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HAIL TO THE CHIEF: EARL WARREN AND THE SUPREME COURT

Dennis J. Hutchinson*


If a man shall be judged by his foes as well as by his friends, then Earl Warren led a charmed life as Chief Justice of the United States. For the sixteen years that he occupied the office, Warren benefited from extravagant praise by public figures and friendly scholars, and from condemnation by racial bigots, Birchers, and religious zealots. When Warren retired from the Supreme Court in 1969 and again when he died five years later, it was said that an era had passed with him: a moral epoch, which somehow he personified, had ended.¹ In the nine years since Warren's death, his enemies and critics have found new targets, but his place in history is now jeopardized by some of his best friends — or at least those sympathetic to the era and the man who symbolized it. G. Edward White and Bernard Schwartz, both of whom must be counted among Warren's friendly critics,² have produced book-length studies that focus on the Chief Justice and the Court over which he presided. Both books claim to be biographies in the traditional sense, but neither is: White has written an extended essay attacking what he sees as the conventional historical stereotype of Warren,³ and Schwartz has produced a term-by-term narrative, with occasional asides, about the Court's deliberations and major constitutional decisions between 1953 and 1969. White's book, for all its scrupulous attention to the facts and its elegant presentation, reads more like a brief than a biography, and a curiously dated brief at that. The issues for White are the issues shaped by Warren's severest academic critics in the 1960s. In order to rescue Warren post mortem, White resorts to rather slippery definitions of

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* Associate Professor in the College and Associate Professor of Law, The University of Chicago. A.B. 1969, Bowdoin College; LL.M. 1974, The University of Texas at Austin; M.A. 1977, Oxford University. — Ed.

¹. For the most recent expression of this sentiment, see Parrish, Earl Warren and the American Judicial Tradition, 1982 A.B. FOUND. RESEARCH J. 1179.


³. Cf. p. 5.
terms such as conservative, progressive, and jurisprudence. Along the way, the reader loses sight of Warren, who is caught in a web of taxonomy.

Schwartz's book is less pretentious but more revealing. The Warren who emerges from Schwartz's year-by-year chronicle did not dominate the Court, spiritually or intellectually; he merely presided over it. The selection of cases and the assignment of opinions were skillful, but the ideas of the Court that set fires in the minds of men and women during the period came not from Warren but first from Hugo Black and then quickly, and for the balance of Warren's tenure, from William J. Brennan. Schwartz's argument belies his subtitle: it was "the Brennan Court."4

I

Professor White lists three "assumptions" about Warren's public life which he seeks to challenge in his book:

The first is that Warren was a conservative California Politician; I shall suggest that neither the term "conservative" nor the term "politician" accurately describes Warren's career as a California public official. The second is that Warren underwent a marked change in his attitudes once on the Supreme Court. I shall argue that his public life can be seen as of a piece and that the surface contradictions in his thought can be seen as manifestations of a deep commitment to a general set of principles that were consistent in themselves. The third is that Warren was not a legal technician and that his jurisprudential views were largely derivative. I shall contend that Warren was merely a different kind of legal technician, unorthodox rather than inept, and that his theory of judging, while uniquely his, was not without its own theoretical integrity. [P. 4.] The agenda is puzzling. The first two assumptions have little to do with Warren's legacy, reputation or importance as Chief Justice of the United States. The elected public official and the Chief Justice presumably have different roles and responsibilities, so the relevance of the relationship between the two is not immediately clear. In any event, White devotes his most sustained and felt attention to the third assumption, which he seems to think stands in the way of Warren's ultimate certification to judicial greatness.

White states that Warren relied for his view of the Constitution not on history, text, or precedent, but on an acute ethical sensibility to the human consequences of the case at hand:

Warren's craftsmanship as a jurist was thus of a different order from that identified with enlightened judging by proponents of judicial restraint. Warren saw his craft as discovering ethical imperatives in a maze of confusion, pursuing those imperatives vigorously and self-confidently, urging others to do likewise, and making technical concessions, if necessary, to secure support. In believing his concessions on matters of doctrine to be "technical," Warren was defining his own role as a craftsman. It was a role in which one's sense of where justice lay and one's confidence in the certainty of finding it were elevated to positions of prominence in constitutional adjudication, and where craftsmanship consisted of knowing what

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results best harmonized with the ethical imperative of the Constitution and how best to encourage other justices to reach those results. [Pp. 229-30.]

The craftsmanship of "ethicism," as construed by White, thus has two components: (1) an intuitive certitude of what the Constitution should stand for in any given case, and (2) a rhetorical capacity to persuade others, at least a majority of the Court, to see things the way he did. White realizes that this version of craftsmanship verges, to say the least, on the solipsistic. He quotes with approval Anthony Lewis' well-known description of Warren as "the closest thing the United States has had to a Platonic Guardian," and asks: "Is Lewis right in suggesting that the posture of an ethicist is fatally dependent on the ethicist's own character?" (p. 359).

Even if one is ethically cock-sure of the bottom line, the future significance of a judicial decision depends largely on how the bottom line is rationalized. White seems to concede that the second component of Warren's craft was often inadequate to the task, and identifies Brennan as "Warren's judicial technician. He was capable, in cases such as Baker v. Carr, or New York Times v. Sullivan, of supplying doctrinal rationales for decisions in which Warren strongly believed" (p. 185).

Brennan's importance, which emerges more vividly in Schwartz's book, goes well beyond what White describes. Not only did Brennan provide the theoretical framework for Warren's ethical intuitions, but he did so in a way that held together majority opinions that might otherwise have splintered into several precedentially-insignificant voices. In a larger sense, Brennan provided a doctrinal coherence — admittedly not accepted by all — for what the Court was doing. To the extent that the Court over which Warren presided has any intellectual legacy that is accessible to those trained in doctrine and not in ethics, it is Brennan who is responsible.

What did Warren provide other than a handy vote and a symbolic figurehead for the legal and social revolution that the Court touched off? Perhaps Warren's stature was assured in the minds of many with the decision during his first term in Brown v. Board of Education. His achievement, widely praised at the time, was not only in authoring the opinion that found state-imposed segregation in public schools unconstitutional, but also — almost more important — in pulling together a unanimous Court for the result and the opinion. For White, Brown is the "crucible" for Earl Warren that shaped his role and his vision of his job (ch. 6), and he recounts the now familiar story of how unanimity was achieved.

Brown may have been Warren's crucible (White's evidence is very circumstantial), but it is too much to say that Warren deserves the lion's share of responsibility for the unanimity that the justices displayed on May 17,

1954. I have argued elsewhere in detail that, far from shaping a unanimous Court for *Brown*, Warren inherited a Court that had been largely prepared, since 1950, to rule unanimously that segregation in public schools was unconstitutional. In that year, the Court held that state-imposed segregation at the college level violated the Equal Protection Clause of the fourteenth amendment. The internal evidence of the Court’s deliberations in those cases, *Sweatt v. Painter* and *McLaurin v. Oklahoma State Regents,* makes abundantly clear that the justices knew what was coming, knew that it would be impossible to rule the other way, and felt that unanimity was an extremely valuable tool for securing compliance with the decision. Warren’s achievement in *Brown* was to head off a possible dissent by Stanley Reed and a threatened concurring opinion by Robert H. Jackson. The achievement is significant, but it is not on the order of achieving the result singlehandedly.

Warren’s achievement in *Brown* was purchased at a high price. The sticking point in the Court’s deliberations prior to Warren’s arrival was not so much result as remedy. Chief Justice Vinson, and Justices Reed, Jackson and Clark had all expressed anxieties more over implementation than over the substantive decision. When Warren became Chief Justice after what too many have viewed as Vinson’s timely death, he immediately realized the problem and solved it in melodramatic fashion: he persuaded the justices to decide the substantive issue first and to delay decision on the decree until later. The tactic worked perfectly, but the divisions over the remedy were postponed rather than defused. When the case was reargued a year later, it became clear that the remedy was, if anything, an even more bewildering question than it had appeared to be under Vinson. Having been unanimous once, the justices felt that they had to be unanimous again or risk undermining the moral force of their first decision. Warren again wrote for a unanimous Court, but this time unanimity was limited to the lowest common denominator among the nine — which produced equivocation and temporization on every line. To a large extent, then, the unanimity Warren helped to consolidate in *Brown I* boomeranged in *Brown II.*

Warren’s tactics for marshalling the Court in the *School Segregation Cases* deserve only qualified praise even from those who view judicial performance, especially by a Chief Justice, as essentially a function of internal administration. Moreover, Warren’s behavior with respect to the opinions in the segregation cases demonstrate the nature and price of craftsmanship based on ethics. In *Bolling v. Sharpe,* the companion case to *Brown* from the District of Columbia, the first draft of Warren’s opinion for the Court held that federally-imposed segregation in the District’s schools violated the fifth amendment, because “[t]he would be unthinkable that the Federal Government should have a lesser duty to protect what, in our present circum-

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stances, is a fundamental liberty.' Warren changed the sentence, and thus the entire constitutional basis for the opinion, when Justices Black and Frankfurter objected to reliance on the substantive due process jurisprudence of the McReynolds era. According to White: "The precise doctrinal steps that the Court took to justify the eradication through constitutional analysis were far less important to Warren than the Court's reaching the result of eradication unequivocally and unanimously" (p. 228).

Whatever one thinks of the appropriateness of the constitutional analysis assumed by White's observation, neither Warren then, nor White now, seem to appreciate fully the costs of Warren's casual attitude toward the theoretical content of opinions issued by the Court. "Deeds without doctrines," as Robert G. McCloskey once called them, are self-defeating. In the short run, they leave the Court vulnerable to attack for being willful rather than rational; in the long run, they provide an empty legacy to inheritors of the faith. Professor Ronald Dworkin, by no means unsympathetic to the mission of the Warren Court, criticized Justice Douglas for the same failure.16

If Douglas's constitutional theories were wishful or transparently fanciful, Warren's were simply empty. "Evolving standards of decency" may be a convenient rationalization, but it hardly provides much guidance for future cases. And to say that a contrary result would be "unthinkable" only invites doubt — at least as a general principle.18 At times, Warren even gave away more doctrinally than he might have wished had he looked down the road: his contribution to the jurisprudence of the Equal Protection Clause in McGowan v. Maryland has been an annoying obstacle in the past decade to those who have tried to carry on the egalitarian revolution that began during his tenure.20 If one looks to Warren for an enduring legacy, as White seems to invite us to do, one finds more symbol than substance.

II

Unlike White, Schwartz does not get bogged down in theory or in elaborate arguments to rehabilitate Warren's reputation for the ages. Facts, not concepts, are Professor Schwartz's meat. He has combed most of the

14. The draft is published in its entirety in Hutchinson, supra note 7, at 93. White's discussion is at pp. 226-28.
21. It does not appear that Schwartz consulted the working papers of Justice Robert H.
available judicial papers for the period (those of Hugo Black, Harold Burton, Tom Clark, William O. Douglas, Felix Frankfurter, and John Marshall Harlan), interviewed former clerks to Warren, and talked on and off the record with all the living justices who sat with Warren except Thurgood Marshall. The materials for a rich and sustained portrait are all on the table: The result is less a biography of Warren than an annotated set of internal minutes for October Terms 1953 through 1968.

The Chief Justice who emerges from this welter of detail is, however, vivid and possessed of a great presence, unlike the bloodless abstraction pictured by Professor White. Thus, of the 1956 Term, Schwartz writes:

In most respects Earl Warren could have been a character out of Sinclair Lewis or Sherwood Anderson. Justice Potter Stewart's comments are worth quoting: “Warren's great strength was his simple belief in the things we now laugh at: motherhood, marriage, family, flag, and the like.” These, according to Stewart, were the “eternal, rather bromidic, platitudes in which he sincerely believed.” These were the foundation of Warren's jurisprudence, as they were of his way of life.

When we add to this Warren's bluff masculine bonhomie, his love of sports and the outdoors, and his lack of intellectual interests or pretensions, we end up with a typical representative of the middle America of his day. Except for one thing — Warren's leadership abilities. As Stewart sees it, Warren may not have been an intellectual, but “he had instinctive qualities of leadership.” [P. 204.]

Warren, as Schwartz shows, also had a temper that could flare when provoked (p. 336) and a consuming vanity. Schwartz shows Warren reacting furiously because he had not been informed that morning coats would be worn at a London reception (p. 284); ticking off the American Bar Association for “deliberately and trickily contriving to discredit the Supreme Court which I headed” (p. 285); and tongue-lashing a journalist who had committed the double sin of painting a favorable picture of Richard M. Nixon and an unflattering one of Warren: “You people are persecuting me because you know I can’t strike back” (p. 337).

If Warren hated anything more than “persecution” by those who saw the world differently than he did, it was Nixon. Professor Schwartz shows that Warren became infuriated at Nixon during the 1952 Republican Convention, when Nixon publically and privately promised to support Warren for President but “betrayed” him by supporting Eisenhower, thus earning Warren's enduring contempt — a contempt that Schwartz says was an “almost visceral repugnance” (p. 21). Forget the bland and self-effacing Warren of his Memoirs,22 the man who had no unkind words for anyone. Warren hated “Tricky Dick — that’s what we used to call him,” “a crook and a thief” (p. 21). According to Schwartz, Warren announced his retirement in 1968 in order to prevent the appointment to the Chief Justiceship from going to Nixon, who he feared would be elected President in the fall.

Jackson. Jackson served only part of one term with Warren, however, and his papers may shed little light on the Chief Justice.

When the maneuver failed, Warren regretted that he had not simply decided to stay in office.23 The book closes with Warren exulting at the news, delivered by Justice Brennan, that the Court had stonewalled Nixon in the tapes case.24 A few minutes later, Warren died.

Warren's place in history depends not on his personality or his choice of enemies, but on his performance as Chief Justice of the United States. Schwartz views leadership as Warren's most important contribution to the Court, and he constantly refers to Warren's leadership and his leadership qualities, although the content of the terms rests largely on inference. Warren frequently was a necessary fourth vote to bring a controversial case before the Court (as in Baker v. Carr,25 where he joined Black, Douglas, and Brennan) (p. 411). He kept his clerks watching for the "right" case to overrule Betts v. Brady26 (p. 458). He assigned opinions fairly and shrewdly, which kept the troops happy (pp. 460-61). And he assigned to himself opinions that he suspected would generate extensive and unpleasant criticism — such as Brown, Miranda v. Arizona,27 and Reynolds v. Sims28 to spare his associates (and to make his place in history?).

If Warren was a great leader, it was due in part to good luck: He enjoyed, from the beginning of October Term 1962 when Arthur Goldberg took his seat, the ideological companionship of four other justices who could be relied on for the most part, at least until the end, to see the Constitution and the Court's mission the way he did (Black, Douglas, Brennan, and first Goldberg, then Fortas). Or as Schwartz puts it: "Even the most inspiring general must, however, have the troops who are willing and able to follow his lead. Chief Justice Warren received his most capable lieutenant after Sherman Minton resigned in 1956" (p. 204).

Minton was replaced by William J. Brennan, who was more than Warren's "most capable lieutenant." He was Warren's intellectual chief-of-staff from the first term in which he sat. Brennan was the man whom Warren called on to produce an opinion where the tentative majority was united as to result but sharply divided over reasoning. Brennan also did the anonymous dirty work of crafting per curiam opinions that were in fact committee reports in highly controversial cases (Cooper v. Aaron29 and Alabama v.

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23. "If I had ever known what was going to happen to this country and this Court, I never would have resigned. They would have had to carry me out of here on a plank—." P. 771.
26. 316 U.S. 455 (1942). "Even before the 1961 Term began, the Chief's new law clerks were instructed by one of the prior term's clerks. 'Keep your eyes peeled for a right to counsel case. The Chief feels strongly that the Constitution requires a lawyer.'" P. 458. Betts was overruled March 18, 1963, in Gideon v. Wainwright, 372 U.S. 335 (1963).
29. 358 U.S. 1 (1958). For other accounts of Brennan's work in the Little Rock school case,
**United States** (pp. 463-64) are the most prominent examples), and it was Brennan who occasionally supplied the theory in a concurring opinion that filled in the gaps left by a bold but incohesive opinion for the Court (his opinions in the controversial religion cases, *Engel v. Vitale* and *Abington School District v. Schempp*, stand out as the de facto opinions for the Court). In addition to writing *Cooper v. Aaron*, for which he received no public credit, Brennan frequently served as Warren's editor before an important opinion was circulated (as in *Miranda v. Arizona* (pp. 590-91)) and occasionally he even helped the most independent author on the Court, Justice Douglas. The most startling fact about the genesis of opinions that Schwartz catalogues is that Brennan, not Douglas, designed the spectral theory of *Griswold*, which Douglas, in a first draft, had tried to dispose of on the basis of freedom of association under the first amendment (pp. 577-80). Warren thought that the case might be handled on the basis of *Yick Wo v. Hopkins* (p. 577). In case after case, Schwartz documents in numbing detail how Brennan would accommodate his own drafts and views in order to preserve an opinion of the Court that was tumbling toward a plurality or worse. Warren may have given the orders, but it was Brennan who put together the General's victories.

