


6-2015

Law, Science, and the Economy: One Domain?

David S. Caudill
Villanova University

Follow this and additional works at: <https://scholarship.law.uci.edu/ucilr>

 Part of the [Law and Economics Commons](#), [Legal History Commons](#), and the [Science and Technology Law Commons](#)

Recommended Citation

David S. Caudill, *Law, Science, and the Economy: One Domain?*, 5 U.C. IRVINE L. REV. 393 (2015).
Available at: <https://scholarship.law.uci.edu/ucilr/vol5/iss2/9>

This Article is brought to you for free and open access by UCI Law Scholarly Commons. It has been accepted for inclusion in UC Irvine Law Review by an authorized editor of UCI Law Scholarly Commons.

Law, Science, and the Economy: One Domain?

David S. Caudill*

In an effort to explore the theoretical and practical promise of ignoring or erasing conventional boundaries and distinctions—such as law/society or inside/outside—in accounts of legal processes and institutions, I consider the problem of financial bias in scientific expertise. I first draw an analogy with science studies, and particularly Latour’s notion of science as a coproduction, which challenges the boundaries (i) between science and society, and (ii) between natural and social influences on the production of scientific knowledge. I then acknowledge the efforts of Philip Mirowski, in his concern that privatization trends degrade science, to overcome an individualistic perspective on financial bias (conflicts of interest, fraud) and to identify indirect, systemic effects of the economy on science. However, as illustrated by Michel Callon’s network model of the economics of science, Mirowski retains a set of conventional distinctions, boundaries, and assumptions (including the primacy of science over technology, and the primacy of academic science over commercialized science) that render his critique ineffective—Mirowski’s examples of systemic effects tend to be individualistic. A more accurate description of the interactions between private firms and public laboratories, including the identification of boundary organizations and hybrid firm/laboratories, advances the effort to recognize and evaluate the systemic and not merely individualistic effects of the economy on science. I conclude that because scientific expertise is a coproduction of law, science, and the economy, and because each of the three enterprises is equally rhetorical, social, institutional, political, and historical, there is no priority or privilege of one domain over another—they are, in those senses, one.

* Professor and Arthur M. Goldberg Family Chair in Law, Villanova University School of Law. The author would like to thank the participants in the “Law As . . .” III symposium, and especially Professor Justin Desautels-Stein, for their critical responses to this Article.

I. A Strong Analogy: Science Studies.....	395
II. Science and the Economy.....	399
III. The Debate Over Distinctions.....	402
IV. Structural Effects of the Economy on Science in Law	408
Conclusion.....	410

[W]e need to get rid of all categories like those of power, knowledge, profit or capital, because they divide up a cloth that we want seamless in order to study it as we choose.¹

Central to this symposium is the question of whether it might be practically and theoretically promising to ignore or erase some of our conventional legal categories.² For example, instead of promoting “law and” projects that conceive of interactions between law and another distinct domain, why not view law as politics or literature (as many already have), or as economics or science (seemingly more challenging)? Such alternative projects question the distinctions between law and society (as independent domains that influence one another), and between what is inside and what is outside of legal processes and institutions, as a first step toward the effort to provide more compelling accounts of how law functions.³

My own focus in this Article concerns the acquisition of scientific expertise into law, which phraseology already suggests two domains: one providing and the other accommodating. Narrowing my ultimate argument, I consider the conventional representations of the problem of financial bias in the science acquired by law (whether in litigation, administrative, or legislative contexts). Now there are three domains—science providing, law making accommodations, and the economy interfering. And the three-domain discourse is often one of intentionality, of good and bad agents, which discourse hides systemic effects that become visible only when one notices that the economy is already in science (whether invited or not), that science is already in law (by law’s adoption of science’s self-image even as law purports to be an evidentiary gatekeeper), and that law is already in the economy

1. BRUNO LATOUR, *SCIENCE IN ACTION* 223 (1987).

2. See generally Christopher Tomlins & John Comaroff, “*Law As . . .*”: *Theory and Practice in Legal History*, 1 U.C. IRVINE L. REV. 1039, 1041 (2011).

3. Indeed, Bruno Latour, on whose actor-network theory I rely on in this Article (with respect to science and society), has in *LA FABRIQUE DU DROIT: UNE ETHNOGRAPHIE DU CONSEIL D’ÉTAT* (2002), translated by Marina Brilman & Alain Pottage as *THE MAKING OF LAW: AN ETHNOGRAPHY OF THE CONSEIL D’ÉTAT* (Polity Press 2010) (2002), challenged such inside/outside distinctions. In the words of one reviewer: “Gone is a purified ‘Law’ and ‘Society’ for Latour. Dividing texts and rules (‘law on the books’) from people (‘law in action’) loses ground underneath the weight of *La fabrique du droit*. . . . For Latour, the force of law is produced through its everyday halting, stammering, hesitating, continually negotiated and overtly hybrid, practices.” Ron Levi, Book Review, 1 LAW, CULTURE & HUMAN. 137, 137 (2005) (reviewing BRUNO LATOUR, *LA FABRIQUE DU DROIT: UNE ETHNOGRAPHIE DU CONSEIL D’ÉTAT* (2002)). Levi is, however, disappointed: “[I]f it is impossible to imagine a ‘social’ without law, one wonders why Latour has here privileged the professional institution, rather than emphasizing the pervasive role of *legality*, running in, through, and beyond the courthouse walls.” *Id.* at 139 (citation omitted). This criticism parallels the criticism that laboratory studies ignore broader societal structures. See *infra* notes 68–71 and accompanying text.

(which is never a free market). Which is not to say that the economy determines science, that science is determinative within law, or that law constructs the economy; it is to say, for example, that scientific expertise is a coproduction—a hybrid of law, science, and the economy—and that no single “domain” is privileged, as so often happens in “law and” scholarship.⁴

I begin with science studies, the basis for my argument, and particularly Latour, because the history of science studies is one of breaking down the formulation of science and society as two domains. I then turn to the economics of science and emphasize the coproduction of science and the economy, by reference to Mirowski’s work on the pernicious effects of commercialization of science, and to Callon’s model of firm/laboratory interactions as an extended example of challenging conventional analytical categories. I conclude that effective solutions to the problem of financial bias in law’s (acquisition of) science require reconceptualizing three domains as one—while at the same time acknowledging that when conventional boundaries are erased, others may take their place.

