

IDEAS HAVE CONSEQUENCES: THE IMPACT OF LAW AND ECONOMICS ON AMERICAN JUSTICE*

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This article empirically studies the effects of the early law and economics movement on the U.S. judiciary. We focus on the Manne Economics Institute for Federal Judges, an intensive economics course that trained almost half of federal judges between 1976 and 1999. Using the universe of published opinions in U.S. Circuit Courts and 1 million District Court criminal sentencing decisions, we estimate the within-judge effect of Manne program attendance. Selection into attendance was limited, as the program was popular among judges of all backgrounds, frequently oversubscribed, and admitted participants on a first-come, first-served basis. We find that after attending economics training, participating judges use more economics language in their opinions, rule against regulatory agencies more often, and impose more severe criminal sentences. We argue that economics, as a rigorous social science, was especially effective in persuading judges. *JEL codes:* D7, K0, Z1.

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I. INTRODUCTION

A growing literature in economics has documented the effects of exposure to information and ideology in electoral politics and public opinion (e.g., [DellaVigna and Gentzkow 2010](#); [Cantoni et al. 2017](#)). But it remains an open question whether exposure to powerful new ideas can directly affect the high-stakes policy decisions of public officials. This article fills that gap by studying the effect of an influential program introducing U.S. federal judges to law and economics. These judges often have to make substantive and precedent-setting policy decisions when the law is unclear. Therefore judicial worldviews and legal ideas, including both positive and normative beliefs ([Bénabou 2007](#)), can potentially influence policy.

Law and economics make up a particularly influential set of ideas in legal academia and the U.S. judiciary. This approach emphasizes cost-benefit criteria, freedom of contract, legal incentives, and more broadly the use of economic analysis in law.¹ Especially compared with the legal communities in other countries, in the United States the influence of economics among law professors and judges is well documented ([Posner 1987, 2008](#); [Ellickson 2000](#)).

In the early years of law and economics, a flagship initiative for sharing these ideas with judges was the Manne Economics Institute for Federal Judges. Started in 1976 by the Law and Economics Center, the Manne program was controversial even in its early years, not least because it was funded by prominent business and conservative foundations ([Butler 1999](#)). By the early 1990s, almost half the working federal judges had attended this intensive two-week training camp.

This article analyzes the effect of economics training on federal judges by linking records on Manne attendance (1976–1998) with a comprehensive data set of appellate decisions in the U.S. Circuit Courts (1970–2005) and criminal sentence decisions in the U.S. District Courts (1992–2011). We use a difference-in-differences design, leveraging the fact that recruitment into the program was oversubscribed and on a first-come, first-served

1. Law and Economics is associated with the Chicago School of Economics, which has had a laissez-faire and generally “conservative” economic outlook (e.g. [Teles 2012](#); [Hovenkamp and Scott Morton 2020](#)). The free-market orientation was particularly strong in early academic law and economics, which has been the focus of judicial training programs of the Law and Economics Center.

basis, minimizing opportunities for selection in response to short-run changes in judges' beliefs/attitudes. Further, we use court-by-year fixed effects (combined with quasi-random assignment of judges to cases) to ensure that treated judges are not selecting into particular types of cases after attendance. Although we cannot completely rule out selection of judges into the timing of attendance, our baseline sample and specification has little imbalance in pre-attendance outcomes. We also show robustness to including the small set of judicial characteristics that predict the timing of attendance, flexibly interacted with time effects, and provide a battery of auxiliary specification checks in the [Online Appendix](#).

The setting is relevant for economic policy because U.S. law makes giants of its judges. The U.S. federal courts (13 circuit courts overseeing 94 district courts) operate in an incremental common law space where judges continually make new rules and legal distinctions that future judges must follow (e.g., [Gennaioli and Shleifer 2007a](#)). Relatively few district court cases are appealed to the circuits, while fewer than 1% of circuit decisions are reviewed by the Supreme Court. Therefore almost all circuit court decisions are final.

We estimate the influence of program attendance on a range of outcomes. First, to understand the effect on economics reasoning, we take a text-as-data approach and compute a word-embedding-based measure of similarity between written appellate opinions and a lexicon of core law and economics terminology. We find that the program increased attendees' use of economics language in the years after attendance. Second, we find that Manne attendees subsequently are more likely to vote against regulatory agencies, in particular on the labor and environmental issues that early law and economics focused on. Third, in the district courts, we find that Manne attendance is associated with more severe criminal penalties—that is, a higher likelihood of a prison sentence.

These results provide evidence on the old question of whether judges are legal formalists or political operators ([Posner 2008](#); [Stephenson 2009](#)). If judges are formalists following the law as written, the program would have no effect. Similarly, if judges are politicians toeing the party line, the program would still have no effect. Neither of these prototypical models can explain the evidence. Instead, our results show a shift in the judge-specific component of decision-making, holding law and political affiliation constant. On this particular point, the best previous

evidence was [Bonica et al. \(2019\)](#), who show in the context of the U.S. Supreme Court that changes in the ideology of selected clerks sometimes shift a justice's votes. Beyond that, the literature has largely attended to legal rules determining outcomes ([Kornhauser 1992](#); [Gennaioli and Shleifer 2007b](#)) or invariant judge characteristics such as political affiliation, average decision tendencies, campaign donation tendencies, or demographics ([Cameron 1993](#); [Martin and Quinn 2002](#); [Epstein, Landes, and Posner 2013](#); [Ash, Chen, and Ornaghi 2024](#); [Bonica and Sen 2021](#)).

This article adds to the literature on the impact of policy ideas, which has mostly focused on the effects of political advertising and biased media on voting and related outcomes ([DellaVigna and Kaplan 2007](#); [DellaVigna and Gentzkow 2010](#); [Enikolopov, Petrova, and Zhuravskaya 2011](#); [Spenkuch and Toniatti 2018](#); [Galletta and Ash 2023](#)). Unlike voting, we can document a direct policy impact, as what these judges decide is law. On this point, a related paper is [Azgad-Tromer and Talley \(2017\)](#), who show that after a finance training program, utility regulators set pricing more in line with standard asset pricing theory.² Our evidence suggests that there is room for policy analysis to influence judicial decision-making.

The remainder of the article is organized as follows. [Section II](#) gives background on the law and economics movement and the Manne program. [Section III](#) explains our various sources of data and measurement strategies. [Section IV](#) describes our

2. Similarly, [Hjort et al. \(2021\)](#) randomize informing mayors in Brazil about the results from economic policy experiments and find that mayors update beliefs and alter policies in response to information about experimental results. [Giorcelli \(2019\)](#) finds that management training increased performance in Italian firms. [Brownson, Colditz, and Proctor \(2017\)](#) explore the diffusion (or lack thereof) of scientific ideas into medical practice. On the ideological side, [Cantoni et al. \(2017\)](#) analyze a staggered Chinese curricular reform that caused students (as intended) to be more skeptical of free markets. Other papers have looked specifically at economics: economics students are less redistributive of potential lottery winnings ([Selten and Ockenfels 1998](#)), view surge prices more fairly ([Frey and Meier 2005](#)), and favor profit maximization in business vignettes ([Rubinstein 2006](#)). [Paredes, Paserman, and Pino \(2020\)](#) find using Chilean data that majoring in economics is correlated with sexism expressed in survey measures. In [Ifcher and Zarghamee \(2018\)](#), a brief economics lesson significantly shifted choices in social interactions such as public goods contributions. In [Stantcheva \(2021\)](#), watching a short video about the economic trade-offs between redistribution and efficiency increased support for progressive taxes (see also [Stantcheva 2020](#)).

empirical approach. [Section V](#) reports the results, while [Section VI](#) discusses their implications. [Section VII](#) concludes.