When the public record is added to Schwartz's numerous behind-the-scenes examples of managing the Court, Brennan emerges clearly as the single most important justice of the period. The list of his opinions for the Court on constitutional questions reads like a syllabus for any comprehensive study of what is usually referred to as the "Warren Court." Under
Warren E. Burger and on a different Court, Brennan's impact has been reduced dramatically. Nonetheless, he is still capable of stitching together majorities, on occasion, that are reminiscent of the old days.\textsuperscript{37}

III

Taken together, White and Schwartz have collaborated unwittingly to put Warren in his place. Although Warren was an important and courageous figure and although he inspired passionate devotion among his followers, as the warm tributes in the \textit{Harvard Law Review}\textsuperscript{38} movingly attest, he was a dull man and a dull judge. His significance has been magnified out of proportion by the enemies he made and by the short-hand by which the constitutional revolution over which he presided became known. Despite the habit of mind that continues to call it the “Warren Court” and the occasional vague encomia that some of his colleagues supplied to confirm the myth,\textsuperscript{39} if any single justice deserves to be identified with the constitutional revolution engineered by the Supreme Court in the last generation, it is William J. Brennan and not Earl Warren.

There are now five biographies of Earl Warren, counting the recent contributions of White and Schwartz.\textsuperscript{40} None does justice to its subject, although Professor Schwartz does Warren the great courtesy of treating him on a human scale and not as larger than life. A critical analysis of the Brennan period is needed now, but five biographies of Earl Warren are enough.


Of Standards for Extra-Judicial Behavior

Russell R. Wheeler*


I

Bruce Allen Murphy delved into reams of manuscripts and other sources to learn more of the policy objectives of Louis Brandeis and Felix Frankfurter and the extrajudicial means they used to achieve those objectives. This work led to three very good law review articles,¹ which received much less notice than they deserved. The same research effort then led to The Brandeis/Frankfurter Connection: The Secret Political Activities of Two Supreme Court Justices, which produced a spate of excited public commentary on what the book purported to reveal and not a little criticism of Murphy’s methods and results.²

The book describes both jurists’ backgrounds and the relationship between Justice Brandeis and Professor Frankfurter that helped Brandeis serve his commitments to Wilsonian Progressivism and to Zionism while on the Court. Murphy then recounts Justice Frankfurter’s contacts with persons here and abroad in pursuit of various foreign and domestic policy goals, and then his efforts to influence appointments to the federal bench. In an appendix, Murphy attempts a chronological review of fluctuations in

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standards of extrajudicial behavior prior to 1916, when Brandeis joined the Court (pp. 345-63).

Arthur Schlesinger, Jr., credits The Brandeis/ Frankfurter Connection with two contributions. First, he says it “makes us think hard about standards of judicial behavior.”3 In addition, Schlesinger asserts that the book “makes us think realistically about the Court itself.”4 Perhaps Schlesinger’s assessment is too generous. If the book really does make us think hard about judicial behavior, the hard thinking does not follow from any searching inquiry that Murphy makes. Murphy looks at those standards (pp. 6-7, 247-75, 341-44), but not in great depth. Rather the thinking the book prompts about the Court and judicial behavior stems mainly from the mass of factual material Murphy provides.

The book’s source of strength — its detailed description of events — also gives rise to major weaknesses: flimsy inferences and occasional factual errors. Professor Robert Cover of Yale Law School, for example, finds a pattern of inaccuracies and, more than that, he claims that “even those important assertions that restate evidence are the product of a selective method which ignores all but the most damning, conspiratorial interpretations.”5 Schlesinger sees the same problem, albeit with a different twist. To him, the book is “disfigured by a host of minor errors” and Murphy “gives Brandeis and Frankfurter too much credit for decisions that were favored by other people and compelled by events.”6

In one sense, such criticisms are not surprising, because Murphy’s presentation largely lacks any overarching theme save that both Brandeis and Frankfurter labored off the bench to promote causes important to them. Yet the book is worth reading, not because of any overall picture it presents, but because of its fascinating extrajudicial short stories, describing incident after incident played out between the Harvard Law School, the United States Supreme Court, the White House, the Congress, and assorted other places. Such a book stimulates a natural tendency to probe for inaccuracies and for questionable interpretations.

It is not unduly charitable to say that Murphy’s interpretations often are plausible. The problem is that he presents them as conclusive when his facts merely create an arguable case for them. A conspicuous instance of the line between the conclusive assertion and the arguable interpretation may be the aspect of the book that has achieved the most notoriety. Stated baldly, Harvard Law Professor Felix Frankfurter was on a retainer to United States Supreme Court Justice Louis Brandeis. Although Murphy cautions that “the Harvard professor cannot be viewed solely as Brandeis’ agent” (p. 43), Frankfurter was, for all intents, Brandeis’ “paid political lobbyist and lieutenant” (p. 10), “the scribe” (p. 153), “the right lieutenant” (p. 33) to do work that Brandeis, for reasons of propriety or appearance, could

3. Schlesinger, supra note 2, at 22.
4. Id. at 23.
5. Cover, supra note 2, at 19.
6. Schlesinger, supra note 2, at 5, 22.
not undertake himself. To help Frankfurter meet the expenses of fighting bureaucratic battles and educating the public, Brandeis, starting in 1916, provided him with monetary gifts. Later, from 1926 to 1938 — the year Frankfurter joined the Court — Brandeis gave Frankfurter an annual stipend of $3,500 (pp. 40-42).7

As Murphy concedes in an endnote, other researchers already had disclosed the existence of the payments (p. 373 n.80). Several of Frankfurter's students, in fact, knew of the disbursements, which often funded research projects aimed at furthering Brandeis/Frankfurter goals.8 Nor is it news, as Murphy recognizes, that Brandeis and Frankfurter worked closely on the national political scene. Tugwell, for instance, referred to the team in 1934 (pp. 176, 416 n.96), and in 1946 Mason described Frankfurter as "tutor to the new administration [who] . . . in turn, sought light and guidance on general policy as well as on specific programs from Justice Brandeis."9

Despite these previous revelations, Murphy has, no doubt, contributed an important piece to this historical mosaic. An endnote typical of much of the book tells how: "This is the first exposition in print of the development and complete extent of both the financial fund and the requests that stemmed from it" (p. 373 n.80).

But how singular was this instance of Brandeis' extrajudicial behavior? Murphy relies on Mason's table listing the extensive gifts that the justice provided between 1890 and 1939 to numerous individuals and charitable causes (pp. 41, 373 n.82). The list shows, for example, that Brandeis gave relatives and friends over $27,000 in 1925; $177,000 in 1929; and $71,000 in 1930 — all told over half a million dollars, about a third of the nearly $1.5 million in gifts accounted for by Mason for the entire 49 years.10

The considerable extent of these gifts leads one to wonder whether Frankfurter, albeit Brandeis' "chief political lieutenant" (p. 170), was the only "lieutenant." Murphy describes Mason's list of gifts as "complete" (p. 373 n.82) even though by Murphy's own account it apparently is not.11

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7. Although Frankfurter viewed "himself as an employee being compensated for services rendered," p. 41, we are not told whether he regarded the payments as taxable income or whether the Justice Department or Senate Judiciary Committee learned of the payments at the time of his nomination to the Court.

8. Murphy concludes, "judging by the lack of knowledge evident in a personal [telephone] interview with one of these students half a century later, . . . many of Frankfurter's protégés will learn here for the first time about the true chain of inspiration to which they were responding," P. 86 (emphasis added). The "student" was Henry Friendly. P. 386 n.49. Another student, though — in Frankfurter's Federal Jurisdiction seminar in 1929-30 — recalled that Frankfurter "told me of the work he was doing for Justice Brandeis and the moneys he received, and the other students in the seminar also knew of these payments." Lewis H. Weinstein, Letter to the Editor, HARV. L. REC., Apr. 16, 1982, at 11. The exchange proves nothing save the need for caution in making declarative statements.


10. Id. at 692.

11. It is doubtful that the total of almost $1.5 million includes the money that Brandeis gave to Frankfurter, because Murphy's documentation of these gifts is evidently derived from correspondence to which Mason did not have access. In a star note, p. 101, Murphy explains how Mason was deprived of access to Brandeis-Frankfurter correspondence "during the thirties," but earlier he characterizes things more broadly: Mason wrote "without benefit of access to the very revealing correspondence Brandeis maintained with . . . Frankfurter while Frankfurter was a professor at Harvard." P. 8. In any event, Mason obviously did not write about
Was Frankfurter the only colleague who worked in response to Brandeis’ requests and who, in turn, benefited from this considerable largesse, for himself or to support still other collaborators (pp. 84, 86)? Brandeis made “extensive use of intermediaries” (p. 73); Murphy constantly uses “lieutenant” to describe people who worked with both Brandeis and Frankfurter. He so describes, for but one example, Brandeis’ allies in the leadership of the Zionist Organization of America (pp., e.g., 31, 55-56, 65-66), an organization that received Brandeis’ financial support (pp. 68, 381 n.104). It would in no way discount the uniqueness (pp. 39, 400) of the “Brandeis/Frankfurter connection” to learn that others received some of Brandeis’ financial support with the understanding that they would pursue his objectives. It might, though, temper Murphy’s description of the relationship as “extraordinary” (p. 43) or as “so unusual . . . [in] that it was designed to free Brandeis from the shackles of remaining nonpolitical while on the bench . . . ” (p. 41). Answering the question might require manuscript searching even more prodigious than that Murphy undertook. As he implies in describing how he came across one source, even more material may await discovery (p. 218 n.*).

Murphy’s inability to tell the story free of the unqualified inference coincides with his inability to tell the story free of the historiographical boast. Boosterism pervades the book, much more than the articles. Thus, references are rarely to correspondence but rather to such self-promotions as a “newly discovered missive,” “which remained hidden in Moley’s unpublished papers” (p. 172) or “[n]ew evidence gleaned from various collections of unpublished letters [that] makes it possible for the first time in print to reconstruct the justice’s efforts here” (p. 330). Throughout, Murphy reminds us that his information came from “a personal interview” (e.g., p. 297) or “a confidential interview” (e.g., p. 312) or “an interview for this volume” (p. 132). In reporting Brandeis’ influence on Frankfurter’s unsigned New Republic articles, Murphy specifies that “until now no volume has revealed the extent to which the true inspiration for many of these pieces was . . . Brandeis” (p. 89). Perhaps Murphy feared that if he did not broadcast the diligence of his efforts, scholars would not recognize what he had discovered, and all readers would not be impressed with his hard work.

The publisher may bear some fault for this belabored tone of secrets discovered. Oxford should have redirected at least some of its resources into the extra costs necessary to carry all notes at page bottom rather than book’s end — and in editing them. Reliable documentation is everything to a book such as this, and the reader must have confidence that the notes have been carefully reviewed. Yet, one note cites twenty pages of an Alan Westin article to support the text’s assertion that Brandeis, “contrary to the prevailing understanding . . . engaged an extensive literary network,

the payments to Frankfurter, although it is possible that the sources he used to construct the list of Brandeis’s gifts included those to Frankfurter, but masked or aggregated with others.

12. See note 1 supra.

13. See, e.g., the suggestions in Kurland, supra note 2, at 10; Cover, supra note 2, at 21.
anchored by Frankfurter, to disseminate his opinions . . .” (pp. 88-89, 387 n.60). Presumably Murphy meant to cite Westin’s assertion that after 1916, “Brandeis said nothing in public about Court matters,” and then summarize his own reinterpretation. If that is what he meant to do, why does the book not do it that way? The language of another note is repeated almost word for word in the text where it is flagged (p. 105, 392 n.21). Another note citing literature “on the disparity between Frankfurter’s religion and his desire for social status” (p. 425, n.56) is flagged at a passage in the text that bears on that point in, at best, a highly tenuous fashion (p. 207). Since Murphy treats the point at issue much earlier in the text (p. 34), the note’s placement may have been a remnant of a previous draft. The index is also in occasional error: University of Virginia Law Professor G. Edward White, for example, is not the Louisiana-born Chief Justice (pp. 20, 473).

C

All this said — even if Murphy’s description of the events is flawed factually or interpretively — he has added to our knowledge in two ways. First, he provides a documentation of numerous activities, and he fills in so many details as to provide a new picture of the lives of these two extraordinary individuals. Cover asserts that Schlesinger documented long ago that Brandeis was “perhaps the dominant influence in the ‘Second New Deal.’” But Schlesinger himself credits Murphy with performing “a first-class job of research,” and of “reconstruct[ing] episodes in the inner history of the Supreme Court, of the New Deal and of World War II (Washington sector), in new detail . . .” Second, and this says something about the nature of scholarly inquiry, Murphy has provided a framework within which others can work. Understanding develops by interpretation and reinterpretation. An initial interpretation, even with flaws, is often the necessary impetus for further analysis that adds even more to our knowledge.

II

What Murphy does not provide, however, is any thorough analysis of the standards that should govern extrajudicial behavior. His work is full of shrug-of-the-shoulder references to the separation-of-powers doctrine, with the erroneous implication that the doctrine must apply to all the varieties of extrajudicial behavior revealed in the book (see pp. 5, 22, and especially p. 15). In an appendix, furthermore, he attempts an analysis of how norms of extrajudicial behavior have evolved. Nevertheless, on the strength of this book alone, one’s thinking about what is proper and improper for justices to do extrajudicially is hardly advanced. It may well be that such advancement was not Murphy’s primary goal (pp. 13-15).

I would like to pose several questions about extrajudicial behavior, mostly by Supreme Court justices, and use some of the book’s rich data to explore how they might be answered.


15. Cover, supra note 2, at 18.

16. Schlesinger, supra note 2, at 5, 22.
One might first ask why justices should engage in extrajudicial activities. This phrasing is at odds with the common formulation: Why should they avoid them? To many, the latter question is answered sufficiently by a few descriptions of what judges have done off the bench — each followed by an exclamation point. A little reflection, however, will suggest benefits from several kinds of extrajudicial behavior, benefits that I summarize here and then discuss in more detail. First, the role of judges in political society may give them unique attributes to bring to other aspects of public policy. At a different level, they bring the special knowledge and perspective of those who have “been there” to debates over how our judicial institutions should be administered and who should be judges. In addition, judges have likely developed perspectives and some degree of political acumen before their appointments that could be put to extrajudicial service. And, by a similar token, an occasional extrajudicial role might maintain the breadth of a judge’s perspectives and inform the judicial mind.

To many, these statements do nothing but illuminate the threats that extrajudicial activity poses to the judicial function. That activity may, for example, deprive judges of the time and energy they need to decide cases fairly and explain their decisions clearly. Extrajudicial contact with a matter may inhibit the impartial consideration of that matter in the context of litigation. Similarly, the desire to stay in the graces of, for example, a President who might bestow the favor of an extrajudicial activity might prevent their considering other matters impartially. Finally, regardless of whether an extrajudicial activity affects justices’ behavior, it may create doubt — an ambiguity — in the minds of those who must have confidence that judges will be fair, those without whose confidence the judicial fiat stands in danger of disrespect.

How do the results of Murphy’s prodigious research help illuminate these purported benefits and costs?

III

Adjudication, especially constitutional adjudication, requires judges to participate in political society in a special way, applying fundamental norms to resolve controversial fact situations. This experience, building on judges’ pre-judicial experiences, arguably creates a unique political perspective and even political skills that might well be of value to the resolution of matters outside case-or-controversy fora. This view was held much more widely in the founding period than it is now. Many then agreed with George Mason, who told the constitutional convention that the judges’ “habit and practice of considering laws in their true principles, and in all their consequences,” laid a strong case that “further use be made of the Judges, of giving aid in preventing every improper law.”17 In fact, John Jay’s major contribution as Chief Justice was to show the dangers of too heavy a reliance on “further use” of judges as commission members and presidential advisers.18

17. 2 M. FARRAND, THE RECORDS OF THE FEDERAL CONVENTION OF 1787, at 78 (rev. ed. 1937). The convention of course rejected the specific objective that Mason was advocating, viz., a Council of Revision, with judicial membership.

The basic notion of judges' obligation to render extrajudicial service has persisted, however, and it may help explain — Murphy makes clear it would hardly explain fully (pp. 304-08ff.) — why Frankfurter defended Jackson's service as special prosecutor at Nuremberg. Frankfurter, despite his public stance that Justices should not "take[e] on other jobs," assured Jackson, not only of "the profound importance" of his mission, but that he "would discharge the task according to the finest professional standards both intellectually and ethically" (p. 306). One of Jackson's colleagues at Nuremberg states the position in a more blunt, if self-serving, fashion: Fourth Circuit Court of Appeals Judge John Parker proclaimed that Jackson's mission was justified because there are occasionally calls "for a judge to do something for his country which no one but a judge can do so well." Murphy, in fact, notes that Frankfurter succeeded in his extrajudicial tasks in part because, as a Justice, he was a "free agent... While nearly everyone in Washington could be suspect of jockeying for a position and status, special attention would be paid to that 'impartial observer, Felix,' who had already reached the pinnacle of his career ambitions" (p. 189).

Obviously the degree to which judges can contribute extrajudicially as judges will vary with the task at hand and with the judge performing it. A desire to grace an important mission with an ornament of impartiality is not enough to justify involving judges in the task. For example, having justices serve on the commission to resolve the disputed presidential election of 1876 appears, in retrospect, to have been a poor idea. Given the venality of the age, and the Court's still-incomplete recuperation from the Dred Scott wound, it was unlikely that the justices' service could have helped resolve challenged election results at the end of the Reconstruction Era. The problem is captured in a Southern newspaper's editorial hope that "if Justice Bradley could withstand the party pressure that reached him [to sustain Reconstruction legislation on the bench], there does not appear to be any reasonable grounds for supposing that he will succumb to such pressure" on the commission. I have serious doubts, for a contemporary example, that the Supreme Court Justices should be directed to set congressional salaries, despite the assertions by two members of the Senate leadership in 1982 that a constitutional amendment to that end would be "the wisest and most apolitical delegation of such compensation setting authority. . . ." Few, however, would contest the basic assumption behind Canon 4 of the American Bar Association's Code of Judicial Conduct. The canon permits judges to write and lecture on the administration of justice, to appear before or consult with governmental bodies or officials on matters concerning the administration of justice, and to serve as members or directors of judicial improvement organizations. In these matters, asserts the commentary, a judge "is in a unique position to contribute," and it encourages judges to do so as their time permits. Procedural rule-making benefits

from their involvement. Their advice on jurisdictional matters, for which Alexander Bickel claimed they are "uniquely expert," is similarly beneficial. And much of the work that Brandeis and Frankfurter, along with colleagues and students, did during the 1920s involved the development of arguments for changes in federal jurisdiction, including the research that was eventually published as *The Business of the Supreme Court* (pp. 84ff.). Even though judges are hardly infallible in shaping judicial administration policies, and although they certainly do not reflect all the perspectives that need to be brought to bear on the process, surely they should be heard.

