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## GOSSIP AND METAPHYSICS: THE PERSONAL TURN IN JURISPRUDENTIAL WRITING

Michael Ansaldi\*

PATTERNS OF AMERICAN JURISPRUDENCE. By *Neil Duxbury*. Oxford: Clarendon Press. 1995. Pp. viii, 520. \$49.95.

AMERICAN LEGAL REALISM AND EMPIRICAL SOCIAL SCIENCE. By *John Henry Schlegel*. Chapel Hill: University of North Carolina Press. 1995. Pp. xii, 418. \$55.

[D]emocracy must be founded . . . on a faith that in the long run ideas are more important than the men who form them.

— Lon L. Fuller<sup>1</sup>

I like persons better than principles, and I like persons with no principles better than anything else in the world.

— Oscar Wilde<sup>2</sup>

The Franco-Rumanian existentialist E.M. Cioran once opined that the two most interesting things in this world are gossip and metaphysics.<sup>3</sup> For those so minded — and I confess I am among them — the combined prospect of gossip *about* metaphysicians, the details of Hannah Arendt's affair with Martin Heidegger,<sup>4</sup> for example, or of Bertrand Russell's physical gifts,<sup>5</sup> provides a special *frisson*, a *ne plus ultra* of satiated prurience. The desire to be in this particular know, for me at any rate, is quite literally irresistible.

It is tempting, of course, to ascribe this to the general depravity of the age of *People* magazine: why should *Luftmenschen* miss out on the simple pleasures of the *homme moyen sensuel*? Enquiring (master-) minds want to know too. Hence, the philosopher as celebrity. For "Wallis and Edward" or "Burt and Loni," just substitute "Jean-Paul and Simone" or "[famous logical positivist] and

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1. LON L. FULLER, *THE LAW IN QUEST OF ITSELF* 122-23 (1940).

2. OSCAR WILDE, *The Picture Of Dorian Gray* (1891), reprinted in *PLAYS, PROSE WRITINGS AND POEMS* 67, 78 (Everyman's Lib. 1972).

3. See Joseph Brodsky, *Isaiah Berlin at Eighty*, N.Y. REV. BOOKS, Aug. 17, 1989, at 44, 45.

4. See generally ELZBIETA ETTINGER, *HANNAH ARENDT/MARTIN HEIDEGGER* (1995).

5. See A.L. ROWSE, *GLIMPSES OF THE GREAT* 13-14 (1985).

[rough-trade boyfriend].” And, as we know full well in this democratic age, we often need our celebrities taken down a peg or two to offset their fame. They’re just people, you know, “no different from anybody else,” or so one imagines the leveler’s gibe. Indeed, one well-known philosophy don at Oxford who turned her hand to literature was accused, for her troubles, of writing not so much novels as Harlequin romances for intellectuals.<sup>6</sup>

In reality, this curiosity about the lives of philosophers is not an entirely recent phenomenon. It dates back at least as far as Xenophon’s and Diogenes Laërtius’ tale-telling biographies of Socrates, not to mention the autobiographies of Augustine of Hippo and Jean-Jacques Rousseau, or Montaigne’s *Meditations*. Nonetheless, what was known popularly about philosophers — as opposed to their philosophies — tended to be limited to vignettes from their lives served up as inspirational *exempla*: Aquinas resisting the prostitutes his brother sent to tempt him;<sup>7</sup> Kant’s nightly stroll through Königsberg, so punctual that housewives set their watches by it; the assiduous Marx facing that daily pile of books in the British Museum. The aforementioned Diogenes Laërtius, who wrote *Lives of Eminent Philosophers*<sup>8</sup> sometime in the third century A.D., had no significant *epigoni*. Apart from him, thus, we lack real philosophical analogues to Suetonius’ *Lives of the Twelve Caesars* or Vasari’s *Lives of Renaissance Artists*, at least until relatively modern times. Indeed, it is probably fair to say that the instinctive reaction of a “premodern” educated sensibility to the inside scoop on Marx’s mistreatment of his children, for example, probably would have been something along the lines of: “What’s that got to do with his ideas? Does it invalidate them? Does it shed any light on them? If not, why should I care?” The unstated premise was that ideas — particularly philosophical ideas — should stand or fall on their own.

Literature offers an instructive comparison here. The lives of poets and novelists long have been a subject of enormous interest. Such interest often rests on the not entirely implausible, but ultimately reductionist, notion, held by much of the public, that their works are, in some direct way, really “about” their lives, and hence that a more detailed knowledge of the latter necessarily will enhance or clarify their appreciation of the former. A strong reaction against this sentiment emerged in the post-World War I era in the form of the so-called New Criticism. The New Critics taught that

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6. This barb was made in a review of a novel by Iris Murdoch, a prolific author of fiction and philosophy. I now, however, no longer can locate the original source.

7. This story is apparently hagiographers’ legend. See W.A. Wallace & J.A. Weisheipl, *St. Thomas Aquinas*, in 14 *NEW CATHOLIC ENCYCLOPEDIA* 102, 103 (William J. McDonald et al. eds., 1967). The fictional nature of the story, however, only underscores my point.

8. *DIOGENES LAËRTIUS, LIVES OF EMINENT PHILOSOPHERS* (R.D. Hicks, trans., Loeb Classical Library 1925).

the meaning of a work of art should be sought without reference to biographical or historical data about how it arose. Many artists — such as the poet W.H. Auden and the novelists Evelyn Waugh and Gore Vidal — reinforced this message by pleading with readers to confine their attentions to words and leave artists' lives alone, taking the position that knowledge of their lives was essentially irrelevant to their works. These protestations, however, reflected an understandable desire for privacy as much as their artistic principle, and Waugh and Vidal may have undercut their position by eventually publishing books of memoirs.<sup>9</sup>

The latest *nouvelle vague* to hit the shores of American legal academe or, at any rate, the latest *vague* I've caught — deconstructionism — by rights ought to leave us uninterested in the low-level folk who merely sling signs and thereby create texts. As we now know, it is not they who give them meaning, but rather I, the reader, happily at work in my interpretive community, humble *lecteur* revealed at last as true *auteur*.

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As may befit a discussion of American jurisprudence, let me start out with a rather Langdellian move, and present some classifications — ideal-types, if you will — of jurisprudential writing.<sup>10</sup> These classifications are all fairly standard, nothing exotic. Furthermore, my listing hardly will be exhaustive; I wouldn't dream of it. I only aim to identify some significant and recurring types.

1. *The Primary Source*: A work in which an author presents his own ideas on one or more abstract questions about law — such as whether it exists, what it is, where it comes from, and its relationship to justice or morality. Such works might include John Rawls's *A Theory of Justice*<sup>11</sup> or John Austin's *The Province of Jurisprudence Determined*.<sup>12</sup> They also might include works of shorter compass, such as Lon Fuller's law review article *Positivism and Fidelity to Law: A Reply to Professor Hart*,<sup>13</sup> to take a famous example. In any event, the jurisprudential ideas of a thinker, directly presented by him, are the hallmark of this type of writing. "Primary jurisprudence" might be another name for it.

2. *The Topical Collection* of primary sources by various authors: Thomas Grey's *The Legal Enforcement of Morality*,<sup>14</sup> for example, or

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9. See GORE VIDAL, *PALIMPSEST* (1995); EVELYN WAUGH, *A LITTLE LEARNING* (1964).

10. Actual works of jurisprudence naturally may exhibit features of more than one type.

11. JOHN RAWLS, *A THEORY OF JUSTICE* (1971).

12. JOHN AUSTIN, *THE PROVINCE OF JURISPRUDENCE DETERMINED* (Wilfrid E. Rawls ed., 1995).

13. Lon L. Fuller, *Positivism and Fidelity to Law: A Reply to Professor Hart*, 71 *HARV. L. REV.* 630 (1958).

14. THOMAS C. GREY, *THE LEGAL ENFORCEMENT OF MORALITY* (1983).

Feinberg & Gross's *Justice: Selected Readings*<sup>15</sup> would fall under this heading. Topical collections may present a wide range of opinion or a narrow one, in which case they approximate my Type 1. The *ideas* illustrated, however, and their worth in explaining legal phenomena, again lie at the heart of such works.

3. *The Critique*: This is an intellectual response to ideas presented in primary sources like the foregoing. Owen Fiss's *The Death of the Law?*<sup>16</sup> and Mari Matsuda's *Liberal Jurisprudence and Abstracted Visions of Human Nature: A Feminist Critique of Rawls' Theory of Justice* belong here.<sup>17</sup> A critique, naturally, also may serve as a vehicle for the presentation, or sharpening, of the critic's own ideas.

4. *The Restatement*: This category includes works, or portions of works, in which the author attempts to restate, perhaps more plainly or vividly, the jurisprudential ideas of another. Robert Gordon's *Unfreezing Legal Reality: Critical Approaches to Law*<sup>18</sup> might sit at the high end of this category. In this work, Gordon's initial approach to a jurisprudential question is one that, he frankly admits, derives from others' work rather than his own,<sup>19</sup> all en route to his own elegant synthesis. Study aids written for students in a jurisprudence course, on the other hand, might fall at the low end of the category.