II. THE LAW AND ECONOMICS MOVEMENT

This section provides some background on the law and economics movement, an influential set of thinkers, professors, lawyers, and policy advocates centered on the Chicago School starting in the early 1970s ([Posner 1987](#)). First, we provide some background on some of the main ideas in economic analysis of law. Second, we discuss the special place of the Manne Program in this movement.

II.A. Background

Three canonical examples from contracts, torts, and criminal law illustrate the potential effects of economic thinking. In contract law, the theory of “efficient breach” gives an explanation for why walking away from a contract should not be penalized, beyond compensating the aggrieved party ([Birmingham 1969](#)). In tort law, the duty of care can be defined economically: the cost of precaution should not exceed the probability of loss times the economic value of the loss ([Posner 1972](#)). In criminal law, finally, the expected penalty—economic cost of the penalty times the probability of detection—should be set high enough to outweigh the expected benefits of crime ([Becker 1968](#)), a prescription at odds with midcentury theories of sentencing according to retribution on behalf of victims or rehabilitation of criminals ([Martinson 1974](#)).

The application of economics ideas to law went from the fringe to the mainstream in the latter decades of the twentieth century. By the 1980s, economics principles had diffused into almost all legal areas ([Posner 1987](#)). Looking at U.S. judicial opinions, [Clarke and Kozinski \(2019\)](#) find that the use of economics terms increased in the 1970s and was most prominent in the 1980s. [Ellickson \(2000\)](#) documents that law and economics has also grown in importance in legal scholarship published in the law reviews.

Law and economics is generally committed to the application of economic principles to jurisprudence and an emphasis on economic efficiency as the main policy criterion ([Posner 2014](#)). In the context of judging, this bundle has at least three components. First, economics can clarify the incidence of legal rules, helping

judges see the effects of their decisions. Second, it provides a positive explanation for past jurisprudence. Third, it provides a set of normative principles—economic efficiency—for judges to try to follow in their decisions.

None of the ideas or modeling approaches of the law and economics movement were outside the bounds of mainstream economics. Yet due in part to the normative emphasis on economic efficiency, law and economics has a recognized association with conservative legal groups. Teles (2012) provides a detailed history of the conservative legal movement and the role of law and economics in particular. As documented further in Hovenkamp and Scott Morton (2020), the Chicago School-oriented law and economics movement was driven at least partly by conservative political goals such as deregulation.

In turn, the conservative or pro-business orientation of law and economics is most often pointed out in the context of administrative law. Law and economics scholars have voiced public-choice criticisms of regulatory policies, emphasizing their negative unintended economic consequences and potential for capture. In labor regulation, law and economics scholars (and judges) wrote extensively against New Deal labor law and union protections (Epstein 1983; Posner 1984). Given that environmental regulation often puts limits on investments in productive property (Blumm 1995), economic approaches have gained a conservative reputation among environmental law scholars (Hornstein 1992). Meanwhile, reliance on economic analysis in antitrust has attained nearly complete consensus (Ginsburg 2010).³ Even judges who have voiced skepticism of judicial economic analysis, such as conservative Justice Antonin Scalia, have famously used cost-benefit reasoning to evaluate federal regulatory standards (Viscusi 1987).

Outside of business, the law and economics movement has gained traction in criminal law by promoting deterrence theory, suggesting that severity of punishment can make up for low

3. By the 1960s, the Supreme Court had read into previous statutes a variety of policy goals, such as protecting small traders from their larger and more efficient rivals, curbing inequality in the distribution of income, and mitigating undue influences of large businesses. The law and economics movement advanced the initially controversial view that the antitrust laws should promote economic efficiency and consumer welfare, rather than shield individuals from competitive market forces or redistribute income across groups of consumers (Bork 1978).

probabilities of detection (Becker 1968). It may be surprising to economists to learn that this idea (deterrence) is quite new and that before Becker criminal penalties were justified on grounds of retribution or rehabilitation (Martinson 1974).⁴ On the other hand, many economists associated with the Chicago School also advocated for legalizing victimless crimes, such as recreational drug use and prostitution (Thornton 2016).

II.B. The Manne Economics Institute for Federal Judges

The influence of economics in legal thought can be traced in part to a controversial economics training program for sitting judges—the Economics Institute for Federal Judges, run by the Law and Economics Center (LEC). The LEC, founded at the University of Miami in 1974, was the first academic research center devoted to law and economics. LEC moved to Emory University in 1980, before its current location at George Mason University.

The judge training course was founded in 1976 and organized by Henry Manne, an influential participant in the early law and economics movement. Manne had previously run a similar course for law professors.⁵ The institute was the the flagship program of the LEC. Substantial funding came from donations by pro-business foundations and corporations.⁶

An excellent summary of the program is provided by Butler (1999), written by a former director. The course ran continuously, once or twice a year, from 1976 to 1998. From the start, all federal judges were invited to apply, yet Henry Manne did not have any existing relationships with federal judges. The LEC made the program attractive by covering all expenses for a beachside hotel

4. In law and economics, rehabilitation and retribution are out of favor (Martinson 1974; Petersilia and Turner 1993; Cullen and Gendreau 2001), and deterrence is viewed as the dominant purpose of criminal justice. Harcourt (2011) suggests that this emphasis on deterrence and increased punitiveness is complementary with laissez-faire economic ideology. By deterring nonmarket opportunism, criminal law incentivizes participation in markets, which leads to higher efficiency. Most recently, the insights from behavioral economics have led to a more nuanced view of how deterrence operates: for example, swiftness, certainty, and fairness might deter crime more than the severity of punishment (Nagin 1998; Kleiman 2009; van Winden and Ash 2012).

5. See Manne (1993) for a history of the LEC, including a discussion of the economics course for judges. For more critical historical perspectives, see Medema (2017), Gindis (2020), and Gindis and Medema (2022).

6. See Barbash (1980). See Online Appendix A for more background and documents related to the Manne Program.

stay and by inviting judges' family members to join. The organizers did not invite particular judges, and the admissions process was first-come, first-served.⁷ This means, importantly, that there was no selection of particular judges for attendance on the side of the program organizers.

On the judges' side, the program was popular among and heavily attended by both Republican and Democratic appointees. Starting in the second class (1977) and into the late 1980s, the course was oversubscribed due to high demand, and the first-come, first-served policy was binding (Butler 1999). The binding attendance cap would have worked against selection into timing of attendance due to short-run shifts in judge preferences about economics. By 1990, 40% of federal judges had attended this program.⁸ Figure I plots the share of Circuit Court cases with a Manne judge on the panel over time. As can be seen, by the late 1990s, about half of cases were directly affected by a Manne panelist.

Online Appendix A provides additional qualitative evidence on how the program was perceived by the public and the judicial participants, along with extensive quotations from judges (both Republican and Democratic appointees) who were enthused about the program. The quotes testify to how much the judges appreciated the program, how demanding the lessons were, and how the judges learned to think about their rulings through cost-benefit analysis rather than more traditional legal reasoning.

Lectures were given by eminent economists including Milton Friedman, Armen Alchian, Harold Demsetz, Martin Feldstein, Paul Samuelson, and Orley Ashenfelter. Topics included the Coase theorem, demand/supply theory, consumer/producer/price

7. This was for two reasons: "First, Manne was sensitive to the possibility of attacks he was recruiting judges targeted by specific contributors. Second, he wanted to avoid any charges of favoritism of appellate over trial judges" (Butler 1999, 356).

8. Manne (1993) writes: "These courses for federal judges have been so popular that for most new judges today the Economics Institute is thought to be almost a requirement." There were also a number of additional advanced judge training courses, including courses on advanced economics, quantitative methods, antitrust, corporations/finance, insurance/torts, and public health. Attendance at these courses required attendance at the Basic Economics Institute, which is the course we analyze. These advanced courses cannot be analyzed individually given the relatively small samples of judges attending them. However, it could be that our treatment effect is partly driven by attendance at these subsequent courses.