Turning to a slightly different category, Murphy devotes most of a chapter to Frankfurter's efforts to promote the judicial candidacies of certain individuals he thought particularly well qualified for the federal bench and to derail those of others (pp. 313-38; Brandeis also attempted to influence appointments, pp. 48-49). Frankfurter had developed a particular view of criteria that should — and that should not — govern judicial selection (pp. 316-17); it would be surprising to find a judge who has not. Judges know, in a way that others cannot, what the judicial office entails, what qualities it needs most, and what kinds of individuals would be appropriate for it. "Merit selection" commissions for state judicial nominations often include judges as members. In Missouri, where the system has been most rigorously probed, Watson and Downing report that of the commissioners, "the judges . . . have evidenced the greatest variety of perspectives on judicial selection." They bring the lawyer's knowledge to the task, but without attendant bar rivalries, and they surely have a special insight into what the job of judging entails. As with judicial administration innovations, sitting judges' perspectives on judicial selection are limited and hardly apolitical, and there are risks, described below, to their involvement. But there are benefits as well.

Judicial-related attributes aside, individuals who manage to get appointed to the bench, especially the highest bench in the land, presumably bring to their chambers more than legal experience and perspective. Almost by definition, they have been actively involved in the affairs of the day. Forbidding all extrajudicial service would, by definition, deprive the nation of the benefits of those personal attributes.

Forbidding extrajudicial activity is, in a sense, at odds with the democratic notion that political society benefits from the participation of its

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26. R. Watson & R. Downing, *The Politics of the Bench and the Bar* 337-38 (1969). The United States Judicial Conference's Advisory Committee on Judicial Activities has accepted "the premise that, as [federal] judicial selection processes become more institutionalized and with wider participation, judges have a responsibility [when asked specifically or by a general call for information] to communicate their recommendations and evaluations to the appointive authorities — the President and Senators — and their selection committees or commissions." Advisory Opinion No. 59, Apr. 16, 1979.
members. Justice Douglas once expressed something of this view. In 1939, the Supreme Court decided *O'Malley v. Woodrough*, 27 upholding the constitutionality of legislation subjecting federal judges to the income tax. “As I entered my vote in the docket book,” Douglas claimed, “I decided that I had just voted myself first-class citizenship. . . . Since I would be paying as heavy an income tax as my neighbor, I decided to participate in local, state, and national affairs, except and unless a particular issue was likely to get into the Court, and unless the activity was plainly political or partisan.”

Douglas's assertion of cause and effect is somewhat disingenuous: even without *O'Malley*, one suspects, he would have decided to “register and vote; . . . fight to raise the level of the [Yakima] public schools [and] become immersed in conservation, opposing river pollution, advocating wildlife protection, and the like . . . [and] travel and speak out on foreign affairs.”

Murphy makes a relatively compelling case that Brandeis' forceful efforts helped to move the New Deal away from the corporate-state mentality that it exhibited in its early years (pp. 185, 343 & passim). He documents that Frankfurter, while on the Court, played an important role in the establishment of various foreign policy efforts of the Roosevelt Administration that broke the isolationist hold dominant in the late 1930s and the early 1940s (pp. 227, 282, 302 & passim).

To say that we have no assurance that justices' activities off the bench will produce “contributions” is to miss the point entirely. We would not think of requiring such assurances before sanctioning the political activities of any nonjudge. Brandeis' role in turning the direction of the New Deal, or Frankfurter's in affecting American foreign policy, would not have unanimously been labelled “contributions” at the time, nor would they today. The test of the propriety of their action is not the degree of approval on the merits, but the costs, if any, to the Court — and to the system of justice generally — of Supreme Court justices' acting extrajudicially.

Finally, it may be that extrajudicial activity can also work to the advantage of the judicial process itself. Justice Douglas offered a stronger reason for exercising his “first-class citizenship” than his status as a taxpayer, a reason captured in his rather cavalier assertion that a “man or woman who becomes a Justice should try to stay alive; a lifetime diet of the law alone turns most judges into dull, dry husks.”

Justice Rehnquist treated a tangential aspect of this question in explaining his refusal to disqualify himself from the Court's reconsideration of *Laird v. Tatum* 31 because of his involvement as an executive department official in matters before the court. Apart from his specific involvement with the matter was the contention, as he summarized it, “that I should disqualify myself because I have previously expressed in public an understanding of the law and the question of the constitutionality of government-

29. *Id.*
30. *Id.* at 469.
tal surveillance.” Rehnquist’s response serves as a reminder that justices of the Supreme Court are drawn from the legal political community in part because they are among its more prominent members. He noted numerous justices who, before they went on the bench, played roles in matters that presented themselves to the Court in the case-or-controversy context, and reasoned that it

would be not merely unusual, but extraordinary, if they had not at least given opinions as to constitutional issues in their previous legal careers. Proof that a Justice’s mind at the time he joined the Court was a complete *tabula rasa* in the area of constitutional adjudication would be evidence of lack of qualification, not lack of bias.32

The question remains whether certain kinds of extrajudicial activities might similarly enhance a justice’s work on the Court. Judging in a democracy is a vital process, and the nation has some interest in knowing that its judges are not permanently cut off from the juices that flow through society. Moreover, it may be that justices see the opportunity for such involvement as an advantage. The reaction of one of Brandeis’ law clerks, J. Willard Hurst, to Murphy’s book is instructive: “The Supreme Court deals with matters of important public policy,” and thus, he said, “[y]ou want people sophisticated in the affairs of the country, not the naive or simple-minded. . . .”33 To seek extrajudicial outlets may be a natural inclination of the kind of people appointed to the Court. *The Brandeis/Frankfurter Connection* certainly leaves the suspicion that both justices may have seriously reconsidered joining the Court if all extrajudicial involvement could, somehow, have been proscribed: They would have been different persons, at least, frustrated by the proscription. Would the nation have benefited from either of those possibilities? This is, to me, the kind of realistic thinking that Schlesinger says the book promotes34 and that serves us well even if it puzzles us. The puzzlement is captured in Murphy’s simple conclusion that both men “found it impossible to curb their political zeal after their appointments to the bench” (p. 9). Is it realism or irresponsibility to accept that inability in some justices? “Perhaps it is time,” Murphy suggests, “that we question more realistically what we can and cannot expect from those who sit on our highest Court” (p. 8).

IV

In *O’Malley*, the case that Justice Douglas claimed liberated him for a life beyond the purple curtain, Justice Frankfurter wrote that judges’ “particular function in government does not generate an immunity from sharing with their fellow citizens the material burden of the government whose Constitution and laws they are charged with administering.”35 Judges do have a “particular function in government,” which takes precedence over any other function. The benefits that extrajudicial activities may bring to

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32. 409 U.S. at 835.
34. See note 3 *supra* and accompanying text.
American political life must be weighed against the burdens those activities may impose on that "particular function."

Weighing those burdens, to be sure, requires a profound judgment. It also requires, much more than commentators have been willing to acknowledge, answers to basically empirical questions, i.e., questions of fact that can, in principle at least, be proved wrong. We are short on facts and long on suspicions about the consequences of extrajudicial activities.

The facts needed to inform our judgment are of various types. Some can come only from the judges and those who work directly with them. For example, what is the impact of extrajudicial activity on judges' time demands and work habits? Although there have been some efforts to measure how judges spend their time, there has been no focus on extrajudicial activities' impact on their judicial work and such a focus would surely be seriously blurred. Our sense of the costs that discrete extrajudicial activities may extract is likely to derive largely from specific examples. Chief Justice Warren, for instance, insisted that he would not give up his judicial duties during the investigation of the Kennedy assassination. After he left office, he told a television interviewer that he "would run back and forth between [the Court and the commission offices across the street]. I don't believe I left my work before midnight any night for ten months." What the impact of the extra burden was on his Supreme Court activities one can only surmise.

Murphy provides a more revealing example. Although Brandeis' extrajudicial work evidently had no effect on his Court workload (pp. 53, 54), Frankfurter's did. During Frankfurter's pre- and early-World War II involvement in all manner of foreign policy matters, his rate of opinion production did not decline. Murphy, however, concludes from interviews with Frankfurter clerks that he delegated a larger share of his judicial work to his law clerks during the period from 1941 to 1943 than he did before or after it. Save for those years, Frankfurter himself prepared the initial drafts of his judicial opinions. From 1941 to 1943, however, his law clerk did so in every case but one (pp. 273-75). Although any difference in the final product has evidently eluded observers of the Court, the shift in work patterns was arguably an abdication of judicial responsibility to pursue extra-
judicial goals. But what of the benefits — if that is what they were — that the arrangement allowed, especially since, if Murphy is to be believed, Frankfurter may have influenced some important events in ways in which others could not?

A judge's judicial administration work — in which the judicial perspective is essential but not sufficient — presents this matter of costs and benefits in sharper contrast. We accept as elementary the normative proposition that each judge should dispose of the cases before him or her as fairly, quickly, and economically as possible. Such case disposition may not be achievable simply if each judge tries hard to do so. The administrative and organizational arts — securing resources, devising procedures, promoting cooperation, and assessing what works — are necessary to the objective, surely, in any large court system, and judges must perform them. The administration of justice is a systemic need that may deserve a judge's time at the expense of prompt attention to an individual case or set of cases.

Perhaps the most frequently asserted cost of judges' extrajudicial activity is bias — the inability to do justice because an extrajudicial contact creates a partiality to one side that affects the judge's decision. What of it when judges are asked to decide questions on the bench that bear a relatively distinct relationship to matters that they touched off the bench, perhaps in a lecture, perhaps in an informal consultation with a government official? Brandeis, Murphy shows, participated in cases that presented questions he had tried to influence off the bench, but he voted in a manner that one would not predict if extrajudicial lobbying foretold judicial behavior. In 1921, "[b]y voting with the Court against the [Lever Food Control] act, after having privately told [Food Administrator] Herbert Hoover how to get it enacted, Brandeis seemingly demonstrated . . . the separation that existed between his judicial and political roles" (p. 55). Another example is United States v. Butler, which declared the Agricultural Adjustment Act unconstitutional. As Murphy says, "[d]espite all his [extrajudicial] admonitions and warnings that he would help dismantle the AAA from the high bench, Brandeis, in dissent, voted to uphold the constitutionality of the act" (p.142).

The late Alexander Bickel took up a related aspect of this question during Senate Judiciary Committee hearings in the wake of the Fortas affair:

[A] judge is supposed to have an open mind, or at least a mind reachable by reasoned briefs and arguments. If he goes on public record concerning issues that are likely to come before him in his judicial capacity, he thereby at least appears to close his mind, to make himself less reachable by reasoned briefs and arguments. And in some measure every man who goes on record in this fashion does in fact close his mind. Here we have some clear questions about how human beings behave. Was Bickel right, for example, in the basic message of his hyperbolic assertion that "[n]othing is more persuasive to ourselves than our own published prose"?

Answers to that question have been consistently intuitive, perhaps re-

38. 297 U.S. 1 (1936).
40. Id.
flecting larger policy objectives. English judges in the eighteenth century justified their practice of giving advisory opinions with the claim that they could change their minds "without difficulty" if arguments at bar showed an earlier advisory opinion to be in error. Vermont Congressman Israel Smith told his colleagues in 1802 that "nothing gives [a judge] greater pleasure than to have it in his power to correct an error, which he may discover in a former opinion." Smith, though, was arguing for abolition of the separate circuit courts created by the Federalist Judiciary Act of 1801, one effect of which would be to restore the justices' dual service as circuit judges. The justices themselves, however, had never wanted the onerous burden of traveling about the circuits. Ten years earlier, in making their case, they told Congress that

appointing the same men finally to correct in one capacity the errors which they themselves may have committed in another, is a distinction unfriendly to impartial justice, and to that confidence in the Supreme Court which it is so essential to the public interest should be reposed in it.

Justice Blair put the question when the Court reviewed one of the circuit's decisions. He recused himself but announced that he held "the impressions which my mind first received," adding parenthetically, however, that he did not know if those impressions persisted "whether through the force of truth, or from the difficulty of changing opinions, once deliberately formed."

It takes nothing from the eloquence of the phrasing — nor the sincerity of the writers — to observe that the debate has not come very far in almost 200 years. Is our knowledge — not suspicion, but knowledge — about the factors that may create extrajudicial bias much more today than it was in the eighteenth century?

The ways in which extrajudicial activity might warp a judge or justice are varied. Impartial decisionmaking might be frustrated by prior contact with an issue off the bench, or perhaps by a justice's desire to please those in a position to award opportunities for extrajudicial service. In fact, the major objection to the first serious instance of a justice's extrajudicial service — Jay's serving as ambassador to Great Britain — was not that he would be unable to decide cases fairly because of any diplomatic contacts with litigated issues. Rather it was that justices would decide cases as the President wished in order to earn prestigious extrajudicial appointments. The same thought shows itself in Frankfurter's opposition to judges who run for office from the bench, namely Douglas. Douglas's votes on cases, Frankfurter

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41. Sackville's Case, 2 Edens Ch. 371-72 (1760).
42. 7 Annals of Congress 706 (1802). Federalist James Bayard saw it differently: To assume a justice would not "be gratified" by an affirmation of an earlier decision "is estimating the strength and purity of human nature upon a possible, but not on its ordinary scale." 7 Annals of Congress 618.
44. Letter of the Justices to the Congress, Nov. 7, 1792, reprinted in 1 American State Papers 52.
46. A Jeffersonian paper complained that it was necessary that Jay be in the country were he needed to preside over any impeachment proceedings, but also "that he should be above the bias which the honor and emoluments in the gift of the executive might create, . . ." Aurora General Advertiser (Philadelphia), May 10, 1794.
feared, were determined by “whether they might help or hurt his chances for the presidency.” He was “writing for a different constituency” (p. 267).

Others might respond that these are meaningless questions, because regardless of whether justices actually become tainted, the citizenry will perceive the judges as biased, and the Court will lose the public support essential to acceptance of its decisions. Murphy stresses the importance of public opinion, but he writes as if the public has the same level of knowledge of the justices’ work (and of sources such as his book, p. 151) as do those who follow the Court closely. He asserts, for an example, that in the early twentieth century, “a forgiving public [had] recently acquiesced for the first time in over forty years to a close advisory relationship between a Supreme Court justice (William Moody) and a president (Theodore Roosevelt)” (p. 17). The evidence suggests, though, that the public knows little of what the justices do on the bench, and it is likely that they know less of extrajudicial activities, even when publicly reported. There certainly appears to be little basis for Murphy’s apparent speculation that, although President Nixon’s forced resignation had little long-term effect on the prestige of the presidential office, efforts to bar Fortas, Douglas, or Haynsworth, from the Court “may permanently lessen public confidence in the Court itself, and hence compromise the ability of the entire judicial branch to have its decisions accepted as law” (p. 14).

Even if John Q. Citizen is unaware of what the justices do — on or off the bench — the Court does have a constituency of those who follow public events, and, more particularly, various segments of the legal community.

47. The visibility of the Supreme Court is not easy to measure, but probably it is lower than might be inferred from popular opinion polls that appear in the press — based on forced-choice responses to questions about which people may in fact have no information. Walter Murphy and Joseph Tanenhaus set about the task of measuring the Court’s visibility in the 1960s, and found that, in 1964 and in 1966, less than half their respondents even attempted to answer an open-ended question seeking to learn what “the Supreme Court in Washington has done that you have disliked . . . liked . . .” Murphy & Tanenhaus, Public Opinion and the United States Supreme Court, in FRONTIERS OF JUDICIAL RESEARCH 273, 276-77 (J. Grossman & J. Tanenhaus eds. 1969). To a question about the Supreme Court’s constitutional role, less than 40 percent could give answers that could be coded according to one of ten broad functions — e.g., “interpret the Constitution,” or “settle basic questions.” Furthermore, this survey was conducted in a period of heightened and presumably visible Supreme Court activity. On the other hand, as Murphy and Tanenhaus note, open-ended questions may underestimate visibility because people have difficulty remembering what they do know. Moreover, visibility increased with education. Murphy & Tanenhaus, supra, at 276-86.

Nevertheless, given these measures of visibility of the Court’s basic functions, one can wonder how visible to the public are a justice’s speech, lecture, or visit with the president.

48. In listing the problems encountered by Fortas, Haynsworth, and Douglas, Murphy also cites “the frequent criticism of” Chief Justice Burger, but I am unaware of any serious claim that he should not be on the Court and thus do not include him among those about whom such claims were made.