5. *The Thematic Study*: This category embraces works such as Robert Summers's *Instrumentalism and American Legal Theory*,<sup>20</sup> the topically organized, chronologically overlapping chapters of James Herget's *American Jurisprudence, 1870-1970*,<sup>21</sup> and Karl Llewellyn's *Some Realism about Realism: A Reply to Dean Pound*.<sup>22</sup> The thematic study attempts to canvass and elucidate manifestations of a jurisprudential idea, or related ideas, in the works of several authors, or perhaps even of one very prolific author. Writing a history of jurisprudence can be approached, indeed, as a large-scale thematic study, or as a collection of thematic studies loosely organized around temporal sequence. This is a classic kind of "history of ideas." The thematic study will, almost by necessity, share features of Types 3 and 4, and could present Type 1 material as well.

The primary source, topical collection, critique, restatement, and thematic study are all unalloyed works of jurisprudence. Jurisprudential ideas *as such* are their direct and immediate concern.

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15. JOEL FEINBERG & HYMAN GROSS, *JUSTICE: SELECTED READINGS* (1977).

16. Owen M. Fiss, *The Death of the Law?*, 72 CORNELL L. REV. 1 (1986).

17. Mari J. Matsuda, *Liberal Jurisprudence and Abstracted Visions of Human Nature: A Feminist Critique of Rawls' Theory of Justice*, 16 N.M. L. REV. 613 (1986).

18. Robert W. Gordon, *Unfreezing Legal Reality: Critical Approaches to Law*, 15 FLA. ST. U. L. REV. 195 (1987).

19. *See id.* at 201 & n.5.

20. ROBERT SAMUELS SUMMERS, *INSTRUMENTALISM AND AMERICAN LEGAL THEORY* (1982).

21. JAMES E. HERGET, *AMERICAN JURISPRUDENCE, 1870-1970: A HISTORY* (1990).

22. Karl N. Llewellyn, *Some Realism about Realism: A Reply to Dean Pound*, 44 HARV. L. REV. 1222 (1931).

All belong to the heartland of jurisprudence. Naturally, however, there are other sorts of jurisprudential writing besides these — more specifically, works in which some other factor besides the ideas themselves intervenes and looms large as one organizing principle:

6. *The Author Study*: William Twining's *Karl Llewellyn and the Realist Movement*<sup>23</sup> and Robert Glennon's *The Iconoclast as Reformer: Jerome Frank's Impact on American Law*<sup>24</sup> clearly belong here. This type of writing contains elements of restatement and critique, and possibly thematic study as well. Because a single individual serves as its focus, the author study blends ideas with biography. It will range from mundane details — where was he born? — to intellectual chronicle — when and how did he come up with this idea? One reason we study a thinker's life is on the premise that his experiences and activities may shed light on his thinking. On the other hand, picaresque or baroque details of the subject's life may make a good story in their own right: the young Llewellyn winning the Iron Cross in World War I; Frank's peculiar eyeglass frames making him a suspect in the Leopold and Loeb murder case. Intellectual biography, it goes without saying, is a well-recognized type of intellectual history.

7. *The Group Study*: This type of work simultaneously pursues a thematic and biographical approach to the works and lives of a number of thinkers. These will have been cohesive enough to form a recognized school or to constitute an interconnected group, either in received opinion or in a particular scholar's view.<sup>25</sup>

8. *The Contextual Study*: This attempts to put jurisprudential ideas in the presumptively illuminating context of something else. Such a work might draw upon contemporaneous social developments or disciplines besides law to elucidate jurisprudential ideas. What historians or economists were thinking, for example, might shed light on the jurisprudence of the same period. The contextual study is clearly a very important subtype of intellectual history.

Types 6, 7 and 8 are still recognizably works of jurisprudence: legal-philosophical ideas lie at the center, though they may share that center with something else. There are, however, two last kinds of jurisprudence-related work that I should mention just briefly:

9. *The Biography*: This differs from an author study in that the biographer minimizes or eliminates the subject's ideas in the interests of presenting the presumably colorful life story of a person who also happened to be, *inter alia*, a legal philosopher. Popular biographies

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23. WILLIAM TWINING, *KARL LLEWELLYN AND THE REALIST MOVEMENT* (rev. ed. 1985).

24. ROBERT JEROME GLENNON, *THE ICONOCLAST AS REFORMER: JEROME FRANK'S IMPACT ON AMERICAN LAW* (1985).

25. Examples that come to mind from outside the law are F.O. Matthiessen's study of the James family, *THE JAMES FAMILY: A GROUP BIOGRAPHY* (1947) and Humphrey Carpenter's *THE INKLINGS* (1978), a study of the lives, works and careers of the Oxford-centered British writers J.R.R. Tolkien, C.S. Lewis, Charles Williams, and so on.

of Justice Holmes would fall into this category. In principle, there is no reason why there should not be a group biography of academic lawyers, other than the general dullness of their lives.

10. *The "Other Hat Worn"*: This type of work explores the nonjurisprudential aspects of a legal philosopher's career, for instance, So-and-So as procedural reformer. The more the other hat worn is substantively related to the jurisprudence hat, the more this type approaches Type 6, the author study.

With the biography and "other hat worn," we finally have left the ranks of jurisprudence altogether, so attenuated has any connection become. Nevertheless, someone with jurisprudential interests perhaps may learn from such a work relevant information he then may turn to more traditionally jurisprudential ends.

What is the point of this apparently essentialist exercise? It is not to suggest that there is, in principle, anything wrong with writing a work that is "not jurisprudence" or "not purely jurisprudence."<sup>26</sup> Rather, it lets me clarify a bias: a work that falls within any of the first eight categories has an a priori claim on my attention *as academic lawyer with an interest in jurisprudence*. It also will help to explain some methodological observations later in this essay.

Now it may turn out that I will come to regard any particular work of jurisprudence as wrongheaded, poorly executed, or otherwise uncongenial, in which case my attention will diminish or indeed entirely lapse. But *ex ante* it claimed my interest by type. Works of the "non-jurisprudence" variety, on the other hand, start out with no such presumption, at least when I am reading through a jurisprudential lens. "Show me," I think, "the burden is on you." As I read it, I may decide that my initial categorization of a work was wrong and move it into a different, explicitly jurisprudential slot. Maybe a work will appeal to me while I wear a different hat. I may conclude that my categories were drawn too narrowly in the first place and that I need to expand or loosen them up. All are possible, but no matter. It goes to show my initial mindset and expectations.

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Neil Duxbury's *Patterns of American Jurisprudence*<sup>27</sup> is a remarkable achievement. It is a thoughtful, detailed *tour d'horizon* of American jurisprudence over the last century and a quarter, from formalism to critical legal studies, with a glance or two beyond into feminism and critical race theory. The scope of his coverage is awe-

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26. And here, let me again intone my apotropaic chant: the above list is not meant to be exhaustive.

27. Neil Duxbury is Reader in Law at the University of Manchester in England.

inspiring: Duxbury apparently has read every important work of American jurisprudence, many not so important ones, plus a good deal more besides. His copious footnotes alone, if nothing else, guarantee that this work will become the academy's standard guide to the last hundred years in American jurisprudence. Duxbury devotes six lengthy chapters to formalism, legal realism, policy science, process jurisprudence, law and economics, and critical legal studies, respectively. They provide an essential starting point for newcomers to these areas and offer more knowledgeable readers an often provocative analysis, and — in the footnotes — a wealth of details.

Duxbury's approach to his topic, however, is not so much jurisprudential as historiographic:

The primary objective of this book is not to explore generally the problems that might arise from employing a handful of concepts and themes to explain a comparably large variety of ideas about law, but to try to demonstrate that our use of concepts and themes affects the way in which we represent the history of legal ideas. [Duxbury, pp. 1-2]

In Duxbury's view, a pendulum-swing model has warped our prior understanding of the history of American jurisprudence:

[F]irst there was formalism, epitomized by the Langdellian revolution; then came the realist revolt against formalism; after which came the renaissance of formalism, exemplified by both process jurisprudence and law and economics, which was superseded by the return to realism in the form of critical legal studies. The pendulum of history swings back and forth, accordingly, between formalism and realism.<sup>28</sup>

Elsewhere employing a pugilistic metaphor, he contends that "the history of modern American jurisprudence does not resemble a boxing contest . . . it is not simply the trading of punches between formalists and anti-formalists" (p. 471). He himself, by contrast, speaks of a "jurisprudential drift" (p. 54) from formalism to realism.

The distortive pendular model, Duxbury believes, has led to a simplistic, excessively schematic understanding of our post-Langdellian jurisprudential history, in which "certain basic themes — in particular, the themes of legal formalism and legal realism" have been developed "in an over-emphatic, sometimes overdramatic, fashion" (p. 2). If we just calm down, it seems, we will discover "complex patterns of ideas." We will see that "[j]urisprudential ideas are rarely born; equally rarely do they die. . . . Ideas — along with values, attitudes and beliefs — tend to emerge and decline, and sometimes they are revived and refined. But rarely do we see them born or die. History is not quite like

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28. P. 2; see also pp. 308-09 (recapitulating the traditional account and describing it as "the episodic conception of the past which seems to permeate American jurisprudential discourse").

that" (pp. 2-3). Duxbury labels his approach "jurisprudence as intellectual history."<sup>29</sup>

Many of us will recognize our own loose talk in Duxbury's description of the pendulum-swing approach, such as when we make in-class, off-the-cuff, quick summations *en passant*. Whether it really has the serious impact on our self-understanding that he suggests is another matter. In so heavily footnoted a work, one would expect to find so central a point corroborated by long string-cites showing all manner of legal academics falling victim — in print and thus presumably at their most cautiously reflective — to this misconception. For the most part I do not see the corroboration and indeed suspect that Duxbury's point would be hard to substantiate fairly,<sup>30</sup> particularly among our major writers in jurisprudence. They are really not as obtuse as all that. The metaphor I would use, instead of a pendulum swing, is drawn from art: there are quick charcoal sketches and full-length oil portraits. Both have their appropriate occasions.