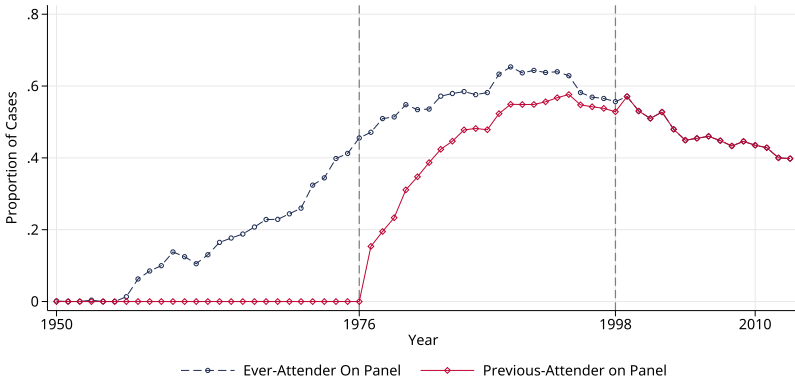


FIGURE I

The Share of Cases with a Manne Judge on the Panel, 1950–2013

The share of cases with a Manne judge on the panel, plotted by year. The blue line with circle markers shows judges who ever attended; the red line with diamond markers indicates judges who have already attended.

theory, bargaining, externalities, expected value/utility, property rights, torts, contracts, monopoly theory, regulation, and basic statistics. The main reading materials were economics articles and textbooks, such as *Law and Economics* by Robert Cooter and Thomas Ulen, and *Exchange and Production* by Armen Alchian and William Allen. An example program agenda, with readings and class schedule, is shown in [Online Appendix Figure A.1](#).

The annual reports include the instructors' views. In terms of the main lessons, the program strove for nominal ideological balance. Both conservative and liberal economic thinkers were invited. Empirical classes, while always a minority of sessions, could include both Orley Ashenfelter and John Lott, for example.⁹ A norm of using first names was established for both teachers and students. It is clear there was an effort to teach economics in a relatively informal and enjoyable, yet rigorous, environment.¹⁰

9. The former director Henry Butler (personal communication, October 14, 2017) writes: "Samuelson [lectured] on whatever the heck he wanted to, usually personal investment strategies; Friedman always started on legalization of recreational drugs; Ashenfelter used climate to predict quality and prices of wine, followed by wine tasting."

10. Notwithstanding this balanced list of instructors, the instruction itself was more emphatically delivered by the conservative instructors. As George Priest, a regularly participating instructor, observed: "[Manne] did not provide

From the judges' perspective, the seminar made a lasting impression. Circuit Judge E. Grady Jolly appreciated "a sound theoretical and rational structure for my decisions . . . the potential effects and foreseeable impact of imposing a duty" (Butler 1999, 371). Supreme Court Justice Ruth Bader Ginsburg wrote: "the instruction was far more intense than the Florida sun. For lifting the veil on such mysteries as regression analyses, and for advancing both learning and collegial relationships among federal judges across the country, my enduring appreciation" (Butler 1999, 358).

II.C. What Are the Expected Impacts?

A strong null hypothesis portends against finding any effect of the Manne program, for at least two reasons (Posner 2008; Stephenson 2009). First, according to a legalist or formalist view, judges apply the law on the books without regard to nonlegal factors. If judges are strictly constrained by statutes and precedents, the Manne program should have no effect. Second, according to an attitudinal view, judges decide cases in line with their partisan affiliation, ignoring legal and policy factors. If Democrat-appointed judges pursue the Democratic Party platform and Republican-appointed judges pursue the Republican Party platform, the Manne program would again have no effect.

Yet in a common-law system, judges have significant discretion in their decisions, and there is a wealth of anecdotal and empirical evidence that nonlegal factors influence decision-making (Posner 2008).¹¹ Moreover, judges are not just politicians (Choi, Gulati, and Posner 2010; Ash and MacLeod 2015). For example, judges appointed by the same political party often dissent against each other, showing the limits of the attitudinal model. Judicial independence arises because judges are skilled and respected

for too much balance . . . [the liberal economists] were cabined by topics far from familiar to them. . . . A liberal economist teaching supply and demand is hardly dangerous" (Priest 1999, 330). Follow-up courses were taught by other economists with a conservative reputation, including James Buchanan and Gary Becker (Butler 1999).

11. As Judge Richard Posner stated in a 2017 *New York Times* interview: "I pay very little attention to legal rules, statutes, constitutional provisions. . . . The first thing you do is ask yourself—forget about the law—what is a sensible resolution of this dispute? . . . See if a recent Supreme Court precedent or some other legal obstacle stood in the way of ruling in favor of that sensible resolution . . . When you have a Supreme Court case or something similar, they're often extremely easy to get around" (Liptak 2017).

professionals with many institutions insulating them from political pressures. Judicial discretion and independence leave space for a training program to influence decision-making. Yet judicial professionalism imposes standards on what types of ideas and information will be persuasive. The variation in exposure to rigorous economics ideas generated by the Manne program lets us test whether these ideas were persuasive for judges and the resulting legal consequences.

To check whether economics ideas are impactful, a simple test is to see whether judges start to use those ideas in their written opinions. Granted, there are many factors contributing to what judges write in their opinions, including strategic and collegial considerations with other judges and the broader policy and political currents of the day (Posner 2008). Furthermore, clerks often contribute significantly to drafting opinions (Choi and Gulati 2004). When taken together across many cases, however, judicial opinions can provide an informative signal of judicial beliefs and intentions (e.g. Posner 1995; Hausladen, Schubert, and Ash 2020).¹² Thus, we will measure the use of economic language using the opinion texts written by federal circuit judges.

Predicting the effect of law and economics on the direction of rulings is more subtle, and hence we take a mostly empirical approach. But the intellectual content of 1970s law and economics suggests some domains to look at. The costs of economic regulation, particularly command-and-control environmental law and legal restrictions on labor markets, were a frequent topic of law and economics scholars, so we would expect effects disfavoring administrative agencies that enforce environmental and labor law. In antitrust, the prevailing law and economics view was that detecting inefficient monopolies was difficult, as the threat of entry would discipline firms even in highly concentrated markets.¹³

In the district courts, we have access to a large data set of criminal sentencing decisions matched to the attending judges. Criminal law was a central focus of law and economics scholar-

12. Richard Epstein, a leading intellectual in early law and economics, has written: “Words are like the critical fortifications on a battlefield. You have to take them in order to win” (Epstein 1995, 366).

13. Henry Manne noted that business support for the program came from its antitrust implications: “I could handle a fund-raising job of raising \$10,000 from ten of them [major corporations]. I wrote to eleven, and I related it heavily to antitrust. . . . Of the eleven I wrote to, within a few weeks I had \$10,000 from ten of them, and the last \$10,000 came in a few weeks later” (Teles 2012, 108).

ship and by Henry Manne himself ([Gindis and Medema 2022](#)), but it was not emphasized in the Manne curriculum. Hence, although this is a high-stakes outcome with major policy significance, the effects on criminal decisions are difficult to predict. One idea would be that judges would follow [Becker \(1968\)](#) and move away from prison toward fines. But federal judges are constrained in imposing fines, so a deterrence approach might recommend increased severity in sentencing. On the other hand, economics training might help judges see the large costs of incarceration on taxpayers and the families of the defendants, as well as the loss in economic productivity when prisoners are not working. Lacking a widely shared model of how economic thinking changes judicial reasoning, we treat these questions primarily as empirical.