As to Murphy’s worries: In 1975 Murphy and Tanenhaus resurveyed those in the original study, see note 47 supra, who had displayed some knowledge of the Court. Although their object was obviously not to test Bruce Murphy’s statement about the effects of questionable extrajudicial activity on support for the Court, their conclusion is revealing: “In the aggregate, diffuse support [i.e., general trust or confidence] for the Supreme Court, despite tremors that shook the entire political system, proved comfortingly resilient.” Tanenhaus & Murphy, PATTERNS OF PUBLIC SUPPORT FOR THE SUPREME COURT: A PANEL STUDY, 43 J. Pol. 24, 29 (1981).
That constituency's attitude toward the Court probably influences the Court's effectiveness, by setting a climate of trust, or distrust, regarding the Court's ability to reach its decisions free from the pressure of improper influence. A controversial matter off the bench — regardless of whether it affects judicial performance — creates an ambiguity, a doubt, that a justice can have a partisan position on one issue (in, for example, a speech off the bench) but maintain a dispassionate, neutral position on the bench on another issue. This doubt is possible even if the two sets of issues are completely distinct for the judge, and probable if they are not.

Although Brandeis voted to sustain the Agricultural Adjustment Act after lobbying against it (p. 142), he may have committed a serious error just the same, simply by threatening a judicial rebuke to the Act. He failed “to observe the most basic stricture for the judiciary, that against using the power of judicial office to further political goals” (p. 141). And, as Murphy wisely observes, Brandeis' action may have led the officials with whom he consulted to believe that they had persuaded a justice how to vote in a case (p. 142). What would be the effect, for another example, on trust in the Court if it were known that one of its members was lobbying actively for the appointment of certain individuals to the bench? There is presumably a limit to how much of this kind of ambiguity the Court's constituency will tolerate before it begins to discount the authority of the judicial fiat.

The implications of this speculation, however, tend to becloud what the speculation is about, viz., empirical questions. How, in fact, does extrajudicial activity affect judges' work on the bench — their ability to decide cases without prejudice — or public confidence in the Court? I do not pretend that we have the methodological tools to answer those questions, but I think we would elevate the debate if we recognized the kinds of questions they are. Murphy's contribution to the debate, however, is significant. He has provided much grist for the mill.
EXPLORING THE ROOTS OF OUR CRIMINAL JUSTICE SYSTEMS

Samuel Walker*


Several years ago Lawrence Friedman admonished his fellow legal historians to leave their traditional realm of appellate court opinions and study the much larger, messier and more mundane world of the day-to-day administration of justice. The reality of the law for most Americans does not consist solely of the fine points of law found in appellate court opinions. In the case of the criminal law, for example, the reality of the law is found in the actions of police officers on the street, of prosecutors and defense attorneys in pretrial proceedings, and only occasionally in actual criminal trials. These stages of the criminal process are what Yale Kamisar once called the “gatehouses” of American criminal procedure. Although largely hidden from public view, they are far more important in terms of the actual impact on peoples' lives than the more visible “mansions” represented by the opinions of appellate court judges.

Professor Friedman has taken his own advice to heart and, in collaboration with Robert V. Percival, one of his students, has produced one of those rare books that can be truly characterized as a landmark. The Roots of Justice reverberates throughout the field of legal history and the realm of contemporary criminal justice studies. The authors give us a better overall view (what the social scientists would call a “model”) of how the criminal justice system works than our criminologists and political scientists have been able to fashion. It is ironic that although The Roots of Justice is a work of history, it may well have more significance for contemporary social science than for the field of history.

This is not to say that The Roots of Justice is flawless. On several points it is less than satisfying, especially where the authors fail to answer adequately the very questions they raise. Indeed, they seem to waffle on the most important point. But such failings pale in significance when measured against the truly impressive accomplishment they have made.

Friedman and Percival set out to produce a “snapshot” of the workings of one local criminal justice system. They chose Alameda County, Califor—

* Associate Professor, Department of Criminal Justice, University of Nebraska at Omaha.
— Ed.
nia (of which Oakland is the principal city) between 1870 and 1910. It was a fortunate choice because an apparently large body of official records has survived. The book is rich in detail, not just with respect to general patterns but about particular cases, which gives it the feel of specificity. We cannot, of course, assume that Alameda County was or is typical of criminal justice systems elsewhere in the country, and the authors make the appropriate disclaimers. But in other ways the snapshot provided by *The Roots of Justice* identifies important general phenomena about the administration of justice. Its principal contribution lies in its effort to grasp one entire local criminal justice system, from police operations to sentencing and punishment of convicted offenders.

*The Roots of Justice* is a commentary on much of the literature in the field of criminal justice. In a curious way it establishes an implicit dialogue with the second book reviewed here, *Conscience and Convenience*, by David Rothman. The dialogue primarily involves methodology. But, as we shall see, issues of methodology are crucial for dealing with the substantive questions of how our criminal justice system works and, in particular, how it changes. Both of these books raise provocative points about the prospects for the "reform" of criminal justice, points that are extremely relevant to current policy debates.

In its effort to grasp the day-to-day workings of one local criminal justice system, *The Roots of Justice* illuminates two major shortcomings of existing scholarship in the field of criminal justice history. The first is the fact that most studies deal with only one institution. Thus, we have studies of the police, courts, or correctional institutions in a particular time or place. Some take a regional or national perspective, but all are essentially partial views. They do not tell us how the apparatus of criminal justice functions as a whole and cannot, therefore, begin to address the fundamental questions of the quality of justice. What we want to know is, to put it crudely, "who got what?" If Friedman and Percival do not quite answer this question to our satisfaction, they have at least brought us to the point where the question can be considered seriously. That alone is an impressive achievement.

The second great shortcoming of criminal justice history has been the neglect of the day-to-day administration of justice. Existing studies tend to be accounts of the creation of institutions. Rothman's *The Discovery of the Asylum* is one of the better examples of the genre. One finds a common dramatic structure in these works. They open with a description of the old order; the rising action involves the mobilization of the reform effort; the dramatic climax is the creation of the new institution; finally the falling action traces the failure of the institution to fulfill the hopes and dreams of its creators.

A number of problems are associated with this approach to history. The most serious is that the story is told from the point of view of the reformers. The narrative is energized by their critique of the old order, their agenda

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3. *Id.*
for reform, their success and subsequent disillusionment. Methodologically, these histories tend to rely primarily on the writings of the reformers, and we are generally asked to accept their view of things at face value. This is essentially a form of intellectual history, an account of ideas about crime and justice told from the perspective of the reformers. *The Discovery of the Asylum* relies heavily on the “pamphlet wars” between the advocates of different approaches to prison design. (Thus, the “war” is really a skirmish between rival groups of reformers.) The point here is not that *The Discovery of the Asylum* is a bad book; rather, because it is one of the best in the field, it illustrates the limitations of the approach it takes.

There are, of course, different fashions in intellectual history. For decades the dominant school of thought offered a liberal-progressive interpretation of criminal justice history. This view took at face value the assumptions of the reformers about such things as the role of police and prisons. If the institutions failed, it was because intervening factors, usually political interference, prevented them from fulfilling their intended mission. More recently, an anti-progressive school has come to the fore. *The Discovery of the Asylum* was and is the most influential statement of this view. The anti-progressive view simply turns the liberal-progressive view upside down. It treats the assumptions of the reformers as inherently suspect and regards the institution as doomed from the start. Anthony Platt's influential account of the juvenile court, *The Child Savers*, joins Rothman’s book as one of the best examples of this approach.

While the anti-progressive school is appropriately more skeptical of reform and reformers than is the liberal-progressive school, it still views events through the eyes of the reformers. Rothman’s current volume, *Conscience and Convenience*, represents a considerable advance, if only because it takes into account the views of a wider range of actors in the drama. Accordingly, it heightens our appreciation for the complexity of the politics of social change.

*The Roots of Justice* takes an entirely different approach. Eschewing intellectual history, it seeks to give us an administrative history of how criminal justice institutions actually functioned. It allows us to begin to grapple with the fundamental questions of social history: What role did these institutions play in the broader context of society? How did the machinery of justice affect peoples’ lives? What was the role of criminal justice in the allocation of power and opportunity in American society?

Given the task, Friedman and Percival necessarily had to employ a quantitative methodology. They needed to identify the general patterns of institutional behavior by examining large data sets. The quantitative approach has emerged as the major alternative to the dominant intellectual/political approach to criminal justice history. Its partisans, led by Eric Monkkonen, have been aggressive in asserting its potential.

A major part of the achievement of *The Roots of Justice* is its common sense application of methodology. Quantitative history has enormous

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promise, not the least of which is its potential for exploring basic questions that other methodologies cannot begin to examine. But it also entails a number of its own problems, not the least of which is determining exactly what all those numbers mean. Arrest rates, for example, are not necessarily a valid indicator of the level of crime and disorder. The quantitative partisans make a strategic retreat in the face of this criticism and argue that the numbers are a valid indicator of bureaucratic activity. True enough, but it begs the question of what factors shape changes in bureaucratic behavior. Why, for example, do arrest rates go up or down? The numbers themselves cannot tell us whether it was because of changes in the level of crime, or changes in public attitudes about criminal behavior, or factors inside the bureaucratic agency itself.

Friedman and Percival manifest a refreshing degree of perception in responding to these complex questions. Steering a middle course, they point out that both traditional and newer quantitative methodologies can be "misused or abused" (p. 15). Thus, they use both methods: "Wherever possible, we drew samples and counted cases; some times we ran simple statistical tests. But at every step of the way we also tried to get behind our figures. We read reams and reams of documents, stuck away in drawers in the courthouse basement" (p. 15). They supplemented the data sets with anecdotal material from particular cases. "Both steps, we felt, were necessary. Without figures, we have only impressions, stories, fleeting moments, vague opinions. Numbers alone, on the other hand, are blind and mysterious: tablets written in an undeciphered script" (p. 15). Amen. The message here is one that partisans on all sides of the methodological wars should heed.

So much for methodology. Given these tools, what did Friedman and Percival find? Ironically, the most important substantive contribution of The Roots of Justice emerges not from the detailed analysis of the numbers but from the general conceptualization of the criminal justice system. The result is a "model" of American criminal justice that vastly improves over any currently available in the social science literature.

There is no single criminal justice system, they argue. Instead, there are several systems functioning simultaneously. Friedman and Percival posit a "wedding cake" model of criminal justice. At the top are a handful of "celebrated" cases which, for one reason or another, are unique and receive an inordinate amount of publicity. Because of the publicity, they shape public thinking about criminal justice despite the fact that they are completely unrepresentative. In the second layer are the routine felonies, and here we find the real business of crime control. Cases here are disposed of in an informal and highly routine fashion. As is true today, trials were rare in the period studied. Finally, the third layer involves the truly petty cases, the minor breaches of the peace and violations of local ordinances. The object is not so much punishment of crime as it is the imposition of discipline on those unlucky enough to be swept up into the system.

Each of these layers functioned in a very different fashion. The celebrated cases in the top layer involved the full-blown criminal process, including that rare event, the jury trial. Many, if not most, of the detailed technicalities of criminal procedure were invoked in such cases. The middle layer involved what we now recognize as a bureaucratized system of
justice. Cases were disposed of in the most convenient manner possible — convenient, that is, from the perspective of the officials maintaining the system. The bottom layer hardly resembled a system of “justice” at all. Legal niceties had little relevance, and cases were processed en masse.

This wedding cake model is a relevant description of contemporary criminal justice. We are familiar with the few celebrated cases, such as the recent John W. Hinckley trial, which have a grossly disproportionate effect on public awareness. We also recognize the informality with which routine felonies are handled. And we know that our municipal courts process cases en masse and pay scant attention to the formalities of criminal procedure. This wedding cake model is a substantial improvement over the unitary models offered by contemporary social scientists that fail to take into account the very different ways in which different categories of offenses are handled.

The policy implications of this model are substantial. Friedman and Percival are telling us that the “roots” of contemporary criminal justice are deep indeed. Consider the matter of plea bargaining. Obviously it is not a new phenomenon, and those commentators who talk of the “twilight” of the adversary system and the “decline” of the jury trial misread our history. The idea that we once enjoyed an adversarial system of justice is a sentimental fiction. The implications for reform are obvious: We will not readily “abolish” or even substantially restrict plea bargaining for there is no golden age of adversarial justice to which we can return.

The wedding cake model also suggests the hazards of confusing events that occur in different layers of the system. The recent uproar following the John W. Hinckley verdict is an excellent example of how the results of one celebrated case may be used to promote “reforms” (in this case the abolition or restriction of the insanity defense) in another layer. Friedman and Percival’s model sensitizes us to the point that the insanity defense, as one technicality of criminal procedure, has little practical relevance for the enormous volume of business in the second layer of the criminal justice wedding cake. Those who think that closing this alleged “loophole” will reduce crime or even substantially modify the processing of felonies are seriously mistaken.

Friedman and Percival conclude, with respect to reform, that “[s]omehow, reforms rarely ‘worked’ in the long run” in Alameda County (p. 323). The criminal justice system is too complex, too resilient, and it “resists deep structural reform” (p. 325). David Rothman has some equally penetrating insights into the nature of reform in Conscience and Convenience. But before turning to this book, I must comment on the major failing of The Roots of Justice.

Friedman and Percival are least successful in answering their own questions about the role of criminal justice in society. The issue has been posed in recent years by radical and Marxist criminal justice scholars: Is the machinery of justice a tool for maintaining systematic social inequities? Does it serve to keep the outcast out and the downtrodden down? In a section on “Class Justice and the Functions of Criminal Law” (pp. 315-18), the authors waffle, and their answer is not really satisfying. “These are ques-
tions,” they write, “that we cannot really answer from our data.” While no definitive answers are likely, more extended consideration seems in order.

Conscience and Convenience is a sequel to Rothman’s earlier study, covering the second great period of institution-building in American criminal justice. Whereas The Discovery of the Asylum explored the development of the prison in the pre-Civil War era, this volume deals with the rapid spread of probation, parole, the indeterminate sentence, and the juvenile court in the Progressive Era. The ideas underlying these institutions had been circulating for decades, and there had been a few tentative experiments prior to 1899. In a remarkable burst of institution-building during the next sixteen years, these institutions became the established norm in American criminal justice. After 1915 only a few states lacked these components of what is now the full criminal justice “system.”

The most significant part of Rothman’s account is not the story of the creation of these institutions but of their subsequent survival in the face of widespread public hostility. By the early 1920’s parole was in disrepute. A large segment of the public accused parole boards of excessive leniency and found them guilty of releasing allegedly dangerous felons into society. The mood and rhetoric of the period bears a striking resemblance to our situation today. Other critics were aware that parole boards lacked any real scientific basis for their decisions concerning the release of prisoners. Parole boards were only the most convenient whipping boys. To indict parole, after all, was to indict not only the indeterminate sentence but the very philosophical underpinnings of our methods of dealing with convicted offenders. Long implicit in criticisms of existing institutions, this philosophical debate did not really surface until the mid-1970s.

Rothman points out that parole survived this pervasive disillusionment, and the explanation is contained in the title of this book: Parole proved administratively convenient for key criminal justice officials. The parole bureaucracy had a vested interest in maintaining the institution. Possibly even more important were the prison officials who found that parole served a number of important administrative needs. Parole was a mechanism both for maintaining control over the inmates themselves and for managing the size of the prison population. Other officials, police chiefs and judges, while unhappy with many features of parole, deferred to the needs of their fellow administrators. Reform may well have sprung from the collective conscience of well-meaning reformers, but the convenience of administrators determined both its ultimate form and its ability to survive.

Conscience and Convenience represents an advance over The Discovery of the Asylum because it takes into account a wider range of actors. The first volume was too much an exercise in intellectual history, relying primarily on the expressed views of the active reformers. It told us too little about the ideas and actions of other key actors, notably the legislators who authorized the first prisons and the judges who were given new sentencing options. The story is perhaps a bit more complex than The Discovery of the Asylum would lead us to believe. Conscience and Convenience, while working with essentially the same methodology, adds new dimensions to the story and enhances our appreciation for the complexity of social change. To be sure, it is still a form of intellectual history. It tells us what key
officials thought rather than how the institutions actually functioned; nevertheless, it is an improved form of the genre.

By a different route, Rothman has reached some conclusions about the nature of criminal justice reform similar to those of Friedman and Percival. The potential for fundamental change in existing institutions is limited indeed. Friedman and Percival tell us that it is because of deeply rooted day-to-day processes. Rothman shows us how those processes are viewed from the perspective of the officials who run the institutions. The parallels between the 1920's, as described by Rothman, and the 1980's are striking. In both periods an era of hopeful reform and experimentation gives way to one of fear and frustration. Now, as in the 1920's, a conservative crime control mood reigns, and we are witnessing a variety of proposals designed to get tough with criminals. Both of these books suggest that this conservative reform effort will not seriously alter the basic processes of American criminal justice. But those who oppose the current conservative proposals should not take any comfort in this, for the sword cuts both ways. Those who have a different view of the problems of our criminal justice system are no more likely to be able, should the political mood shift, to effect any fundamental changes either.

The message implicit in these two books is depressing indeed. It is all the more depressing because the books are so persuasive. These two books are the products of mature scholars at the height of their powers. Friedman and Rothman have thought deeply about the nature of our criminal justice system and have presented us with two extremely important works. We can only hope that they will continue their respective lines of inquiry and that the junior member of this group, Robert Percival, will pursue the scholarly inquiry he has begun.
In a profession whose practitioners are not ordinarily distinguished by a capacity for silence, historians have kept one secret fairly well. The legal variety of Clio's calling is being resurrected. Ever since the late 1960's historians in increasing numbers have discovered American law as an object worthy of close scrutiny.¹ Nowhere is the resurrection more evident than in the work now under way on the colonial origins of our law. Indeed, the outpouring of books, articles, dissertations, theses, and papers, of which the three books under review here are but more recent examples, threatens to turn legal history into the new wave in early-American studies. That is an encouraging sign. It holds the promise that, finally, the investigation of legal things may someday be joined to the larger field of colonial history, where it properly belongs. Additionally, it serves notice of the need for an occasional evaluation of the direction and content of current research. The appearance of Messrs Konig's, Nelson's and Roeber's books provides one such opportunity.

