” assessments of harm that can be isolated from any kind of social context or influence. Many low level and uncertain risks featured in toxic tort litigation have been bolstered by concerns about loss of public control over various areas of science and technology. As messy as such litigation may appear to technocrats, it plays an important role in educating and facilitating public participation in the future technological direction of society. Writers such as Jasanoff have noted the significance of such forms of litigation in developing a more publicly sensitive regulatory culture. The history of regulation suggests that public concerns stimulating litigation and regulation of toxic wastes and new technologies have not been such an obvious “bogey” to stimulating technological development. In fact, there is a growing body of technological innovation studies which suggest the need

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community.”); Faigman et al., *supra* note 14, at 1811; Gregg L. Spyridon, *Scientific Evidence vs. “Junk Science” – Proof of Medical Causation in Toxic Tort Litigation: The Fifth Circuit “Fryes” a New Test* (Christophersen v. Allied-Signal Corp.), 61 MISS. L.J. 287 (1991); Klein, *supra* note 47, at 2222; Lee Loevinger, *Science and Legal Rules of Evidence, A Review of Galileo’s Revenge: Junk Science in the Courtroom*, 32 JURIMETRICS J. 487, 501 (1992) (book review) (“Ultimately, the most practical and effective method of dealing with the problem of junk science in the courts, as well as in other branches and agencies of government, is to undertake the scientific education of all citizens. . .”).

<sup>142</sup> GALILEO’S REVENGE, *supra* note 5, at 146. Compare Spyridon, *supra* note 141, at 311 (“Nor can we hold the defendant hostage to ‘junk science’ theories which prey on the public hysteria generated by the media’s portrayal of the ‘toxic age’ in which we live.”).

<sup>143</sup> See DOROTHY NELKIN & MICHAEL BROWN, WORKERS AT RISK: VOICES FROM THE WORKPLACE (1984); CHARLES PERROW, NORMAL ACCIDENTS: LIVING WITH HIGH-RISK TECHNOLOGIES (1984); Brian Wynne, *Knowledges in Context*, 16 SCI. TECH. & HUM. VALUES 111 (1991). See also Schuck, *supra* note 43, at 3 (“[T]hese conflicts are not only inevitable but socially desirable in a democratic-liberal-technocratic-polity. . .”).

<sup>144</sup> PERROW, *supra* note 143, at 306-24; THE SOCIAL AND CULTURAL CONSTRUCTION OF RISK: ESSAYS ON RISK SELECTION AND PERCEPTION (Branden B. Johnson & Vincent T. Covello eds., 1987); KRISTIN SHRADER-FRECHETTE, RISK AND RATIONALITY: PHILOSOPHICAL FOUNDATIONS FOR POPULIST REFORMS (1991); Brian Wynne, *Public Understanding of Science*, in HANDBOOK OF SCIENCE AND TECHNOLOGY STUDIES 361, 381-82 (Sheila Jasanoff, Gerald E. Markle, James C. Petersen & Trevor Pinch eds., 1995).

for stricter environmental safeguards that can act as an important stimulus to technological change and be a source of competitive advantage for innovative firms.<sup>145</sup>

I. *Reluctance of Junk Science Model Proponents to Engage with the History, Philosophy, and Sociology of Science*

¶ 76 Discussion of junk science is often facilitated by setting up a strict and oversimplified model which implies that any acknowledgment of the continuities between scientific knowledge and the need to consider the social context in which those claims are generated is equivalent to “morally suspect” philosophical nihilism. Contributions from the history, philosophy, and social studies of science are regularly rejected as irrelevant:<sup>146</sup> “Many a philosopher is so intrigued by the shag that she will deny the existence of the carpet entirely. There is simply no meaningful line to be drawn between pseudo-science and science, she will assure you, or at least nothing that can be rigorously demarcated and defined.”<sup>147</sup>

¶ 77 Huber desires to disentangle matters of scientific “truth” from the modern philosopher’s blessing. Nevertheless, in many places in his text his analysis relies on the very historians and philosophers of science he wishes to exclude. For instance, there are footnotes to Jerome Ravetz for the ability of social pressures to allow serious science to be replaced by a so-called “confidence game,” and a quote from Thomas Kuhn for propositions relating to the central nature of scientific work – namely that “a scientific “fact” is the collective judgment of a scientific community. “Good science” is defined not by credentials but by consensus.<sup>148</sup> We have already discussed widespread support for philosopher Karl Popper’s scientific method doctrine of falsificationism. Proponents of the junk science model typically employ this asymmetrical style of argument. They are willing to enroll philosophers and historians of science when it suits their cause, but reject them when they do not.