Beyond simply influencing the direction in decision-making, it could be that economics provides a toolkit to help judges make the correct decision. In line with this idea, [Baye and Wright \(2011\)](#) show that judges who attended law and economics training were less likely to have their antitrust decisions appealed. Building on this notion, we look at measures of decision quality, such as citations and the probability of promotion to higher courts.

III. DATA

This section describes our data sources and judicial outcome measures. Some additional information and summary statistics are reported in [Online Appendix B](#).

III.A. Overview

There are three layers in the U.S. Federal Court system: the local level (district court), intermediate level (circuit court), and national level (Supreme Court). Federal judges (numbering roughly 180 in circuit courts and 680 in district courts) are appointed by the president, confirmed by the Senate, and serve with life tenure. They are responsible for adjudicating disputes involving common law and interpretation of federal statutes. Their decisions establish precedent for adjudication in future cases in the same court and in lower courts in the same geographic

boundaries. The 13 U.S. circuit courts (courts of appeals) take cases appealed from the 94 district courts.¹⁴

The lower courts handle hundreds of thousands of cases per year—roughly 67,000 in circuit courts and 330,000 in district courts. In comparison, the Supreme Court hears only 100 cases a year. Circuit court decisions make up the vast majority of what law students are reading and what judges are applying.

1. *Circuit Court Cases.* Our key data set is the set of judicial decisions published by the U.S. Circuits of Appeal for 1970 through 2005. The cases come from Bloomberg Law and are cross-checked against other data sets, including the Songer Database, Federal Judicial Center’s Administrator of Courts data set, and information from LexisNexis.

The data set comprises about 200,000 cases with associated opinions. For each case we have the set of judges working on the three-judge panel. Of these judges, we have the authoring judge, as well as whether either of the other judges wrote a dissenting opinion. We have a topic code with eight categories, from which we identify economics cases as those involving labor or regulation.¹⁵ Economics-related cases make up about 30% of the data set.

2. *District Court Cases.* We obtained data on criminal sentencing by federal district judges from Transactional Records Access Clearinghouse (TRAC). Extensive descriptions of these data are available in Yang (2014). The data come merged with judge identity for 1992 through 2003, with approximately 1.03 million cases.

3. *Federal Judge Biographies.* We have biographical information on federal circuit and district judges from the Federal Judicial Center. The data set includes detailed information on

14. The First through Eleventh Circuits preside over groups of three to nine states. The Federal Circuit and D.C. Circuit have specific topic jurisdictions, rather than jurisdiction over groups of states. The vast majority (98%) of circuit court decisions are final. In the remaining 2% that are appealed to the Supreme Court, 30% are affirmed.

15. Non-economics cases are due process, criminal appeals, civil rights, first amendment, privacy, and other. Online Appendix Table A.1 tabulates the case counts by category.

judicial careers, the party of the appointing president, cohort/region of birth, and education.¹⁶

4. *Manne Program Attendance.* To the FJC data we have added the record of attendance by all federal judges to the Manne program. Butler (1999) contains a list of all the judges that had attended through 1998, when the program as such ended (other economics trainings continued but were on more specific topics, e.g., antitrust, or were smaller in scale, e.g., two- to three-day workshops). We supplemented this list with years of attendance from annual reports obtained by Freedom of Information Act requests and through correspondence with the Law and Economics Center at George Mason University.¹⁷

III.B. *Measuring Economics Style in Judicial Language*

The first way that we measure the influence of law and economics on the judiciary is through the written opinions. To this end, we draw on recent methods in natural language processing to construct a measure of economics language using word embeddings applied to an index of terms. The starting point is the corpus of majority opinions written by the judges. The opinions are pre-processed by removing capitalization and punctuation and representing them as lists of words.

We combine these opinion data with an index of law and economics terms used by Ellickson (2000) for identifying law and economics articles in a law journal corpus. This index includes 11 words and phrases that are characteristic of the use of economic analysis in legal contexts.¹⁸ One approach to measuring economics style would be to simply count these terms in judicial opinions. However, the terms are quite rare in judicial opinions, so a count-based measure produces a large number of zeros and

16. See [Online Appendix B](#) for the enumerated list.

17. Due to data limitations, the attendees in 1984 and 1985 were obtained as a single list that could not be disambiguated. Attendance year for that group was assigned to 1984.

18. Ellickson used the following wildcards: *externalit**, *transaction_costs*, *efficien**, *deterr**, *cost_benefit*, *capital*, *game_theo*, *chicago_school*, *marketplace*, *law1economic*, *law2economic*. From these phrases, we obtained the words *externality*, *externalities*, *transaction*, *transactions*, *cost*, *costs*, *efficient*, *efficiency*, *deterrence*, *benefit*, *benefits*, *capital*, *market*, *markets*, *marketplace*, *economic*, *economics*.

fails to capture meaningful variation across opinions (see [Online Appendix Figure A.8](#)).

To address this issue and measure the more implicit, subtle, contextual use of economics reasoning, we draw on word embeddings—a deep-learning method from natural language processing often used for machine translation. Word embedding is a word vectorization algorithm that learns dense numerical representations of words based on co-occurrence statistics in large corpora ([Mikolov et al. 2013](#); [Pennington, Socher, and Manning 2014](#)). A word, normally an item in a large vocabulary, is “embedded” in a lower-dimensional space, where semantically related words tend to appear near each other. For example, “economics” and “markets” will tend to be closer to each other than “economics” and “constitution.” But “economics” and “economy” would be even more similar, and therefore get a higher measured similarity. Thus, word embedding provides a continuous measure of semantic distance, solving the issue of sparseness we find with counting words from a lexicon.

There are several word-embedding algorithms to choose from and a number of options for model training. Our implementation uses the algorithm from [Mikolov et al. \(2013\)](#), with the default settings from [Řehůřek and Sojka \(2011\)](#). Previous work has shown that downstream measurements in social science contexts are not that sensitive to these choices ([Ash, Chen, and Ornaghi 2024](#); [Rodriguez and Spirling 2021](#)). We take words that are semantically close to the Ellickson lexicon and then compute the semantic distance between the judicial opinions and these words. [Online Appendix Figure A.7](#) shows the set of words that are closest to the Ellickson vector, where the size of the word corresponds to the closeness to the Ellickson lexicon in embedding space. They are clearly economics related. [Online Appendix D.1](#) shows example sentences from the judicial opinions that rank highly on closeness to the Ellickson vector. Reassuringly, these sentences are all directly related to economics and most are applying economic reasoning. [Online Appendix Figure A.8](#) shows the distribution of the embedding-based measure and highlights that it is relatively normally distributed, contrasting with the sparseness of a count-based measure that requires exact matches to the lexicon.

For robustness, [Online Appendix D.4](#) describes an alternative measure of economics language constructed using a supervised-learning approach predicting how similar opinions are to opinions on economics cases. The measures are correlated, but not

strongly. We find similar empirical results using the supervised-learning measure instead of the embedding-similarity measure.

III.C. Judicial Decision Outcomes

The rest of our outcomes are coded from judicial decisions. We list them in turn.

1. *Labor and Environment Regulation.* Our main outcome for circuit court decisions is a machine-coded measure for voting against regulatory agencies. We look at regulatory cases where the government is a party to the case. Specifically, we identify labor agencies as including the National Labor Relations Board, Office of Worker's Compensation Programs, U.S. Department of Labor, Federal Labor Relations Authority, and Occupational Safety and Health Administration. The included environmental agency is the Environmental Protection Agency. We construct measures based on judges' voting. We consider voting against the government in a regulation case as in line with a deregulatory policy objective.