For legal historians, the central theme of the colonial period has been to

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¹ Fixing a date for such beginnings is always difficult to document precisely, and so when the so-called "second revival" of legal history started is subject to debate. A useful index is when the architects began to write in terms of new beginnings. By that measure, the late sixties and early seventies marked the start of a renewed interest in legal history. See, e.g., Flaherty, An Introduction to Early American Legal History, in Essays in the History of Early American Law 3 (D. Flaherty ed. 1969); 5 Perspectives in American History (D. Flaherty & B. Bailyn eds. 1971); Gordon, Introduction: J. Willard Hurst and the Common Law Tradition in American Legal Historiography, 10 Law & Soc'y Rev. 9 (1975).
emphasize the transfer and transformation of common law in its American setting. From the 1880's to the 1960's, that accent caused them to regard colonial law in a particular way. First, by training and temperament, they were inclined to investigate what Robert W. Gordon has called the internal aspects of law; that is, to treat it as an independent entity, whose autonomous features must be understood. Their concern was therefore with the development of colonial precedents, procedures, courts, and jurisdictions, and in explaining these technicalities, they assumed, sometimes quite without compelling proof, that certain English rules or customs dictated direct American borrowings. Attorneys, law professors, or judges, they frequently glossed over the seventeenth century, which they regarded as populated with legal primitives, preferring instead to devote their energies to discovering the circumstances leading to such things as the rise of the professional bar. Next, these scholars, because they trained or taught at the great eastern law schools, tended to equate the New England and the Middle Colonies with the center of the colonial universe. Thus their work took on a decidedly Yankee flavor to the point where it sometimes seemed as though nothing of importance occurred south of Pennsylvania. Finally, they undertook the printing of the stuff of legal history — statutes, court records, and other such documentation. Again, with a few noteworthy exceptions, New England sources comprised the bulk of what was rendered into print.

The attention given to the autonomy of early American law had a consequence that was as predictable as it was harmful. By stressing technicalities, legal scholars engendered the myth that only they who spoke the special language of law were capable of interpreting the law's history to others. It is quite understandable how such men, given their orientation, should seek to cloak their discipline with professional mysteries. Moreover, the nature of law, like that of natural sciences, does require special skills of its historians which others obviously do not need. But, by making the mysteries and skills appear so unusual, if not downright arcane, they erected a body of knowledge so peculiar that it seemed to say almost nothing to the uninitiated colonialist. And so, colonial historians tended to ignore their period's legal history and its sources altogether.

While their emphasis now seems misplaced, the old legal scholars did make important contributions. They were the pioneers who pointed ways for others to follow. Many of their institutional studies are now classics that are fundamental to modern students' grasp of the outlines of early legal history. What is more to the point of this essay, however, is the effect of their interpretations. There is a direct link between disaffection with the older views and the resurgent interest in doing the colonies' legal history.

What are the impulses that caused the dissent? In the first place, even in its heyday, internal legal history never lacked critics, just as there were always venturesome scholars who followed independent directions. Of them, none has been more influential than James Willard Hurst. A prolific author, his writings have a transcendent importance because of their singular

2. Gordon, supra note 1, at 9-11. Gordon's article also provides a general summary of the growth of American legal historiography down to 1975, and it is upon this summary that my short sketch rests.

3. See Flaherty, supra note 1, at 20-32.
insights. The most valuable of these for the colonial era is one that now seems quite obvious: there is more to the American legal system than its rules and institutional buttresses. Apart from the technicalities, law has a social function. It defines and governs the social order, but not in splendid isolation; instead, it is susceptible to economic, political, or social imperatives because these are the obligations that give it definition. Thus to study even the most ordinary components of the legal order through time is to illuminate society’s values, as well as how they change with the passage of years.4

David H. Flaherty also had a hand in the revival. His article, “An Introduction to Early American Legal History,” which opens the collection, Essays in the History of Early American Law, was a skilled survey of the writings on colonial law as of 1969. He repeatedly drew attention to the gaps in existing knowledge while he simultaneously suggested topics or sources that awaited exploration. Among other things, he reminded his readers that much remained to be done on the seventeenth century and the southern colonies. The entire essay reads as a brief for fresh conceptions and approaches to the legal problems of the settlement, growth, and matur- ing of colonial America. Flaherty was also among the first to recognize the possible application of Hurst’s intuitions to colonial legal studies. Both in the “Introduction” and elsewhere he advanced the proposition that with careful employment Hurst’s ideas might take colonial legal history “beyond the traditional framework.”5

Even as Flaherty wrote, other changes were afoot. A harbinger of things to come was the publication of a spate of New England town studies. Their relevance to reviving interest in colonial law lay in their advocacy of what is now called “the new social history” as a way of interpreting the past.6 The new social historians concern themselves mainly with the past’s little people, and in writing history from the bottom up, they accord weight to every fact, irrespective of its significance. What makes such a democracy of data manageable is the reliance upon quantitative methodologies first developed by French and English historical demographers. The new learning also draws intellectual sustenance from the social sciences, principally cultural anthropology, economics, and psychology, as well as sociological theories about the family and modernization. Given the serial nature of court records, plus some social historians’ own employment of them, it was inevitable that scholars with interest in law should be attracted to the new methodologies. After all, these methods, with their reliance upon statistical sampling techniques and computers, did ease the tasks of sorting through


6. Five of these were published in 1970. They were subsequently reviewed at length by John M. Murrin in II Hist. & Theory 226 (1972). Those books were also to influence southern colonial history. The major statement of the new social history for the colonial South is THE CHESAPEAKE IN THE SEVENTEENTH CENTURY: ESSAYS ON ANGLO-AMERICAN SOCIETY (T. Tate & D. Ammerman eds. 1979).
great masses of records. Then too, some of the theories undergirding social history seemed to form links to Hurst's ideas. Here were techniques and thinking that could aid the quest for a basis upon which to reassess early American law, and they soon began to influence how legal history is written. This effect is clearly seen in the work of Professors Konig, Nelson, and Roeber.

In *Law and Society in Puritan Massachusetts*, David Konig investigates the growth of law in Essex County. He is concerned not only, as he puts it, with "the internal development of legal doctrine and procedure, but [with the discovery] of the demands that social, economic, and political contingencies placed on legal institutions and how those institutions changed in order to become important in society." Also, he examines "the position of the legal system in a society with a multiplicity of institutional power sources," as he develops the theme that "the county magistracy and legal system must be viewed as serving many of the functions previously identified with the town or congregation" (p. xiv).

Konig has limited the study to the years between the founding of Massachusetts and the crown's grant of the second charter. What is striking about Essex in those years was the Essexmen's propensity to sue one another. For example, he finds that between 1672 and 1684, county residents filed some 3000 actions in the courts at Ipswich and Salem, a remarkable number indeed, when the reader recalls the expense of filings and the dangers of travelling about in a colony wracked by Indian wars.

For Konig, the question becomes why so much litigation? Traditionally, Puritan historians have maintained that such contentiousness evidenced the decline of missionary zeal among Bay colonists. As that sense of piety eroded, settlers turned more aggressive and acquisitive, and they looked to the courts to resolve their differences, thereby making those bodies instruments of restraint. Konig casts that formulation aside, preferring to replace it with another possibility. An increase in the amount of litigation does not prove Massachusetts Bay to have been a pathological society; instead, it indicates the movement toward social stability.

Konig makes the point that from the start the saints always had courts. In those early years, when the sense of mission was keenest, the legal system served largely to reinforce common ideals. In time, the force of those ideals slackened, and with that loosening came the transformation of the legal order. It became the pattern after which a coherent society might be fashioned. At that point, it was the chief conservator of order as well. Lawsuits were the agency of change, and rather than being a signal of decay or a way to exact personal revenge, litigation supplied the Puritans with the means to resolve ordinary community problems or to gauge acceptable personal behavior. Constant resort to the courts also was the device for testing what was useful about the English legal heritage in an American setting.7

*Dispute and Conflict Resolution in Plymouth County, Massachusetts, 1725-1825*, grew out of William E. Nelson's earlier investigation of how common

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law was Americanized. Like Professor Konig, Mr. Nelson in this slim little book concentrates his attentions upon a single geographic region in the Bay Colony. Where Konig treated a community in the throes of being and becoming, Nelson deals with one that had existed for a hundred years, and he is curious about how its inhabitants handled disputes and conflict during its second century. Early on, he concludes the impossibility of ascertaining “how disputatious eighteenth-century New England was,” and so, he determines to “focus upon the techniques by which disputes were resolved rather than upon the frequency at which they arose.” To obtain these objects he finds it necessary to construct two “typologies” which he contends were used in Plymouth County’s several towns. Some towns, he argues, settled their disputes “without recourse to outside institutions such as courts,” while others “frequently did turn to the courts” (p. 4).

Nelson identifies three principal agencies of conflict resolution: the town meeting, the courts, and the church congregation. Town meetings worked primarily as instruments of governance. The inhabitants turned to them only in case of “group conflict,” and Nelson concludes that “the town meeting was not a prominent institution for adjudicating disputes” (p. 22). The courts were more likely to control what is called “the dispute resolution process.” Their main purpose “was dispute resolution, even though the Court of Sessions, in particular, did possess some broader governmental jurisdiction” (p. 26). While the third agency, the church congregation, worked in a fashion similar to the courts, it was distinguished from them by the greater frequency of its meetings and its informality, as well as by the different sanctions it employed and its judging of moral rather than criminal misdeeds.

Having described the devices for settling disputes, Nelson next compares what conditions in the fourteen towns, of which Plymouth County consisted, led their residents to turn to “their local congregation for the resolution of their disputes with the circumstances under which they brought their disputes to formal legal institutions” (pp. 74-75). Most of the time the church, at least down to the onset of the Revolution, appears to have been the preferred agency. But certain conditions, such as the recalcitrance of suitors who refused to submit to church discipline, the presence of dissenters or schismatics, and what are denominated “commercial litigants” dictated recourse to the courts.

The Revolution worked great changes in these patterns. Both during and after the conflict for independence, citizens of all the towns turned more fractious, and the informal agencies declined in their effectiveness as more suitors sought relief through the courts. There were several reasons for this change in Nelson’s estimation. There were more suits involving commercial litigants, suggesting that the intrusion of more worldly men into the affairs of Plymouth County eroded the bonds of religious cohesion. Then too, Nelson finds an increase in the number of dissenters living in the county, and clearly, these people had little inclination to turn for help to the Congregationalists. There was also the effect of the rise of party politics in the 1790’s, which became institutionalized early in the new century. By

1810, therefore, these intrusions into a formerly stable community bonded by common agreement in religion and social mores led to the emergence of groups who saw the courts, not the church and town meetings, as the vital forum for adjudicating disputes. In short, that change signalled the dissolution of the social contract which held the county together since its beginnings.

A. G. Roeber paints on a much grander canvas than those of Konig and Nelson. His subject in *Faithful Magistrates and Republican Lawyers* is the place of lawyers in what he terms “Virginia legal culture” throughout the century and a quarter following 1680. In portraying that object, he endeavors to challenge Charles S. Sydnor’s conclusions about the impact of the Revolution on the Old Dominion’s law and political institutions.

Some thirty years ago, Sydnor wrote the classic exposition of Virginia politics in the age of George Washington. Lawyers did not occupy a prominent place in the Sydnor formulation. Although some in the Virginia dynasty trained in the law, he maintained “planters, not lawyers, dominated politics.” Those planters controlled that first stepping stone to political preferment, the county courts, and that monopoly insured their dominance, as well as the initiation of those whom they chose for advancement. They helped make a revolution, but the struggle to cut the English ties left them pretty much as they had entered it, at the head of society.9

Roeber regards this as a superficial assessment of the situation, and so he sets about constructing an alternate interpretation. A necessary ingredient to his purposes is what he discerns as the professionalization of the colony’s law and the modernization of its society (p. xvi). Throughout the eighteenth century, the law’s complexity grew, the number of lawyers proliferated, and something he calls “print culture” spread across a “nonliterate plantation society.” Before 1750, however, no colonist talked of modern society, of law as science, or of lawyering as a professional calling. Instead, Virginians “interpreted events according to their identification with one of two opposing traditions—‘Court’ and ‘Country’” (p. xvii).

By these lights, most Virginians were Countrymen. That is to say, down to mid-century, they conceived law to be of a highly local and personal nature. Lawyers they distrusted for being lawyers, but more so because trained barristers were Courtmen who viewed themselves as a group with a collective interest in centralizing power in the capital. The colonists had long existed without lawyerly pettifoggery; indeed, early on few trained lawyers went to the Old Dominion because of the slender pickings there. Every county had its bench comprised of members drawn from the leading planter families, and at their regular sessions, these judges dispensed a justice rooted deeply in the demands of deference and dependence which defined English and Virginia Country ways. These sessions were important beyond their judicial purpose; they were encrusted with rituals whose performance served as a reminder of every man’s place in the rural social order. In fact, Roeber maintains the proposition that these rituals, thickly described, reveal the extent to which Virginians adhered to Country ideals.

Although temperamentally hostile to Courtmen, Virginians came

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grudgingly to accept them. A reason for their acceptance, Roeber informs his reader, was tied to the development of towns and commerce. These indices of modernity engendered litigation that transcended county boundaries as it revealed the incompetence of local magistrates to manage more complicated legal issues than those they ordinarily confronted. Moreover, as the eighteenth century unfolded, there was a greater need for the law's revision and standardization, as well as for formally trained men to administer a growing corpus of law. As statutes increased in quantity and complexity, the professional bar, committed to the centralizing tendencies in Court philosophy, emerged. Its members began to subvert the oral culture of Country society, and on the eve of the breach with England, the lawyers had elbowed themselves to places of leadership.

Lawyers not only revolutionized the legal order, but they helped lead the Old Dominion through the Revolution. They did so by persuading their Country cousins that Courtmen could be as good republicans as they. All the while, the professionals continually eroded the power of the local magistracy until their triumph came in the complete overhaul of the commonwealth's entire court structure. In the end, then, Virginia's legal culture was "created by both faithful magistrates and republican lawyers" (p. 261).

Law and Society in Puritan Massachusetts is the most compelling of these volumes. Its success is due to Konig's judicious development of its major themes, a result of evidence carefully considered and cogently employed. Konig draws freely from, among other things, those contemporary English treatises and manuals that influenced the saints' attempts to tailor the home country's legal customs to new uses. The arguments are usually persuasive because their author writes with flair. He owns a keen eye for the apt quotation, just as he has an ear for the neatly-turned phrase. What mars though is the occasional lapse into social-science-speak, which clouds rather than illuminates the exposition. On balance, the book stands as a fine illustration of work that takes the examination of colonial law beyond traditional boundaries toward considering the law's wider implications for the transfer of British culture to the New World. It is, in short, the sort of study Willard Hurst has called for.

Analyzing the connection between law and dispute resolution is a worthy enterprise, but Nelson's treatment is unsatisfactory. Too often Nelson exhibits excessive concern for the statistical purity of his evidence, creating thereby the perhaps unwarranted impression that means count for more than results. What results are produced frequently state the obvious, inflate conclusions, or obscure the significant. Is it edifying to learn that in the 1790s most Plymouth men still farmed? Is it in the least surprising to discover that Quakers were more likely to take their troubles with the saints to court instead of to church? And, is every disagreement over hiring or maintaining a Congregational minister necessarily a sign of religious disputa
tion? Plainly, some of this difficulty derives from Nelson's resort to statistical methodology. Its employment automatically has a limiting effect upon prose style. There are just so many ways by which tabular evidence may be rendered into words.

Even allowing generously for such limitations, Nelson's writing is very nearly impenetrable at times, and comprehending the book requires strict concentration. That done, comparing it with Konig's leaves a reader to
wonder if the two works do not come to opposite conclusions about the place and role of legal institutions in Bay communities. Konig downgrades the significance of the town meeting and the church congregation, just as he sees in rising numbers of court cases a sign of Essex’s vitality and stability. Nelson regards the town meeting and the congregation, especially the latter, as important agencies for settling conflicts. He interprets the greater recourse to the courts in the period after the Revolution as a sign of the breakdown of an old social consensus. His stand seems closer to that of earlier Puritan students. This contradiction, if such it be, cannot be reconciled without further inquiry.

Faithful Magistrates and Republican Lawyers piques one’s interest because of its subject matter, but reading it soon dulls that curiosity. The book is misconceived in ways that severely flaw it. Revising Charles Sydnor’s work is a plausible enough undertaking. Classic though it is, it is not the last word on eighteenth-century Virginia. But Sydnor was quite unconcerned with the place of lawyers in the Old Dominion’s golden age. He dismissed them as unimportant. Hence, he is more Roeber’s straw-man than his foil.

Roeber also tries to fit his facts into a curious view of colonial politics and society. They will not go into the mold because the viewpoint is wrong. He insists on tying the professionalization of lawyers to what he perceives as a wider contest between English imperial authorities and growing numbers of colonial republicans. In itself, the rise of the Virginia bar is a significant, though by no means new, concern of legal historians. Actually, it is the topic of Alan McKinley Smith’s 1967 dissertation and E. Lee Shepard’s recent article, both of which have more to offer on the subject than Roeber presents.10 In Roeber’s hands, the Court-Country model distorts events and evidence to the extent that it loses whatever usefulness it might have. The technique of “thick description” as a device to illuminate the attachments of lawyers and magistrates to the ideals of Court and Country obfuscates rather than clarifies. How details relate to actions or changed circumstances is seldom revealed. Phrases like “legal culture,” “print culture” or “non-literate plantation society” lack precise definition, causing the reader to ponder their meaning as well as their import. At the last, he is never sure whether Roeber is doing legal history, social history, or a little of both.