I. A STRONG ANALOGY: SCIENCE STUDIES

When I say there is no inside/outside distinction, I mean that we should not believe in the existence of inside and outside. We should sit exactly at the place where the inside and the outside of the network are defined. . . . [W]e have to see inside-and-outside as an active category, created by the actors themselves, and it has to be studied as such.⁵

The field of science studies is ill-defined, referring directly to a collection of social science subdisciplines—science and technology studies, sociology of science (or sociology of scientific knowledge), science and society—and indirectly to the history and philosophy of science. “Science studies” is decidedly not one of the natural sciences, which are its objects of study, and while early sociologists of science might have attempted to show how science at its best is unaffected by cultural forces, contemporary science studies reveal scientific knowledge as coproduced, if not constructed, by social structures, including, for example, language, rhetoric, values, institutions, money, guiding paradigms, and experimental conventions. While no one would deny the existence of the foregoing as constituting a *context* of supports for the scientific enterprise, the notion that scientific knowledge is determined (“social constructivism”) or at least coproduced is controversial among nearly all scientists and scientific theorists, hence the “science wars” in the 1990s.⁶

4. See Penelope Pether, *Language, in LAW AND THE HUMANITIES: AN INTRODUCTION* 315, 318 n.7 (Austin Sarat et al. eds., 2010) (explaining that law is typically the “privileged element” or object of interdisciplinary inquiry in the law and literature dyad, insofar as the inquiry is typically concerned with what literature—both its texts that represent law and lawyers and its critical methodologies—can tell us about or *do* for law).

5. Thomas Hugh Crawford, *An Interview with Bruno Latour*, 1 CONFIGURATIONS 247, 257 (1993) (statement by Bruno Latour concerning the relation of science and society).

6. In response to a perceived attack on the natural sciences by science studies, various authors fought back. See PAUL R. GROSS & NORMAN LEVITT, HIGHER SUPERSTITION: THE ACADEMIC LEFT

Bruno Latour occupied an uncomfortable position in the science wars—he became the signifier of, and whipping boy for, science studies in the eyes of its critics, notwithstanding the riven state of science studies and its cottage industry of criticizing Latour. Latour’s association with science studies is clearly justified, both because he (with Woolgar) practically invented laboratory studies,⁷ and because his books have a substantial (though not exclusive) following among science studies scholars. But in the science wars, the enemies of science studies were attacking social constructivism as a critique or reduction of science’s accomplishments; and while very few science studies scholars would fit that particular caricature, Latour is famous for his rejection of social constructivism for its maintenance of the social/natural distinction—for merely replacing natural explanations of scientific progress with social explanations.⁸ Having taken (or engendered) both the linguistic *and* the naturalist turn in science studies, Latour does not privilege the “social”—scientific facts are too discursive, and too natural, to be reduced to the “social” (which they are, as well).⁹ Latour recognizes his lack of credibility on such matters for scientific critics of science studies, and imagines their response:

“You can dust your hands with flour as much as you wish, [but] the black fur of the critical wolf will always betray you; your deconstructing teeth have been sharpened on too many of our innocent labs—I mean lambs!—for us to believe you.”¹⁰

To be fair, even if such critics rose above oversimplification and stereotyping through a more sophisticated engagement with Latour, they probably would not like him. Just as important, and despite his influence in science studies, Latour can be just as maddening to his fellow sociologists of science for his rejection of critique, of denouncement, as the task of social scientists:

Many see the . . . task of the intellectual[s] . . . as a critique of the foundation. They need to develop a metalanguage that will unveil and denounce the false appearances.

. . . .

. . . [A]ll debunking makes people believe in the thing being debunked. The attitude of unveiling and denouncing the falseness of the scientific method

AND ITS QUARRELS WITH SCIENCE (1994); STEVEN WEINBERG, DREAMS OF A FINAL THEORY (1992); LEWIS WOLPERT, THE UNNATURAL NATURE OF SCIENCE (1992); Alan D. Sokal, *A Physicist Experiments with Cultural Studies*, LINGUA FRANCA, May/June 1996, at 62, 62.

7. See generally BRUNO LATOUR & STEVE WOOLGAR, LABORATORY LIFE: THE CONSTRUCTION OF SCIENTIFIC FACTS (Princeton Univ. Press 2d ed. 1986) (1979) (an early study of a laboratory that was highly influential among later science studies scholars engaged in ethnographic research).

8. See LATOUR, *supra* note 1, at 132–34.

9. BRUNO LATOUR, WE HAVE NEVER BEEN MODERN 6 (Catherine Porter trans., Harvester Wheatsheaf 1993) (1991).

10. Bruno Latour, *Why Has Critique Run Out of Steam? From Matters of Fact to Matters of Concern*, CRITICAL INQUIRY, Winter 2004, at 225, 232.

always reinforces the argument of the scientist . . . [Because it] makes you believe it is important to find the true method.¹¹

One way to understand Latour's concern, and the criticism of Latour (for not taking sides), is to see Latour breaking down conventional categories and distinctions. The flaw in social constructivism was to preserve the nature/society dualism and to privilege the social; the flaw in modernism was to divide natural science from social science, nonhuman from human; the flaw in postmodernism was to believe in the modern as the object of denunciation. "So then the question is can we play another game? Can we redefine the task of the intellectual so that it is no longer denouncing from one of two poles?"¹² To the extent that the science wars represented the realism/constructivism debate, Latour promises that "you do not need to believe in . . . either of these two poles."¹³

Things become active, and the collective becomes made of things — circulating things . . . [And] these hybrids (quasi-objects) start resembling what our world is made of. It is not that there are a few hybrids; it is that there are *only* hybrids. And the . . . purely social relation . . . and the purely natural construction . . . don't exist.¹⁴

Careful divisions "between what is natural and what is social" do not interest Latour.¹⁵

Another distinction in science studies worth mentioning in this context is "upstream" and "downstream" contexts in the production of scientific knowledge. The term "ELSI"—ethical-legal-social-implications—became popular in science studies to designate a role for the social scientist in the production of scientific knowledge. Instead of waiting "downstream" for the results of science, and then considering the implications for society, the suggestion was made to move "upstream," into the laboratory, with a team of ethicists and lawyers and sociologists.¹⁶ However, that conception risks eclipsing the already present ethical, legal, and social aspects of laboratory life, and unwittingly grants social independence to the scientific enterprise.¹⁷

With respect to the role of economics and the economy in the scientific enterprise, Latour and Woolgar's *Laboratory Life* highlighted the issue in their

11. Crawford, *supra* note 5, at 250, 255. For example, Latour finds that Feyerabend's "denouncing presupposes the existence and importance of what is at stake." *See id.* at 255. *See also* Andrew Barry & Don Slater, *Technology, Politics, and the Market: An Interview with Michel Callon*, 31 *ECON. & SOC'Y* 285, 303 (2002) (quoting Michel Callon: "Bourdieu has not only to reinforce the [economic] macro-structures that are supposed to exist, but he has also to explain why the truth is on one side and not the other.").

12. Crawford, *supra* note 5, at 258.

13. *Id.* at 261.

14. *Id.*

15. *Id.* at 262.

16. *See, e.g.*, Paul Rabinow, *Prosperity, Amelioration, Flourishing: From a Logic of Practical Judgment to Reconstruction*, 21 *LAW & LITERATURE* 301, 304 (2009).

17. *See* David S. Caudill, *Synthetic Science: A Response to Rabinow*, 21 *LAW & LITERATURE* 431, 432 (2009).

