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Quantitative Legal History

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# Quantitative Legal History

Daniel M. Klerman

## **Abstract**

Legal historians seldom use statistics, but this is a missed opportunity. Quantitative methods are particularly helpful in understand core legal history issues, including the effect of legal change and the influence of multiple factors on legislation, judicial decisionmaking, and citizen behavior. Recent work by Gavin Wright, Paul Mahoney, and Michele Landis Dauber shows how tables, graphs, and regression analysis can be woven into persuasive historical narrative and analysis. Collaboration between legal historians and quantitative social scientists also provides an untapped avenue to enrich the field.

Quantitative Legal History  
For *Oxford Handbook of Historical Legal Research*  
Daniel Klerman\*

Abstract

Legal historians seldom use statistics, but this is a missed opportunity. Quantitative methods are particularly helpful in understand core legal history issues, including the effect of legal change and the influence of multiple factors on legislation, judicial decisionmaking, and citizen behavior. Recent work by Gavin Wright, Paul Mahoney, and Michele Landis Dauber shows how tables, graphs, and regression analysis can be woven into persuasive historical narrative and analysis. Collaboration between legal historians and quantitative social scientists also provides an untapped avenue to enrich the field.

Quantitative legal history is in a rather sorry state. Only about a quarter of recent works of legal history use even simple quantitative methods (such as tables or graphs), and articles or books with more sophisticated methods, such as regression analysis, are extremely rare. The limited use of quantitative methods in legal history reflects, in part, the marginal place of numbers in scholarship produced by historians. On the other hand, given the increasing prominence of empirical work in law more generally, and given the emergence of new techniques for the quantitative analysis of texts, the infrequent use of statistical methods is surprising.

The infrequent use of quantitative techniques is also a missed opportunity. Scholars from other fields, including economics, sociology, and political science, are using statistics to analyze legal history. Such analysis is particularly helpful in understanding the effect of legal change and in analyzing the influence of multiple factors on legislation, judicial decisionmaking, or citizen behavior. While it is wonderful that people from other disciplines are using statistics to analyze legal history, legal historians might do it better, given their penchant for archival work and their superior knowledge of historical sources, context, and theory.

Legal historians could learn basic statistical analysis in graduate school or on their own. Or they could fruitfully collaborate with statisticians or social scientists with quantitative expertise. While co-authorship is relatively common in the social sciences, it is surprisingly rare in legal history. Collaboration between legal historians and quantitatively sophisticated social scientists presents a terrific opportunity for interdisciplinary work that could enrich the field.

This chapter first assesses quantitatively the use of quantitative methods in legal history. (Section I). It then discusses a few examples of the successful use of numbers and statistics in recent books addressing legal historical topics. (Section II). Section III discusses the future of quantitative legal history.

## I. A Quantitative Analysis of the Use of Quantitative Methods in Legal History

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As is fitting in a chapter on quantitative methods, this section attempts to document numerically the extent to which quantitative methods are, in fact, used in recent works of legal history. That is not a trivial task, because the field of legal history is not well defined and the range of techniques that could be considered quantitative is vast. Table 1 below takes one approach, although, as with all quantitative analysis, other approaches are possible.

Table 1. Quantitative Analysis in Recent Works of Legal History

	Articles published in 2016		Books reviewed in 2016		Prize-winning articles, 1988-2015		Prize-winning books, 1980-2016	
	Number	%	Number	%	Number	%	Number	%
Any quantitative tables or graphs	10	14%	14	26%	15	22%	20	38%
Two-way tables	3	4	14	26	13	19	15	28
Regressions	0	0	1	2	0	0	1	2
All (including books or articles without tables, graphs or regressions)	71		54		67		53	

Table 1 examines four categories of scholarship: articles published in legal history journals in 2016, books reviewed in legal history journals in 2016, prize-winning articles, and prize-winning books. Even these four categories, of course, required further choices as to which journals and which prizes. The articles examined were those published in the main English-language legal history journals: *Law and History Review*, the *American Journal of Legal History*, the *Journal of Legal History*, and *Law and History*. The first two are published in the United States, the *Journal of Legal History* is published in England, and *Law and History* is the journal of the Australia and New Zealand Law and History Society. The books examined were those reviewed in these same four journals. The prizes are the Hurst Prize, awarded by the Law and Society Association for the best work in “socio-legal” history, and four prizes awarded by the American Society for Legal History – the Surrency Prize (best article in *Law and History Review*), Sutherland Prize (best article on English legal history), the Cromwell Article Prize (best article in American legal history published by an early career scholar), the John Phillip Reid Prize (best monograph on Anglo-American legal history by a mid-career or senior scholar), and the Cromwell Book Prize (best book in American legal history by a junior scholar).