Part and parcel of the pendulum-swing notion is an allegedly binomial, either-or pairing of "formalism" and "realism-antiformalism." Writers on American jurisprudence, this appears to suggest, have been wont to gloss over complications and to assign jurisprudential thinkers and ideas to one or the other category. Let us consider an example, however, that I think will illustrate a danger of writing "jurisprudence as intellectual history." At one point, Duxbury attempts to provide a microlevel example of his overarching theory: "[S]ome commentators," he says, have charted "a straight and uncluttered path from Holmes to the legal realists" that produces "oversimplified intellectual history" by concentrating on the antiformalist side of Holmes and ignoring the conflicting proformalist strains of his work (p. 46). In support of this allegation of "oversimplified intellectual history" are adduced four items of evidence: Wilfrid E. Rumble, Jr.'s 1968 book *American Legal Realism: Skepticism, Reform and the Judicial Process*;<sup>31</sup> Bernie R. Burrus's 1962 article *American Legal Realism*,<sup>32</sup> Ralph J. Savarese's 1965 article of the same name;<sup>33</sup> and William Twining's discussion

29. See pp. 1-7.

30. To do this fairly would require not just isolated quotations, but an assessment of overall context, and perhaps even of a large body of a writer's work.

31. WILFRID E. RUMBLE, JR., *AMERICAN LEGAL REALISM* 38-44 (1968). The introduction to the book indicates that Mr. Rumble was on the faculty at Vassar College.

32. Bernie R. Burrus, *American Legal Realism*, 8 *HOW. L.J.* 36, 37-38 (1962). Professor Duxbury mistakenly gives the cite as 6 *HOW. L.J.* Mr. Burrus is identified there as shortly about to begin as an Assistant Professor at Georgetown University Law School.

33. Ralph J. Savarese, *American Legal Realism*, 3 *HOUS. L. REV.* 180, 186-87 (1965). The text identifies Mr. Savarese as a member of the District of Columbia and New York bars.

of Holmes in his 1973 study of Karl Llewellyn.<sup>34</sup> As I read them, however, two of these sources, Rumble and Twining, provide no support at all for the thesis of a commentator-distorted Holmes. The other two, Burrus and Savarese, provide, most charitably construed, only the flimsiest kind of support. One might, indeed, have thought it *infra dig* to rely on them at all. I pick the actual nits in a footnote.<sup>35</sup> My conclusion, however, is that Duxbury here does

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34. TWINING, *supra* note 23, at 15-20.

35. Duxbury does not provide a definition of formalism, and I certainly shall not attempt it in a footnote. For purposes of making a methodological point in a book review, I proceed on the assumption that we have an intuitive sense of what formalism and antiformalism are. Interested readers doubtless will wish to consult Thomas C. Grey, *Langdell's Orthodoxy*, 45 U. PITT. L. REV. 1 (1983).

Wilfrid Rumble indeed does provide a list of features of Holmes's thought that might be described as antiformalist and that certain realists hence found attractive. Presumably, to support Duxbury's thesis, one then needs to make the *argumentum e silentio*: since Rumble's discussion does not mention the (pro-)formalist sides of Holmes's thought, it impliedly is representing that it did not have any, and that this allegedly significant silence distorts the "real" Holmes. Even granting — for the sake of discussion — that an argument from silence can, in principle, have this kind of probative value, it would be outborne by Rumble's accompanying text, which is ringed with caveats: "The correspondence between these [Holmesian antiformalist] ideas and those of the legal realists will become clear later in this study. The immediate purpose is simply to indicate *those points* in Holmes's *overall* picture of the judicial process which profoundly influenced the views of the legal realists." Rumble, *supra* note 31, at 41 (emphasis supplied). "Any attempt to draw parallels between the views of Holmes and those of the legal realists is, of course, dangerous. It is likely for one thing to de-emphasize the tensions within Holmes's thinking. . . . [L]ike most great thinkers, his ideas are not without their own inner stress." *Id.* at 43. "An exposition of points of similarity is likely, also, to overemphasize the doctrinal unity within the realist movement." *Id.* at 44.

William Twining's discussion, which Duxbury claims "treats Holmes purely as an antiformalist" (p. 46 n.147), indeed does cite many of Holmes's well-known antiformalist chestnuts: the attack on Langdell as legal theologian; the antithesis of logic and experience; the bad man, indifferent to "axioms or deductions," trying to determine what courts will in fact do. Any chance that we would come away with an image of Ollie-one-note, however, is diminished and perhaps eliminated by accompanying discussion and citations of some formalist strands in Holmes's thought as well: "[Holmes] dismissed as unenlightened the practical minded who undervalued jurisprudence ('We have too little theory in the law rather than too much')." TWINING, *supra* note 23, at 17 (quoting OLIVER WENDELL HOLMES, *The Path of the Law*, in COLLECTED LEGAL PAPERS 167, 198 (1920)). Noting that "Holmes was careful to dissociate himself from the anti-intellectualism and narrow-mindedness of some men of affairs," Twining goes on to quote another famous proformalist passage: "'An intellect great enough to win the prize needs other food besides success. The remoter and more general aspects of law are those which give it universal interest.'" *Id.* at 18-19 (quoting HOLMES, *supra*, at 202).

The discussions of Holmes by Burrus and Savarese are each a total of three paragraphs in length, and the majority of the text in each case is devoted to quotation of the same three *loci classici*: the "logic and experience" passage from *The Common Law*, and the "bad man" and "cynical acid" passages from *The Path of the Law*. With one exception in Savarese, Duxbury basically has to rely on the probative value of the *argumentum e silentio*. In principle, such value is, at best, very weak. Here, it approaches zero: anyone who expects a rounded picture of a well-known Great Mind in three paragraphs deserves to be misled. Furthermore, I argue in the main text that a reader of a pure jurisprudence text — as opposed to texts of other kinds — has no legitimate expectation of receiving a complete portrait of the authors on which the text relies. See *infra* text accompanying notes 36-37.

At one point in his discussion, Savarese does veer somewhat off the edge with a reference to Holmes's "entirely empirical and skeptical definition of law in his celebrated essay *The Path of the Law*." Savarese, *supra* note 33, at 187. I would have amended the adverb to

precisely what he accuses others of doing: writing oversimplified intellectual history. Hence, when he puts on the trunks to get in the ring with the pendulum-swing-binomial-pairing model of American jurisprudential history, one winds up somehow feeling that Professor Duxbury has just K.O.-ed his own straw man.

How does Duxbury's overconstrual of a number of secondary sources about Holmes illustrate a generic danger of writing "jurisprudence as intellectual history"? To try to explain my point, let me first propose a number of imaginary jurisprudential statements, such as might be made in the different types of jurisprudential writing:

1. "Experience and intuition are what judges use when they decide cases." (Primary Source)
2. "It is not experience and intuition, but logic, that dictates the outcome of cases." (Critique)
3. "When we consider the various ways jurisprudential thinkers have sought to explain the judicial process, we can recognize two divergent strands: an experiential-intuitionist approach and a logico-rationalist approach, with the latter generally predominating in standard academic discussions of the matter." (Thematic Study)
4. "While Jones's antiformalist *bons mots* are the more frequently quoted, they in fact are equaled in number by expressions of his proformalist sentiments." (Author Study — Intellectual Biography)
5.
  - a. "Jones's well-known antiformalism can be understood better if we look at what his Pragmatist friends were writing at about the same time." (Contextual Study — Intellectual History)
  - b. "Smith misinterprets Jones by referring to him as an antiformalist pure and simple." (History of Ideas — Intellectual History).
6. "The workings of the judicial process can be explained best as a mixture of logic and experience. Overemphasizing either one distorts the reality." (Primary Jurisprudence)

When one chooses, as Duxbury has done, to write about jurisprudence historically, there is, right at the outset, a hybrid mixture of perspectives and approaches. The core of jurisprudence is abstract ideas about law, while history ultimately relates to persons. With the benefit of a hundred years of discussion behind us, one may, for example, conceivably feel the stirrings of a "purely" jurisprudential statement like number 6, but because one is also writing history, one feels compelled to illustrate it — or at least the second half of it — with a statement like number 5(b). Conversely, there

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"largely" myself. But this hyperbole is mitigated by the implications of Savarese's earlier statement that "early recognition by Holmes of the significant role of nonlogical factors in the judicial process can be found in [the logic-and-experience passage]." *Id.* at 186-87. To belabor the obvious, "a significant role of non-logical factors" necessarily suggests some role for logical ones.

