voracious reader and amply enjoyed the library privileges of his association with Harvard University. Lippmann's bookishness and elitism have earned him the title of 'public philosopher,' and that moniker is apt also because he spoke of his work as the work of "public reason," a phrase that was dear to Enlightenment philosophers. Following this ideal, Lippmann understood the journalist's role to be like that of the scholar in search of truth emancipated from authority, prejudice, and interest. The public space was not a marketplace of ideas and not an arena for the contest of passions; it was a space in which to inscribe the work of informed argument.

Goodwin anoints Lippmann as a "public economist" and rightly notes that there has been none like him. It may be that Goodwin intends us to treat Lippmann as an economist. If that is so, the record of his originality is not compelling. I would endorse the alternative that Lippmann is a public economist in the sense of pursuing "public reason" on matters of economic policy. Lippmann did not believe that the public, on its own and even with his aid, could have the knowledge and discipline to govern. Lippmann was not a "persuader" in the style of his friend Keynes, or Milton Friedman or Paul Krugman, seeking to mobilize a popular outcry. Lippmann was not an "explainer" in the style of Leonard Silk or David Warsh, soliciting deference to the work of experts. Matters of economic policy required, for Lippmann, a higher court than the testimony of credentialed experts or an assembly of newspaper readers. Only the use of reason—vivid, synthetic, and conclusive—was fit to sit in judgment of civic matters. There never will be another Lippmann, because this plausible and old-fashioned ideal is ill fitted to a public culture that is bitterly polarized and cynical. Public intellectuals are not dead, but they are no longer men of reason.

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Herbert Hovenkamp, *The Opening of American Law: Neoclassical Legal Thought, 1870–1970* (Oxford and New York: Oxford University Press, 2015), pp. 460, $53 (hardcover). ISBN 978-0-19933-130-7.

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The author of this book requires little presentation. Professor Herbert Hovenkamp is a leading authority on American law history and is among the most acclaimed experts on antitrust law and economics. Above all, he is a rare—indeed, almost unique—example of a legal scholar who has always paid attention to the history of economic thought and to the role played by economic ideas in shaping American law on

business-related matters. His 1991 volume, *Enterprise and American Law, 1836–1937,* is a must-read reference on the relationship between classical political economy and nineteenth-century corporate, regulatory, and antitrust law. The present book starts where that one ended—not chronologically (in this sense, a substantial overlap exists between them), but conceptually. While the previous work focused on classical economics, *The Opening of American Law* deals with neoclassical analysis and explores the impact of marginalism on American legal thought.

Hovenkamp's main thesis is that starting from the end of the nineteenth century, American law was drastically modified by two intellectual revolutions: the theory of evolution by natural selection, and the marginalist theory of rational economic behavior. Both theories, readers are told, triggered an array of novel interpretations and applications of the law that changed forever the nature of the relationship among the market, the government, and the law itself. The new theories, Hovenkamp adds, could encompass alternative approaches, including radical views on both the right and the left of the political spectrum. Darwinism paved the way to the extreme laissez-faire of the supporters of the "survival of the fittest" principle as well as to the highly interventionist penchant of progressive reformers; marginalism showed that market failures were ubiquitous and that ample room existed for efficiency-enhancing redistributive policies, but it could also be used to demonstrate the theoretical superiority of the free market as an allocative system. With his usual display of legal scholarship and a masterful handling of the relevant case law, Hovenkamp details how these twin revolutions affected American law in fields as diverse as taxation, risk management and insurance, corporate governance and finance, IPR law, labor policy, railroad regulation, and, of course, antitrust law. He also claims that the differential ability of various interest groups (social reformers, trade unions, corporations, etc.) to avail themselves of the new theories has been crucial in determining the law's direction in each of those very fields. In this sense, Darwinism and marginalism have been instrumental since the 1880s to leading American law towards directions that were totally unexpected only a decade earlier. The bottom line is clear: according to Hovenkamp, the transition from the so-called classical legal thought of the late nineteenth century to the modern, so-called progressive approach of the first half of the last century has been largely due to the combined action of the two revolutions—the marginalist one being, for several reasons, that with the most long-lasting effects.

The thesis is fascinating. Even more importantly, it is extremely pleasant for us, historians of economics, in that it grants high visibility to our field, which, in Hovenkamp's reconstruction, inevitably rises to the status of fundamental ingredient for any proper understanding of twentieth-century American law. It would escape no HET scholar perusing *The Opening of American Law* that the book opens remarkable opportunities—indeed, almost invites us—to join forces with law historians and pursue the largely unexplored path of the peculiar influence of economic ideas on American legal minds. And not just for building a sort of historically oriented branch of law and economics, but as an exciting enterprise capable of demonstrating our discipline's power to cast new light on any field of social interaction where economic ideas have had a role—which means practically everywhere. Readers of Hovenkamp's volume would find scores of examples where this research method is applied at its best, with copious fruits in terms of explanatory insights on the economic how and why of so many facets of American law.