Chapter 5, entitled “Cycles of Credit.”¹⁸ As D. Wade Hands points out, “Cycles of Credit” was not an exercise in “the economics of science” in many of the senses of that term.¹⁹ First, the chapter was neither an attempt to “apply” economics to science, nor to model science as a “competitive market process” (Wade suspects that Latour and Woolgar would “consider neoclassical economics to be naively reductionist, narrowly individualist[ic], and in general a quite uninteresting approach to studying (any) social process”²⁰). The term “economics of science” more often refers to works “in the language and discursive format of contemporary economics,” such as Diamond’s theory of the rational scientist maximizing a utility function under constraints, or Wible’s explanation of fraud in science by specifying first-order conditions and optimization uncertainty.²¹ Second, Latour and Woolgar describe a quasi-economic “market for credibility that determines what scientists work on,” using economic *analogies* to describe science and scientists in capitalist terms (e.g., supply, demand, and value of information).²² Finally, Latour and Woolgar do not address the issue of the effects of the economy on science, except to say (in passing) that the “link between the scientific production of facts and modern capitalist economics is probably much deeper than a mere relation,” and that “[s]cientists’ final realisation of capital, through their movement into clinical studies, industry, and culture, is not examined” in *Laboratory Life*.²³

That examination *has* been carried out by Philip Mirowski, an economist, science studies fellow traveler, and critic of Latour (and of actor-network theory, especially its apparent economic theorist, Michel Callon).²⁴ Mirowski’s critique of neoliberalism and its privatized science regimes goes a long way toward revealing, and shifting attention toward, the systemic effects of the economy on science; *and* he offers examples of the economic effects on science *in law*.²⁵ In the end, however, he relies upon and reifies categories and distinctions that render his critique of commercialization in science ineffective.²⁶

18. LATOUR & WOOLGAR, *supra* note 7, at 187–233.

19. D. Wade Hands, *The Sociology of Scientific Knowledge: Some Thoughts on the Possibilities*, in NEW DIRECTIONS IN ECONOMIC METHODOLOGY (Roger Backhouse ed., 1994), *reprinted in* SCIENCE BOUGHT AND SOLD 515, 523–24 (Philip Mirowski & Esther-Mirjam Sent eds., 2002).

20. *Id.* at 526–27.

21. *See id.* at 535–36 (referring to Arthur M. Diamond, Jr., *Science As a Rational Enterprise*, 24 THEORY & DECISION 147 (1988); James R. Wible, *Fraud in Science: An Economic Approach*, 22 PHIL. SOC. SCI. 5 (1992); James R. Wible, *Maximization, Replication, and the Economic Rationality of Positive Economic Science*, 3 REV. POL. ECON. 164 (1991)).

22. *See* Hands, *supra* note 19, at 525.

23. LATOUR & WOOLGAR, *supra* note 7, at 231 n.9, 233 n.22. Michel Callon chooses to “get rid of notions like market[s] of ideas, scientific capital, credibility cycles, etc., which erase the differences between activities that are obviously different.” Michel Callon, *From Science As an Economic Activity to Socioeconomics of Scientific Research*, in SCIENCE BOUGHT AND SOLD, *supra* note 19, at 277, 281 n.4.

24. *See* Philip Mirowski & Esther-Mirjam Sent, *Introduction*, in SCIENCE BOUGHT AND SOLD, *supra* note 19, at 38–39.

25. *See id.* at 54–55.

26. *See id.* at 55–59.

II. SCIENCE AND THE ECONOMY

*[O]ne of the most repeated complaints about Latourian actor-network analysis is that it leaves everything pretty much just the way it found it*²⁷

*[I]nstead of arguing [for] and reinforcing the strict opposition between open science . . . and a market with clearly defined boundaries . . . , [network theory] aims to show the possibility of scientific research that is both autonomous and strongly connected [with] firms.*²⁸

The growing privatization of science raises questions about whether science is degraded or enhanced by relationships with (or financial support from) commercial industry. Does increased collaboration between scientists and industry decrease the collaborative behavior of scientists (i.e., freely sharing knowledge) toward one another? Everyone knows science costs money—it always has—but then the debate begins, with three identifiable positions on financial interests or entanglements: (i) they are relatively benign because the *internal* workings of scientific methodology are unaffected by *external* support (except in the case of fraud, the occasional bad apple); (ii) they are seriously influential and generally good for science by fostering efficiency and innovation; (iii) they are seriously influential with pernicious effects that degrade the quality of science. The first position is represented by physicist David Goodstein, who has written extensively on scientific fraud and was called upon by the federal judiciary, after the U.S. Supreme Court’s *Daubert* decision,²⁹ to explain how science works.³⁰ The second position is found in Paula Stephan’s *How Economics Shapes Science*, which raises no cause for alarm—there’s always room for improvement, of course, but “[s]cience costs money and incentives play a key role in science.”³¹ The third position has been taken up by Philip Mirowski, who fiercely claims to be neither naive nor Pollyannaish about any purported golden age of disinterested science,³² and also insists that the commercialization of science is not in itself a marker of decline—“accusations of corruption must be judged on a case-by-case basis.”³³ Taken as a whole, however, Mirowski suspects that commercialization has changed the structure of science for the worse, even as he concedes the difficulty of measuring that decline.³⁴

To be clear, Mirowski is trying not to talk about individual responsibility,

27. *Id.* at 58.

28. Callon, *supra* note 23, at 277, 281.

29. *Daubert v. Merrell Dow Pharm., Inc.*, 509 U.S. 579, 589 (1993) (confirming the active, gate-keeping role of judges in evaluating scientific reliability).

30. David Goodstein, *How Science Works*, in REFERENCE MANUAL ON SCIENTIFIC EVIDENCE 67, 68 (2d ed. 2000); *see also* DAVID GOODSTEIN, ON FACT AND FRAUD: CAUTIONARY TALES FROM THE FRONT LINES OF SCIENCE (2010).

31. PAULA STEPHAN, *HOW ECONOMICS SHAPES SCIENCE* 1 (2012).

32. *See* Philip Mirowski & Robert Van Horn, *The Contract Research Organization and the Commercialization of Scientific Research*, 35 SOC. STUD. SCI. 503, 508 (2005) (raising the question of whether “images of the disinterested operation of science ever actually corresponded to research practices on the ground”).