In any event, I am not aware that either the Burrus or the Savarese paragraphs have been crucial to the way we understand Holmes.

may be an obvious sequential progression, through a number of logical stages, from one jurisprudential concept to another. Yet because one also is writing history, because there is a story to be told, one feels a need to illustrate the stages of that progression with concrete individuals whose writing "exemplifies" them. Indeed, one whose view of prior historiography is premised on a dialectic model will go out looking for stages and doubtless will find them. The simultaneous imperatives of the jurisprudence-history hybrid, however, may make it hard to do either one full justice. For example, an Italian legal philosopher writing in another context may have an obvious riposte to a jurisprudential position taken by an American thinker, but such a response lies outside of American jurisprudential *history*. Alternatively, people may be used distortingly as proxies for ideas they occasionally expressed, or their words may be overconstrued so as to fill in a slot in the dialectic schema. Ideas are dramatized, and sometimes overdramatized, with instantiating "representatives," when, to do real justice to the actual people involved, an author study is what is really called for. Thus, as with his discussion of commentators on Holmes (p. 46), I did from time to time feel that Duxbury was providing somewhat tendentious readings of his sources. This did not occur often enough, however, to shake my basic faith in him as fair-minded guide. Admittedly, it is not always easy to distinguish "tendentious" from "different from my interpretation."

There is a further methodological point to be made about the difference between jurisprudence and intellectual history. When discussing Holmes as the realists' precursor and criticizing those later commentators who draw a straight line from themselves back to the great jurist, Duxbury reproaches the realists: "Various realists gleaned from Holmes all that corresponded with their particular versions of antiformalism, and left behind them all that did not" (p. 46). To which, however, the appropriate response can be only: "If so,<sup>36</sup> so what?" To the extent that the realists pursued a Type-1 jurisprudential program — by which I mean they explored or advanced their abstract ideas about law — they had no obligation to present a rounded portrait of Holmes. To a jurist acting solely as such, Holmes legitimately might be regarded as a source of

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36. In support of his thesis of realists themselves distorting Holmes, Duxbury cites two sources: Morris R. Cohen, *Justice Holmes and the Nature of Law*, 31 COLUM. L. REV. 352, 356, 363 (1931) and Thomas C. Grey, *Holmes and Legal Pragmatism*, 41 STAN. L. REV. 787, 818 (1989). In each case, there is exactly one specific instance mentioned, and it is the same one in both: JEROME FRANK, *LAW AND THE MODERN MIND* (1930). I would be delighted to hear an explanation of why constantly citing the most extreme representative of a particular movement — or "mood," as Duxbury would have it — as though he were equal to legal realism as a whole is not an example of "overdramatic, oversimplified" jurisprudential history.

ideas, a quarry of pithy phrasings of antiformalist insights.<sup>37</sup> Such thinkers may have found Holmes's formalist side unpersuasive or contrary to what American jurisprudence then needed to hear. Perhaps they were campaigning for a temporary divorce of logic and experience, or perhaps a permanent one. In any case, I see no problem with selective quotation. Critics of realism, in turn, were equally free to cite only Holmes's more formalist observations. In a Type-5 author study, by contrast, which simultaneously must restate its subject's overall jurisprudence and present his intellectual history, such selectivity would be a glaring defect. I judge it inappropriate for Duxbury to hold the realists to the standards of another subfield — the author study — when they were undertaking primary jurisprudence. It is inappropriate — a category mistake — to treat jurisprudence like intellectual history. This is not to say that intellectual history should not be written about jurisprudence. But it pays to keep different types of ideas separate.

Earlier on, I stated a bias in favor of a certain brand of jurisprudential writing, and my final critical observation must be viewed against that background. I would have liked for Professor Duxbury to talk more "across" the chapters, stepping back from the stories in each to give us his own views about which, if any, of the arguments approached a "right" answer.<sup>38</sup> A distinctive auctorial point of view would have made it more the sort of jurisprudence text I prefer. While I regret its absence, I also recognize that it would have made an already very long book even longer.

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But enough carping about methodology. When all is said and done, Duxbury's work tells a richly detailed story — interspersed with the odd lapidary phrasing — of the intellectual enterprise called "American jurisprudence since Langdell." Its overall quality stands, despite my disagreements with various of his judgments. To my mind, his work reaches its best level not when summarizing, paraphrasing, and marshaling books of jurisprudence and law review articles, but rather when it puts a particular school, movement, or "mood" in that jurisprudence into a nonlegal context. Especially good, in my view, are the discussions of legal realism's background in artistic and social-scientific realism and in institutional economics; the Chicago school's rootedness in the economic thought of the rest of the University of Chicago; and the connections between the

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37. Moreover, commentators on realism legitimately might emphasize those features of Holmes's thought that influenced the realists.

38. Duxbury is, however, quite good about putting his finger on the intellectual weaknesses of the jurisprudential patterns he discusses, and so to that extent he does take substantive positions, albeit only negative ones. See, for example, his assessment of John Hart Ely's version of "process" jurisprudence. See pp. 292-93.

nascent critical legal studies and the "New Left." Along the same lines, I regret that we did not hear more about pragmatism in the chapter on legal realism.<sup>39</sup>

*Formalism:* Duxbury's introductory chapter on legal formalism seeks to demonstrate that there are two strands of legal formalism, only one of which involves deductive legal reasoning, which he calls "academic formalism." He notes that the Socratic method, the anointed vehicle for the inculcation of legal formalism, actually arose at Columbia, not Harvard, and that Langdell was only its first Harvard practitioner. Langdell's pupil, James Barr Ames, was its true apostle, changing its whole orientation from teaching doctrine to teaching legal reasoning. Duxbury makes the interesting observation that the philosophy underlying the case method — which Langdell apparently saw as analogous to contemporaneous innovations in the teaching of chemistry — was itself inherently Darwinist: survival of the fittest cases.

The more "famous" realists like Karl Llewellyn and Jerome Frank tended to train their fire primarily on academic formalism. Duxbury, however, believes that one also should understand formalism to include a different, forensic formalism: the laissez-faire, social Darwinist approach of late-nineteenth- and early-twentieth-century courts to, for example, labor contracts, an approach that deemed all parties legally equal, whatever their real economic power. Duxbury is not the first to undertake this renewed emphasis: Joseph Singer was making a similar argument in 1988,<sup>40</sup> and doubtless there were others.

Duxbury finds it inapt to speak of a "revolt against formalism"<sup>41</sup> in law in the sense of a sudden break with tradition by the realists. He tries to show, as I mentioned above, a "jurisprudential drift" from formalism to realism, describing the movement from one to the other as "very slow and hesitant" (p. 3). To do this, he looks at the thought of four "in-between" figures — Oliver Wendell Holmes, Jr., Benjamin Nathan Cardozo, John Chipman Grey, and Roscoe Pound — each of whom had a foot in both camps. I doubt that anyone today will take exception with Duxbury's claims about the four. Readers who think a "revolt" necessarily involves unprecedented suddenness will agree with Duxbury's conclusion that the realists of the late twenties and early thirties did not revolt. Others may think it legitimate to use that word to refer to a period when antiformalist elements in the thinking of certain high-visibility legal academics and others had come to predominate, such that it was no

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39. Duxbury's discussion of pragmatism is a little over a page long. See pp. 127-29.

40. See Joseph William Singer, *Legal Realism Now*, 76 CAL. L. REV. 465, 475-95 (1988) (reviewing LAURA KALMAN, *LEGAL REALISM AT YALE: 1927-1960* (1986)).

41. See p. 3. The phrase is Morton White's.

longer possible to speak of a balance of opposing tendencies. As Engels said somewhere, changes of quantity eventually end up becoming changes of quality.

*Realism:* In any case, Duxbury judges realism to have been just a mood rather than a movement, *pace* Karl Llewellyn<sup>42</sup>, and certainly not a school. Having just quibbled over the word "revolt," I do not want to go through the same process all over again. I just would note that one possibly alarmist view of Duxbury's general strategy is to see him as minimizing realism's substantive significance ("no big deal") while conceding its power to generate sound and fury ("much ado about nothing"), only later to pronounce it a failure.<sup>43</sup> Readers who wonder how a mood can be a failure should not look to me for an explanation.

In discussing the famous Llewellyn-Pound debate of the early 1930s, Duxbury reaches the conclusion that "there are no good historical or conceptual reasons for demarcating the prerealists from the realists, and that realism accordingly should be regarded as the continuation of a particular trend — namely, the growing dissatisfaction with legal formalism — rather than as the beginning of something substantively new" (p. 77). This is rather like saying that because *Homo sapiens* is not all that genetically different from chimps, *Homo sapiens* is nothing new. It is all a matter of with whom you are comparing him, I suppose. On that logic, because there are no obvious lines of demarcation, we could make the same statement about orangutans, sloths, and for that matter proto-plasms. Man, after all, is nothing new: he is still just organic matter.

It is true that Llewellyn was not far removed from "protorealists" like Holmes, whom he pronounced our greatest jurist,<sup>44</sup> or Cardozo, to whom he dedicated his early study of American case law.<sup>45</sup> But was he not, to leave Jerome Frank quite out of the picture, already much further away than either of them from formalism? Think, for example, of Llewellyn's exposition of what he called the "Janus-faced case method,"<sup>46</sup> an early example of the radical indeterminacy thesis. At what point is it legitimate to recognize a change in quantity as a change in quality? On Duxbury's ontology, apparently never. Yet the change in quality is, I believe, precisely the difference between divergent strains in establishment

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42. See Llewellyn, *supra* note 22, at 1233-34.

43. See, e.g., pp. 158-59, 298.

44. See KARL LLEWELLYN, *RECHT, RECHTSLEBEN UND GESELLSCHAFT* 30 (1977) (1932).

45. See KARL LLEWELLYN, *THE CASE LAW SYSTEM IN AMERICA* xxxi (Paul Gewirtz ed. & Michael Ansaldi trans., Univ. of Chicago Press 1989) (1933).