2. *Conservative Judicial Decisions.* As a more general measure of conservatism, we have a hand-coded measure of decision direction from the Songer-Auburn database (Songer and Tabrizi 1999). The sample is hand-labeled for vote valence: liberal, conservative, or neutral/hard to code. For example, a conservative vote includes rejecting the defendant in a criminal procedure case, rejecting a plaintiff asserting violation of First Amendment rights, and rejecting the Secretary of Labor who sues a corporation for violating child labor regulations.

An upside of the Songer-Auburn measure is its generality and that it incorporates expert knowledge about law and politics. But we still consider it as a secondary outcome because of the following downsides. For one, hand-coding leads to potential coding errors and subjective decisions, for example, being driven by the reasoning rather than ruling in a case. The biggest downside is that it is only available for 5% of cases, and only until 2002. Furthermore, the sampling was not done uniformly across courts and over time.

Figure II shows the trend in conservatism over time. It has increased since the late 1970s, especially in economics cases (those on labor and regulation).

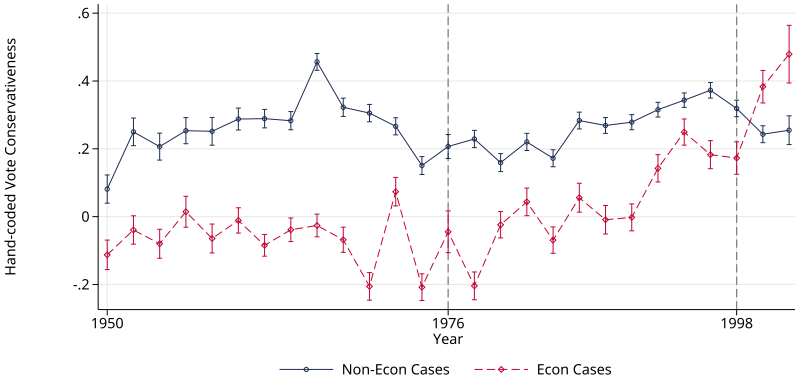


FIGURE II

Increasingly Conservative Rulings in U.S. Federal Courts

Average conservative vote rates in circuit courts using 5% hand-coded Songer-Auburn data, plotted by year and separately by economics and non-economics cases. Error spikes give the standard error of the mean. Data are weighted to treat judge-years equally.

3. *Criminal Sentencing Decisions.* We produce measures of sentencing severity from the district court criminal case records. Besides the judge and sentencing date, we have detailed information on the type of crime and the sentence imposed. We drop life sentences, the death penalty, probation, and fines (all relatively infrequent outcomes) and focus on prison sentence outcomes. The main outcome is a binary variable for whether any prison is imposed. In the [Online Appendix](#), we report supporting results with sentence length in months.

IV. ECONOMETRICS

This section outlines our identification strategy. We use a difference-in-differences design to estimate the short-run effect of Manne attendance relative to colleague judges who attended the Manne program in different cohorts. After providing an overview, we address different threats to identification in subsequent subsections. Additional information on the research design is included in [Online Appendix C](#).

IV.A. Overview

The identification strategy is difference-in-differences, where we estimate within-judge changes in outcomes after Manne attendance relative to colleagues. In particular, we leverage short-run exogenous timing in attendance conditional on application to the program, driven in part by first-come, first-served variation. That is, we compare changes between judges who attend in a given year, relative to judges who will attend but have not yet attended, as well as relative to judges who have already attended.

More concretely, we take as our baseline estimation sample the set of attending judges. Our identification assumption is that within the sample of attenders, changes in untreated potential outcomes are the same across all treatment cohorts, either before or after treatment. This assumption allows us to include the already-treated and not-yet-treated as a control group. That setup is different from the one in [Callaway and Sant'Anna \(2021\)](#), which requires parallel trends in potential outcomes in treated units relative only to the not-yet-treated and/or never-treated. As many judges attend the program early on, restricting attention only to the not-yet-treated would cost too much statistical power. We thus require parallel trends in the already-treated.

Never-treated judges (never-attenders) are excluded from the main sample. As discussed in [Online Appendix C.3](#), never-attenders are not a plausible control group because they are differentially selected on both levels and trends of pretreatment outcomes. Hence, our identification assumption is that timing of attendance is exogenous only conditional on attendance, and we do not require that attenders and never-attenders are on parallel trends.¹⁹ Of course, a limitation of this design is that our results are not informative about how never-attenders would respond to Manne attendance.

Another limitation of this design is that it can only capture short-run effects of the Manne program. Relative to current attenders, not-yet-attenders will attend later and catch up in terms

19. For completeness, regressions including never-attenders in the sample are described and reported in [Online Appendix C.7](#). The main results for effects on labor/environment cases and effects on criminal sentencing are robust to including never-attenders. Economics language, however, does not show a significant treatment effect when never-takers are included in the control group, consistent with never-takers being on a steeper positive trend in learning about law and economics from other sources.

of changes in an outcome variable. Thus, using not-yet-attenders as a control group (rather than never-attenders) means that we cannot estimate long-term effects.

IV.B. Specification

We model outcome Y_{ijct} (a decision, vote, or text metric) for case i by judge j in court (circuit or district) c during year t as

$$(1) \quad Y_{ijct} = \alpha_j + \alpha_{ct} + \gamma Z_{jt}^{\text{post}} + \mathbf{X}'_{ijct} \beta + \epsilon_{ijct},$$

where α_j is a judge fixed effect and α_{ct} is a court-year fixed effect. Z_{jt}^{post} is an indicator variable for the years after judge j attended the Manne program. $\mathbf{X}'_{ijct} \beta$ can include other covariates, as described below, and ϵ_{ijct} is an error term. For the event studies, we report the coefficients and confidence intervals produced from estimating

$$(2) \quad Y_{ijct} = \alpha_j + \alpha_{ct} + \sum_{k \in K} \gamma_k Z_{jt}^k + \mathbf{X}'_{ijct} \beta + \epsilon_{ijct},$$

where now we have indicators Z_{jt}^k , which correspond to the leads and lags of Manne attendance, for $k \in K$.²⁰ Standard errors are clustered by judge.

The core identification assumption is the standard condition that errors, conditional on controls, are mean zero:

$$E[\epsilon_{ijct} | \alpha_j, \alpha_{ct}, \{Z^k\}, X_{ijct}] = 0.$$

We assess various threats to this assumption. With judge fixed effects and randomization of case assignment within court-year, the primary threat is unobserved judge-time variables that are correlated with both the outcome and attending the Manne program.

20. Following [Borusyak, Jaravel, and Spiess \(2024\)](#), event-study coefficients for the first year in the window ($k = -W$) and the year before attendance ($k = -1$) are excluded. As discussed in [Borusyak, Jaravel, and Spiess \(2024\)](#) Proposition 1, in the case of no never-treated units and inclusion of both unit and time fixed effects, dropping just $k = -1$ still leads to collinearity of the year fixed effects with the time-to-treatment dummies. Results are robust to dropping other early leads and lags ([Online Appendix Figures A.15 and A.29](#)), or to using other lengths of event-study windows ([Online Appendix Figures A.11 and A.25](#)).

IV.C. Conditional Random Assignment of Cases to Judges

The court \times year fixed effects hold constant any time-varying court-level factors. In particular, the fixed effects are important because cases are randomly assigned within a court-year block, so including court-year fixed effects makes case portfolios comparable across judges. Otherwise, our observed effects could be driven by changes in the types of cases that treated judges rule on, rather than changes in their decisions. Furthermore, to address the issue that some courts and years have more cases than others, we reweight case observations such that judge-years count equally (Solon, Haider, and Wooldridge 2015).

Online Appendix C.1 provides further detail on case assignment. Previous work has assessed judge randomization through interviews of courts and orthogonality checks on observables (Sunstein et al. 2006; Boyd, Epstein, and Martin 2010; Chen and Sethi 2011). In our setting, we can check whether Manne training has an effect on the types of cases that judges sit on or author. Online Appendix Figure A.3 shows that Manne judges are not more likely to sit on cases published on economics topics, and they are not disproportionately selected from the three-judge panel to author more economics cases.