33. *Id.*

34. *See* PHILIP MIROWSKI, SCIENCE-MART: PRIVATIZING AMERICAN SCIENCE 266 (2011).

scientific fraud, or the intentional manipulation of data by a scientist with an obvious conflict of interest due to funding. In the tradition of science studies, Mirowski views science and the economy as “mutually constituted”—for example, in the reengineering of “university science around more commercial pursuits,” we notice that “some forms of funding and organization of credit [i.e., from authorship to ownership of intellectual property] promote certain kinds of creative or innovative activity, while other forms actively discourage them.”³⁵ This is a challenge to the scientific notion that social interests are not “systematic or structural; [and] merely serve to focus our attention. . . . [They do not affect] how we do research or what it is that we find there.”³⁶ In the view of science studies, however, “different social alignments can produce different scientific outcomes.”³⁷ While everyone acknowledges that decades of industry funding “may have had some minor influence on changing the *means* by which research is prosecuted, . . . [few imagine that] it transformed the *ends* of science [, i.e.,] whatever it is that we get at the end of the process.”³⁸

Examples of systemic analyses of science and the economy as coproductions include David Tyfield’s study of the “economic impacts on the directions of scientific thought, argument and controversy,” which conceptualizes science and the economy as a “process of ongoing (re)construction [wherein] each conditions the development of the other.”³⁹ Like Mirowski, who attempts to trace the harms of commercialization upon “good” science,⁴⁰ Tyfield argues that “[s]ocial influences’ on science are not . . . only relevant when science goes wrong or is corrupted in some way.”⁴¹ Another example of systemic analysis is Daniel Lee Kleinman’s study of the commercialization of academic science, which acknowledged the popular criticism that “commercially motivated collaborations between university biologists and science-based companies can skew research agendas, prompt inappropriate restrictions on the flow of information, and create conflicts of interest.”⁴² Kleinman, however, is more interested in the “subtle landscape” of academic capitalism—not the direct results of corporate influence, but how “corporate domination of a field of scientific investigation early in its development,” even absent subsequent industry funding, “can indirectly affect the questions that are asked and the answers that are acceptable at a later time.”⁴³ The laboratory studied by Kleinman was neither restricted by industry sponsors nor

35. PHILIP MIROWSKI, *THE EFFORTLESS ECONOMY OF SCIENCE?* 117, 132 (2004).

36. *Id.* at 24.

37. *Id.* at 21.

38. Mirowski & Van Horn, *supra* note 32, at 531–32.

39. 1 DAVID TYFIELD, *THE ECONOMICS OF SCIENCE: A CRITICAL REALIST OVERVIEW: ILLUSTRATIONS AND PHILOSOPHICAL PRELIMINARIES* 25–26 (2012).

40. *See* MIROWSKI, *supra* note 34, at 160.

41. 1 TYFIELD, *supra* note 39, at 18.

42. DANIEL LEE KLEINMAN, *IMPURE CULTURES: UNIVERSITY BIOLOGY AND THE WORLD OF COMMERCE*, at x (2003).

43. *Id.* at xi.

showed any signs of “egregious violations of academic norms”⁴⁴ (e.g., secrecy, conflicts of interest); but “indirect, systemic effects”⁴⁵ of commercial culture were identifiable, including “the ways scholarly writing in the field is framed, the way experiments are organized, the measures of success that are used[,] and the tools that are available. . . . [N]o direct intervention . . . by industry needed to occur for the influence to be felt.”⁴⁶ Mirowski, notably, does not limit his analysis to the commercialization of university science, but identifies the parallel “creation of new social structures of research, . . . new forms of intellectual property, new communication technologies, new research protocols, new career paths, and new institutions of command and control.”⁴⁷ As examples of the decline in science brought on by commercialization, Mirowski first identifies “just-in-time science,” or “the forced inducement of quick and dirty techniques to produce attenuated results on schedule, under budget, and within the parameters of contractual relations.”⁴⁸ Second, Mirowski identifies the “sound science” (or anti-junk-science) movement wherein hidden organizations promote industry-friendly, or in terms of litigation, defense-friendly, science, by casting doubt on good science, demanding more research of good science, or harassing good scientists.⁴⁹ Finally, Mirowski identifies the degradation of patent quality, a phenomenon that finds support in IP literature.⁵⁰

My question is whether Mirowski has identified systemic economic pressures; his examples do not appear to be “good science,” but rather to fall into his category of individual responsibility (the first and third examples) or fraud (the second example), thereby undermining his claim that he is not talking about the *obvious* deleterious effects of commercialization on science. I want to link that blind spot in Mirowski’s work to his criticism of Latourian actor-network theory, and in the economics of science literature, to Callon’s application of actor-network theory as a response to Mirowski’s distinction (crucial to Mirowski’s analysis) between open and privatized science.

44. *Id.* at 6.

45. *Id.* at 17.

46. *Id.* at 4–6, 17, 88–89 (offering the example of the dominance of the chemical industry in agricultural pest-control research).

47. Mirowski & Van Horn, *supra* note 32, at 504 (discussing the rise of contract research organizations).

48. MIROWSKI, *supra* note 34, at 290.

49. *See id.* at 297–99.

50. *See id.* at 305–06; *see also* Rebecca Henderson et al., *Universities As a Source of Commercial Technology: A Detailed Analysis of University Patenting, 1965–1988*, 80 REV. ECON. & STAT. 119, 126 (1998) (describing an increase in “low-quality” patents granted to universities); Shawn P. Miller, *Where’s the Innovation: An Analysis of the Quantity and Qualities of Anticipated and Obvious Patents*, 18 VA. J.L. & TECH. 1, 2 (2013) (estimating that about twenty-eight percent of new patents would be found at least partially invalid).

III. THE DEBATE OVER DISTINCTIONS

[S]ince . . . we wish to attack scientists' hegemony on the definition of nature, we have never wished to accept the essential source of their power: . . . the very distribution between what is natural and what is social and the fixed allocation of ontological status that goes with it. We have never been interested in giving a social explanation of anything, but we want to explain society.⁵¹

Rejecting the “social explanation genre,” Latour and Callon seek to “obtain” nature and society through “network building, or collective things, or quasi-objects.”⁵² Tired of being labeled conservative or reactionary for their supposed realism, they claim to be genuine coproductionists (and scold their science studies critics for being “exactly as reactionary” as their scores of realist critics):

The perfect symmetry in the misreading of our work by “natural realists” and by “social realists” alike is a nice confirmation that we are in a different, although for them unthinkable, position.⁵³

It's not *either* social relations (intentional human subjects) or things (brute material objects), but “the circulation of network-tracing tokens, statements, and skills” to be observed and documented.⁵⁴ And it seems to be the agency of nonhumans that attracts the accusation of realism; for realist readers of Callon's study of St. Brieuc Bay fishermen, the scallops must either be “out there [forcing] themselves on naive realists, or . . . in there made of social relations of humans talking about them”; but Callon and Latour do not attribute “out-thereness” to the various forms under which “scallops exist.”⁵⁵ The key to the accusation, in any event, is the criticism that accounts of science by Callon or Latour do not differ from those of conventional historians of science—explaining a scientific discovery by “granting agency to things in themselves.”⁵⁶ Callon and Latour concede the difficulty of finding an “unbiased vocabulary,” but “negotiation” is not “discovery,” and “actant” is not “actor.”⁵⁷

This (minimalistic) background is important because Mirowski has joined the harshest science studies critics of Latour and Callon. Steve Fuller, for example, blames the Parisians and their actor-network theory for the decline of science studies—its “aversion to normative judgments and . . . open antagonism to the

51. Michel Callon & Bruno Latour, *Don't Throw the Baby Out With the Bath School! A Reply to Collins and Yearley*, in *SCIENCE AS PRACTICE AND CULTURE* 343, 348 (Andrew Pickering ed., 1992).