About fourteen percent of articles published in 2016 and twenty-six percent of books reviewed in 2016 had quantitative tables or graphs. This could be seen as reasonably wide usage. The greater use of numbers in books may simply reflect their greater length, which allows more space for different types of analysis. Prize winning articles and books use tables and graphs at somewhat higher rates (twenty two and thirty-eight percent), which indicates that prize committees are receptive to quantitative work. It is also possible that the difference between prize-winning works and works reviewed or published in 2016 reflects greater use of quantitative

methods before 2016. The prize data comes from a longer period (1988-2015 and 1980-2016), and there is some evidence that earlier works were more likely to use statistics, although the number of works analyzed here is insufficient to test that hypothesis rigorously. One particularly noteworthy early prize-winner was Lawrence Friedman and Robert Percival, *The Roots of Justice: Crime and Punishment in Alameda County, California, 1870-1910* (1981), which has sixty-eight quantitative tables.<sup>1</sup>

A much smaller percentage of works contain more sophisticated quantitative analysis. Only four percent of articles published in 2016 contain two-way tables, and the use of two-way tables is lower for prize-winning books and articles as well. Two-way tables are tables that analyze a single outcome with two possibly explanatory factors. For example, while a one-way table might display the number of cases per year over ten years, a two-way table might display the number of cases per year over that same time period broken down by subject matter, region, or court. Such tables allow the researcher to see the way that two factors (e.g. year and court) affected the outcome variable (cases per year). It is also notable that most of the articles with two-way tables came from a single special issue of *Law and History Review* devoted to digital humanities. If that issue were excluded, only a single article published in the four legal history journals in 2016 would have had a two-way table.

While tables can be effectively used to analyze the effect of two factors, they become more cumbersome when analyzing three or more factors. Regression analysis is the standard way of exploring the effect of multiple factors. It was used only twice in the works analyzed in Table 1. This is surprising and disappointing, as legal historians are often trying to tell complex stories about the effect of multiple factors, such as time, race, and gender. When quantitative data are available, regression analysis is an appropriate and powerful tool. It is notable that neither of the authors of the two works in Table 1 which used regression analysis have Ph.D.'s in history. One, Michele Landis Dauber, received her doctorate in sociology, and the other, Paul Mahoney, does not have a doctorate, although he is well known for his work in law and economics. As will be discussed in Section III, one reason there is so little quantitative analysis in legal history is that history graduate schools, which, over the last few decades have trained the most prominent legal historians, do not generally teach quantitative methods and have relatively few faculty who use quantitative methods. The next section will discuss Dauber's and Mahoney's books in some detail as examples of the way statistical analysis can enhance works of legal history.

## II. Examples of Quantitative Legal History

While it is relatively rare for legal historians to use quantitative techniques, there are excellent examples. This section discusses three in depth.

### 1. Gavin Wright, *Sharing the Prize* (2013)

Gavin Wright's *Sharing the Prize* provides a compelling illustration of the way that relatively simple tables and graphs can be used to shed light on one of the central questions of modern legal history – whether the civil rights laws of the mid-1960s worked. He focuses, in particular, on the South, arguing that the Civil Rights Act of 1964 and the Voting Rights Act of 1965 had

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<sup>1</sup> The list of Tables on pp. ix-xiii mentions 71 tables, but three of them were not quantitative. For example, two of them listed judges, their terms, and their dates of birth.

their biggest impact on this region, and that it is best to analyze their effects on a regional basis. The civil rights acts, of course, also affected the north, but Wright argues that, given regional differences, it is helpful to focus on the south. Prior studies, he argues, which analyzed the entire U.S., provide a less clear picture of the success of the civil right revolution, because African-Americans started from a higher baseline in the north, and because controversies over busing and urban unrest led to different political and economic dynamics. Wright argues that, if one focuses on the south, the civil rights legislation of the mid-1960s, along with the litigation and civil and political mobilization it enabled, produced unambiguous economic gains for Southern blacks, without harming Southern whites.