¶ 78 Consider also the comments of Ayala, Black, and Saffran-Brinks: “We make no claim to philosophical rigor, or to resolving the positivist versus relativist and other debates about the nature of science. Instead, our discussion aims to present a picture of science in accordance with the way most scientists actually practice their profession.”<sup>149</sup> In such a Manichean model we either have simplistic positivism or total naïve relativism. These commentators appear to be blissfully unaware of the variety of approaches to science and policy that flow from a more sophisticated awareness that scientific knowledge does not need to be defined as having some

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<sup>145</sup> JASANOFF, *supra* note 9; ROY ROTHWELL & WALTER ZEGVELD, INDUSTRIAL INNOVATION AND PUBLIC POLICY: PREPARING FOR THE 1980S AND THE 1990S (1981); Jacqueline Cramer & Walter C.L. Zegveld, *The Future Role of Technology in Environmental Management*, FUTURES (June 1991); Johan Schot, *Constructive Technology Assessment and Technology Dynamics: The Case of Clean Technologies*, 17 SCI. TECH. & HUM. VALUES 36 (1992).

<sup>146</sup> See GALILEO’S REVENGE, *supra* note 5, at 223.

<sup>147</sup> *Id.* at 223 (“It is simply unacceptable for any judge to insist that there is no such thing. With or without the modern philosopher’s blessing, courts must still make calls between the manufacturer of Bendectin and the child born without arms, between the woman with breast cancer and the operator of the streetcar. With or without a philosophically certain demarcation between science and pseudoscience, courts are still going to issue certain judgments. Judging is the ultimate exercise in positivism, a faith in facts strong enough to justify transferring fortunes, ruining reputations, and putting people to death. Anyone who does *that* for a living has a moral obligation to maintain faith in external, discoverable truth. Those who can’t should practice their uncontained credulity elsewhere.”). *Id.* at 215-16.

<sup>148</sup> *Id.* at 226.

<sup>149</sup> Black et al., *supra* note 20, at 753. Some exceptions among the legal writers include O’Connor, *supra* note 21; JASANOFF, *supra* note 9; Ronald Allen, *Expertise and the Daubert Decision*, 84 J. CRIM. L. & CRIMINOLOGY 1157 (1994); Rochelle Cooper Dreyfuss, *Is Science a Special Case? The Admissibility of Scientific Evidence After Daubert v. Merrell Dow*, 73 TEXAS L. REV. 1779, 1804 (1995).

simple correspondence with “truth.”<sup>150</sup> It is also worth noting that many of the calls for the clean decisive positivist ideal of scientific “truth” would only encompass those scientific “truths” which proponents of the junk science model find politically and philosophically “acceptable.”

#### IV. CONCLUSION

¶ 79 We have outlined some of the problems associated with use of the ill-defined junk science model. While the model and its proponents have not gone without criticism, much of the attack has failed to transcend many of the assumptions and presuppositions underlying the model. Some critics have identified the selective political use of the concept “junk science” and the tenuous nature of arguments blaming “junk science” for a “crisis” in the legal system, but very few have challenged the concept of “junk science” itself in a detailed, well documented textual manner.

##### A. *Junk Science Model Critics*

¶ 80 A number of scholars have commented on the politically loaded nature of the junk science model and the tendency for it to be deployed in polemical and inconsistent ways in political contexts. There have been three dominant, often intersecting, approaches to the criticism of the model – and especially of Huber’s gloss. First, it has frequently been pointed out that the actual areas of science categorized as “junk” are highly politically motivated and that the selection of examples displays a lack of scholarly rigor and empirical veracity. Yet, this style of criticism implicitly accepts that there is indeed some kind of simple overarching demarcation between “good” and “junk” science. It is merely the choice of particular areas of science that is challenged.<sup>151</sup> A second critique is that the magnitude of the problem of junk science has been exaggerated. In this approach, the legal system is seen as not being in the state of crisis that junk science model proponents often describe.<sup>152</sup> Again, it is not the junk science model itself which is questioned, but rather its prevalence and socio-legal significance. The third critique is that junk science model proponents have oversimplified the division between “junk” and “good” science. This approach shares similar concerns to those explored in this paper. Nevertheless, they lack an attempt to articulate in depth why

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<sup>150</sup> Barnes points out there is no need to adopt such an extreme dichotomy. BARRY BARNES, THE ELEMENTS OF SOCIAL THEORY 94-128 (1995); Barry Barnes, *How Not to Do the Sociology of Knowledge*, in RETHINKING OBJECTIVITY 21 (Allan Megill ed., 1994); see also IRWIN, *supra* note 9; Steve Rayner, *Risk and Relativism in Science for Policy*, in THE SOCIAL AND CULTURAL CONSTRUCTION OF RISK: ESSAYS ON RISK SELECTION AND PERCEPTION 5 (Branden B. Johnson & Vincent T. Covello eds., 1987).