Having thus praised the author's method, this reviewer cannot fail to notice a major flaw in the specific application offered in the present book. Indeed, it is a flaw that, I regret to say, undermines no less than Hovenkamp's main thesis. Briefly said, it is simply false that the neoclassical revolution in economics had any significant impact on American law until at least the 1920s. This, for the very simple reason that, as historians of American economics know all too well, most US economists of the Gilded Age and Progressive Era were not themselves neoclassicals, and, for different reasons, vehemently opposed this very approach; so it is very unlikely that an approach that was still disputed and minoritarian on its home turf could have been so strong and established and recognized to sensibly affect—indeed, *revolutionize*, because this is what Hovenkamp actually claims—a nearby field such as the law. To put it bluntly: Where could a judge of the Gilded Age have possibly learned his marginalist economics, given that the approach was seldom taught at the time, let alone when that judge was reading law as young man?

Alas, what makes the book disappointing from this reader's viewpoint is that the author himself—so well read in HET as he undoubtedly is—often gives the impression of being perfectly aware of that bug in his thesis. Despite the assertion that marginalist economics triggered the transformation of American law as early as the late nineteenth century, time and again readers are informed that in each of the specific fields scrutinized, the classical heritage was still alive and kicking, and that the real turnaround came only much, much later: no earlier than 1920 in some circumstances, between the two world wars in the majority of the cases, and as late as the 1950s or 1960s in the remaining ones. In other words, as soon as the analysis truly penetrates the details of a single area of the law—and, of course, Hovenkamp is as masterful as ever at doing that—the simplistic picture, according to which by 1890 everyone in the US had magically turned neoclassical, breaks into pieces, revealing a reality of conflict, uncertainty, and, above all, enduring confidence in the eternal validity of classical doctrines. A bit provocatively, it might even be said that the whole book is an excellent, fact-laden, truly illuminating *confutation* of (a major component of) its main thesis. Marginalist economics, readers may well conclude, did *not* really influence American legal thought until—to give an approximate median date—1930. Until then, US jurists were still by and large under the influence of classical political economy.

Mine, it should be noted, is not mere historical quibble about an impalpable issue such as "when did an idea started being influential?" The point is, on the contrary, very substantial for at least two reasons. First, because by so late a date as 1930, the influence of Darwinism (be it Social or Reform) had largely vanished. This undermines Hovenkamp's claim about the *combined* action of the two revolutions: when the first was exploding, the second one was negligible, and vice versa. Again, the author is the first to deny substance to his own argument in that Darwinism all but disappears after Part I of the book—and inevitably so, given that Hovenkamp constantly reminds us that the real neoclassical breakthrough in American law came only after (sometimes, *long* after) WWI.

The second reason is, if possible, even more serious. It is understandable from the viewpoint of a law historian to take as the true turning point in American law the constitutional revolution of the 1930s, and therefore to consider the 1890s not so different from the 1920s, in that both decades, and those in between, fall under the traditional headlines of "laissez faire constitutionalism," "formalist legal thought," or "*Lochner* era"

(the book aiming *inter alia* to undermine these stale historical labels by showing that, due to Darwinism and marginalism, none of them makes real sense). However, this is not so for the history of US economics, for which these very decades were absolutely pivotal. In HET-speaking, 1889 is a galaxy apart from 1919 or 1939. In between these landmark years falls the entire "struggle for the soul of economics," so beautifully described by, say, Malcolm Rutherford, Yuval Yonay, Tim Leonard, Luca Fiorito, Charles McCann Jr.—to name just a few of the most recent contributors. That struggle, involved the different approaches that animated the pluralist landscape of American economics during those decades. The details are too well known to JHET readers to deserve repetition. Suffice here to remember that the American Economic Association was founded in 1885 *against* the mainstream economists of the time (a strange occurrence were it true that everyone was at the time already neoclassical!); that when the debate on the minimum wage exploded in 1910, only a small minority of the participants (economists, social scientists, jurists) made use of neoclassical marginal productivity theory, while most of them referred to some informal notion of "living wage"; that economics, including neoclassical economics, did not become "formalist" until the late 1930s (so it could hardly overlap with so-called legal formalism, which, by so late a date, had been buried for good); and that the use of mathematics (including marginalist ideas) did *not* represent a distinctive trait of any single school in American economics, in that one thing were the (occasional) tools used by economists, but another were the policy prescriptions that those very tools might at best uphold, but never determine.