Wright most often shows the effect of the civil rights legislation by using graphs and charts that compare some variable (e.g. per capita income or department store sales) over time. He can nearly always point to a significant positive improvement starting around 1965, shortly after enactment of the Civil Rights Act of 1964 and Voting Rights Act of 1965. Often those southern trends are compared to national trends, to show that, while the whole country was doing well in the 1960's, the South was doing even better and that its divergence from national trends started around 1965. For example, in showing that public accommodations laws benefited the south, he shows not only that retail sales grew in the 1960s, but that retail sales in the south, as a percentage of total US sales were actually falling in the early sixties, but started rising in 1965 (p. 96). Similarly, Wright analyzes median black male income by region. Black male income rose in all regions between 1955 and 2000, but black male median income in the 1950s was less than half of black median income in the northeast and midwest. By 2000, black median income in the south had risen so much that it was virtually identical to black male income in other regions (p. 146). Similarly, southern male incomes for both whites and blacks grew between 1955 and 2000, but black southern incomes were less than half of white male incomes in the 1950s. By 2000, the gap had narrowed dramatically, although it was still significant (p. 147).

Wright also skillfully uses net migration as a vivid indicator of economic change (p. 143). From 1900 to 1960, people, both white and black, left the south. For African Americans, this exodus is often called the "Great Migration." The net migration of whites during the same period, while smaller both in raw numbers and as a percentage of the southern white population, is evidence that segregation, Jim Crow, and discrimination did not really benefit the white southern population. Starting in the 1970s, migration patterns for African Americans reversed themselves, and hundreds of thousands of African Americans migrated to the south, where economic, political, and cultural conditions were perceived as more auspicious and welcoming. The reversal of migration flows shows that it was not just statistics that improved for southern blacks, but that African Americans themselves perceived the change and altered their residential choices in response. Net white migration patterns also shifted, albeit a decade earlier. Wright argues that the fact that whites also found the south more attractive during and after the civil rights revolution shows that economic and political gains to African Americans did not come at the expense of whites, but instead whites and blacks "shared the prized."

Another particularly vivid and largely forgotten effect of the civil rights revolution was the swift desegregation of southern hospitals. Threatened with loss of Medicare and other federal funding, southern hospitals desegregated almost completely in a just a few months between July and December 1966 (pp. 237-38). The ensuing improvements in health outcomes were dramatic. Black infant mortality in the south, which, in the 1950s and early 1960s, was roughly double black infant mortality in the north, converged to northern rates by 1975, with the most dramatic drops occurring in the late 1960s (p. 239). Although white infant mortality (both in the north and

south) also declined, and black infant mortality in the north did as well, the improvements among southern African Americans were much larger. Of course, these declines reflect not just better access to higher quality hospitals, but other changes, including improvements in education, nutrition, and sanitation. Nevertheless, these other changes also reflect the civil rights revolution, which desegregated schools, increased incomes (and thus access to food), and enhanced African American political power and thus helped ensure that public investments in sanitation benefited African Americans as well as whites.

Wright also addresses head on some of the “downsides” of the civil rights revolution, including negative effects on black business districts, the persistence of poverty, and the “possibility that the political and ideological legacy of Civil Rights era has served to impede further progress in recent decades” (p. 223). He is also careful to note that the landmark legislation of the 1960s did not, of itself, change conditions on the ground. Instead, litigation, local organizing, and individual courage was usually necessary to transform “law on the books” into change “on the ground.” Nevertheless, by using extensive quantitative evidence about business, employment, politics, schools, and politics, Wright is able to persuasively illustrate the dramatic impact of civil rights legislation on the South, especially on African Americans living and choosing to migrate there.

Although Gavin Wright is aware of (and discusses) the extensive econometric literature on civil rights, the book eschews complex statistical analysis for easy-to-understand graphs and tables. In this way, Wright’s work may serve as a model for many legal historians, who neither know statistics nor have an inclination to learn it. Of course, the fact that Wright’s graphs and tables are easy to understand does not mean that they were easy to create. In fact, like elegant narrative history, effective presentation of quantitative analysis takes many drafts, experimentation with alternative formats, and attention to reader feedback.