IV.D. Endogenous Selection of Judges into Attendance and Timing

The central concern is endogenous selection into the program. With judge fixed effects, the primary threat to identification is that there are time-varying judge characteristics that influence both attendance and judicial decision-making. As discussed in Butler (1999), there is little selection on the program side, as no judges were specifically recruited. On the judges' side, however, it could be that judges who at some point decide they like economics or conservatism then decide due to this ideological shift to attend the Manne Program.

We assess selection into attending Manne on other observable judge variables in Online Appendix Tables A.3 and A.4. We use all control variables selected using elastic net as predictive of attendance (with regularization parameters chosen by cross-validation). Unsurprisingly, there are significant differences between Manne and non-Manne judges (columns (1) and (2)). Republican appointees are a little more likely to go, but (as noted in

Section II.B), many Democrat appointees also attended and endorsed the program.

Importantly, many of the covariates that predict attendance do not predict the specific year of attendance. Notably, Republican affiliation (from the nominating president) is not a statistically significant predictor for timing (and is even dropped by elastic net in the circuit courts). The covariate balance in ever-attenders lends credence to the exogenous-timing assumption, driven in part by the first-come, first-served rule governing attendance. Up until the late 1980s, classes were oversubscribed and the judges applying later were bumped to subsequent sessions. Most of the circuit judges in our sample attended during this early heyday period. In these cohorts, especially, opportunities were reduced for selection of specific types of judges to specific episodes of the course, suggesting that timing of attendance was exogenous given application. The exception to this is age, as older judges were more likely to attend and more likely to attend earlier.

Online Appendix Table A.5 provides complementary balance checks based on the main outcome variables for use of economics language and labor/EPA decisions. First, we compare never-attenders to not-yet-attenders on the same court at the same time (i.e., including circuit \times year fixed effects) and show that they differ significantly in both outcomes. Second, we limit the sample to not-yet-attenders and show that neither outcome varies significantly with attendance year. Overall, this provides additional support for our identification assumption of parallel trends when limiting to the sample of ever-attenders.

To help address issues of selection into cohorts, we adjust for observables as follows. We allow for flexible time effects of all elastic-net-selected characteristics that predict the timing of attendance. Specifically, we set $\mathbf{X}_{ijct} = \lambda_t \mathbf{X}_j$, where \mathbf{X}_j includes judge covariates selected by elastic net as predictive of the timing of Manne attendance (see Online Appendix C.2), and λ_t includes time fixed effects corresponding to five-year periods.²¹

21. We also constructed averages of pretreatment or pre-1976 outcome variables by judge as potential selection variables for elastic net. They were not selected. The choice of five-year periods for λ_t is for convenience; short time periods (e.g., a year or a month) are too restrictive and absorb too much variation for the event studies to be estimated properly. Results are robust for a period of three years or more (see Online Appendix Figures A.16 and A.30).

As an alternative approach to address selective timing, we report specifications limiting to the early “heyday” period, when classes were oversubscribed and the first-come, first-served rule was binding. Still, we cannot fully rule out endogenous timing of attendance along unobservable judge characteristics correlated with judicial outcomes.

IV.E. Negative Regression Weighting?

Our identification assumption requires parallel trends in the already-treated, which rules out time-varying treatment effects and associated negative weighting issues that are the concern of the recent difference-in-differences literature (e.g. [Goodman-Bacon 2021](#)). [Online Appendix C.6](#) presents diagnostics from [De Chaisemartin and d'Haultfœuille \(2020\)](#) and [Jakiela \(2021\)](#) to show that negative weighting is only occurring for a small part of our sample, and it does not appear that effect heterogeneity is a major concern ([Online Appendix Table A.7](#)). This combination of limited negative weighting and limited heterogeneity gives us confidence that our design is not vulnerable to misspecification of the control groups, despite our lack of a clean set of never-treated judges. See [Online Appendix C.3](#) for a discussion of this design choice.

IV.F. SUTVA Violations

Another concern is that judges are communicating among themselves, particularly in a circuit. As discussed, judges serve on three-person panels on a variety of economic and noneconomic issues and interact a great deal while deciding cases. Further, circuits prioritize within-circuit precedents as legal guidance in decision-making. Controlling for circuit-year fixed effects is necessary for random assignment of cases but raises concerns about spillovers. [Online Appendix C.5](#) shows there is little evidence of spillovers.

To address concerns about stable unit treatment value assumption (SUTVA) violations, we estimate a specification that adjusts for peer share. For each judge j in court c at time t , we define \bar{Z}_{ct}^{-j} as the share of peer judges (weighted by caseload) on the same court (besides j) who have attended the Manne program. Given that there are likely heterogeneous effects of peer share across

judges, we set $\mathbf{X}_{i,jct} = \alpha_j \bar{Z}_{ct}^{-j}$, allowing for a judge-specific effect of peer attendance.²²

V. RESULTS

This section reports the empirical results on how attending the Manne program affected judge decisions. First, we look at effects on the use of economics language in the circuit courts, and second, we examine circuit court decisions about regulatory agencies. Third, we look at results for criminal sentencing. This section reports the main event-study estimates. [Section VI](#) summarizes the magnitudes ([Tables I and II](#)). Further supporting material and results are reported in [Online Appendices D](#) (economics language), [E](#) (regulatory decisions), [F](#) (conservative voting), [G](#) (antitrust), [H](#) (criminal sentencing), and [I](#) (additional supporting results).

V.A. *Effect of Economics Training on Judge Opinion Language*

We start by answering the basic question of whether judges who attend economics training actually use the language of economics in their opinions. We look at the vector similarity of a case to a lexicon of economics language in word-embedding space, as described in [Section III.B](#). The sample includes majority-opinion authors and excludes nonauthor panel members.

[Figure III](#) reports the event-study estimates for the embedding-based measure of economics language. Formally, the markers give the point estimates for $\hat{\gamma}_k$ from [equation \(2\)](#) limiting to the event-study window, with 95% confidence intervals computed using the associated standard errors (clustered by judge). The first specification (blue circles) reports the baseline with judge fixed effects and circuit-year fixed effects. The second specification (red diamonds) reports the baseline with the addition of elastic-net-selected controls (predicting time of attendance), interacted with time fixed effects. The third specification (green triangles) is the same as the baseline but limited to the early period (pre-1987) when courses were oversubscribed. The fourth specification (purple squares) is back to the baseline sample and

22. These controls are included in the main results and all regression results. For some of the event studies reported in the [Online Appendix](#) with a small sample (for example, the results with the hand-coded conservative voting outcome), the peer-share controls specifications are too noisy and therefore are excluded.

TABLE I
REGRESSION ESTIMATES: THE EFFECT OF THE MANNE PROGRAM IN CIRCUIT COURTS

	Economics language			Voting against regulators				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Manne	0.363 (0.1340)	0.430 (0.1805)	0.010 (0.0947)	0.440 (0.1559)	0.162 (0.0675)	0.175 (0.0739)	0.169 (0.0474)	0.177 (0.0649)
<i>N</i>	5,261	3,214	10,305	4,108	2,650	2,082	4,244	2,581
Conservative vote (econ case)								
Post Manne	0.265 (0.1316)	0.126 (0.1177)	0.043 (0.0710)	0.070 (0.0722)	0.059 (0.0741)	0.035 (0.0928)	0.022 (0.0476)	0.064 (0.0893)
<i>N</i>	804	586	1,589	766	2,408	1,538	4,859	1,959
Court-year fixed effects	X	X	X	X	X	X	X	X
Judge fixed effects	X	X	X	X	X	X	X	X
Sample	Short run	Short run	Long run	Long run	Short run	Short run	Long run	Long run
Pre-1987		X		X		X		X

Notes. Summary of the estimated effects of Manne training on circuit court case outcomes, indicated by column headers. Specifications are the same as detailed in the associated regression tables for each outcome. All regressions include court-year and judge fixed effects. For economics language, the sample is limited to economics cases and opinion authors. "Short run" indicates the event-study sample. "Long run" includes ever-attenders for all years. Pre-1987 means limiting to years 1986 and earlier. Standard errors (in parentheses) are clustered at the judge level, and observations are weighted to treat judge-years equally.