52. *Id.*

53. *See id.* at 349–50.

54. *Id.* at 351.

55. *See id.* at 353 (referring to Michel Callon, *Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St. Brieuc Bay*, in *POWER, ACTION, AND BELIEF: A NEW SOCIOLOGY OF KNOWLEDGE?* 196 (John Law ed., 1986)).

56. Callon & Latour, *supra* note 51, at 354.

57. *See id.* (“We should be credited with having tried to [establish a symmetrical vocabulary], and when no other solution was available, to have chosen a repertoire which bears no insult to nonhumans.”).

adoption of ‘critical’ perspectives.”⁵⁸ By their invocation of “*natural* factors,” Latour and Callon have apparently caused science studies to jump the shark:

Not surprisingly, scientists . . . have welcomed the Parisian turn, since it clearly reopens the door to traditional . . . explanations of science that incorporate both social and natural factors “interacting” to produce . . . an experimental outcome. It would seem, then, that we have reached one of those . . . [Molièrian] moments . . . when a move that appears radical within the terms of a paradigm is equivalent to the prose that everyone else outside the paradigm has been always speaking (albeit now with a French accent).⁵⁹

Fuller is suspicious because “a seemingly radical innovation that quickly acquires widespread currency probably serves some well-established interests that remain hidden” in its reception.⁶⁰ After all, didn’t fascist ideology combine “an animistic view of nature, a hyperbolic vision of the power of technology, and [a] diminished sense of individual human agency,” just like actor-network theory?⁶¹

Mirowski frames *Science-Mart*, his study of the negative effects of privatization on American science, as a story about the fictional, “intrepid academic researcher Viridiana Jones.”⁶² Early in the book, she meets a few fellow faculty members in the field of science studies, which sounded interesting enough for her to go to a conference at her university to hear famous representatives like Bruno Latour and Steve Woolgar.⁶³ But she “was distressed to find that when they weren’t indulging in opaque jargon about ‘actants,’” they “tended to confuse ‘excellence’ (whatever that was) with the crudest sorts of proxy measures for scientific output.”⁶⁴ Mirowski drops an endnote here to Latour’s *Reassembling the Social* and Latour and Woolgar’s *Laboratory Life*, among others.⁶⁵ Fast forward fifty pages, in a discussion about how most “intellectuals simply took both the priority and primacy of science over technology as gospel before 1980,” and most “disavowed it afterward,” we read that some, like “Bruno Latour, went so far as to claim there had existed only one ontological entity called ‘technoscience’ all along.”⁶⁶ Mirowski sees this “loss of faith” in science as having “everything to do with the economy”—the bending of

58. Steve Fuller, *Why Science Studies Has Never Been Critical of Science: Some Recent Lessons on How to Be a Helpful Nuisance and a Harmless Radical*, 30 PHIL. SOC. SCI. 5, 6 (2000).

59. *Id.* at 8.

60. *Id.* at 18 (emphasis omitted).

61. *Id.* at 23 (as to its diminished sense of human agency, Fuller paradoxically *also* sees an eerie similarity between “totalitarian and actor-network theorists” in their “glorification of the heroic practitioner”!).

62. See MIROWSKI, *supra* note 34, at 1.

63. *Id.* at 3.

64. *Id.* at 3–4.

65. See *id.* at 351 n.5.

66. See *id.* at 54. This echoes Latour’s argument that we have never been modern (“We have never been cut off from our past; we have never been different.”), and that the supposed scientific revolution never happened. See Crawford, *supra* note 5, at 259.

science and technology to economic ends.⁶⁷ The ultimate heresy is then attributed to Callon—“scientific knowledge does not constitute a public good as defined in economic theory”⁶⁸—and thereafter the “neoliberals had won.”⁶⁹ Latour, in this narrative, adopts the “trademark neoliberal doctrine” that science has always been commercial, “an utter travesty of the actual history.”⁷⁰ If Latour and Callon are not card-carrying neoliberals, but rather accidental ones, it is because they turned their “attention to microscale studies of laboratory life, ignoring how the laboratory’s macroscale relationship to society was being reengineered all around, not to mention the changing identities of the paymasters for all those DNA sequencers and inscription devices.”⁷¹ Fuller echoes this concern, remarking that when laboratories become “objects of fascination,” science studies scholars might fail “to see how science reflects larger societal forces.”⁷²

In discerning whether this set of criticisms is fair—indeed, whether Latour and Callon are the appropriate targets of a critique of neoliberal privatization of science—the accusation that laboratory studies ignore social and economic forces is far from compelling. Of course an ethnographic study is neither a grand theory nor a broad quantitative analysis. Nevertheless, the chapter on cycles of credit in *Laboratory Life* traces several (actual, not metaphorical) economic forces on the scientific community (grants, money, equipment);⁷³ more importantly, the laboratory provides “a model on which you can actually do empirical studies about the technologies of society and knowledge production.”⁷⁴ The remaining criticisms reveal Mirowski’s position in the debates over the economics of science, which Callon has described as a plea for the restoration of the autonomy of an open, academic science, in opposition to a “market with clearly defined boundaries.”⁷⁵ Callon does not dispute the need for nonprofit research and scientific autonomy (from the economic market), but wants to show, using network theory, “the possibility of scientific research that is both autonomous and strongly connected [with] firms.”⁷⁶

67. See MIROWSKI, *supra* note 34, at 54 (quoting BENOÎT GODIN, MEASUREMENT AND STATISTICS ON SCIENCE AND TECHNOLOGY: 1920 TO THE PRESENT 287 (2005)).

68. See *id.* at 66 (quoting Barry & Slater, *supra* note 11, at 301 (quoting Callon)). This is an unfair criticism because while Callon says that “[s]cientific knowledge does not constitute a public good as defined in economic theory,” see Michel Callon, *Is Science a Public Good?*, 19 SCI. TECH. & HUM. VALUES 395, 407 (1993) (emphasis added), he also states that “[s]cience is a public good, not because of its intrinsic properties but because it is a source of diversity and flexibility,” see *id.* at 395; see also *infra* note 117 and accompanying text.

69. MIROWSKI, *supra* note 34, at 66.

70. *Id.* at 327.

71. *Id.* at 90. Mirowski suggests that while Latour and Woolgar “gleefully resort[ed]” to economic metaphors in *Laboratory Life*, they “essentially ignor[ed] any substantive economic structures.” *Id.* at 360 n.6.