## 2. Paul Mahoney, *Wasting a Crisis: Why Securities Regulation Fails* (2015)

Paul Mahoney’s *Wasting a Crisis* analyzes the history of securities regulation with heavy reliance on regression analysis. In his view, prior historians have been bamboozled by the self-serving statements of legislators and regulators, who have touted their work as serving the public interest. In fact, Mahoney argues, much of securities regulation has benefited special interests – such as small banks or established high-end brokerages houses – at the expense of consumers. To reach these conclusions, Mahoney uses tools of political economy to analyze the political coalitions that lobbied for legislation and then uses the statistical tools of financial economics to explore the effects of regulation.

Mahoney starts by analyzing blue sky laws, which were state laws enacted between 1911 and 1931, ostensibly to prevent fraud in securities markets. Mahoney finds that the states that enacted the most rigorous “merit review” statutes, were not those that had a greater incidence of securities fraud, but rather were states where small banks were common and stockbrokers were scarce. Small banks feared that potential depositors would invest in stocks rather than putting their money in savings accounts and CDs, so small banks sought regulation that would reduce securities offerings. Their concern, Mahoney argues, was market share not consumer protection. While larger banks in the pre-New Deal era could compete by entering the securities business themselves, and so were not as threatened, small banks did not have the ability to compete in this way and thus sought legislation to hobble the securities industry, their competitor for savings and investment. In states where the securities industry was already well established, and thus where

there were many stockbrokers, the securities industry could effectively lobby to prevent costly legislation. In states where the securities industry was weak, such as Kansas, Arizona, and North Dakota, small banks were more effective politically and could get the stringent regulation they sought. In contrast, where small banks were weak and the financial industry strong, such as New York, New Jersey and Delaware, blue sky legislation was weaker (involving ex post litigation about fraud rather than pre-offering screening for merit or fraud), even though securities fraud was undoubtedly a larger problem in New York than in North Dakota (p. 33). Mahoney also analyzes the effect of blue sky laws and finds that the more stringent laws resulted in increases in bank profits, consistent with the idea that bank rather than consumer protection was a key motivation for the legislation (p. 35). Unfortunately, there is not good data on the incidence of securities fraud, so Mahoney cannot test whether blue sky laws reduced fraud and thus benefited consumers.

In his analysis of blue sky laws, Mahoney makes extensive use of regression analysis. While he uses tables to show that rural states with weak securities industries adopted the strongest regulation (p. 22), he rightly does not stop there, because many factors could affect the adoption of stringent legislation, including the incidence of securities fraud and the strength of small banks, the Democratic party, progressive politicians, and the securities industry. When analyzing so many factors, tables become very cumbersome, and regression analysis is the most appropriate way to identify the important factors.

Later chapters address a variety of other issues in the history of securities law. Perhaps the most interesting is the analysis of whether the Securities Exchange Act of 1934 actually improved public disclosure. The “market failure narrative” that Mahoney aims to debunk asserts that the public generally had very poor information about publicly traded securities before 1934, and that the Act’s mandatory disclosure provisions provided valuable information to market participants. Mahoney (and a collaborator, Jianping Mei) test that theory using techniques from financial economics. If disclosures mandated by the Act aided traders, then prices would more accurately reflect the value of traded firms and likely future disclosures of profits and other information. As a result, the superior mandatory disclosures required by the Act should mean that, when a firm later announces earnings, that announcement should have a smaller impact, because traders would previously have had better information with which to predict future earnings (p. 85). Thus, by comparing reactions to earnings announcements before and after the implementation of the Act’s disclosure requirements, one can assess whether mandatory disclosures improved information to market participants. Mahoney finds they did not for a sample of 201 companies listed on the New York Stock Exchange. Nor is he surprised, because the SEC’s disclosure requirements were modeled on the requirements already imposed by the New York Stock Exchange. While the SEC’s requirements were somewhat different (and arguably more stringent), the differences were not large enough, Mahoney concludes, to measurably improve information available to the market.<sup>2</sup>

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<sup>2</sup> Nevertheless, because Mahoney’s analysis was restricted, for data availability reasons, to firms listed on the New York Stock Exchange, it is possible that the SEC disclosure requirements improved information about firms listed on other exchanges that previously had more lax disclosure requirements. This public benefit may not, however, have outweighed the costs. As Mahoney points out, the effect of regulation is often to enforce “best practices” on lower-cost firms and organization, thus driving them out of business and lessening competition. Indeed, about a quarter of exchanges went out of business within the first few years of the SEC’s existence, and another quarter were acquired or scaled back their

Mahoney's book should serve as a warning to legal historians, who often rely heavily on what historical actors themselves said. Mahoney argues that what legislators and regulators stated was often wrong or misleading. They made assertions that they thought were politically expedient, that they thought would be convincing to the public, and that would make their actions seem to be in the public interest. Nevertheless, Mahoney argues that quantitative analysis of what legislators and regulators actually did and of the effects of their actions provides superior insights into policymakers' motivation and whether policies actually benefited the public.

