CHAPTER 19

QUANTITATIVE LEGAL HISTORY

DANIEL KLERMAN*

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 analyse legal history. Such analysis is particularly helpful in understanding the effect of legal change and in analysing the influence of multiple factors on legislation, judicial decision-making, and citizen behaviour. While it is wonderful that people from other disciplines are using statistics to analyse legal history, legal historians might do it better, given their penchant for archival work and their superior knowledge of historical sources, context, and theory.

* The author thanks Scott Altman, Sam Erman, Bob Gordon, Ariela Gross, Philip Hoffman, Naomi Lamoreaux, Paul Mahoney, Chris Tomlins, and Gavin Wright for helpful comments and suggestions.
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

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 19.1 below takes one approach, although, as with all quantitative analysis, other approaches are possible.

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* The author is also grateful to a number of sources analysed in senior scholar, and then by a junior scholar.

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1 The list of tables on pp. i, e.g., two of them displayed j


Table 19.1 Quantitative Analysis in Recent Works of Legal History

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* The author is also grateful to USC Law librarians for their assistance in locating and searching the large number of sources analysed in Table 19.1.

... in the field of legal history is considered quantitative with all quantitative articles published in legal journals in 2016, prizes in two categories, of course, the annual prizes. The articles language legal history of Legal History, the ro are published in the England, and Law and History Review, Sutherland article Prize (best article 1980–2016), and there is some evidence that earlier works were more likely to use statistics, although the number of works analysed 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.1

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1 The list of tables on pp. ix–xii mentions seventy-one tables, but three of them were not quantitative, e.g., two of them displayed judges, their terms, and their dates of birth.
A much smaller percentage of works contain more sophisticated quantitative analysis. Only four per cent 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 analyse 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 the 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 the *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 used effectively to analyse the influence of two factors, they become more cumbersome when analysing 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 analysed in Table 19.1. This is surprising and disappointing, because 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 19.1 that used regression analysis have PhDs 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 discusses Dauber’s and Mahoney’s books (as well as Gavin Wright’s *Sharing the Prize*) 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.

A. Gavin Wright

Gavin Wright’s *Sharir* relatively simple table questions of modern worked. He focuses on the Voting Rights Act of 1965 to analyse their effect on the North, but focuses on the South. Pr less clear picture of Americans starting from over busing and urban Wright argues that if c 1960s, also enabled, produced u harming Southern w

Wright most often used charts that compared sales over time. He c 1965, and the Voting Rights national trends, to the South was doing c 1965. For exam the South, he shows n in the South, as a per 1960s, but started rising income by region. Blac male median inc in the Northeast and risen so much that it w Similarly, Southern me 2000, but black South 1950s. By 2000, the gap Wright also skilfully From 1900 to 1960, Americans, this exodi of whites during the 2 3 ibid
A. 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 on the South, arguing that the Civil Rights Act of 1964 and the Voting Rights Act of 1965 had their biggest impact on this region and that it is best to analyze their effects on a regional basis. Civil rights legislation, 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 rights 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 American 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 the enactment of the Civil Rights Act of 1964 and the 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 1960s, 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 U.S. sales, were actually falling in the early 1960s, but started rising in 1965.2 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.3 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.4

Wright also skilfully uses net migration as a vivid indicator of economic change.5 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

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2 Gavin Wright, Sharing the Prize (2013), at 96.
3 Ibid., at 146.
4 Ibid., at 147.
5 Ibid., at 143.
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 just a few months between July and December 1966. 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. 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.' 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 were usually necessary to transform 'law on the books' into change 'on the ground.' Nevertheless, by using extensive quantitative evidence about business, employment, 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 model for many legal to learn it. Of course, does not mean that t effective presentation with alternative forms.

B. Paul Mahoney

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Paul Mahoney's Wasi with heavy reliance on bamboozled by the sel touted their work as s of securities regulatio established high-end t these conclusions, Mal coalitions that lobbied economics to explor t Mahoney starts by between 1911 and 1931, finds that the states th not those that had a g where small banks we feared that potential d money in savings acco reduce securities offeri consumer protection. V by entering the securi small banks did not have legislation to hobble t investment. In states w and thus where there w effectively to prevent leg such as Kansas, Arizona politically and could be small banks were weak. Jersey, and Delaware, bl about fraud rather than securities fraud was un
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.


Paul Mahoney’s *Wasting a Crisis* analyses 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 analyse 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 analysing 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 lobby effectively to prevent 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. Mahoney also analyses the effect of blue sky laws and finds that the more stringent laws resulted in increases in bank profits, consistent with the idea that a key motivation for the legislation was the protection of banks. Unfortunately, there is no 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, 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 analysing 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 modelled 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.

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10 Ibid., at 35.
11 Ibid., at 22.
12 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 organizations, thus driving them out of business and lessen within the first few years the operations to avoid SEC n the losses from less compi

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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.