72. See Fuller, *supra* note 58, at 27.

73. See LATOUR & WOOLGAR, *supra* note 7, at 187–233.

74. See Crawford, *supra* note 5, at 253.

75. See Callon, *supra* note 23, at 279–81.

76. See *id.* at 281. Callon highlights “the importance of the role of scientific research performed

I will only briefly summarize the intricate details of Callon's model of interactions between laboratories and firms, which is concededly speculative, as is Mirowski's suspicion that these interactions degrade science—both theorists are engaged in analytical groundwork (even though they both offer examples to support their current assessments and their predictions). My own focus is on some of the features of Callon's model relevant to his implied critique of conventional divisions and categories in the economics of science. At the outset, it bears mentioning that Callon does not seem to share neoliberal tendencies, as he grants to governments “a central role in regulating interaction between firms and laboratories,” but even there the categories are blurred, as governmental “intervention contributes to the constitution of these relations. In other words, the state must not be seen as an external player; it is caught in this process of inventing and consolidating new types of practices and relations.”⁷⁷ Callon likewise acknowledges, as does Mirowski, the role of the rules of property rights, and also, reflecting European trends in the public understanding of science, the role of “concerned groups and laypeople in the discussion on the technical options and sometimes even on the choice of research objectives.”⁷⁸ And as to Mirowski's criticism (and that of others, like Fuller) that Callon is not critical—Callon does indeed say that we have to abandon “the idea of critique of the hard economists, which is intended to show them they are wrong”⁷⁹—Callon *also* claims (in that same passage) a right to contribute his own market perspective,⁸⁰ which model is clearly a critique of numerous conventional categories and a normative theory.⁸¹ But in keeping with Latour's distaste for denunciation, it is not a critique that is on the same terms (or that grants the same categories and distinctions) as the target of the critique.⁸²

For example, Callon sees the ideal of the autonomy of academic science as an institutional frame, born of historical contingency but transformed into the theoretical necessity of viewing science as a public good.⁸³ In the terminology of network theory, creative and productive forces tend to “overflow[]” such frames, thus economists of science should construct “new analytical tools . . . to account for the proliferation of links and interactions crossing over boundaries, and to identify the consequences so that new material and institutional frames can be discussed and decided.”⁸⁴

Moreover, with respect to the term “decided,” the controversy over

in nonprofit organizations, as a constant source of technological diversification that thwarts the natural tendency of markets to render techno-economic trajectories irreversible.” *Id.* at 280 (citing Callon, *supra* note 68).

77. Callon, *supra* note 23, at 307.

78. *See id.* at 308.

79. MIROWSKI, *supra* note 34, at 66 (quoting Barry & Slater, *supra* note 11, at 301 (quoting Callon)).

80. *See* Barry & Slater, *supra* note 11, at 301.

81. *See infra* note 102 and accompanying text.

82. *See* Crawford, *supra* note 5.

83. *See* Callon, *supra* note 23, at 279.

84. *Id.* at 279–80.

appropriate property rights regimes and over the appropriate role of university (or public nonprofit or government) laboratories is “simultaneously scientific and political.”⁸⁵ Thus a biotech firm, for example, engaged in “knowledge production, technological developments, and construction of demand, in close interaction with consumers,” provides an empirical case and a “veritable social laborator[y] in which new arrangements and devices and original rules of the game are tried and argued.”⁸⁶

To establish the need for network theory in the economics of science, Callon challenges the notion that the characteristics of a scientific “good” (i.e., knowledge or information) is *intrinsic* to science—such characteristics include the status of a scientific “good” as (i) either *rival* (one must compete to get it, like an embodied scientist) or *nonrival* (codified knowledge that anyone can use, like computer code), or (ii) either *appropriable* (one can own it and easily exclude others, like a microscope) or *nonappropriable* (one cannot easily exclude others, like a trade secret, “because its disclosure cannot be easily contained”).⁸⁷ In terms of the conventional public/private distinction, “codified knowledge demonstrates a high degree of nonrivalry and its appropriation cost is high,” so it is a public good, while “embodied knowledge is rival and the cost of appropriation is lower,” so it is a private good.⁸⁸ And in terms of the distinction between basic (general, universal) and applied or technological research, the former conventionally “takes (by its very nature . . .) or should take (for reasons of efficiency . . .) the form of codified statements. Consequently it is a public good. . . . [And it] must be organized according to the rules of academic science . . . [where] disclosure is the norm.”⁸⁹ Overflow is, in this conventional perspective, inefficient and must be contained because it “leads to the privatization of basic science.”⁹⁰

Callon suggests that “rivalry, appropriability, and generality of knowledge” are actually not intrinsic properties of science, but rather that their extent “varies according to the structure of the networks in which they are produced.”⁹¹ Basic science is conventionally presumed to be codified statements accessible without regard to context, but because “no research finding (scientific statements) contains its own meaning” (which “is given to it extrinsically by the network of laboratories and competences within which it circulates”), reproducing knowledge requires a laboratory—“basic science [cannot] be reduced to . . . information.”⁹² A scientific theory is not *intrinsically* universal, but is rather diffused and translated as generality is gradually created in a network and its infrastructures.⁹³ Distinguishing between

85. *See id.* at 280.

86. *See id.* at 281.

87. *See id.* at 284–85.

88. *See id.* at 286.

89. *Id.*

90. *See id.*

91. *See id.* at 287.

92. *See id.* at 287–88. “[U]niversality is networked . . . [and] valid only in the rare places that have been configured to cater for it.” *Id.* at 289. It “is an outcome and not a starting point.” *Id.* at 290.

93. *See id.* at 290–92. A scientist producing knowledge must “interest, enroll, and ally with other

emergent and consolidated networks (as extremes on a continuum), Callon reverses the notion that nonrivalry, nonappropriability, and universality are given (or intrinsic to, or characteristic of) a new discovery—emerging scientific knowledge is actually “rival, appropriable, and specific” (or local) until it is replicated (at some cost) and becomes a consolidated configuration similar to Kuhn’s normal science.⁹⁴ Emergent and consolidated (or aligned) networks “are of course ideal types, purified forms that do not exist as such in reality. What we observe in the real world are hybrid evolving forms.”⁹⁵ Even the common distinction basic and applied research is questioned by Callon, who suggests the term “fundamental research” for emergent configurations, because the “capacity to produce basic knowledge” depends on participation in consolidated networks, where “basic” and “applied” research interacts.⁹⁶

A firm engaged in competition will align with laboratories having “competences and knowledge that are sufficiently different from its own to allow the opening up of new technological options, but sufficiently similar to make communication possible at a reasonable cost.”⁹⁷ This may involve tensions and trade-offs between (i) “openness, unexpected events, and new connections” and (ii) closed-ness, “known trajectories, and the reinforcement of links.”⁹⁸ University laboratories compete for these alliances to extend their networks, even as they might also compete for government grants to support teachers who engage in emergent (fundamental) research, publish, and attend conferences to facilitate relationships in the field.⁹⁹ But laboratories may *need* nonpublic funds, which engenders “a hybrid environment, a chain of intermediaries between . . . laboratories and firms.”¹⁰⁰ Alliances between laboratories and firms also presuppose “a sort of hybridization of firms, i.e., the integration by those firms of a core group of researchers” creating collaborative *boundary* organizations, perhaps “firm-laboratories.”¹⁰¹

Returning to the implied debate between Mirowski and Callon, it is difficult to extract a normative argument from Callon, paralleling and opposing Mirowski’s concerns over the degradation of science, that firm/laboratory interactions produce *better* science, but Callon does argue that his description is more accurate: overflows (from conventional frames) happen, they are likely “irresistible and irrevocable,” and laboratory autonomy is not threatened by “connections between

actors.” *Id.* at 293. “Little by little socio-technical networks emerge from such relationships, through progressive learning, iteration, negotiation, and adaptation.” *Id.* at 294.