In conclusion, these books invite comment on the condition of the legal history revival. Apparently, few scholars have heeded Flaherty’s plea for a more intensive investigation of the colonial South. Massachusetts Bay still garners a disproportionate share of attention. If that imbalance does not abate, we may be verging on that dreadful day when we know more about the Bay Colony than we need or ought to know. Another result is evident in the Roeber volume. It is a pioneer study that is clearly impaired by its author’s misapprehensions of Virginia’s legal and political arrangements.

Law in Colonial America

Had there been a fund of basic research on the courts, their officers, and the laws themselves, Roeber might well have avoided many of those mistakes.

All three are part of the series Studies in Legal History published under the auspices of the American Society for Legal History. Since its inception, it has achieved distinction as an important outlet for new work. Such prominence merits recognition, as well as continued support, for the Society, just as it places special responsibilities upon the series editors. They must exercise rigorous care in considering and preparing manuscripts for print.

Sad to report, such attention to these volumes is not always evident. Nelson's and Roeber's are especially marred by sloppy preparation. Typographical and spelling errors, as well as infelicitous phrasing and imprecise usage afflict both. If the authors failed to catch these, then the editors should have. After all, the process of publication is a cooperative venture, the purpose of which is to produce books that express the author's findings clearly, concisely, compellingly. These three books do not always meet the standard, and to the degree they do not their worth is diminished.

The purpose for commenting upon these editorial slips is to make this point in the words of Ben Johnson: "Neither can his mind thought to be in Tune, whose words do jarre, nor his reason in frame, whose sentence is preposterous... How shall we look for wit from him whose leasure and head, assisted with the examination of his eyes, yield you no life or sharpness in his writing?" Each of us who does legal history should keep sight of our unique dependence upon the written word. If we write poorly, we shall be condemned to obscurity.

Before the social historians' techniques and assumptions work their way too deeply into the legal scholar's thinking, it is well worth a hard look at the implications of such a commitment. Two of these, one technical, the other philosophical, possess the greatest moment for the new legal history.

The utility of quantitative methods is not in doubt; some of its results are. Quantification is too often employed to measure the obvious, and the product it achieves necessarily seems banal. Its disciples frequently labor under the misguided conviction that the very use of quantifiable evidence somehow invests findings with superior authority. Colonial court records seldom were kept in a fashion that allows reconstruction of what they recall with mathematical precision. At best, no matter the technique, all the records reveal an approximation of reality, which is all any mode of historical investigation is capable of achieving.12

By the same token, legal scholars should approach social history's theories with an open skepticism.13 How valuable is Roeber's use of an anthropological concept like "thick description," for example? Describing cultures is very much a part of what anthropologists do, but description performs a much less prominent role for legal historians, who are about the business of interpreting the significance of their evidence as it relates to

13. See id. at 300-04 (criticizing the "eclectic manner in which historians have "shopped around" in sociology for usable concepts).

change over time. If they are content merely to describe every fact they find, they have interpreted nothing. Anthropological description and historical interpretation are different species of intellectual enterprise. The latter implies a willingness to discriminate among bits and pieces of evidence and to decide that some are more significant than others, just as it means arraying the chosen facts in a fashion that gives them meaning. And what of an idea like modernization, which has gained such extraordinary popularity lately? Created by social scientists as a theoretical device to describe the emergence of developing nations in the modern era, colonialists have found it a most seductive theory. It is, in the words of one enthusiast, "far more critical to an understanding of the first two centuries of American life and far more worthy of scholarly attention than the American Revolution."  

D.H. Murdock terms such expressions of enthusiasm "regrettable," observing that modernization posits a crudely linear historical development. It also imposes artificial categories on the growth of social entities, as it "leaves no room for the discontinuities, divergent developments and persistent survivals of the obsolete which characterize the history of most societies."  

Raising such criticism does not automatically brand the critic a Luddite. The reason for calling attention to them is not to condemn social history, let alone to argue that it holds no relevance for the legal historian. Quite clearly, it does. In the hands of a sensitive, skillful, and imaginative scholar like David Konig, its approaches yield impressive results. The point of the objection is that social history is no sovereign cure for the problems of a newly emergent legal history; it has its limitations. And so, before rushing to quantification and social scientific theory willy-nilly, legal historians should heed the advice of that ancient precept, caveat emptor.

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THE MEDIEVAL ENGLISH COUNTY COURT

Stephen D. White*


Before Dr. Robert C. Palmer began his intensive work on the medieval English county court, the legal history of this institution had never aroused much interest among modern historians of the common law, who wrote about it, if at all, only cursorily. Because it was thought that medieval county court judgments continued to be rendered by mere landholders, long after the time when judicial power in some other courts had been assumed by professionals, the county court could be regarded as a curiosity. Its nonprofessional and vaguely democratic character looked backward, perhaps, to the era before the Norman conquest of England. Its primary role in post-conquest legal history was to suffer certain decline, though not a total demise, when it was confronted by the advance of centralized, bureaucratic, professional legal procedures. Because the medieval county court eventually took on the task of selecting knights of the shire to sit in Parliament, it had some claim on the attention of historians. But as the modern study of medieval legal history became almost as specialized and professionalized as the medieval legal system itself, that claim on scholarly interest, like suit to the old county court, could be discharged by others — the constitutional (as opposed to legal) historians.1

This way of looking (or sometimes not looking) at the medieval county court was recently challenged by Palmer in several impressive articles,2 and has now been completely undermined by the arguments set forth in his even more impressive book. In this new work, Palmer's objective is to rescue the once-despised medieval county court from what E. P. Thompson calls "the enormous condescension of posterity."3 But Palmer's rescue strategy differs totally from that of recent writers on the past history of down-trodden people, marginal cultures, or obsolete institutions, which, like the old county court, eventually faded away.4 Palmer wishes to accord some dignity to the medieval county court. But this is not because he sees

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* Associate Professor of History, Wesleyan University; Visiting Member, School of Historical Studies, Institute for Advanced Study, Princeton, New Jersey, 1982-83 (fall term). A.B. 1965, Harvard College; Ph.D. 1972, Harvard University. — Ed.


4. On the ultimate demise of the old county court, see J. Baker, supra note 1, at 22.
in it, as Thompson now sees in the jury, "a lingering paradigm of an alternative mode of participatory self-government" that can rival more up-to-date paradigms reflecting "the rule over the people by bureaucrats, 'experts', or a substitutionist vanguard." Instead, having found incontrovertible proof, through much reading and analysis, that the medieval county court was actually dominated, not by local landholders, but by expert professional lawyers, their noble patrons, and royally appointed sheriffs, Palmer establishes the importance of this body — if not its dignity — by showing that in the long march of English law towards professionalism and modernity, the county court stood, at least briefly, in the vanguard.

In ten closely reasoned chapters, Palmer first argues that the county court was run by real professional lawyers, and not by those whom he regards as "amateurs." He further maintains that instead of focusing primarily on the "decline" of this court, legal historians should closely examine its changing place in the medieval legal system. They should show how, by the thirteenth century, this type of court first became integrated into a unified judicial structure, or "legal system," but later lost its close association with other courts, as the king's court assumed many of its judicial powers (p. 306), and as English judicial organization lost much of its earlier unity. Palmer also shows how the study of this one species of court can illuminate other subjects, such as the origins of the legal profession.

Palmer's work is therefore valuable for its substantive and methodological contributions to medieval legal history; and while he does not really substantiate his insistent claim that the county court generally proceeded fairly and justly, this belief of his is not essential to what is, in other respects, a convincing and important historical argument.

Palmer first sets out to discuss "The Institutional Framework and Personnel" of the medieval county court (Part I: chs. 1-5). Here, his objective is both to answer certain central questions about the workings of county courts and to use his answers to challenge received opinions about how these judicial bodies really functioned. When and where did the county court meet? What role did the sheriff and his subordinates play in this institution? What sorts of people owed suit to this type of court? And who actually served as its judges? Who carried on the work of pleading in the

6. Palmer considers "a person involved in legal activities a professional lawyer when, for a period of years, that person appears to be spending the major part of his time in legal functions and deriving the greater part of his income from those activities or, at least, from the investments made from that income, and when that person possesses a specialized knowledge differentiating him from laymen." P. 89 n.1.
7. On the claim that the county court was an "amateur" body, see, e.g., pp. 56 & 112. It is worth noting that nonprofessionals need not be amateurs.
8. Although Palmer frequently uses the term "legal system," (see, e.g., pp. xiii, xiv, 141, 173, 182, 297, 300, 301 & 304), he does not clearly define it. When using this term, he is concerned more with the relationship between courts than with the kind of law that courts administer.
9. Thus, in the fifteenth century, Palmer maintains, the English courts did not constitute "a legal system." P. 304.
10. In his two articles, see note 2 supra, Palmer has additional things to say about the origins of the legal profession.
Medieval County Courts

county court? And what roles did these pleaders play outside this forum? Having found new evidence bearing on "every topic of importance" concerning the county court (p. xi), Palmer uses his new findings as elements in a complex discussion of medieval English judicature. Thus, his treatment of venue and scheduling (ch. 1) really serves an argumentative purpose, as well as a purely descriptive one. He finds, for instance, that the timing of royal hundred court meetings was not dictated by a provision of the Magna Carta calling for meetings every three weeks. It evidently was determined instead by the sheriff's wish to coordinate the schedules of these lesser courts with the established schedule of county court meetings. From this, he can argue, first, that the county court was "the primary royal court" in every county, and, second, that careful planning, implemented by the sheriff and "a sophisticated staff of competent bailiffs," went into the scheduling of county court meetings (p. 27). While one may wonder how much sophistication was really required for the performance of this task, Palmer's findings on this point certainly help him to move toward his goal of showing that the county court was under royal control and formed part of an integrated legal system.

Palmer provides further support for this general thesis in chapter two, where he demonstrates that some of the county officials who managed county court meetings and/or enforced county court judgments had close connections at the royal court. He also tries to justify his own favorable judgment on the county court by arguing that the viscontiel bureaucracy, composed of the sheriff and his subordinates, was relatively efficient, responsible and law-abiding. Between 1180 and 1340, he shows, the workings of the county courts were shaped by the crown policy of appointing sheriffs with close ties to royal government. These men held their offices "for a limited time and in a succession of counties" and did not regard them simply as a source of monetary gain. This appointments policy, he maintains, was "vital to the rapid implementation of judicial innovations and the avoidance of legal chaos" (p. 31). Palmer acknowledges that sheriffs wielded power in the county courts and profited financially from county court proceedings (p. 36), and that they sometimes abused their power there. But he stresses that their oath of office made them legally liable for "unjust and inefficient conduct" (p. 37), that they were sometimes prosecuted for breach of these obligations, and that they even had "a vested interest in making the county court operate efficiently and justly" (p. 37).

Similarly, Palmer first notes that some of the sheriff's underlings purchased their offices for fixed fees and thus had a financial incentive to take more than what he calls a "fair" profit for their work (pp. 50, 53). But he then points out that these men were legally accountable to the sheriff and were bound by contract to perform their duties properly. Sometimes they were even fined for their failure to honor this obligation (p. 53). These facts suggest to Palmer that a sheriff's bailiff normally possessed "a knowledge of the law sufficient to fulfill his duties competently, so that he could avoid amercement and make a profit" (p. 53). Palmer therefore concludes that while the viscontiel bureaucracy was hardly "a faultless servant of the public interest," it was probably not "as corrupt as the occasional virulent protest might indicate" (p. 55). One need not share Palmer's sunny view of medieval county bureaucracy to appreciate the persuasiveness of the claim
that, when viewed from one or perhaps several perspectives, the work of the
sheriff and his underlings was carried out quite successfully.

Palmer's claims about the efficiency of the county court's proceedings
and about its close ties to other judicial bodies are supported in a particu-
larly arresting way by his original and important findings about what sorts
of people actually judged county court cases. Introducing this exceptionally
well-argued section of his book (ch. 3), Palmer writes:

Medieval legal theory, the words of the common law writs, and modern
legal historians identify suitors as the judges of the county, hundred and
baronial courts. They had a specific tenurial obligation to attend the court
and render judgments. . . . Since the obligation of suit was tenurial and
not based on professional qualifications, the position of the suitors as the
judges of the lower courts has made the county and hundred courts seem
irrevocably amateur [and "democratic" (pp. 88, 113)] to the historian. . . .
Moreover, it has been assumed that the doctrine, the actual obligation, and
the consequence of suit of court continued through and well beyond the
thirteenth century. [P. 56.]

Moving from a detailed discussion of the Cheshire county court to an anal-
ysis of county courts in general, Palmer tears down this received view.

He first shows that in Cheshire, a clear distinction was regularly made
between "judges" and "suitors" (p. 60): the former held the real power of
judging cases, while the latter had only an opportunity to influence the for-
mer's decisions (pp. 64-65). Although "judges" and "suitors" thus played
different roles in county court cases, members of both groups were fulfilling
generally similar obligations to do "suit" to the county court. This obliga-
tion was normally imposed, not on townships or individual people, but on
manors, and it could be fulfilled either by the immediate lords of manors or
by people sent in their place (p. 69). The difference between mere "suitors"
and "judges" (who also did "suit" to the county court) seems to have arisen
simply from the fact that by ancient custom, some manors were obliged to
send "judges" to the county court, while other manors were bound only to
provide mere "suitors" (p. 74). The mere "suitors" of the Cheshire county
court were apparently an obscure lot, about whom little can be learned (p.
74); but the "judges" Palmer skillfully identifies as "the bailiffs or senes-
chals of those who owed suit and were obliged to find a judge" (p. 72).
After arguing that the Cheshire county court was not "atypical," Palmer
goes on to show that after about 1270, the duty of suitors to attend the
county court was allowed to lapse, as this institution itself began to lose
importance (p. 81). Even before that date, however, the county court judges
had been professionals. In Cheshire and elsewhere, powerful landholders
had

a vested interest in maintaining a high level of excellence in the county
court and in seeing to it that their interests were intelligently and forcefully
represented. The seneschals and bailiffs were among the most skilled legal
people working in the county and they were the logical choice to be judges
in the county court. [P. 72.]

Thus, certain political facts, along with a certain sort of logic, determined
that the medieval county court would not be "a democratic assembly of

11. P. 74; cf. pp. 74-78.
knights of the county.”

Equally significant for Palmer’s overall argument is his discovery that real professional lawyers were working in the county courts as pleaders, thus performing another complex activity demanding great “legal expertise and knowledge” (p. 91). Palmer draws several conclusions from this finding. First, he uses it to question the traditional assumption that professional secular lawyers only began to appear in England in around 1200, when they can be found practicing in the king’s courts. Having found bailiffs and seneschals practicing in the county courts in the earlier twelfth century, he suggests that these men were “the first secular lawyers in England” and that lower courts, like the county court, were actually “more important than the king’s court in the genesis of the [legal] profession” (p. 136). The finding that county court pleading was carried out by professional lawyers also aids Palmer’s effort to demolish established views about the “amateurism” of this court. These lawyers, he finds, were real professionals. Hired on occasion for an individual case (pp. 93, 112), a typical local lawyer was usually retained for life by around two dozen clients, each of whom paid him an annual fee of up to 20s. (pp. 112; 94-97). These lawyers also served concurrently as bailiffs, pledges, undersheriffs, county clerks, sheriffs, members of county commissions, and attorneys in the king’s court (p. 112). They were seen as “specialists, people with uncommon skills, and were paid accordingly” (p. 112). They were not sustained solely by “high professional ability”; a lawyer’s “power was based on the wealth of the aristocrats he served as well as on his knowledge of the intricate world of legal technicality” (p. 89). Since English barons used the county courts and the bailiffs who represented them and their tenants there to consolidate their own feudal power, Palmer can claim that “the county courts embodied the ties between lord and man” (p. 113). Palmer does not think, however, that the county court therefore served only as an instrument of baronial rule. In his view, these highly trained, professional clients of medieval nobles made the county court “a professional and legally respectable institution, rather than the amateur court presented by modern historians.” Moreover, the training and career patterns of these professional lawyers ensured that county court proceedings would not be shaped solely or even predominantly by local political interests or regional legal traditions. Because these men also used their craft in other courts, including the king’s court, they created, albeit inadvertently, “strong bonds of common legal thought between the different courts of the country” (p. 89). Thus, legal professionalism and royal control prevented local magnates from completely dominating the county court and made this institution part of a unified legal system.

In Part II of his study, which treats county court jurisdiction, Palmer covers many new topics, but continues to sound the same themes first heard in Part I. He again shows that the county court was part of an integrated court system, and his claims about the basic fairness of medieval English law become more insistent. “One of the most basic principles . . . which informed English legal procedures,” he says, “was the principle that a litigant

13. See pp. 113-119.
gant should receive a fair hearing and be accorded the benefits of customary process" (p. 141). Because in certain instances, which he enumerates, English courts might turn out to be "irremediably biased" (p. 141), considerations of fairness required that litigants be allowed to escape from a potentially biased court, or else to prosecute a court that had treated them unfairly. While conceding that this basic commitment to fairness was seldom expressed in medieval England, he still insists that it provided the underlying rationale for several processes by which lawsuits could sometimes be transferred into or out of the county court. Toll, pone and the grand assize, he thinks, were all "designed to avoid injustice before it had occurred" (p. 151), and if a litigant still suffered injustice in a lower court, he could, by securing a writ of false judgment, gain access to the king's court, where impartial justice, Palmer believes, could be regularly obtained (pp. 149-50). Although Palmer cannot really prove that English court procedures were fair, he can still show that these procedures, along with several others, constituted "institutionalized bonds between the courts which exerted constant pressure towards unifying and making common the varying customs in England." Thus, procedures like toll and pone contributed to the process by which English courts were "welded" into "a legal system" (p. 173).