<sup>151</sup> Chesebro, *supra* note 4, at 1722 (“... given the massive promotion of Huber by the Manhattan Institute and its affiliated conservative organizations, the reader must not assume that the sheer notoriety of Huber’s scholarship reflects real merit.”); Jennifer Sparks, *Admissibility of Expert Psychological Evidence in the Federal Courts*, 27 ARIZ. ST. L.J. 1315. (1995) (“We can see now the damage done by the conservative domination of the debate that led to *Daubert*; Tamarelli, *supra* note 126. The conservative triumph in the war of the slogan making it the “junk science” debate has shriveled the dialogue. Conservatives have shrunk the question to “How can we exclude bad science, the science that does not conform to Legend, non-consensus, non-uniform science?”).

<sup>152</sup> See CARNEGIE COMMISSION ON SCIENCE, TECHNOLOGY, AND GOVERNMENT, SCIENCE AND TECHNOLOGY IN JUDICIAL DECISION MAKING 13 (Mar. 1993) (“... many of the [junk science] concerns are greatly exaggerated... it does not appear that the federal courts are being inundated with fringe science.”); Chesebro, *supra* note 4, at 1643, 1653-54, 1658, 1705 (“The analysis of Huber’s treatment of facts... reveals a work that relies almost exclusively on anecdotal information and inflated rhetoric, misrepresents numerous aspects of its subject matter, and presents no considered, objective, or empirically based measure of the extent of the ‘junk science’ problem.”); Green, *supra* note 58, 669-70; Joseph Sanders, *Scientific Validity, Admissibility, and Mass Torts After Daubert*, 78 MINN. L. REV. 1345, 1389 (1994); Jeff L. Lewin, *Calabresi’s Revenge? Junk Science in the Work of Peter Huber*, 21 HOFSTRA L. REV. 183, 186 (1992) (book review).

such an approach is too simplistic. Thus, while we generally agree with this perspective, we have provided a more detailed examination and explanation of why such categories are inadequate. Other critics tend only to make passing reference, rather than engage in more thorough or sustained appraisals.<sup>153</sup>

¶ 81 While most commentators have challenged the existence of a dichotomy between “good” and “junk” science without drawing from the literature in the history, philosophy, and social studies of science, a notable exception is Sheila Jasanoff. In her recent work, *Science at the Bar*, Jasanoff suggests that:

To correct the perceived deficiencies of scientific assessment in toxic tort cases, courts have been enjoined to take their cues from “mainstream science”. . . . I have argued that “mainstream” scientific positions (if they exist at all) are constructed in part through the very flow of litigation. Knowledge that the tort system may eventually be willing to live by emerges from the dialogic interaction of law and science, often in conflicts about the safety of technology. The case of “clinical ecology,” frequently cited as an example of “junk science” by the advocates of “mainstream science,” shows this process at work. It suggests that the adjudicatory process is not structurally incapable of taking into account differences between so-called mainstream and peripheral views in science, but that (as within science itself) it takes time and active work by professional bodies to develop consensual knowledge around new areas of dispute.<sup>154</sup>

¶ 82 Jasanoff’s critique is more sophisticated than those of other critics in that it acknowledges the social processes involved in the construction of law-science knowledge, and the difficulty in using simplistic philosophical demarcation criteria between science and non-science.<sup>155</sup> However, Jasanoff does not pursue these criticisms by engaging in detailed analysis or deconstruction of the junk science discourse.<sup>156</sup>

#### A. *Trashing “Junk Science”*

¶ 83 Drawing from an extensive body of law/science literature, we have developed and critiqued the junk science model. We have shown how it relies on an image of science based on naïve notions of the unique efficacy of science, motivations of scientists, scientific norms, and scientific method. The argument that junk science is the product of the legal distortion of “normal” scientific practices was challenged, and it was argued that this claim failed to acknowledge: 1) scientific disagreements outside of legal contexts are prevalent; 2) tensions between law and science are better thought of as problems of deconstruction rather than distortion; and 3) such

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<sup>153</sup> See Blomquist, *supra* note 12, at 635 (“. . . [Huber’s] categorization of all scientists as either “good” or “bad” is too simplistic.”); Chesebro, *supra* note 4, at 1647; Koukoutchos, *supra* note 51, at 2244. See also Robert F. Blomquist, *The Dangers of “General Observations” on Expert Scientific Testimony: A Comment on Daubert v. Merrell Dow Pharm., Inc.*, 82 KY. L.J. 703, 717 (1993-94); Jasanoff, *supra* note 95, at 345.