### 3. Michele Landis Dauber, *The Sympathetic State* (2013)

Michele Landis Dauber takes a very different approach. In Chapter 7 of her book, *The Sympathetic State*, she uses quantitative analysis of letters to Eleanor Roosevelt to uncover the "moral economy" of public assistance. These letters usually asked Mrs. Roosevelt for material assistance and provide a narrative that the author hopes will convince Mrs. Roosevelt that he or she is deserving of aid. Like Mahoney, Dauber is skeptical of the veracity of what people wrote. By comparing the content of letters to census information, Dauber shows that the letters are often misleading --- omitting important information, and sometimes asserting facts that are likely to be flatly wrong. Nevertheless, Dauber thinks it very informative to analyze the letters, because their omissions and distortions reveal what their writers thought would be convincing.

Although Dauber skillfully discusses several illustrative letters, she notes that "it is impossible to reliably detect systemic variation in a set of 529 letters simply by reading them" (p. 212). With so many letters, statistical analysis is necessary. Nearly any point could be "proven" by quoting from one or two letters. Only by analyzing the letters quantitatively and by controlling for multiple factors can reliable general conclusions be drawn. One of Dauber's findings is that the excuses in the letters did not vary much by gender, region, or indicia of social class (p. 213). From this Dauber infers that letter writers shared a common view of what counted as a valid excuse (or at least what they thought Mrs. Roosevelt would consider as such). In Dauber's words:

This practical imperative [to make a persuasive case for their own lack of fault] seems to have overwhelmed any systemic differences in the background of writers, producing more agreement on what constituted a good letter than would seem likely given the high degree of regional, class, gender, and racial diversity among the writers. This agreement is a signal of the underlying moral economy in which these writers expected their letters to be read. (p. 218).

Nevertheless, the letters were not uniform. Letters that requested assistance with unpaid debts tended to include more excuses, probably because debt "raises the question whether the debt represents a flaw in the debtor's moral character. .... willingness to spend beyond her means." (pp. 213, 219).

One of the strengths of Dauber's approach is that she embeds her statistical analysis in compelling narratives. She starts the chapter with summaries and quotations from several letters so that the reader gets a real feeling for the nature of her sources and the actual words used by the letter writers. Quantitative analysis need not be dry, and it does not need to displace the more

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operations to avoid SEC regulation (p. 99). Whether the benefit of increased information outweighed the losses from less competition is an open question.

traditional narrative and analytic methods used by legal historians. It can usefully supplement them, especially when historians are fortunate to possess large amounts of source material. In those situations, statistical analysis can allay suspicions that the historian is emphasizing unrepresentative pieces of evidence and can uncover patterns and factors that would otherwise be invisible.

#### 4. Other works

The three examples discussed in this section represent, of course, just a small sampling of legal historical works that use quantitative methods. Other scholars have used statistical analysis to address a wide range of issues, including the causes and effects of the nineteenth-century expansion of women's rights, the effect of the colonial imposition of common or civil law legal systems, the economic effects of the Glorious Revolution, the dynamics of medieval litigation, and the nature of slave captain contracts.<sup>3</sup> I chose to discuss in depth the books by Gavin Wright, Paul Mahoney, and Michele Landis Dauber because they illustrate the variety of ways quantitative methods can be used, and because they combine statistics with narrative and other methods in a way that legal historians are likely to find more persuasive. Some works of quantitative legal history emphasize the numbers so much and seem to abstract so much from context that legal historians (and other not committed to quantitative history) are unlikely to find them persuasive. But these three works show how statistics can be woven into a broader theoretical and narrative context to form a cohesive whole.