A more correct appreciation of the history of US economics invalidates Hovenkamp's general thesis. Leaving aside Darwinism, the reality is that neoclassical economics did *not* lay behind the dramatic transformation of American law he so effectively describes. To make just one example: How can the legal doctrine of substantive due process (the Holy Grail for all laissez-faire jurists of the Gilded Age and Progressive Era) be represented as a deliberate, though vain, "effort to hold off the marginalist revolution that was infecting many aspects of legal thought" (p. 9)? The doctrine dates back to, at the latest, the 1890s, and was enshrined by the Supreme Court in the famous *Lochner vs New York* decision of 1905. By that time marginalism was still struggling to affirm itself within American economics. Claiming that it was already so powerful to affect American constitutional law is simply against historical evidence.

The best piece of evidence for the weakness of Hovenkamp's thesis is the fact that, until after WWII, never in the case law he refers to did courts make recourse to the true epitome of the neoclassical approach to legal matters: cost-benefit analysis. Comparing the costs and the benefits of a law—say, a police power intervention—is the smoking gun, the acid test for establishing whether the economics underlying a court's decision is classical or neoclassical. Classical analysis admitted only of government interferences of a win–win kind: i.e., those bringing gains to all market participants without infringing anyone's rights. Beyond them, only a well-specified list of admissible interferences existed (Adam Smith's famous duties of the sovereign), or, better, a well-specified boundary of sacred individual rights no law or conduct could ever trespass. In legal jargon, classical economics was tantamount to a categorical, yes-or-no approach to adjudication. Neoclassical analysis admits that no such well-defined categories exist, so much so that any law, regulation, or behavior must be assessed in terms of the costs and the benefits it brings. This, in legal jargon, is the essence of a judgment by

degree, where no rule is per se lawful or unlawful. The latter approach, as Hovenkamp rightly observes, was pioneered in the 1880s and 1890s by progressive hero Oliver Wendell Holmes. But the fact that a legal giant like Holmes probably took the idea from the early marginalist economics of his time does not authorize us to conclude that the latter had any significant influence on American courts—not, at least, for a very long time. Indeed, as Hovenkamp's book clearly shows, judges and justices of the Gilded Age and Progressive Era never employed cost-benefit analysis, and continued to follow categorical reasoning, even in those areas of the law where the other approach would seem more natural—like, for example, in the case of the so-called rule of reason of antitrust law, which the *Standard Oil* Court of 1911 still presented in fully categorical terms.

Despite its relevance for neoclassical thought, Hovenkamp hardly mentions cost-benefit analysis throughout the book. Indeed, he builds a lot on the idea that the key notion of neoclassical economics is the rational, forward-looking, expectations-driven economic agent. While this may be a fair synthesis of the post-WWII neoclassical representation of the economic agent, it is totally ahistorical when applied to the marginalist economics at the turn of the twentieth century. The HET scholarship has amply demonstrated that the forward-looking character of business behavior was a peculiar trait of the *classical* entrepreneur, one that was entirely lost with the advent of the overly narrow view of entrepreneurial activity typical of the earliest neoclassical generations. Progress in mathematical techniques eventually allowed economists to bring uncertainty and expectations back into their analysis, but this happened much later. As a confirmation, just look at the most significant instance of early courtroom application of "neoclassical" notions of forward-looking value and uncertainty: viz., the jurisprudence on rate regulation. The fact is that the Supreme Court proclaimed the key doctrines of present value and reproduction cost in the landmark *Smyth vs Ames* case (1898) *without* any help from economic theory, but borrowing exclusively from established accounting and engineering techniques. How to reconcile the new doctrines with the still largely classical economics underlying the Court's regulatory jurisprudence was a puzzle that kept occupied some of the best economic and legal minds of America until the 1930s!

In the end, a question naturally arises: Should HET scholars read this book? Despite my previous concerns, the answer is undoubtedly positive. Even setting aside the role neoclassical economics surely had in later, post-WWII jurisprudence (Hovenkamp is totally right about those decades, but it is to be hoped that he will dig deeper in future works), and even leaving behind the usual display of scholarship and generous provision of insightful analysis characterizing here, like always, Hovenkamp's research, the main reason why I found *The Opening of American Law* fascinating and worth reading is that it clearly demonstrates the strength and resilience of *classical* political economy in affecting American law, even long after the approach had been challenged, and eventually displaced, by alternative theoretical paradigms. Unintended as it might have been, showing that the forty years bracketing 1900 were still the time of classical economics in American law opens the door to multiple research questions that HET practitioners, possibly in cooperation with law historians, should endeavor to answer.

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