46. See *id.* at 46-50; K.N. LLEWELLYN, *BRAMBLE BUSH* 74-76, 179 (Oceana Publications 1951) (1930).

thought and the thinking of anti-establishment mavericks. People paid more attention, and the implications of their thought penetrated the consciousness of more and more people.

I also think that it is just plain wrong to argue, as Duxbury does (pp. 130-31), that because realists had aspirations of predicting case outcomes, they were covert formalists. I see no necessary connection between a belief that case outcomes can be predicted, albeit with less than one hundred percent certainty, and formalism. Llewellyn, for one, thought that a fair amount of certainty was attainable, but only such certainty as would result *not* from the application of some formula but from the exercise of judgment by a trained lawyer focusing his attention not simply on legal logic but on social, sociological, and institutional factors as well. His was certainty over the run of cases, rather than certainty as regards any particular case: macro- but not microcertainty.<sup>47</sup> Hence, I find Duxbury's contention here unpersuasive.

I earlier complimented Duxbury for his detailed "situating" of legal realism in the context of realism in art and social sciences,<sup>48</sup> and for profiling the background of the more economic side of realism in institutional economics. His story also goes over some familiar terrain: the social-science leanings of certain Yale and Columbia faculty, the creation of the Johns Hopkins Institute for the Study of Law, and legal realist ideas for reforming legal education. He broaches less familiar territory with a discussion of realism on the Constitution, wherein he discerns the influence of Charles Beard, and with his brief but illuminating account of "Realism and the regulatory state." Here, Duxbury suggests that realist jurisprudence and the New Deal coincided only in a shared belief that law was or could be "a tool for shaping social policy" (p. 155). Also interesting is his treatment of Jerome Frank. He demonstrates that Frank's invocation of psychology in *Law and the Modern Mind*<sup>49</sup> was not as novel as I previously had thought. Duxbury leads off his section on Frank in quite an original way — with a discussion emphasizing Frank's use of concepts of symbolism and word magic.

Was realism ultimately just a failed mood? I am not sure that the success-failure matrix is the best one for assessing jurisprudence. We're not talking about Willy Loman here. But accepting the success-failure dichotomy for the moment — if you highlight realism's advocacy of an indissoluble marriage of law and social sci-

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47. Quite apart from *THE COMMON LAW TRADITION* (1960), written towards the end of his life, Llewellyn already was making such arguments in the early 1930s. See, e.g., LLEWELLYN, *supra* note 45, at 11-12, 76-82; see also Michael Ansaldi, *The German Llewellyn*, 58 *BROOK. L. REV.* 705, 775-77 (1992).

48. Here, I also would add the contemporaneous *verismo* movement in Italian opera as evidence of a transnational "mood."

49. FRANK, *supra* note 36.

ence to follow the temporary divorce of Is and Ought, as Duxbury does,<sup>50</sup> I too probably would have to find it a failure. Duxbury, however, goes on to state that the realists failed to construct "a convincing alternative" to "the Langdellian pedagogic framework" (p. 158). I am not quite sure what this means. If it means that realists still used the case method in their teaching, my response would be that they apparently found it a good tool for their purposes, and thus felt no need to come up with an alternative. In any case, as Joseph Singer has noted, realism fundamentally has altered the way we talk about cases.<sup>51</sup> Furthermore, the "cases and materials" teaching text the realists pioneered<sup>52</sup> has become the standard, replacing the old-style Langdellian compilation of cases *tout court*. Also, while legal education has not become as clinical in approach as Jerome Frank might have wanted, it has moved much further in that direction, with increased simulated and external learning opportunities for law students. Law schools now pay much more attention to skills other than appellate advocacy, a development of which Llewellyn doubtless would have approved, even while perhaps finding it insufficient.

What Duxbury may mean by a failure to come up with a pedagogic alternative to Langdellianism, however, is that realists had nothing to put in the place of specious deductive certainty: the "normative vacuum" at the heart of realism. If so, then he and I just view the same thing differently. What he sees as a failure, I regard as a success. Furthermore, I am not at all sure that they had nothing to put in its place; what they had to offer just may have been more modest and "realistic."

*Policy Science:* Surely it is some indication of realism's potency, if not "success," that some significant part of the jurisprudence that followed felt obliged to respond to it, particularly to that "normative vacuum." In other words, realism got people thinking hard. Duxbury treats the "policy-science" jurisprudence of Harold Lasswell and Myres McDougal as a "failed neo-realist initiative," but one that now consciously poured democratic political values into that normative hole (p. 164). It failed because those values proved to be as manipulable as the vacuum itself. Once again, there was no there there. Logic had failed, and so too would Western political values.

Duxbury's chapter provides a very clear account of this rather puzzling movement. He shows how policy science took one of the

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50. See p. 158. He is not alone in this assessment: see, for example, Elizabeth Warren, *Comments on Professor White's Paper*, ANN. SURV. AM. L. 49-55 (1988).

51. See Singer, *supra* note 40, at 473-75; see also JOHN HENRY MERRYMAN, *THE CIVIL LAW TRADITION* 66-67 (2d ed. 1985).

52. See LAURA KALMAN, *LEGAL REALISM AT YALE: 1927-1960*, at 78-97 (1986).

weaker strains in realism — its largely long-distance love affair with the social sciences — and pursued it to an incomprehensible extreme, to a belief that “the social sciences may be an invaluable source of normative guidance” (p. 172), to a “utopian vision . . . of a new world order in which social policy is generated, primarily, by social science” (p. 175). It may be one thing to say that society needs to stipulate to some values so as to be able to get along; it is quite another to try to wish away the contingent character of the stipulation by sending “science” out there to “find” the values for you. People were obviously desperate for a there to be there.

Duxbury notes that Lasswell’s background was as a propagandist (p. 166), and it seems obvious, from a cynical post-Vietnam vantage point, that policy science was largely about the preaching of American values. These basic values — individualism, power, respect, enlightenment, skill, wealth, well-being, rectitude and affection<sup>53</sup> — were a discovered datum, and their worth “a matter beyond political or moral debate — they exist, as it were, ‘beyond ethics’ ” (p. 178). It certainly strikes one as *echt Amerikanisch* to go around holding truths self-evident, while ignoring real-world complications. Individualism is obviously a swell thing, but how far does it go? When the father of two young children decides he needs to “find himself” and skips for the coast, that is a highly individualistic thing to have done, even though it probably conflicts with the wealth and well-being of the wife and kids.<sup>54</sup> Do we applaud Dad for his individualism? Reply hazy; ask again later.

Policy science also wanted to change legal education to mold “lawyers of the future” and strove to create a curriculum “suitable for training lawyers to put democratic values into policy” (p. 180). Its efforts constituted, Duxbury notes, the first attempt to conceive of lawyering as “an overtly political endeavour” (p. 164). It never really caught on, at least in part because it seemed too elitist and impractical. It does rather smack of a three-year civics course.<sup>55</sup>

*Process Jurisprudence:* Or then again, maybe the there did not matter as much as how you got wherever it was you were going. Duxbury’s title for the chapter on legal process jurisprudence, “Finding Faith in Reason,” nicely evokes his theme. Formalism had “logic” and “rules”; now came “reason” and “principle.” What’s the difference between “rule” and “principle”? Principle has three syllables? And then too, reason *is* a fair bit squishier than “logic.”

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53. The list changed a fair bit, and the above is my working out of what Duxbury suggests was a later final form. See pp. 178, 183.

54. This example is suggested by the work of Andrew Hacker.

55. While policy science, Duxbury notes, was generally a failure in the sense that its values ultimately were recognized as nonobjective and its program for reform of legal education was not accepted, it survives as an important school in international law. See pp. 196-99.

Emphasizing his antipendular point, Duxbury cautions us against the standard story in which process jurisprudence makes its debut in the post-World War II era as yet another response to realism. He contends instead that process jurisprudence “parallels if not precedes legal realism itself” (p. 205). On the other hand, “[c]ertainly process jurisprudence began to flourish once the mood of realism began to wane” (p. 205). What then is process jurisprudence? It, too, is not a school (p. 206), but this time an “attitude” toward law (p. 207) — and one “remarkably difficult to pin down,” “tend[ing] to bobble to the surface of, rather than to dominate, the works of those who shared it” (p. 207). Its stirrings can be traced in Langdellianism and legal realism itself, and still more palpably in jurists of the mettle of Robert Maynard Hutchins, John Chipman Gray, Roscoe Pound, and Benjamin Cardozo. Duxbury also identifies its presence in the works of a distinguished and highly diverse assortment of later scholars, including Lon Fuller, Henry Hart, Albert Sacks, Alexander Bickel, Harry Wellington, John Rawls, Herbert Wechsler, and John Hart Ely. Indeed, he finds it “embedded in modern American jurisprudential discourse” (p. 208).

What was distinctive about process jurisprudence, these “lawyers with an attitude”? In retrospect, they seemed to have two quite sensible if not particularly remarkable insights, plus a metaphysical belief. Process jurisprudence quite appropriately focused attention, first, on questions of institutional competence: “which institution within the legal process might be considered best equipped to deal with which problems?” (p. 233). Or, in another version, “the issue of what courts are good for — and not good for . . . [and] the whole range of questions as to the appropriate relationship between the federal courts and other organs of federal and state government.”<sup>56</sup> It should, however, be pointed out that Llewellyn, a realist, already was thinking along similar lines in the late 1920s.<sup>57</sup> Its second insight was simply that statutes should be interpreted with an eye to their purpose (pp. 228-30). Today that sounds almost fatuous, but obviously once it did not.