### III. The Future of Quantitative Legal History

Sophisticated quantitative analysis is likely to remain marginal to legal history. A key reason is that legal historians are increasingly receiving their training as Ph.D. students in history departments, where quantitative methods are not usually taught or used by those who teach in such departments.<sup>4</sup> Until the late 1970s, quantitative analysis and "scientific history" more generally were important parts of historical studies.<sup>5</sup> Statistics were important to many kinds of historians, including Marxists, cliometricians (historians using modern non-Marxist economic models and large datasets), and followers of the *Annales* school. Nevertheless, in the late seventies, many elite historians, including those mostly likely to train the next generation of scholars, turned away from social science and statistics towards more traditional narrative models. One of many reasons for this shift was the bitter debate over Fogel and Engerman's analysis of slavery in *Time on the Cross*. One consequence of the increasing statistical

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<sup>3</sup>For works on all of these topics, see Daniel Klerman, 'Economics of Legal History' in Francesco Parisi (ed.) *Oxford Handbook of Law & Economics* (2017). For prior and related surveys, see Ron Harris, 'The Encounters of Economic History and Legal History', (2003) 21 *Law and History Review* 297ff.; Daniel Klerman, 'Statistical and Economic Approaches to Legal History', (2002) *University of Illinois LR* 1167ff.

<sup>4</sup> On the aversion of most academic historians to social science and to the quantification that usually accompanies it, see Phillip T. Hoffman, 'Opening Our Eyes: History and the Social Sciences', (2006) 6 *Journal of the Historical Society* 93ff; Jan de Vries, 'The Return from the Return to Narrative', Max Weber Lecture Series, European University Institute (2013). Both Hoffman and de Vries, of course, argue for greater use of social science by historians.

<sup>5</sup> Lawrence Stone, 'The Revival of Narrative: Reflections on a New Old History,' 85 *Past & Present* 3ff (1979).

sophistication of the cliometricians and historians' turn to narrative is that quantitative historians were more likely to be found in economics departments than in history departments, and quantitative history was more likely to be published in economic journals.<sup>6</sup> For example, Gavin Wright is employed in the Stanford economics department, as is another distinguished economic historian, Avner Greif. There are, of course, notable exceptions. For example, Naomi Lamoreaux, although she is a member of the Yale economics department, is also chair of Yale's history department, where she has the opportunity to influence the large number of legal historians who receive their training at Yale.<sup>7</sup> Economic history is also increasingly published in economics journals.<sup>8</sup> Similar trends can be found in legal history. As noted above, legal history journals and books considered of legal historical interest by the editors of those journals contain almost no sophisticated quantitative analysis. In contrast, economic and statistical analysis of legal history flourish in journals devoted to law and economics, economics, and political science.<sup>9</sup>

Of course, doctoral students in history can take courses in other departments and thus learn statistics and gain exposure to those in other departments who use quantitative methods for historical and other purposes. Similarly, those pursuing a doctorate in other fields can choose to focus on legal history topics. Nevertheless, as long as quantitative work remains so marginal to history departments, it is likely to be underused in legal history as well.

There is some reason to be optimistic that quantitative work may become more central to the field of history. Jean De Vries has even predicted a "Return from the Return to Narrative" and thus an increased use of social scientific methods in history. If historians turn to the social sciences, they will they almost certainly use more statistics, because quantitative analysis is an integral part of most social science research.<sup>10</sup> Relatedly, here has been a recent upsurge in work on the history of capitalism, and that may presage greater interest in economic history and in quantitative work. On the other hand, much work in the history of capitalism eschews statistical analysis in favor of intellectual history or other approaches.

The emergence of digital humanities may also revive interest in quantitative approaches. Digital humanities uses new quantitative approaches designed particularly for the automated analysis of texts. Because legal historians so often deal with texts, digital humanities may prove an attractive approach. The fact that *Law and History Review* recently devoted a special issue to "digital legal history"<sup>11</sup> and that this Handbook includes a chapter on "Legal history and digital humanities" is encouraging in this regard. On the other hand, legal historians may, at least for some time, prefer more traditional approaches to statistical analysis. Quantitative approaches have, until recently, relied primarily on the expertise of the researcher to convert legal sources

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<sup>6</sup> Claudia Goldin, 'Cliometrics and the Nobel,' 9 *Journal of Economic Perspectives* 191ff (1995).

<sup>7</sup> Her own distinguished work on the comparative history of legal business forms puts her in a good position to advise young legal historians. See, e.g., Naomi R. Lamoreaux and Jean-Laurent Rosenthal, 'Legal Regime and Contractual Flexibility: A Comparison of Business's Organizational Choices in France and the United States during the Era of Industrialization'. (2005) 7 *American Law and Economics Review* 28ff.