94. *See id.* at 291–92.

95. *Id.* at 297.

96. *See id.* at 302.

97. *See id.* at 305.

98. *See id.* at 306.

99. *See id.* at 311.

100. *See id.*

101. *See id.* at 310–11 (explaining that integration might include giving the firm “access to the laboratory’s seminars,” or receiving scientists from public laboratories into the firm’s “own research centers”).

fundamental research and the economic market.”¹⁰² For my purposes, I have emphasized how Callon proceeds by challenging the assumptions and categories that provide the foundation for Mirowski’s concerns.

IV. STRUCTURAL EFFECTS OF THE ECONOMY ON SCIENCE IN LAW

*Economists stage a play with double-bound actors: [firms and laboratories] must interact but without crossing boundaries! The only relationships that these straitjacketed actors are allowed are those that emerge from the circulation of information. The conditions for their production are simply ignored. This is simple exchange economics, whereas one of the key points of science studies is that the circulation and production of knowledge are inseparable: they are one and the same process.*¹⁰³

The scientific enterprise can be conceived as virtually independent from the economy—except in cases of fraud driven by monetary greed, *external* financial support of science does not affect the *internal* workings of science. It is perhaps an advancement in the economics of science for some to argue that financial support is an unqualified good, as it fosters efficiency and innovation—they point to “hard-won” “[a]dvances in medical and surgical care” due to industry collaborations.¹⁰⁴ But others, while conceding the inevitability of funding as well as the “social forces and political agendas . . . result[ing] in significant scientific progress,” warn that the pressure of commercialization could reduce collaboration among scientists, undermine “scientific progress, and contribute to the premature application of technologies.”¹⁰⁵ The “statistically significant association between industry sponsorship and pro-industry conclusions” in one study has raised questions about whether we can trust biomedical research findings.¹⁰⁶ This too is an advancement, to recognize both positive and negative effects of the economy on science. Mirowski’s work constitutes yet another advance, by distinguishing between an individualistic and a systemic or structural perspective on the potential problems of interaction between the economy and science. If the problems are conflicts of interest, low methodological quality, or “accept[ing] remuneration geared to the outcome of a research project,” on the part of individual scientists, then we encourage transparency and disclosure, better methodology, and ethical guidelines.¹⁰⁷ But those solutions do not begin to address the sort of recent structural changes brought about by economic forces on science.

102. See *id.* at 312–13. Moreover, Callon notes that he has adopted “a point of view that is largely normative,” notwithstanding that the “sociology of science . . . proclaims itself agnostic.” Callon, *supra* note 68, at 418.

103. See Callon, *supra* note 23, at 287.

104. See Thomas P. Stossel, *Opinion: What’s Wrong with COI?*, SCIENTIST (June 12, 2012), <http://the-scientist.com/?articles.view/articleNo/32190/title/Opinion-What’s-Wrong-with-COI/>.

105. See Timothy Caulfield, *Opinion: Pressured to Commercialize*, SCIENTIST (May 28, 2012), <http://the-scientist.com/?articles.view/articleNo/32158/title/Opinion-Pressured-to-Commercialize/>.

106. See Justin E. Bekelman et al., *Scope and Impact of Financial Conflicts of Interest in Biomedical Research: A Systematic Review*, 289 JAMA 454, 454 (2003).

107. See Sylvia Rowe et al., *Funding Food Science and Nutrition Research: Financial Conflicts and Scientific Integrity*, 89 AM. J. CLINICAL NUTRITION 1285, 1288 (2009). On the issue of methodological quality, a

Recall Latour's version of the sociology of scientific knowledge, where the phenomena under analysis "have the characteristics of being narrative, collective, and outside [of us]. They are quasi objects; they are not of our own making. . . . That is it: real, narrated, social."¹⁰⁸ In the discourse concerning financial bias in science, many of the solutions offered to counteract the pernicious effects of the economy on science seem aimed at ensuring that scientific knowledge is not rhetorical or social. Get rid of the bad apples who fabricate data for financial gain, and look past or behind the false promises of the bad pharmaceutical company to discern the promotion of the "quick and dirty result over the calm and measured finding. . . . [Ultimately, getting] those new discoveries out the door and into the world as soon as possible."¹⁰⁹ Even Mirowski's critique of litigation science (research performed after a case has been brought), allegedly a structural analysis of the commercialization of science,¹¹⁰ is also an avowed critique of science studies. In *Daubert* on remand to the Ninth Circuit Court of Appeals, Judge Kozinski expressed a preference for science that preexists litigation because research "conducted independent of the litigation provides important, objective proof that the research comports with the dictates of good science. . . . [Experts relying on] existing research are less likely to have been biased toward a particular conclusion by the promise of remuneration."¹¹¹ That conclusion is questionable, given that existing pharmaceutical research is sometimes tainted; and there is no structural reason that litigation science is necessarily of a low quality.¹¹² (Judge Kozinski went on to say that law-enforcement forensic science, which is clearly litigation science, does not raise the same concern;¹¹³ that conclusion is also questionable in light of the recent National Academy of Sciences study of forensic science, which raised concerns about the independence of laboratories administered by law enforcement agencies and recommended public forensic laboratories.¹¹⁴) Yet Mirowski joins in the condemnation of litigation science, (i) viewing it as a corporate enterprise (even though in cases against pharmaceutical companies, it is the plaintiffs that need litigation science), (ii) labeling it just-in-time science and retaining the boundary

perceived problem, most investigations into commercial funding of scientific research have "found no association between sponsorship and overall methodological quality." Sergio Sismondo, *Pharmaceutical Company Funding and Its Consequences: A Qualitative Systematic Review*, 29 CONTEMP. CLINICAL TRIALS 109, 109 (2008).

108. See Crawford, *supra* note 5, at 264.

109. See MIROWSKI, *supra* note 34, at 289.

110. See *id.* at 288 ("[T]he problem of scientific fraud will not be central to my current inquiry. Rather, I want to set out some relatively tractable notions of 'good science'" (citation omitted)).