These *aperçus*, which seem more in the nature of midlevel political theory than jurisprudence, have little if anything to say about non-statutory adjudication. They were accompanied, however, by a

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56. P. 236 (quoting HENRY M. HART & HERBERT WECHSLER, *THE FEDERAL COURTS AND THE FEDERAL SYSTEM* xii (1953)); see also pp. 255-62 (reviewing Hart and Sacks on institutional competence).

57. See LLEWELLYN, *supra* note 45, at 66-68; cf. *id.* at 101-04. (This work was published in 1933, but the relevant portions had been completed by the beginning of 1930.) See *id.* at xxxvii. Lon Fuller, who was one of Duxbury's process jurisprudents to focus attention on institutional competence, see *infra* text accompanying note 61, had read this work of Llewellyn's. See Lon L. Fuller, *American Legal Realism*, 82 U. PA. L. REV. 429 (1934); L.L. Fuller, Book Review, 82 U. PA. L. REV. 551 (1934).

related faith in reason and a belief in consensus, which turn out to be not especially helpful for common-law judging, despite initial appearances. In an attempt to differentiate appropriate roles for courts and for legislatures, process jurisprudence ventured that while legislatures may set policy, courts must decide by principles, which they can locate through reason. What made something a principle rather than a policy? Why, the fact that courts could use it but not the other.

When they stepped off the semantic merry-go-round, process lawyers generally failed to make things any clearer. They were chasing a distinction without much if any substantive difference. Over and over and over again, principle and policy collapsed into the reality of judicial and legislative choice. You say to-may-to, I say to-mah-to; the fact that one plant grows in my garden and the other in yours does not make either of them any less a *Lycopersicon esculentum*.

If reason, ultimately, is just shorthand for the idea that judges and other decisionmakers should reflect before they act, amen to that. Yet, somehow I find apodictic assurances that reason — or reasoned elaboration,<sup>58</sup> or whatever you call it — will lead to unarguably right answers consistently underwhelming. I have never really gone in much for the oracular style of argumentation. Lowering a *deus ex machina* onto the set remains as plausible as it ever was. Sometimes, alas, it seems as if Walter Wheeler Cook, Jerome Frank, and Karl Llewellyn might never have written a word.

Of the whole process movement, Alexander Bickel's philosophy of judicial prudence, which postulated that judges should sometimes stifle themselves by making the essentially political judgment not to follow a principle out to its *n*th implication, came closest to practical wisdom — not to mention the reality of what goes on. A principle that does not have to be followed all the time sounds suspiciously like one policy colliding with a competing policy. Such a situation obviously calls for judgment, not reason.

*Law and Economics:*<sup>59</sup> Although there certainly were American lawyers interested in law and economics before the rise of the Chicago school, in Duxbury's opinion there was nothing cohesive enough to be called a law and economics movement. Those who

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58. See p. 259 (discussing Hart and Sacks on reasoned elaboration).

59. Indulging a version of his antipendular bent yet again, Professor Duxbury begins his chapter on the law and economics movement with a perhaps less than entirely riveting disquisition on whether that movement is or is not related to legal realism, critical legal studies, and process jurisprudence, and if so in what way and how much. It is not that the topic is out of place in such a work, but that it is out of place at the beginning of a chapter. Such a relatively detailed discussion would be better placed in an appendix: how can readers possibly assess the merits of his genealogical analysis before being told about either law and economics itself or critical legal studies, the subject of the following chapter?

did discuss law and economics during that time tended to distrust courts as makers of economic policy due to judicial adherence to laissez-faire principles; they looked instead to legislatures to fulfill that role. They also tended to regard market failures as a common phenomenon, fully correctable through governmental regulation.

Most of modern law and economics springs from very different sources. As I have noted already, Duxbury provides a quite interesting account of the competing personalities — including Milton Friedman — and theories in the University of Chicago's economics department, before and after its institutional embrace of neoclassicism. Disagreement in that department, in fact, led to the appointment in 1939 of a junior professor from its ranks to become the law school's first economist (p. 335).

Once again, Duxbury guides us away from an overly simplified history. There was no neat division between the "old" Chicago school of the 1940s and 1950s, which supposedly applied economic theory only to the more obviously economic areas of law like antitrust, tax, corporate law, and public utilities, and the "new" Chicago school of the 1960s and after, which purportedly then sought to extend such analysis into "non-market areas of legal activity" as well (pp. 380-81). While the simplification is not entirely devoid of truth, we learn in fact that during the mid-1940s "the seeds of the 'new' law and economics [already] were being planted" (p. 341).

Be that as it may, in that earlier period antitrust law unquestionably served as a key focus of the Chicago school. Under the guidance of Aaron Director, an economist who came to be what Duxbury calls a general *éminence grise* of the Law School, the Chicago school took the position that "the prevalence of private monopoly" was "exaggerated" (p. 338). It contended that since "it is virtually impossible to eliminate competition from economic life . . . monopolies are essentially unstable [because] new competitors will emerge" (p. 345). Accordingly, it concluded that "regulation is the proper function of markets rather than governments" (p. 343). This view of antitrust law generated a great deal of criticism, much of it coming from Harvard. Nevertheless, by the 1970s, "Chicago-inspired economic analysis was beginning clearly to dominate debate about antitrust policy in the United States" (p. 355), and this domination has continued, in part, because of the appointment of Chicago-style law and economics jurists to the federal bench during the Reagan and Bush presidencies.

As mentioned, this style of economic analysis eventually would be applied extensively to other areas of law, such as property law, that at first blush did not seem amenable to economic treatment. A key stage in this development was Ronald Coase's pioneering work on transaction costs, as well as his enunciation of the "Coase theo-

rem,” which held that in the absence of transaction costs “the legal assignment of property rights to parties involved in negotiation would have no effect on the eventual allocation of resources between them” (p. 387). As with its antitrust theories, the newer “nonmarket” applications of Chicago law and economics did not go unchallenged. This time the most pointed criticism emanated from Yale. Guido Calabresi thus summarized law and economics’ limitations:

“[E]conomic theory . . . cannot tell us how far we want to go to save lives and reduce accident costs. Economic theory can suggest one approach — the market — for making the decision. But decisions balancing lives against money or convenience cannot be purely monetary ones, so the market method is never the only one used.” . . . [C]ertain activities may be sustainable in market terms, [but] they may be prohibited for the simple reason that “there are some things we do not want in our society regardless of costs.”<sup>60</sup>

Duxbury concludes his chapter with an account of the career of the most vigorous academic advocate of an expansivist application of economic analysis to law: the “demonized” Richard Posner and his efficiency and wealth-maximization principles.

*Critical Legal Studies:* Duxbury finds the pendulum theory to have some validity here, though not in the way one might at first imagine: CLS is not a revival of realism, but it *is* a reaction by some segments of American law against the appeals to consensus and individualism implicit both in process jurisprudence and law and economics. Furthermore, unlike the “amorphous” mood that was realism, CLS qualifies as a real “movement” because it has been “more co-ordinated, institutionalized and clearly self-identified” (p. 425).

Duxbury begins his discussion with “the political backdrop,” i.e., the New Left, university-based beginnings of the movement, as well as the “law and development” programs at Harvard, Yale, Stanford, and Wisconsin, which attempted “to formulate valid generalizations about the relationships between law and the major economic, social, and political transformations associated with industrialization.”<sup>61</sup> These programs provided the training ground for the “law and society movement,” which Duxbury depicts as a partial precursor to CLS. But some who had originally been more sympathetic to the law and society movement ultimately grew critical of its apolitical style of analysis and its failure to recognize law’s ideological side.

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60. P. 393 (quoting GUIDO CALABRESI, *THE COST OF ACCIDENTS: A LEGAL AND ECONOMIC ANALYSIS* 18, 100 (1970)).

61. P. 437 (quoting David Trubek, *Toward a Social Theory of Law: An Essay on the Study of Law and Development*, 82 *YALE L.J.* 1, 21 (1972)).

CLS was "invented" in the late 1970s as a perception began to form that there was a "growing leftist presence in the law schools" (p. 447). A committee organized to plan a conference and draw up a list of who was to be invited. The first Conference on Critical Legal Studies, held in May of 1977 at the University of Wisconsin, produced both good scholarship and a fair amount of backbiting.

Duxbury surveys the works of a number of the best-known "Crits," including Morton Horwitz, Roberto Unger, and Duncan Kennedy. He observes that Horwitz's scholarship "endeavours to demonstrate that American judges, from the late eighteenth century onwards, began to mould common law doctrine so that it favoured mercantile, as opposed to other group interests within society" (p. 451). Unger's work, by contrast, he views as visionary, aimed at total critique and social transformation. He notes, however, that "the nature of the post-liberal, solidaristic society is something about which Unger is only able to speculate" (p. 455). Finally, Duxbury summarizes Kennedy's intellectual project throughout the 1970s, the so-called indeterminacy thesis, as an "elaborat[ion] . . . the tensions inherent in liberal legal thought and doctrine" (p. 457).