<sup>154</sup> JASANOFF, *supra* note 9, at 131.

<sup>155</sup> SHEILA JASANOFF, *THE FIFTH BRANCH: SCIENCE ADVISERS AS POLICYMAKERS* (1990); Sheila Jasanoff, *Science and the Courts: Advice for a Troubled Marriage*, 2 NAT. RESOURCES & ENV’T 3 (1986); Jasanoff, *supra* note 95. See also Margaret G. Farrell, *Daubert v. Merrell Dow Pharmaceuticals, Inc.: Epistemology and Legal Process*, 15 CARDOZO L. REV. 2183 (1994).

<sup>156</sup> See, e.g., Jasanoff, *supra* note 95, at 354 (“At the same time, the sociological perspective on science alerts us to be cautious about statements like the following from a recent popular article about science and the courts: ‘Some will always insist that all truth is relative and subjective, that science must be viewed as a chaotic heap of unconnected and contradictory assertions . . . [citing Huber]. Neither the neutrality of the law nor the positivism of science has stood up well enough to tests of empirical research to justify uncritical belief. Fortunately, however, the alternative is not nihilism, at least so long as we remember that the ultimate goal of the courts is not the impossible one of finding objective truth but the more attainable one of dispensing justice.’”).

approaches fail to acknowledge the growth and importance of new areas where law and science are indissolubly integrated as hybrids. We also explained how the junk science model gains “support” from both inadequate histories of science and a sociologically naive view of the public perception of risk. Our final strand of criticism examined the way proponents of the junk science model simultaneously embrace and reject the work of modern philosophers and historians of science, evidently based on how well it serves their purposes at any given juncture.

¶ 84 The rejection of a simple dichotomy between “good” and “bad” science facilitates discussion in a number of areas otherwise precluded. For instance, questions relating to the efficacy of various sciences, their objectives, and the ethics of their practitioners can be examined in more specific local terms, freed from the need to anchor them to over-arching, unworkable, mythological images of science. Discarding the junk science model also reduces the tendency for scientific disagreements in legal contexts to generate emotionally charged deprecatory accusations between, and among, scientists and legal practitioners. “Junk science” dialogues are unlikely to encourage constructive informal learning processes between the various participants involved in difficult areas of law-science interactions. Such areas include those where there are scientific uncertainties and disagreement, and sociological uncertainties relating to the setting of the boundaries for appropriate professional expertise in the case of law-science hybrids.

¶ 85 “Junk science” discourse tends to favor a technocratic image of politics which contains numerous problems. Technocratic political solutions tend to favor the reduction of political decisions to technical problems. Such reductionism is simplistic in the case of law-science interactions. Given the rich diversity of activities constituting modern science and the contexts in which modern science and law are brought together, the act of distilling simple apolitical scientific solutions out of complex debates is implausible. It represents an attempt to impose certainty and closure in dynamic areas of modern society where disagreements about science carry with them different images of the direction of future society, political choices about risk, and debates about the trustworthiness and form of decision-making institutions. Avoiding technocratic “solutions” provides opportunities for policies to be developed diachronically, responsive to changes in political directions which reflect change in the scientific-technical knowledges involved. This places less pressure on the development of synoptic approaches to decision-making, which are likely to be inappropriate in contexts of uncertainty and change.<sup>157</sup> In short, the junk science model tends to convert what should be more visible political debates into narrow, inadequately framed, pseudo-epistemological debates.

¶ 86 At its worst, the junk science model represents a defeatist pessimism toward the democratic process, and a turning away from the complex realities of modern political society – especially at the intersection of science, technology, politics, and law. In what could be described as epistemological nostalgia, it harks back to the myth of a golden age when science was science and law was law.

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<sup>157</sup> See BRIAN WYNNE, RISK MANAGEMENT AND HAZARDOUS WASTES: IMPLEMENTATION AND THE DIALECTICS OF CREDIBILITY (1987); Brian Wynne, *Technology, Risk and Participation: The Social Treatment of Uncertainty*, in SOCIETY, TECHNOLOGY, AND RISK 83 (Jobst Conrad ed., 1980); Dorothy Nelkin & Michael Pollak, *Public Participation in Technological Decisions: Reality of Grand Illusion?*, 81 TECH. REV. 55 (1979); Brian Wynne, *Risk and Social Learning: Reification to Engagement*, in SOCIAL THEORIES OF RISK 275 (Sheldon Krinsky & Dominic Golding eds., 1992).