<sup>8</sup> Ran Abramitzky, 'Economics and the Modern Economic Historian,' NBER Working Paper 21636 (October 2015). On the paucity of economics, and social science more generally in history journals, see works by Hoffman and de Vries above in fn. \_\_.

<sup>9</sup> See works cited in Daniel Klerman, 'Economics of Legal History' in Francesco Parisi (ed.) *Oxford Handbook of Law & Economics* (2017).

<sup>10</sup> de Vries (n. \_\_).

<sup>11</sup> *Law and History Review*, volume 34, Number 4 (2016).

into quantitative data. That is, the author generally must read the sources and “code” them. Digital humanities approaches often reduce the human contribution to the analysis by having the computer “read” and classify the sources. At the moment, computers are rather primitive readers, and it is yet to be seen whether analyses that rely heavily on textual comprehension software can provide genuine insight.

Legal historians might also be influenced by subfields of political science, such as American Political Development, where quantitative analysis is common.<sup>12</sup> Or, as Phillip Hoffman has pointed out, historians (including legal historians) could usefully employ tools from cognitive psychology and game theory, which already “are revolutionizing fields such as law or political science.”<sup>13</sup> Because these tools are usually tested with statistical analysis of data, their use would almost inevitably lead to increased quantification. Similarly, techniques developed to analyze the effect of politics, institutions, and panel composition on modern judicial decisionmaking could be fruitfully applied to judges in the more distant past.<sup>14</sup> In this regard, it should be noted that the Supreme Court Database<sup>15</sup> has recently been updated to include information on Supreme Court cases and justices back to 1791, so much of the relevant data have already been coded. Of course, historians may want to focus on other courts, and will thus need to code the data themselves. Even so, the Supreme Court Database and the many articles that analyze Supreme Court data can serve as useful models.

It is possible that legal historians will be inspired by this essay or other works to learn and apply more quantitative techniques. They could do so by taking statistics and research methods courses offered by political science, economics, sociology, and other departments. Or they could teach themselves. A very useful resource for autodidacts could be Lee Epstein and Andrew D. Martin, *An Introduction to Empirical Legal Research*, a book specifically intended for legal scholars (although not necessarily legal historians) who have minimal quantitative training. Although it teaches some basics of statistical analysis, it also addresses important topics that statistics texts often ignore, such as research design, collecting and coding data, and persuasive presentation of results. Those who teach themselves quantitative methods (and indeed all who use them), would be well-advised to share ideas and drafts with social scientists in other departments. Those conversations can help the author avoid mistakes as well as spark more general conversations and interchange on methods as well as substance.

Perhaps the most promising avenue for quantitative legal history would be collaboration between legal historians and quantitative social scientists. Co-authorship is extremely common in the social sciences, but strangely rare in legal history. Collaboration provides a way to incorporate sophisticated methods without requiring independent mastery of those techniques by the legal historian. In collaborating, the legal historian is likely to learn considerably about statistics and quantitative research. Similarly, the legal historian can ensure that the quantitative analysis takes proper account of historical context and that narrative and other techniques are effectively used in the exposition.

#### IV. Conclusion

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<sup>12</sup> See, e.g., Daniel P. Carpenter, *The Forging of Bureaucratic Autonomy: Reputations, Networks, and Policy Innovation in Executive Agencies, 1862-1928* (2001).

<sup>13</sup> Hoffman (2006) 6 *Journal of the Historical Society* 94.

<sup>14</sup> See, e.g., Lee Epstein, William M. Landes, and Richard A. Posner, *The Behavior of Federal Judges: A Theoretical and Empirical Study of Rational Choice* (2013).

<sup>15</sup> <http://supremecourtdatabase.org> or <http://scdb.wustl.edu/>.

Although relatively few legal historians currently use quantitative techniques, there are some who do and their work shows the potential power of statistics to disentangle the influence of multiple factors, to reveal the effect of legal change, and to uncover patterns in large quantities of text. The recent interest of historians in digital humanities and the history of capitalism may foreshadow increasing use of quantitative methods by historians, which may in turn influence the next generation of Ph.D. legal historians. Or current legal historian could benefit by educating themselves in quantitative methods or by collaborating with empiricists from other schools and departments.

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