In its heyday, the 1980s, CLS grew, in Robert Gordon's words, "so exotically varied and internally divided as to defy characterization almost entirely."<sup>62</sup> Although older themes continued to be developed, new ones came to the fore, including "the inadequacy of rights discourse and the viability of nihilism as a basis for a legal theory" (p. 470). Duxbury also notes the emergence of "trashing" and the publication of the first edition of the CLS reader *The Politics of Law*.<sup>63</sup> Roberto Unger, meanwhile, continued to write about the society of the future, to which Duxbury responds with no little exasperation and a rather tart in-joke:

Assuming that the types of institutional revision deemed necessary for the transition from liberalism to post-liberalism could ever be effectuated, why should we wish to commit ourselves to such an upheaval? Yet again, Unger confesses and avoids. The first three volumes of *Politics* "merely suggest the outline of a vision that needs to be worked out later." So many books, pages and words, and Unger is still promising to deliver the goods next time. God, it seems, has not yet spoken.<sup>64</sup>

Here, Duxbury reveals his basic good sense: considering that law professors are generally the sort of people with whom one prefers to avoid even having lunch, one *certainly* does not want to live in a

62. P. 467 (quoting Robert W. Gordon, *Law and Ideology*, 3 *TICKUN* 14, 15 (1988)).

63. *THE POLITICS OF LAW* (David Kairys ed., 1982).

64. P. 481 (quoting ROBERTO M. UNGER, *FALSE NECESSITY* 560 (1987)).

society designed by them. One faculty meeting will convince anyone of that.

As with Judge Posner, Duxbury tells of the demonization of CLS, recounting the faculty tensions at Harvard — once described by David Trubek as “the Beirut of legal education” — and the brouhaha generated by a suggestion that adherents of CLS did not belong in legal academe. Duxbury draws a nice parallel between the Crits’ reactions to the latter and the earlier relationship between the New Left and the universities: Academe was an evil club in which the dissidents nonetheless wanted full membership.

It may be time to switch to the past tense when discussing CLS, Duxbury implies, because it “peaked in the American law schools in the middle of the 1980s and has been losing momentum ever since” (p. 426). If CLS is moribund, why? There are any number of reasons, of which Duxbury highlights three: the appeal of neopragmatism, generational conflicts within the movement, and the fissuring off of feminism and critical race theory. Here, in any event, I revert to my earlier question: does the success-failure model, to which the “death” idea is obviously related, provide the correct framework for judging jurisprudence? Even if the CLS movement dies, — in the sense, for example, that people who call themselves Crits cease to foregather as such — we still must ask whether, and how much, such a way of thinking has enriched or sharpened our knowledge of the law, our own personal jurisprudence. That, I think, is the question we need to ask about all the schools, or movements, or moods that Duxbury has so thoroughly and ably chronicled.

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What and whomever else he may have meant, we know for certain that Emil Cioran cannot have been referring to American jurisprudence or its creators when he alluded to the fascinations of gossip and metaphysics. On the metaphysical side, the most enduring achievements of American jurisprudence have, to my mind, largely been negative. Of a profession that so vaunts its allegedly Socratic methods, it is particularly fitting to say that, metaphysically, we are stalled in the aporia of the early Platonic dialogues. And gossip? *Odium philologicum*, the renowned pettiness of bookmen, schemes and squabbles over matters of little import — these would be mostly our topic here, supplemented no doubt by the odd drunk or seducer. Fairly tame stuff; entertaining enough in the hands of a C.P. Snow, David Lodge, or F.M. Cornford.

Neil Duxbury seeks to write jurisprudence as intellectual history. John Henry Schlegel,<sup>65</sup> author of *American Legal Realism and Empirical Social Science*, writes in a rather different key:

I believe that intellectual history, as traditionally understood as a history of ideas embodied in texts, is an essentially empty exercise, though intelligible as the practice of a group of historians who participate in a professional identity that sees history as a largely autonomous enterprise of academics responding to other academics. Rather than a history of ideas, intellectual history needs to be the history of intellectuals, people who do things with ideas — in this case in an academic setting. [Schlegel, pp. 4-5]

In an afterword to his book, he writes, in a still more radical vein:

It is time we consider giving up the history of ideas, giving up intellectual history as a history of the ideas of humans set apart from the rest of their lived experience, and to begin to write the history of intellectuals. . . . Let us simply stop the pretense that it is the dance of reason that we chronicle in intellectual history, if only in the name of more accurately representing the thinkers of the past as that humanistic ideal — people trying their best to get from Monday to Tuesday in as honorable a job as they have managed to find. Let us stop looking for the dance of reason and record the whole dance of life. [Pp. 260-61]

Schlegel's subject, a relatively small slice of Duxbury's, was largely unknown until Schlegel's publication of a series of articles, now reworked and expanded in this book. His subject is the empirical legal research done by some, but by no means all, of the historical legal realists — including Dean Charles E. Clark, William O. Douglas, W. Underhill Moore, Walter Wheeler Cook, Herman Oliphant, Leon Marshall, and Hessel Yntema.<sup>66</sup> So important was

65. Professor of Law, State University of New York at Buffalo School of Law.

66. Schlegel is certainly free to study whatever scholars, or groups of scholars, he sees fit, and the Columbia-Yale-Johns Hopkins focus makes obvious sense. But I fear his pique at Karl Llewellyn's disparaging remarks about the empirical work of his *enfant protégé* Underhill Moore and that of the Hopkins quartet, *see* pp. 146, 200, has led him to slight Llewellyn's place in the story he is telling of realist involvement with the social sciences. When describing how the focus of his own work would be different from that of William Twining's 1973 study of Llewellyn, *see* TWINING, *supra* note 23, Schlegel writes: "Since Llewellyn's major interests were jurisprudence and commercial law, [Twining's] biography could not have given a central place to empirical research by the Realists *because Llewellyn never really engaged in any.*" P. 6 (emphasis supplied).

While it is certainly true that the amount of empirical research done by Llewellyn was not of the same order as that of, say, Moore or Clark, Schlegel appears to have forgotten about KARL LLEWELLYN & E. ADAMSON HOEBEL, *THE CHEYENNE WAY* (1941), or the numerous summers in the late 1940s that Llewellyn did field work among the Pueblo in the American Southwest, yielding, admittedly, little other than "extensive field notes." TWINING, *supra* note 23, at 359. *See generally* Wolfgang Fikentscher, *Die Erforschung des lebenden Rechts in einer multikulturellen Gesellschaft: Karl N. Llewellyns Cheyenne- und Pueblo-Studien*, in *RECHTSREALISMUS, MULTIKULTURELLE GESELLSCHAFT UND HANDELSRECHT: KARL N. LLEWELLYN UND SEINE BEDEUTUNG HEUTE* 45 (Ulrich Drobnig & Manfred Rehbinder eds., 1994) [hereinafter *DROBNIG/REHBINDER COLLECTION*]. Naturally, this was not quantitative work, but it nonetheless qualifies as "empirical." Schlegel perhaps might respond that, even so, most of the data collection in each case was done by people other than Llewellyn himself. *See* TWINING, *supra* note 23, at 155, 358. But the same also could be said about most, if not

this work, Schlegel seems at times to suggest, that it may necessitate a revision of our basic concept of what legal realism was, to bring it in line with his view of intellectual history: "realism is best understood as something that the realists did."<sup>67</sup> At other points, he seems less insistent on the sole correctness of defining realism in his way and no other.<sup>68</sup>

That is just as well. It would make no sense to me to exclude the realists' jurisprudence, their most enduring contribution to American law, from consideration altogether. Sensibly, Schlegel does not do so. In principle I am either, as jurisprudential reader, open to having my understanding deepened by the presentation of jurisprudential ideas set against the backdrop of a writer's whole life and work, or, as nonjurisprudential reader, open to being told an interesting nonjurisprudential story.

On my earlier schema, Schlegel's work probably would be described as a mixture of the clearly jurisprudential "Group Study" (Type Seven) on the one hand, and the nonjurisprudential "Biography/Other Hat Worn" (Types Nine and Ten) on the other. Based on his earlier programmatic credo, I am quite sure Professor Schlegel would not be bothered by a claim that his work was "not jurisprudence," on my fairly traditional understanding of the same.

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all, of the realists Schlegel studies. Indeed, one aptly might describe the relationship between Llewellyn and his Columbia junior colleague Hoebel in roughly the same way Schlegel conceives of the relations between Moore, Clark and William O. Douglas, on the one hand, and the professional social scientists Dorothy Thomas and Emma Corstvet on the other: providing technical expertise on social-scientific methodology. Even though Llewellyn and Hoebel's field work among the Cheyenne would not satisfy the methodological requirements of contemporary anthropology, see Fikentscher, *supra*, at 54-55, *The Cheyenne Way* can nonetheless be said to have "inaugurated a new style of legal anthropology." Fikentscher, *supra*, at 55.

Not precisely germane to Schlegel's observation on Llewellyn's engaging in empirical research, yet still obviously related, is Llewellyn's posthumously published *RECHT, RECHTSLEBEN UND GESELLSCHAFT*, *supra* note 44, a theoretical work in which Llewellyn sought "to 'open up' the field of legal sociology, to offer it a possible program . . . tentatively proposing a few daring hypotheses and 'fantasies' about the way legal sociology might eventually organize its observations of the 'life of the law' in the context of the larger society." Ansaldi, *supra* note 47, at 746 (quoting LLEWELLYN, *supra* note 44). Perhaps Schlegel would count this as "cheerleading" for empirical social science, but such a characterization only would tend to assimilate Llewellyn to one of Schlegel's subjects, Walter Wheeler Cook. One wonders, in fact, whether any realist of the period, with the possible exception of Moore, thought about social science as systematically as Llewellyn did in this 1932 work. All in all, it is rather odd for one whose primary reputation abroad is for his sociology of law, see William Twining, *The Idea of Juristic Method: A Tribute to Karl Llewellyn*, in *DROBNIG/REHBINDER COLLECTION*, *supra*, 71, 72-73, Schlegel, pp. 45-186 *passim*, to figure so queerly in a work on American legal realism and the social sciences, empirical and otherwise.