Links between the county courts and the central government are also treated in chapter 7, which discusses the so-called viscontiel writs. These writs, which were addressed to the sheriff and originated cases in county courts (p. 174), became writs de cursu under Henry II (1154-1189). What concerns Palmer is why they were instituted at all, when county court cases could be initiated without writ, and what effects these writs had on English court structure. He finds that the institution of the viscontiel writs served to tighten the bonds between the county court and the king's court. He also claims that for reasons that he closely examines, at least some of the viscontiel writs provided litigants with "better" procedures (p. 182) than were otherwise available in the county court and made these procedures available to more litigants than the king's court could accommodate. Moreover, the fact that some of these procedures led to reviews of county court cases by the king's court meant that lower court practice was "constantly brought before the best legal minds in the county and, if unjustly aberrant, criticized and perhaps even ignored in county process" (p. 146). Indeed, Palmer claims that "[t]he viscontiel courts were . . . [a part] of the reason why the common law by 1215 was perceived to be necessary and beneficial to England as a whole and not just to the king" (p. 219).

Palmer next sets out to explain certain interesting changes that had occurred by the fourteenth century in the types of cases handled by county courts (ch. 8). By the 1330s, at least some of these courts were apparently dealing with "a narrower range of forms of litigation, fewer cases initiated by writ, and, in debt, claims for smaller sums" (p. 228). These changes suggest that the county courts were declining in importance and becoming less closely tied to the king's court. But because Palmer takes the unusual but plausible position that litigants may actually have preferred local courts

16. See, e.g., pp. 142-147.
to the king's court, he is unwilling to explain this change by making the conventional assumption that litigants were eagerly and sensibly by-passing the county court and flocking to Westminster. In the end, he argues very convincingly that these changes were "the long-term results" of royal inquiries in the 1270s into the organization of local government (p. 228). Given his favorable view of the county court, he cannot claim, as he does when treating the origins of the viscontiel writs, that these changes were signs of real progress and instead views them as examples of "the untoward influence of statutory action" (p. 220). Nevertheless, he can still incorporate these developments into his general argument by treating them as examples of "the complicated and highly technical coordination of the king's court with the local courts" (p. 220).

A final expository chapter (ch. 9) treats the county court's changing relationship to other judicial bodies. Within the county itself, he finds, the county court held a position of superiority, which it did not lose after 1300, even though there were shifts in its relationship to liberties, courts of ancient demesne and lesser courts without the franchise of return of writs (pp. 286, 296). Nevertheless, the county court's relationship to the king's court altered so that the position of the former in English court structure changed as well. The establishment of the nisi prius system, he argues, drove a "wedge . . . between the county courts and the king's court, each functioning more and more without reference to the other" (p. 228), while the general eyre disrupted county litigation (p. 289) and did damage to county litigants (p. 291). As the county court and the king's court drifted apart in most respects, the election of the knights of the shire to sit in the court of Parliament became the county court's "most important prerogative" (p. 294). The county court could still perform this function because it still embodied "the community of the county" (p. 296). For Palmer, therefore, a court dominated by lawyers, who represented barons, and by sheriffs, who represented the king, also represented a county community, whose nature and composition Palmer leaves mysteriously undefined. In a carefully argued conclusion (ch. 10), Palmer suggests that when studying medieval judicial organization from the twelfth to the fourteenth century, it is less useful to show that a court was gaining or losing power, relative to the king's court, than it is to determine "the degree to which the various courts in England were bound together into a legal system." Historians should therefore focus on the process of "curial integration" (p. 297).

Through his own work on the county court, he has found that the history of this process falls into two distinct phases. Down to the reign of Edward I (1272-1307), English courts became bound together so as to constitute a real "legal system," but in the fourteenth and fifteenth centuries, "the bonds that unified the courts were soon broken or even served to isolate the courts from one another" (p. 302). In the process of "curial integration," he claims, the most powerful moving forces were not people, but legal procedures, like the ones set in motion by the viscontiel writs. Nevertheless, sheriffs and lawyers also played a significant, if unconscious, role in linking different courts together. Palmer also shows that the energy sustaining the process of curial integration did not all come from the king's

court; sometimes, the impetus for this process came from the procedures or personnel of the counties (p. 301). Palmer thus suggests that conventional ways of picturing medieval legal change need to be revised so as to take account of the fact that the king’s court at Westminster was not the only dynamic element in medieval legal culture.

Palmer’s book is valuable and important for several reasons. It presents some novel substantive findings about medieval county courts and proposes some interesting new ways of approaching the history of medieval judicial organization. It also identifies many different forces that shaped the legal history of the county court: royal efforts to control local life; the political appetites of local magnates; the bureaucratic skills, financial interests, legal obligations and political ties of county officials; the professional skills, career patterns and political interests of lawyers; and a commitment to impartial justice. While he never really explains precisely why these different forces combined to produce curial integration in one segment of the middle ages and curial disintegration in another, he successfully shows how these forces, down to about 1270, served to make English judicial organization more unified. He is less successful, I think, in his efforts to show that these forces — rather like the ones represented in pluralist models of democratic politics — either neutralized each other or else worked harmoniously together so as to create courts that would generally render “fair” and “just” decisions.

Perhaps the clearest reason why this part of his argument is unconvincing is that he does not look seriously at contemporary statements about how unfair or unjust the English courts could be. To support his contention that the king’s court was impartial, he quotes, without comment or qualification, Glanvill’s assertion that “His Highness’s court is . . . impartial” (pp. 149-50). But after quoting a biting remark from Fleta about how seneschals exerted undue influence on local court proceedings, Palmer claims that “[i]t is doubtful, of course, that the seneschals were all that corrupt” (p. 120). Evidently, Palmer sees no point in quoting other passages in which contemporaries questioned the fairness of English courts, or queried the honesty and good intentions of lawyers, judges or local officials.

In an elaborate passage likening the king’s court (in the broad sense of the term) to hell, the late-twelfth-century satirist Walter Map had this to say about the viscountiel bureaucracy and the oaths taken by those who belonged to it:

There are sent . . . from the court those whom it styleth sheriffs, undersheriffs, beadle, whose duty it is to pry cunningly. These men leave nothing untouched, nothing untried, and, like bees, they light on flowers to draw forth some of the honey: they punish what is innocuous, but the belly goeth clear of punishment. And yet, at the outset of their office, in the presence of the highest judge, they do swear to serve faithfully and honestly God and their master, rendering to Caesar the things that are Caesar’s, to God the things that are God’s; but bribes pervert them so that they tear the fleece from the lambs, leaving the foxes unharmed, inasmuch as they win favour by their money, knowing that “giving requireth ingenuity.”

18. W. MAP, DE NUGIS CURIALUM (COURTIERS’ TRIFLES) 7 (F. Tupper & M. Ogle trans.)
Later, speaking of the royal court whose "impartiality" Glanvill lauds, Map observes:

Aye, the king in this court is like the husband who is the last to know the sin of his wife. The courtiers slyly [sic] send him forth to sport among birds and hounds to prevent his seeing what they do within during his absence. While they make him take his sport, they themselves are busy with serious matters; they sit upon their tribunals and bring both righteousness and unrighteousness to the same judgment. When, however, the king returneth from his hunting or his hawking, he showeth his spoils to them and giveth them a share; they do not disclose theirs to him.\(^1\)

Perhaps these pointed remarks by a skilled satirist should be treated by legal historians as inadmissible evidence. But here are some remarks on local justice by the author of an early twelfth-century legal treatise that Palmer uses from time to time.\(^2\) In a passage with direct bearing on the issues most central to Palmer's argument, the author of the *Leges Henrici Primi* observed of the county court in the reign of Henry I (1100-1135):

"The laws of the counties themselves differ very often from shire to shire, according as the rapacity and the evil and hateful practices of lawyers have introduced into the legal system more serious ways of inflicting injury."\(^2\)

After continuing in this vein, the same author concluded: "The vexations of secular legal proceedings are beset by wretched anxieties of such number and magnitude, and are enveloped in so many fraudulences, that these processes and the quite unpredictable hazard of the courts seem rather things to be avoided."\(^2\)

What are the legal historians to make of such "occasional but virulent protest[s]" (p. 55)? Palmer's method, it seems, is to dismiss them as summarily as earlier legal historians dismissed the medieval county court, or else to treat them as warily as a lawyer would treat a hostile or lunatic witness. In his generally successful and often brilliant effort to write a balanced, judicious and impeccably documented history of the medieval county court, he feels it necessary to dismiss manifestly unbalanced statements of some contemporary observers. The county court, he suggests, could not, "of course" (p. 120), have been as unfair and unjust as people like Walter Map, the author of *Fleta*, or the author of the *Leges* said it was. Perhaps not. But why not adopt a more straightforward and less dutifully judicious position and take more account of the fact that some medieval people (who knows how many?) thought that it was? Even those of us who take this position, however, should be grateful to Dr. Palmer for having done so much to illuminate the workings — fair or unfair — of the medieval county court.

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1924). This passage comes from the chapter "On Night Birds," which begins by discussing people at the royal court whom Map likens to "the owl, night-hawk, vulture, bubo, whose eyes the darkness love and hate the light."

19. *Id.* at 320.
20. *See* pp. 78-80, 113-29, 151.
22. *Id.*
This is an important book. The argument is complex, but its complexity is likely to be lost in the clarity of the exposition. Each of the pieces of the exposition is carefully crafted and full of insight, but the contribution of each piece to the structure of the overall argument is not revealed until the end, by which time many readers will not have the patience or the ability to put it all together. This reader has the patience (although he is not at all sure that he has the ability) and offers the following in the hope that those with more ability but less patience will find it a useful guide.

The book was written in the “firm conviction that at the present time there is little knowledge of why law changes when it does, or why it changes in the direction that it does, or when and why it responds to pressure” (p. vii). This may be the central problem of legal history, if not at all times, at least in our time, and many, including this reviewer, would rank it among the central problems of the academic study of law. Professor Watson looks at the problem of legal change in the context of the basic division between civil law and common law legal systems and asks how this basic division came about. He concludes that

[...] the basic differences between civil law and common law systems are explained in terms of the legal traditions themselves; that is, the differences result from legal history rather than from social, economic, or political history. Above all, the acceptance of Justinian’s Corpus juris civilis, in whole or in part, as authoritative or at least as directly highly persuasive[,] determined the future nature of civil law systems and made them so distinctive. [P. viii.]

While this conclusion is not new, Watson reaches it with a force and in a manner that this reviewer cannot recall encountering before. The method by which Watson gets to his conclusion gives the book both its originality and its complexity.

The book may be divided into two parts. The first seven chapters deal with the civil law prior to codification; the next five chapters deal with codification and its aftermath. The first part of the first chapter and the last chapter should be excluded from this basic division, since the former defines civil law and the latter offers general observations on legal change. This material is best considered after we examine the core of the book.

Within this core the order of topics is roughly chronological. We begin
with a discussion of the *Corpus juris civilis* itself (pp. 10-13), and we note (ch. 2) what Watson calls the "block effect" of Roman law, a striking metaphor for the fact that the materials in the *Corpus juris* are arranged so that it is possible to outline schemes of rules for a given area of Roman law without much reference to the other areas. Roman law then is a highly "transplantable" body of law—a body of law appropriate for study by any group of lawyers who lack a body of literature concerning their native law and who choose to study law generally. These were the conditions facing European lawyers in the central Middle Ages and may well have accounted for the peculiar attraction of the *Corpus juris civilis* for those who revived its study in the twelfth century. Study of the *Corpus juris civilis*, however, had substantial methodological consequences (ch. 3). The book was studied at the feet of teachers who did not need to be connected with practice. Their teaching acquired a distinctly international character. This international character, in turn, led to the book's dominating university legal education. The style of reasoning became one of "formal rationality." Problems were solved by cutting facts down to their bare bones and by searching for the commentator who "got it right." This in turn gave a timeless character to the statements of the problems and to their solutions. All these characteristics may be found in the case decisions which were published in the civil law countries in Europe prior to codification (ch. 4).

The study of the *Corpus juris* fixed the mode of legal thought. Examples drawn from Roman law were used to illustrate techniques of argumentation, comparisons were drawn between Roman law and local law, and writing about local law made use of Roman law categories (ch. 5). In particular, the categorization system of Justinian's *Institutes* became the way by which authors described their local legal systems. This became particularly important when the humanists in the sixteenth century showed that the medieval developments of Roman law had carried it far beyond what the classical authors had in mind (ch. 6). Natural law thinking in the seventeenth and eighteenth centuries began by breaking away from Roman law, but ultimately it was tamed and forced into Roman law categories (ch. 7).

With the discussion of modern codifications (ch. 8), Watson changes his style of argumentation, offering more explicit consideration of possible contrary arguments. The chapter begins with a recognition that political backing is necessary for any kind of codification, but argues that neither the difficulty of finding the law nor social upheaval adequately explains why modern codifications took place. What is shown, and shown quite powerfully, is that both the French and the German codes are deeply rooted in the particular legal traditions of the countries and that the important differences between them can be explained in terms of those traditions.

Watson next analyzes the effects of codification by contrasting codes with institutes as sources of law. Codification leads to even greater abstraction, to a total loss of the sense of the history, to a loss of the international character of law, and to a fixity which makes codes difficult to change. On the other hand, drafters of new codes frequently undertake comparative work (ch. 9). Roman law distinctions embedded within the codes are critical to the modern divisions of law: the public-private distinction, the development of specialized tribunals for handling administrative matters,
separation of commercial law from the law of obligations generally, even the divisions within the law of obligations (ch. 10). The sources of modern civil law are considered (ch. 11), and Watson shows that, although the role of statute differs from what it was in the precodification period, the role of the jurist and of case law is a direct descendant of what had come before.

Thus the story is told — and well told. So far as this reviewer is able to judge, the book, with a few minor exceptions, is accurate and well-presented, and it is notable for its clarity and its interest. The book is not an elementary textbook of the history of the civil law. The exposition requires too much knowledge for the beginner. As a sophisticated retelling of a well-known story, however, the book deserves high marks. Watson has done good service for the English-speaking reader who knows something of European legal history and wants to know more.

But the book was not written with exposition as its primary purpose, and whether its argument is ultimately convincing will depend on whether the definition of the problem found in the first chapter and its solution found in the last prove to be convincing. Watson has chosen to develop his argument out of a distinction between civil law systems and common law systems. That the distinction exists is generally recognized by all who work in comparative law, but there is disagreement as to exactly what the distinction is. “Civil law” turns out to be a convenient shorthand for describing any one of a number of characteristics which tend to exist in countries whose legal traditions are not derived from England. The characteristic which Watson focuses on in his first chapter is the greater influence of the Corpus juris civilis. Defining civil law in this way requires that Watson exclude the Scandinavian countries and the socialist countries from his definition of civil law, and he says very little about the countries of the Far East. My difficulty, however, is not with the fact that Watson groups the countries this way in order to make the definition of civil law more precise. The legal systems of the excluded countries are frequently recognized as quite different from the systems of central and western Europe and from those of, for example, South America, whose legal systems are derived from the central and western European legal systems. My difficulty is that Watson's definition of civil law comes perilously close to rendering his conclusion tautological. He defines civil law as a legal system in which the influence of the Corpus juris civilis is strong, and he demonstrates that the characteristics of the civil law system depend upon the influence of the Corpus juris civilis.

The conclusion is not tautological, however, because Watson's point is that the influence of the Corpus juris civilis is the cause of many of the dis-

1. This is the mandatory “nit-picking” footnote: There is no Thomas M. Green at the University of Michigan, but there is a Thomas A. Green (p. viii). Pages 82 and 160 n.34 have typesetting errors which produce noticeable stray marks on the page. On p. 129 “influential” should be so spelled. On p. 134 René David's name needs an accent mark. On p. 145 the statement that the Twelve Tables rigorously excluded administrative, constitutional and public sacral law seems odd in the light of Tables 9.1-2, 10 (is this “private” sacral law?), 11.2, 12.5. The discussion of partnership and laesio enormis on pp. 164-65 is overly compressed. In particular the effect of the Uniform Partnership Act on the so-called “aggregate theory” might have been mentioned. Page 175 would have benefited from a citation to Dawson, Effects of Inflation on Private Contracts: Germany, 1914-1924, 33 Mich. L. Rev. 171 (1934). Considering the wide range that the book covers in such a short space, the fact that I could find so little of the detail to criticize convinces me that this is a remarkably accurate piece of work.
tinctive characteristics of the legal system in those countries in which its
influence was strong. Codification is the most important variable explained
by the influence of the Corpus juris civilis. The role of the jurist, the style of
deciding cases and the authority given to him, the separation of civil from
commercial law, and separate tribunals for public and private law are other
characteristics which are seen to depend on the influence of the Corpus
juris, although the argument here is less powerful, because the phenomena
themselves are not observed, or not observed to the same degree, within all
the selected countries.