TABLE II
REGRESSION ESTIMATES: EFFECT OF THE MANNE PROGRAM IN DISTRICT COURTS

	Any prison given		Sentence length	
	(1)	(2)	(3)	(4)
Post Manne	0.0617 (0.0202)	0.035 (0.0135)	-0.026 (0.1724)	0.045 (0.0709)
N	70,784	260,516	70,482	259,107
Court-year fixed effects	X	X	X	X
Judge fixed effects	X	X	X	X
Sample	Short run	Long run	Short run	Long run

Notes. Summary of the estimated effects of Manne training on district court outcomes, indicated by column headers. Columns (1) and (2) are OLS, while columns (3) and (4) are Poisson models. Regressions include court-year and judge fixed effects. “Short run” indicates the event-study sample. “Long run” includes ever-attenders for all years. Standard errors (in parentheses) are clustered at the judge level. OLS regressions are weighted to treat judge-years equally.

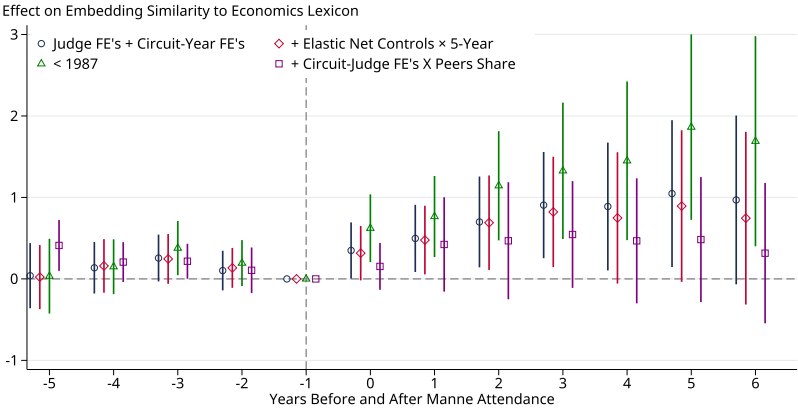


FIGURE III
The Effect of the Manne Program on Economics Language

The figure presents event-study estimates of the effect of Manne attendance on word-embedding similarity to the law and economics lexicon (from Ellickson 2000). The sample is limited to economics cases and case authors in the event-study window. Regressions include judge and circuit-year fixed effects (blue circles), with additional specifications adding elastic-net-selected controls interacted with time fixed effects (red diamonds), limiting to the pre-1987 period (green triangles), and adding peer-share controls interacted with judge fixed effects (purple squares). Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals, with standard errors clustered by judge.

includes peer Manne attendance shares interacted with circuit-judge fixed effects.

Across the four specifications, we see that judges who attended the Manne program tended to increase their use of economics style in written judicial opinions. There is a discrete jump in the years after attendance, and the post-attendance effect is significant for all series. The effect is persistently positive, and significant for three years after the program.²³ Meanwhile, there are no significant effects in the pre-trend period. The effect is notably larger when limiting to the early period (green triangles), reflecting that the effects on language are stronger in the early period (when law and economics is relatively new) and weaker in the later period (when law and economics is already relatively familiar). Furthermore, for all specifications, we run the test from [Rambachan and Roth \(2023\)](#) and rule out substantial nonlinear pre-trends ([Online Appendix Figure A.10](#)).

[Online Appendix D.2](#) reports an extensive set of supporting results and robustness checks on the use of economics language in judicial opinions. Complementary difference-in-differences regression results are reported in [Online Appendix Table A.9](#) (see [Table I](#), columns (1)–(4) for the baseline specifications). The main estimates looking at short-run effects on attenders (columns (1)–(11)) are consistent with the event-study estimates and also robust to controls for judge party (interacted with year) or case topic (columns (3) and (7)). The estimated effect is much larger and more significant when limiting to courts and years with relatively few (below median) post-Manne judges (column 5). The estimate is not robust to dropping the weights, which upweights courts and years with more cases (column 8); that is in part mechanical as the Manne effect is concentrated in the early period and the caseload is larger in the later period. The baseline result holds with winsorized weights, however (column (9)). Statistical significance is not sensitive to alternative specification of standard errors (columns (10) and (11)). The long-run effects (columns (12)–(22)), meanwhile, are generally not significant. Overall, the results suggest that Manne attendance increases the short-run

23. For all of the specifications, here and in subsequent results, we see somewhat larger confidence intervals at the beginning and end of the period. This is due to an unbalanced sample of judges with fewer judges at the tail ends, as some judges enter or leave the court within six years of Manne attendance.

use of economics-oriented language by about 0.36 standard deviations.

As described in [Online Appendix D.3](#), we produced similar measures of embedding-based economics similarity in the district courts (that is, the trial-court level below the circuit courts). We collected the universe of published opinions and matched them to the authoring judge ($N = 508,325$). We produced similar event-study estimates for the effect of Manne attendance on economics language in the district courts. As in the circuit courts, there is a positive and significant effect of attendance on economics language.

[Online Appendix D.4](#) reports analogous circuit court results for the alternative measure of economics language using a supervised learning approach. That outcome is a machine prediction, based on the text of an opinion, of how similar it is to an opinion written on an economics topic. The results are consistent, with a statistically significant positive event-study effect from the Manne program ([Online Appendix Figure A.20](#); [Online Appendix Table A.10](#)).

[Online Appendix D.5](#) reports supporting results with additional language measures. First, it could be that judges are picking up more academic language in their approach to law, rather than a more economic approach. To check for this, we produce a measure of noneconomic academic language: similarity to a corpus of law journal articles published in recent decades. We find no effect of Manne attendance on a legal scholarship style ([Online Appendix Figure A.21](#)), consistent with an economics approach mattering more than an academic approach. Second, we would like to know whether judges are adopting the conceptual reasoning of economics, the statistical/quantitative tools, or both. We produce a measure of statistical/quantitative language based on distinctive terms, and we find that there is no effect on the use of statistical/quantitative language ([Online Appendix Figure A.22](#)). Hence, the effect on language seems to be more on the conceptual use of economics, rather than the use of econometric analysis.

V.B. Effect on Circuit Judge Decisions in Regulatory Issues

Next we look at voting against federal regulatory agencies, particularly those entrusted with enforcing labor and environmental regulation. We focus on two types of agencies that the law and economics movement specifically criticized: the labor

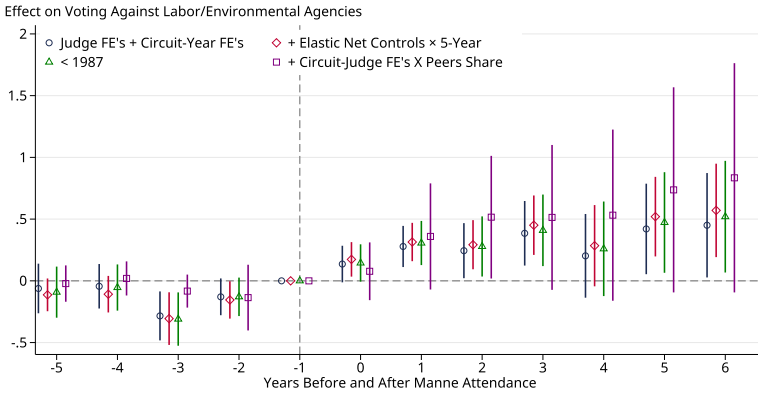


FIGURE IV

Effect of the Manne Program on Votes Against Labor/Environmental Agencies

The figure illustrates event-study estimates of the effects on voting against a government agency on labor and environmental issues, relative to the year before attendance at Manne economics training. Regressions include judge and circuit-year fixed effects (blue circles), with additional specifications adding elastic-net-selected controls interacted with time fixed effects (red diamonds), limiting to the pre-1987 period (green triangles), and adding peer-share controls interacted with judge fixed effects (purple squares). The sample is limited to the event-study window. Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals, with standard errors clustered by judge.

agencies (especially the National Labor Relations Board and Department of Labor) and the EPA. Our outcome is whether a circuit judge votes against one of these agencies on appeal.