67. P. 2. The phrase seems to echo Llewellyn's famous sentence in the *Bramble Bush* lectures, "What these [legal] officials do about disputes is, to my mind, the law itself." LLEWELLYN, *supra* note 46, at 3.

68. "I suppose that there is nothing wrong with jurists discussing Realism as jurisprudence, timeless answers to what are taken as timeless questions. For historians, though, it is a matter of some consequence whether Realism is seen as a jurisprudence rather than Realists[ ] as having or sharing a jurisprudence." P. 4.

Whether his work tilts toward one or the other end of the spectrum will depend on whether I think the nonjurisprudential material he presents illuminates my understanding of ideas. If it does not, or not primarily, the burden falls on him to show why I should be interested in his material. Perhaps his story is intrinsically interesting; perhaps it manifestly bears on other important concerns of academic lawyers; perhaps Schlegel's analysis exposes some latent bearing on those concerns.

*Clark and Douglas:* The realist social scientists named above all were associated with Columbia, Yale and Johns Hopkins, or some combination thereof. Charles Clark and William O. Douglas of Yale each began his empirical work in connection with some progressive law reform project. In Clark's case, it was procedural reform. The perceived need for such reform reflected in part a widely shared belief that "procedure was too technical and complicated and, as a result, allowed lawyers imbued with 'the sporting theory of justice' to avoid decisions on the merits of claims by playing procedural games" (p. 83). The narrow topic of Clark's project, on which he collaborated with Douglas, was the adjudication of civil cases by Connecticut courts. In the course of this work he learned of and ultimately adopted some of the values of the newly quantifying social sciences.

After a few months of field work, he concluded that state court civil litigation "was largely administrative," and he found that "jury trials were infrequent" (p. 83). From this, Clark "wonder[ed] about the appropriateness of using complex judicial machinery to resolve such apparently simple disputes" (p. 83). He later expanded his study to courts outside Connecticut, to criminal cases, and to a longer time period.

The results of the study of criminal cases flew in the face of the conventional wisdom: rather than finding widespread delay, "eighty-five percent of the cases were disposed in two months" (p. 89). In fact, Clark consistently concluded that the criminal process was almost too efficient. The study's sponsors, however, who had vested interests in the original reformist thesis, excluded these findings from the final report. On the civil side, two findings — that federal courts' diversity jurisdiction accounted for under twenty percent of filed cases and that those that were filed were fairly simple cases — upset some sponsors and delighted others, depending on the types of reforms they endorsed. The data also showed that "[e]ssentially uncontested matters — collections, divorces, and foreclosures — together with the largely settled matters, primarily tort cases, dominated the docket" (p. 92). Such evidence, however, proved "by and large irrelevant to the cause of reform as [Clark and his collaborators] knew it, since in a system where most cases are

uncontested or settled, technical procedure played little part, and thus to expend effort at its reform made little sense" (p. 92). Schlegel describes Clark's ensuing bewilderment and disappointment over the sponsors' conclusions that:

the fact that Clark's fact gathering was a "scientific" enterprise made no difference. Fact gathering that did not advance an immediate reform objective was scholarship not worth publishing, just as fact gathering that did not fit their model of how the world was structured was an "irrelevant jumble of figures." They would give or withhold their support for the newer empirical research in law just as they had for the older, less structured research. [pp. 97-98]

Clark undertook another empirical study, this time of the compensation received by auto accident victims, while Douglas studied business failures and focused in particular on the administration of the bankruptcy laws. Two nonlawyer professional social scientists — Dorothy Swaine Thomas and Emma Corstvet — collaborated on both projects, attempting to perfect the methodologies being followed. Yet lawyers and social scientists, Schlegel says, gradually drifted apart: the lawyers were called away to seemingly more attractive activities, having proved less than fully willing to regard facts as having much independent value, apart from some specific purpose; the social scientists, on the other hand, remained chiefly interested in making social observation a thoroughly professional, quantitative science.

*Johns Hopkins:* One item of fallout from the "Deanship Crisis" on the Columbia Law School faculty in 1928 — engendered when Columbia's President Nicholas Murray Butler passed over Herman Oliphant for the deanship — was the departure of Oliphant and two colleagues, Hessel Yntema and economist Leon Marshall, for Johns Hopkins University. There they joined Walter Wheeler Cook, who was then in the process of planning "a nonteaching, research institute for law" (p. 147). Each of the four faculty members "was to head up a 'practically independent' research unit" (p. 159). The research projects ultimately would encompass a study of the Ohio and Maryland court systems; litigation in New York; installment sales; and concurrent jurisdiction of the federal and state courts. In reality, however, these lawyers-empiricists generally lacked a clear sense of the purpose of their ultimately short-lived Institute. Schlegel's account indicates that there was not an enormous amount of enthusiasm among the four participants for actually doing empirical research of the sort that Clark and Douglas would be doing at Yale. That lack of enthusiasm, coupled with the administrative chores associated with a newly founded institution, the absence of secure funding, the incomprehension and jealousy of the rest of the Hopkins faculty, and the Depression, ultimately killed the Institute off.

*Underhill Moore*: Perhaps the most impressive achievement of Schlegel's book is its revisionist account of Underhill Moore — professor first at Columbia, then at Yale. Both Moore and Cook, as Schlegel describes them, belonged to one of the first generations of law teachers whose professional identity was as academics who were supposed to participate in the grand scholarly enterprise of keeping the post-Langdell formalist edifice of the law in good repair (pp. 25-26). Their lives ran parallel in other ways as well, their paths often crossing. They both began their careers in “colonial service” at law schools west of the Hudson and east of the Rockies doing traditional sorts of scholarship, then each gradually worked his way back to “civilization” on the East Coast (p. 67). Also, they jointly encountered the pragmatist philosopher John Dewey at Columbia, and that shared experience changed both their professional lives: Cook became a cheerleader for law and the social sciences, while Moore sloughed off his law professor identity and assumed that of a social-scientist wannabe (pp. 24-25, 57-62). Indeed, of all the realist lawyers who did social science work, Moore, in Schlegel's account, took most seriously the methodological demands of the field, and he thus broke free of the nexus between empirical research and reform.

Before the Schlegelian rehabilitation, it is safe to say that many people regarded Moore as a sort of law professor-madman, sitting outside in shorts and pith helmet counting cars. Schlegel's not inconsiderable achievement is to have converted our reaction from ridicule to pity, and perhaps even to outright sympathy. In all Moore's empirical research — from his study of banks' debiting of direct discounts and bank practices on recrediting stopped certified checks, to the famous parking studies — his methodological understanding of the social sciences grew progressively more sophisticated, whether he was studying the relation between law and custom or applying stimulus-response psychology to law-related behavior. This sophistication came about in part through his collaboration with Thomas and Corstvet, whose advice, admittedly, he did not always follow. But the rub was that he was lonely and misunderstood: the other Yale law professors at best did not understand him and at worst mocked him, and the professional social scientists at Yale's Institute for Human Relations considered him an amateur.

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I have barely outlined the “story” Schlegel tells about the empirical work done by legal realists. A number of essentially set pieces that help to provide context fore and aft accompany his tale; they include sections on the professionalization of law teaching, the “colonial service,” and the last chapter on the not particularly edifying subsequent history of social science research in American law

schools. All are illuminating and well done. Schlegel also must be commended for his expanded definition of the realist controversy, which includes not just the Llewellyn-Pound debate, but also the earlier Columbia Law Review symposium on Jerome Frank's *Law and the Modern Mind*.<sup>69</sup>

But again I must revert to my starting point. Schlegel's book is not jurisprudence, or even jurisprudential history, in anything approaching a classical sense. Truthfully, I do not find it to have thrown much of a sidelight on jurisprudential ideas as such. But such was not his intent. If I read his book through different spectacles, I find much that is quite interesting, including, most especially, his above-mentioned set pieces and his own analyses of his collected data, placed at the end of each of the three middle chapters.

On the other hand, there is all that data itself, narrative and personal detail. Must we really be frogmarched through every last letter and memo of anyone who ever wrote on any aspect of the Hopkins Institute, and through every last methodological problem in questionnaire design? After a while, it gets to be about as stimulating as biblical genealogy. While Professor Schlegel disclaims any desire "to do a von Ranke" (p. 12), his presentation of such voluminous data, in his effort to show us *wie es eigentlich war*, is nonetheless quite Germanic in its power to overwhelm with detailed thoroughness. Quite frankly, a lot of it is not very interesting and easily could have been condensed without any loss of confidence in the subsequent discussion and analysis. The book also needed a good copy editing. But it would be churlish to end on the warts; so instead I will finish by stating that students of realism, and of legal academe more generally, are in Schlegel's debt for his rescue of realism's forgotten legacy.

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69. See *Law and the Modern Mind: A Symposium*, 31 COLUM. L. REV. 82 (1931).