Working out the implications of all of this for the general theory of legal
development is largely left to the reader. The final chapter, in which one
might expect to find a conclusion, in fact contains a theoretical discussion,
derived from an earlier article, which operates on an abstract level and is
difficult to connect with the rest of the book. Watson clearly believes that
general political, social and economic developments do not explain why a
legal system is the way it is. The most powerful evidence for this proposi-
tion, stated a number of times although never elaborated, is the fact that
England and the Continental countries with which Watson is dealing have
gone through basically the same political, social and economic develop-
ment and yet have arrived at very different legal systems. Some, of course, would
want to argue the uniqueness of the English political experience. Certainly
England had in place very early a group of institutions which allowed it to
develop its native law in a way that was comparatively free from the all-
pervasive effect of the Corpus juris that existed on the Continent, particu-
larly in the early modern period.

The difficulty with Watson's book, however, is deeper than the quibbles
which one might raise about the comparison that he draws. The difficulty is
that his evidence, his methodology and his theory are never juxtaposed in
such a way as to allow one to see how they all fit together. There are a
number of methodological suggestions in the book, but they are never
treated systematically. I find myself attracted both to the suggestion that
historical explanations must be judged on the basis of their plausibility, not
their inevitability (pp. 96-97), and to Watson's use of Ockham's razor (pp.
186-87), which suggests in the context of legal history that explanations in-
ternal to the legal system are to be preferred to explanations which require
the use of forces external to the system. But it is difficult to see from the
way Watson tells his story whether these methodological principles are to
be regarded as always at work or only at work in those particular sections
where he recognizes them, and both principles are sufficiently problematical
as to require more justification than they receive.

Perhaps more problematical still is the final, theoretical chapter, which
presents a complex general model of legal change and which, at least in its
emphasis on forces for and against change, seems to undercut much of what
is said in the book. If it is true, as the model seems to suggest, that basic
changes are, at least in part, the result of "pressure forces," should we not
have been told more about those forces at the points in the book where
major changes are discussed? The chapter on codification, as we noted,
concedes that political support must exist for codification, but the analysis
of what those forces were in the examples considered is scattered. The con-
sideration of the political forces supporting codification is not treated sys-
tematically; the influence of the *Corpus juris* is. Thus, the influence of the *Corpus juris* is made to seem more powerful than that of the various political forces.

Finally, and perhaps most importantly, many common law readers will question Watson's focus on formal legal rules and their organizing schemes. To argue that these elements in a legal system owe a great deal to what went before in the intellectual tradition is almost too easy. Many of us who have argued that there is a stronger relationship between law and society than Watson sees do so on the basis of a realist emphasis on the decisions in actual cases. Watson does not offer us much systematic discussion of decisions in actual cases; indeed, he offers us relatively little in the way of middle-level rules. The story he tells is a fascinating one, but in the end this reader was left unsatisfied because of Watson's failure to address this question: Does this difference really make a difference?

The systematic study of legal change is, as Watson notes, in its infancy. The great German legal historians, Koschaker and Wieacker, who deal with many of the same topics that Watson covers, do not consider the theoretical issues that Watson raises. Watson's book, then, is path-breaking, and a new path is not always easy to follow. I, for one, hope that he returns to the issues he has raised in this book.
Peter H. Irons' *The New Deal Lawyers* recounts the struggle of three attorneys — Donald Richberg of the National Recovery Administration (NRA), Jerome Frank of the Agricultural Adjustment Administration (AAA), and Charles Fahy of the National Labor Relations Board (NLRB) — to defend New Deal agencies against the hostility of a politically and constitutionally conservative federal judiciary. Ultimately, the Supreme Court struck down the NRA\(^1\) and the AAA\(^2\) but upheld the NLRB\(^3\). That agency's survival is generally credited to the "switch in time that saved nine"; the Court, threatened by Roosevelt's court-packing scheme, responded with politically motivated hospitality toward the NLRB. *The New Deal Lawyers* suggests an additional reason for the success of the NLRB — the superior legal "style" of its chief attorney.

Irons contends that the distinctive traits of the agencies' chief attorneys were reflected in their enforcement programs. Irons characterizes Richberg of the NRA as a "Legal Politician," an ambitious and politically astute man who "parlayed his relationship with Roosevelt into an unofficial post as 'Assistant President'" (p. 29); Frank of the AAA as a "Legal Reformer," a legal realist and self-described reformer (p. 120); and Fahy of the NLRB as a "Legal Craftsman," a lawyer who saw his job not as making social policy, but as enforcing a statute "through the presentation of carefully selected cases in the courts, with meticulous attention to detail and the formulation of narrowly drawn issues the keys to success" (p. 235). Richberg preferred political influence to litigation, while Frank concentrated on negotiation. Neither began his administration with a carefully developed litigation strategy to test his agency's power before the Supreme Court. Only after becoming embroiled in enforcement suits did they realize the value of such planning. By then, Irons observes, they had lost much control over the timing and choice of cases. Fahy, by contrast, at the outset constructed a litigation course to force an early Supreme Court test on a strong case for the NLRB. His meticulous planning paid off, as he presented to a receptive Court a package of four viable cases headed by *NLRB v. Jones & Laughlin Steel Corp.*\(^4\).

Fahy had the additional advantages of relative freedom from interference by both the head of his agency and the Justice Department. Warren Madden, the chairman of the NLRB, approved of Fahy's litigation scheme and allowed him considerable autonomy. Richberg and his superior, Hugh Johnson, usually agreed on most policy issues, but vied for control of the NRA and for influence at the White House, which crippled the agency's effectiveness. Jerome Frank and the chief of the AAA, George Peek,

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4. 301 U.S. 1 (1937).
clashed violently — personally and professionally — from the day Frank was appointed as general counsel until Peek was forced out. Unfortunately, while Frank's personal relations with Peek's successor were far more cordial, serious policy differences divided them.

"The relentless bureaucratic imperialism of the Justice Department" further diminished Richberg's and Frank's power over their agencies' enforcement and litigation policies (p. 11). Anxious to dominate government litigation, the Justice Department, with Roosevelt's acquiescence, wrested control over all litigation from the NRA and AAA. Consequently, Richberg and Frank were forced to defer to the Justice Department's decisions regarding which cases were appealed and what arguments were made. The Department's distaste for litigation and ignorance of the subject ensured disaster. Fahy, however, avoided this pitfall since the Wagner Act placed primary control of litigation with the NLRB, allowing the agency's lawyers to represent the Board in any court.

Fahy did not escape the hostility of the federal judiciary, however. All three general counsels had to defend the constitutionality of New Deal legislation before a bench composed predominately of elderly, conservative, and partisan Republican judges. Most of the courts were un receptive; some were outright incompetent. Irons relates Fahy's disheartening exchange with one octogenarian judge. Informed that the constitutional issues in the case centered around the Commerce Clause, the judge sent for a copy of the Constitution, thumbed through it, and inquired of Fahy: "Does this case involve Indian tribes?" No, the puzzled Fahy answered. "Does it involve trade with foreign nations?" No, again. "Then it must be commerce between the states," [the judge] concluded triumphantly (p. 256)."

The ultimate test for Richberg, Frank and Fahy was, of course, in 1936 when Jones & Laughlin was heard before the Supreme Court. Irons acknowledges that the Court was ripe for a case which would permit it to uphold New Deal legislation on a new, broad reading of constitutional grants of power to Congress. But he proposes that Fahy's victory was as much the product of craftsmanship and control of the NLRB's enforcement program as of fortuitous timing. Conversely, he implies that Richberg's and Frank's lack of planning, internecine feuds with their agency heads, and territorial contests with the Justice Department would have seriously undermined their chances for success even if they had faced a less intractable Supreme Court majority.

Irons's use of the "style" of influential people to explain history derives from James David Barber's studies of American presidents. The approach sharpens his comparison among the three general counsels and organizes his highly detailed discussions of the personalities and politics of the era.

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5. Fahy was appearing before Chief Judge Buffington, age 81, in NLRB v. Pennsylvania Greyhound Lines, 91 F.2d 178 (3d Cir. 1937), rev'd, 303 U.S. 261 (1938).
6. Irons notes that Judge Buffington remained on the bench until 1958, when he was 103. P. 327 n. 11.
This clarity, though, comes at the cost of artificially confining engrossing, complicated characters within rigid categories. The reader is left wishing for a fuller description of the personalities of Richberg, Frank, and Fahy.

Only two previous works have undertaken comparable analyses of the roles played by individual lawyers in shaping momentous constitutional litigation. Benjamin R. Twiss, in *Lawyers and the Constitution*,8 examined the attorneys who argued for a jurisprudence based on states-rights federalism and laissez-faire economics. Richard Kluger's *Simple Justice*,9 investigated the lawyers who represented the cause of racial equality. The constitutional revolution of the New Deal has been exhaustively studied from the perspectives of the Court, the President, and Congress; The New Deal Lawyers' emphasis on the role of these three attorneys makes Irons' book a welcome contribution to this too limited body of literature.

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Throughout a lifetime that included a career as a private lawyer, involvement in national and international political issues and a twenty-three year tenure on the Supreme Court, Louis Brandeis was constantly active in reform. In *Louis D. Brandeis and the Progressive Tradition*, Melvin Urofsky attempts to demonstrate an essential consistency in Brandeis' philosophy and his approach to reform throughout those years. Urofsky does not undertake an exhaustive history of Brandeis' reform activities. Instead, Urofsky adopts a thematic approach, identifying various strains of thought underlying Brandeisian reform and documenting the recurrence of those ideas in each phase of his career. This thematic approach results in an oversimplification of Brandeis' philosophy and reform methods. Though adequate as an introduction to Brandeis and his beliefs, this approach is likely to disappoint readers seeking a more comprehensive treatment of the subject.

According to Urofsky, Brandeis' political and social philosophy reflected that of the progressive movement. Brandeis, contrary to his critics' allegations, did not desire radical change in American society (p. 18). In fact, Brandeis was committed to the free enterprise system in which he and his family had prospered. But Brandeis recognized defects in the market system, most of which were caused, he believed, by industrialization. Brandeis saw large industry as a corrupting force that degraded the laborers responsible for its success and inhibited the economic freedom of the common man. Brandeis sought a return to an earlier America, when businesses were small and hard work and merit ensured advancement. In this attempt to return to the past, Brandeis and the progressives were fundamentally conservative, not radical (pp. 18-21). In Urofsky's view, Brandeis consistently adhered to this conservative philosophy from his childhood until his death.

Urofsky's conception of Brandeis' philosophy has been criticized as simplistic.¹ By portraying Brandeis as committed to certain "immutable values" (p. 1) cherished by the progressives, Urofsky discounts the possibility of growth and development in Brandeis' outlook. On the contrary, it seems clear that development did occur. Allon Gal points out Brandeis' change from mugwump to progressive, and notes that Urofsky ignores this transition completely.² Urofsky himself notes that Brandeis "imbibed" the values of Bostonians after moving to that city (p. 4) and changed his position on several issues, particularly Zionism (p. 90).

These changes, moreover, were the natural corollary of Brandeis' world view. His orientation toward facts made this growth in philosophy inevitable. As Urofsky emphasizes, Brandeis believed that the law should grow out of the facts of each situation; indeed, Brandeis' stands on issues were so

2. Id.
fact-specific that they were often considered inconsistent with each other.³ It is difficult to imagine a man committed to learning from the particular facts of a situation clinging to an unchanging set of values for an entire lifetime, particularly in light of the many different social, economic and political circumstances Brandeis experienced. In addition to being inaccurate, this simplification limits Urofsky's analysis. By insisting that Brandeis subscribed consistently to the progressive philosophy, Urofsky fails to illuminate Brandeis' attitudes toward issues outside the range of progressive ideology. As Gal points out, Urofsky's analysis sheds no light on Brandeis' attitude toward the New Deal and the issues it raised because such issues were never considered during the life of the progressive movement.⁴

Within the context of this static progressive philosophy, Urofsky emphasizes various methods common to Brandeisian reform. As previously noted, one of the most widely recognized Brandeisian characteristics was an emphasis on facts. Brandeis believed that all law must be firmly based on the facts of particular situations; law divorced from the facts was essentially dead (p. 50). Urofsky presents examples of Brandeis' appetite for facts from all phases of his career. As a private lawyer in Boston, Brandeis made an effort to know the facts of his client's business as well as, if not better than, the client. He felt that in order to serve as an effective counselor, a lawyer had to impress clients with "superior knowledge" and "know [clients'] affairs better than [do] they" (p. 7). As the "[p]eople's [a]ttorney" (p. 47) in Muller v. Oregon,⁵ Brandeis succeeded in persuading the Supreme Court to uphold an Oregon statute prohibiting workdays of longer than ten hours for women. Of his one-hundred page brief, only two pages were devoted to legal citations; the remainder was devoted to an exhaustive presentation of statistics concerning women and the effects of long workdays (pp. 51-53). Finally, as a Supreme Court Justice, Brandeis advocated and attempted to apply a "living law." In his second dissent from a decision by the Court, Brandeis stated that "the judgment should be based upon a consideration of relevant facts, actual or possible — ex factor jus oritur [the law arises out of the fact]. That ancient rule must prevail in order that we may have a system of living law."⁶ In illustrating Brandeis' use of facts, Urofsky is neither profound nor original. Nevertheless, his point is made clearly and effectively.

Another characteristic of Brandeisian reform was the offering of a remedy for every evil attacked. This practice distinguished Brandeis from other progressive reformers of his day, many of whom showed no capacity for devising realistic solutions. Again Urofsky uses several examples to illustrate his point. Brandeis proposed the creation of savings bank insurance as an alternative to the corrupt industrial workers' life insurance policies (pp. 31-39); he introduced sliding scale rates to the public utility industry in Boston to combat inefficiency and high rates (pp. 26-30); and he suggested the institution of a "preferential union shop" and a protocol to handle labor grievances as a solution to the major conflicts in the New York garment

⁴. See Gal, supra note 1, at 708-09.
⁵. 208 U.S. 412 (1908).
⁶. Adams v. Tanner, 244 U.S. 590, 600 (1917).
industry (pp. 62-67). Also indicative of Brandeis’ impulse to provide reme-
dies was his frequent suggestion to clients that he be “counsel to the situa-
tion” rather than advocate of one side (p. 12). Lacking ties to one side of a
conflict, Brandeis was free to search out lasting solutions. To Urofsky,
Brandeis always undertook reform in a comprehensive manner. To attack
an evil without providing a remedy would have been inadequate.

Recently, the most widely publicized aspect of Brandeisian reform has
been the use of surrogates.7 Brandeis began to use surrogates when his per-
sonal secretary, Alice Grady, became Deputy Commissioner of the savings
bank insurance system. Grady provided a constant link to the system
through which Brandeis could monitor its progress and make suggestions
for its improvement (p. 39). But the use of surrogates did not flourish until
Brandeis became a Supreme Court Justice and propriety forced him to take
a less active role in reform activities. At this time, according to Urofsky,
Felix Frankfurter was “undoubtedly [Brandeis’] most important surrogate”
(p. 155). Brandeis gave Frankfurter a salary to support his reform activities;
in return Frankfurter served “as a conduit for funneling information from a
variety of reform groups to Brandeis, advising him of their problems and
progress and in turn conveying his advice” (p. 156).

Frankfurter was not Brandeis’ only surrogate during his Supreme Court
years. Brandeis turned to his family in his efforts to improve the University
of Louisville Law School (p. 148) and relied on Zionist allies to orchestrate
and accomplish the ouster of the corrupt Lipsky Administration of the Zi-
onist Organization of America (pp. 152-56). Urofsky does not discuss the
ethical problems arising from this use of surrogates by a sitting Supreme
Court justice8 and does not undertake a detailed analysis of Brandeis’ fun-
neling of funds and advice through such lieutenants. In the context of
Urofsky’s thematic examination of Brandeisian reform, the use of surro-
gates is simply evidence of Brandeis’ continuing involvement in reform ac-
tivities even when he could no longer do so openly.

This thematic approach enables Urofsky to provide a coherent view of a
complicated life. Ultimately, however, Urofsky’s approach is disap-
pointing. In detailing these themes, Urofsky omits details and inconsisten-
ties and thus fails to provide a complete picture of Brandeis’ reform. For
instance, in emphasizing Brandeis’ ability to find consistently “reasonable”
and “workable” solutions (p. 15), Urofsky quickly passes over Brandeisian
reforms that failed. Rather than analyze these failures and Brandeis’ reac-
tion to them, Urofsky either blames the failure on others or states, without
much further explanation, that the reform did not succeed. As a result,
Urofsky neglects a significant part of Brandeis’ development as a reformer.
Similarly, Urofsky states that Brandeis experienced difficulties in making
the transition from progressive advocate to impartial judge. Urofsky states
that Brandeis made “several mistakes” and occasionally “violated his own
canons of conduct” (p. 122), but the book does not examine those mistakes.
Brandeis’ errors in this transitional period, like his failures in reform, could
reveal much about the nature of the man. In his insistence on showing

7. See B. Murphy, The Brandeis/Frankfurter Connection (1982) (reviewed in this
volume).
consistency and success in Brandeis' reform, Urofsky sacrifices a more complete understanding of the Justice's complex character.

Nevertheless, Urofsky's portrait of Brandeisian reform is a useful addition to the literature, albeit in a limited way. Other books are more detailed,\(^9\) providing fuller portraits of Brandeis and his involvement in reform. These books are also of more use as research tools since assertions and quotations are documented; Urofsky does not use footnotes and provides only a brief note on sources. Detailed books, however, are not necessarily satisfactory introductions. *Louis D. Brandeis and the Progressive Tradition* can fill this role. Urofsky's simplification, a liability for one seeking a complete understanding of Brandeis, is not necessarily detrimental in an introduction. Urofsky presents a clear, concise and generally accurate view of Brandeis, uncluttered by inconsistencies and extensive detail.\(^10\) Urofsky's book should suffice for the reader seeking an overview of Brandeisian reform.\(^11\)

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