111. *Daubert v. Merrell Dow Pharm., Inc.*, 43 F.3d 1311, 1317 (9th Cir. 1995) (citations omitted).

112. See Sheldon Krimsky, *The Ethical and Legal Foundations of Scientific 'Conflict of Interest,' in LAW AND ETHICS IN BIOMEDICAL RESEARCH: REGULATION, CONFLICT OF INTEREST, AND LIABILITY* 63, 63–81 (Trudo Lemmens & Duff R. Waring eds., 2006).

113. See *Daubert*, 43 F.3d at 1317 n.5.

114. See COMM. ON IDENTIFYING THE NEEDS OF THE FORENSIC SCI. CMTY., NAT'L RESEARCH COUNCIL, STRENGTHENING FORENSIC SCIENCE IN THE UNITED STATES: A PATH FORWARD 183–84 (2009).

“between purpose-built science for litigation . . . and academic science in standard peer-reviewed journals,” and (iii) blaming science studies for taking a symmetrical approach toward assessing critically both litigation science and peer-reviewed science: “[t]his is not the first time we observe science studies beginning to make a pact with neoliberal conceptions of knowledge, but it is certainly one of the most dispiriting.”¹¹⁵ Notwithstanding his starting point, that commercialization alone is not a marker of the decline of science,¹¹⁶ Mirowski seems to revert to an earlier (Mertonian) sociology of errors in science—only bad science is characterized by coproduction. Given that Mirowski’s examples of the structural effects of the economy on science *in law* are quite individualistic (corporate production of defense-friendly science, and applying for low-quality patents, although the latter is arguably a result of systemic commercial pressures), we might say that it is difficult to sustain a systemic or structural analysis of science and the economy while maintaining such normative distinctions between academic science and commercial pursuits, open and privatized science, and basic and applied science. As an example of an analysis of the interaction between science and the economy that does not assume such distinctions, we have Callon’s model, which blurs conventional boundaries to include hybrids (i) of basic and applied science (in consolidated networks), (ii) of academic and commercial pursuits (in boundary configurations and hybrid firm/laboratories), and (iii) of public and private domains (e.g., science is not intrinsically a public good, but becomes a public good in the network as “a source of diversity and flexibility”).¹¹⁷

CONCLUSION

*[When sociologists of science] try to reconnect scientific objects with their . . . web of associations, . . . we always appear to weaken them, not to strengthen their claim to reality. . . . [W]e want to add reality to scientific objects, but, inevitably, through a sort of tragic bias, we seem always to be subtracting some bit from it.*¹¹⁸

I am beginning to appreciate Latour’s goal of description in advance of critique, of groundwork, of trying to understand how given or conventional categories and distinctions might eclipse certain features of the interactions between law, science, and the economy. It is not simply that we need to acknowledge the complexity of that interaction by creating new categories (such as hybrids), or by adding more pieces to the analysis,¹¹⁹ but that we need to acknowledge a particular

115. See MIROWSKI, *supra* note 34, at 304–05.

116. See Mirowski & Van Horn, *supra* note 32, at 507–08.

117. See Callon, *supra* note 68, at 395, 416 (“[P]ublic and private science are complementary despite being distinct: each draws on the other. . . . A firm that funds diversity by supporting new collectives is producing a public good and the government agency that contributes to a yet stronger linkage between the research it funds and the perfecting of Tomahawk missiles are supporting a science that can doubtless be called private.”).

118. See Latour, *supra* note 10, at 237.

119. See Theodore M. Porter, *Thin Description: Surface and Depth in Science and Science Studies*, 27 OSIRIS 209, 211 (2012) (“While thick description does tend to get complicated . . . it does not refer merely to an abundance of detail.”).

kind of complexity that is not visible when conventional boundaries are honored—we may need to get rid of some assumed pieces (such as inside/outside distinctions) and do away with shortcuts (whether that shortcut is assuming commercialization always degrades science or always fosters efficiency and innovation). Distinctions can remain—science is neither *the* law nor *the* economy—but the boundaries are blurred when a scientific expert appears in court or a scientific report is delivered to a governmental agency, because the acquired science is a coproduction or hybrid of legal recognition, scientific practices, and economic structures. Expertise is mutually constituted and conditioned by all three enterprises in one domain; and because each enterprise is equally rhetorical, social, institutional, political, and historical, there is no priority or privileging based on one enterprise escaping from culture. Establishing that analytical framework is a descriptive project, and is easily criticized for leaving everything as it is—“fine, it is a coproduction, but the scientific enterprise, the economy, and the law open for business tomorrow and do not change or improve on the basis of a more accurate description.”

On the other hand, to say that all of the science that is appropriated by law (on terms established in law but informed by science) is structured and constituted, not merely influenced or supported, by economic forces, is a critique. It is a critique of Judge Kozinski’s categorization of litigation science as suspect (and the categorization of law enforcement laboratory forensic science as reliable in *Daubert* on remand);¹²⁰ it is a critique of David Goodstein’s “insider” description (for federal judges) of how science works (with social and rhetorical supports that have little effect on the results of research);¹²¹ it is a critique of Paula Stephan’s mundane acknowledgment of the economy “shap[ing] science” with no cause for alarm;¹²² and it is a critique of Professors Thomas McGarity and Wendy Wagner, who in *Bending Science: How Special Interests Corrupt Public Health Research* sound the alarm, but focus on *direct* influences on science by bad actors.¹²³ These critiques, however, are only constructive if judges and regulators can understand that revealing science as a coproduction of social, rhetorical, and natural “forces” is not a critique of the scientific enterprise, but an arguable description of our state of affairs.

Whether law, science, and the economy are one domain, once conventional boundaries are erased, remains a question. I return to my epigraph about getting rid of all the usual categories “because they divide up a cloth that we want seamless in order to study it as we choose”¹²⁴—it is seamless for a moment, but then new categories and boundaries are generated by any critical analysis. Even Callon, in his critique of Mirowski for maintaining traditional distinctions, assumptions, and categories, immediately introduces new distinctions, assumptions, and categories

120. See MIROWSKI, *supra* note 34 and accompanying text.

121. See Goodstein, *supra* note 30 and accompanying text.

122. See STEPHAN, *supra* note 31.

123. See David S. Caudill, *Economics and/of Science: The Meaning(s) of Financial Bias and the Ideal of Interest-Free Science in Law*, 16 SMU SCI. & TECH. L. REV. 247, 264–65 (2013).

124. See LATOUR, *supra* note 1 and accompanying text.

(like “emergent” and “consolidated” configurations). Law and literature can be conceived as one domain, but we immediately start to address what type of literature law is (not a novel or a poem or a play, but literature or literary nonetheless). In breaking down distinctions between law and society, we do not stop identifying analytical categories—we simply get rid of some old ones. Law, science, and the economy are one, in a sense, but not in the sense that there are no new boundaries to replace conventional categories and distinctions.