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 provided a narrative that the author hoped would convince Mrs. Roosevelt that he or she was deserving of aid. Like Mahoney, Dauber is sceptical of the veracity of what people wrote. By comparing the content of letters to census information, Dauber shows that the letters were often misleading—omitting important information and sometimes asserting facts that are likely to be flatly wrong. Nevertheless, Dauber thinks it very informative to analyse the letters, because their omissions and distortions reveal what their writers thought would be convincing.

Although Dauber skilfully 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’. With so many letters, statistical analysis is necessary. Nearly any point could be ‘proven’ by quoting from one or two letters. Only by analysing 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. 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

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

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.\textsuperscript{15}

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... willing[ness] to spend beyond her means.'\textsuperscript{16}

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 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.

\section*{D. 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.\textsuperscript{17} 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 others 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.

\textsuperscript{15} Ibid., at 218.
\textsuperscript{16} Ibid., at 213, 218.

Sophisticated quantiti...
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 PhD students in history departments, where the faculty seldom use or teach quantitative methods. Until the late 1970s, quantitative analysis and 'scientific history' more generally were important parts of historical studies. 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 1970s, 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. A consequence of the increasing statistical sophistication among cliometricians and of historians' turn to narrative is that quantitative historians are more likely to be found in economics departments than in history departments, and quantitative history is more likely to be published in economic journals.

For example, Gavin Wright is a member of 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.

Economic history is also increasingly published in economics journals. Similar trends can be found in legal history. As noted above, legal history journals and books

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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.23

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.24 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 the Law and History Review recently devoted a special issue to 'digital legal history'25 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 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 sub-fields of political science, such as American Political Development, where quantitative analysis is common.26

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24 de Vries (n. 18).
28 See, e.g., Lee Epstein Theoretical and Empirica
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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.27 Because these tools are usually tested with statistical analysis of data, their use would almost inevitably lead to increased quantification. Similarly, techniques developed to analyse the effect of politics, institutions, and panel composition on modern judicial decision-making could be fruitfully applied to judges in the more distant past.28 In this regard, it should be noted that the Supreme Court Database29 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 analyse Supreme Court data can serve as useful models.

It is possible that legal historians will be inspired by this chapter 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

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 legal historians. Or current legal historians could benefit by educating themselves in quantitative methods or by collaborating with empiricists from other schools and departments.

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THE OXFORD HANDBOOK OF

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Edited by
MARKUS D. DUBBER
and
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For much of the period spanning the second half of the 20th century, the world was dominated by the Cold War. The United States and the Soviet Union engaged in a decades-long struggle for global influence and control. This struggle had profound effects on international relations, economics, and culture. The Cold War also had a significant impact on individual lives, with many people living in fear of nuclear annihilation.

The Cold War began in 1947 with the Truman Doctrine, which declared the United States' commitment to supporting free peoples against threats to their independence. This was followed by the Marshall Plan, which provided economic aid to Western Europe to support democracy and prevent the spread of communism. In the 1950s, the Soviet Union responded with the Marshall Plan for Eastern Europe, which aimed to create a socialist system.

The nuclear arms race reached its peak in the 1960s, with the United States and the Soviet Union stockpiling huge arsenals of nuclear weapons. The Cuban Missile Crisis in 1962 brought the world to the brink of nuclear annihilation. The Cold War ended in 1991 with the collapse of the Soviet Union, which marked the end of the bipolar world order and the rise of a unipolar world led by the United States.

The Cold War had far-reaching consequences for international relations. It led to the establishment of alliances such as NATO and the Warsaw Pact, and it shaped the political and economic landscape of the world. The Cold War also had a significant impact on culture, with the rise of the counterculture movement and the growth of international tourism.

In the decades following the end of the Cold War, the world has become increasingly interconnected, with the rise of global communication technologies and the internet. This has led to new challenges, such as the spread of terrorism and the rise of authoritarian regimes. The world is now facing a new set of challenges, including climate change, economic inequality, and the rise of nationalism.

Legal historians have played a key role in understanding the history of law and legal institutions. They have used a variety of methods to study legal history, including legal archives, oral history, and archaeological evidence. This has allowed them to gain a deeper understanding of how legal systems have evolved over time and how they have shaped society.

Some of the key figures in legal history include Charles Beeman, the founder of the American School of Legal Studies, and the historian of law John Phillip Ruddock. Beeman's work laid the foundation for the study of legal history, and his students went on to become some of the most influential legal historians of the 20th century.

Legal history has also had a significant impact on contemporary legal systems. By studying the history of law, legal scholars can gain a deeper understanding of the legal principles that underlie contemporary systems. This has allowed them to develop new approaches to legal problem-solving, and it has also led to the creation of new legal institutions.

In conclusion, legal history is an important field of study that has had a significant impact on our understanding of the legal systems that govern our world. By studying the history of law, we can gain a deeper understanding of how legal systems have evolved over time and how they have shaped society. This knowledge is essential for anyone who wishes to understand the world in which we live.