Figure IV shows the event-study estimates for equation (2) with votes against regulatory agencies as the outcome. As with the language outcomes, we report a baseline specification (blue circles), with elastic net controls interacted with time (red diamonds), limiting to the early period (green triangles), and with judge-specific peer attendance share controls (purple squares). The sample is limited to the event-study window. Across specifications, we see that Manne-trained judges exhibit a significant increase in propensity to vote against federal labor and environmental regulatory agencies. The effect is quite robust to the inclusion of elastic-net-selected controls, limiting to the oversubscribed period, or adjusting for peer attendance by judge.

For the first three specifications, we see a statistically significant negative pre-trend coefficient three years before attendance. These pre-trends could indicate that our estimated Manne effects

reflect selection bias, where judges moving in that direction already enroll in Manne. However, the significant pre-trend is not robust to minor variations on our baseline specifications, nor do we see similar pre-trends in any other outcomes. For example, the pre-trend is not observed when including the peer-attendance controls (spec. 4 with purple squares; see [Online Appendix C.5](#) for additional discussion on potential peer spillovers). [Online Appendix Figure A.23](#) reports some further robustness checks on the pre-trend. One of the drivers is imbalance in the sample around attendance; when we add indicators for missing observations in the pre-Manne years, or when we add pre-Manne average voting outcomes, interacted with time, the pre-trend becomes insignificant, while our main effect remains highly significant. However, according to the test from [Rambachan and Roth \(2023\)](#) we cannot rule out substantial nonlinear pre-trends in this outcome ([Online Appendix Figure A.24](#)). Overall, we cannot fully rule out that these pre-trends indicate endogenous timing, in which judges are experiencing preexisting shifts in a more conservative direction that are reinforced by differential Manne attendance.

[Online Appendix E](#) provides additional results and robustness checks on the Labor/EPA analysis. The regression results for [equation \(1\)](#) are reported in [Online Appendix Table A.11](#) (see [Table I](#), columns (5)–(8) for the baseline specifications). The results hold across a range of specifications, both in the short run (columns (1)–(11)) and long run (columns (12)–(22)). The results are robust to including alternative controls, different samples, and different clustering. The results do not hold with unweighted regressions where courts and periods with more cases are weighted more in the estimates (columns (8) and (19)), but the baseline results hold with winsorized weights (columns (9) and (20)). Overall, the results are consistent with a 15% increase in the probability of voting against labor and environmental regulation agencies after attendance at the Manne program.

Next, to complement the results on regulatory decisions, we undertake a similar analysis using alternative outcome data in a smaller sample of cases (5% through 2002) where the ruling has been hand-coded as conservative or liberal by the Songer-Auburn project. [Online Appendix Figure A.31](#) shows the event-study estimates for the effect of Manne attendance on conservative voting, where the coefficients in red come from the subset of economics-related cases (labor and regulation), and the coefficients in teal come from the subset of non-economics-related

cases (everything else). From the event-study figure, we can see a clear positive trend break in the conservativeness of votes in economics cases, relative to non-economics cases, after Manne program attendance. The difference between the trends persists over five subsequent years.

The accompanying regression results (Table I, columns (9)–(16) and Online Appendix Table A.12) show that in economics cases, Manne attendance is associated with a 30 percentage point increase in conservative vote rate in the short run (within six years). There is no effect on cases unrelated to economics. Given the relatively small sample size, however, these results are less robust and should be interpreted with caution (see Online Appendix F).

Finally, we consider the category of antitrust law, a priority of the Chicago School and Henry Manne. Our outcome is a newly collected data point coded as voting against antitrust rights, unfortunately only available for a small number of cases (see Online Appendix G). Online Appendix Table A.14 reports difference-in-differences regressions, and we observe mostly noisy and null estimates. The estimated coefficients are mostly positive, and one of them is statistically significant. There are no statistically significant negative effects. Although these results are mixed, overall they are more consistent with the Manne program's focus on more permissive, rather than more aggressive, antitrust enforcement.

V.C. Effect of Economics Training on Criminal Sentencing

Now we move from appellate decisions in the circuit courts to criminal sentencing decisions in the district courts. Our district court sample is considerably later than the appellate court sample, beginning only in 1992, so the judge pool is more likely to have been influenced by law and economics in law school, muting the effect of the program. District judges also decide sentences individually and every year, so the influence of previous-attending peers or sample imbalance are less likely to contaminate our estimates. Further, the Manne program's effect on criminal sentencing is somewhat difficult to predict, as an incentives approach might recommend stronger penalties to increase deterrence, or a reduction in penalties given their social costs, or fines rather than jail (see Section II.C).

Here we focus on the main sentence outcome of each district court case, conditional on conviction: whether a defendant is

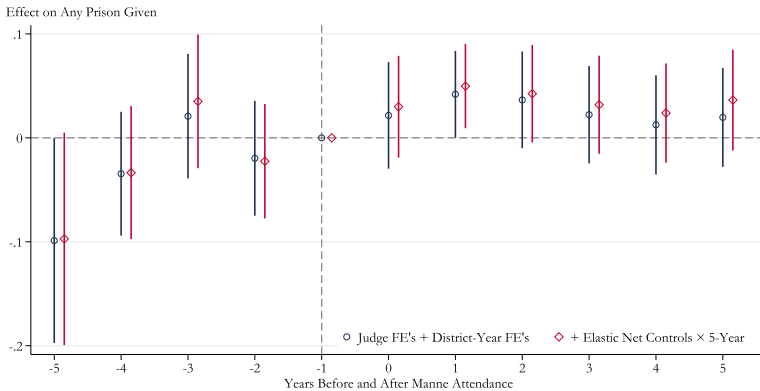


FIGURE V

The Effect of the Manne Program on Giving a Prison Sentence

The figure illustrates the event-study estimates of the effect of Manne attendance on criminal sentencing outcomes in district courts, 1992–2003. The outcome is any prison sentence given. Regressions include judge and district-year fixed effects (blue circles), plus elastic-net-selected controls interacted with time fixed effects (red diamonds). The sample includes ever-attenders for all years. Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals.

incarcerated. Given mandatory sentencing guidelines during this time period (1992–2003), judges had limited discretion in the actual length of the sentence imposed. Therefore we would not expect much of an effect on sentence length, if any. Results with that outcome are reported in the [Online Appendix](#).

The event-study estimates from [equation \(2\)](#) for giving a prison sentence are reported in [Figure V](#). We report two specifications: the baseline (blue circles) includes judge and district-year fixed effects, and the second specification (red diamonds) adds elastic-net-selected judge characteristics (predicting time of attendance) interacted with time fixed effects, both in the ever-attenders sample.²⁴ For both specifications, we see a positive

24. The coefficient plot has two fewer specifications than those above. First, we don't have the pre-1987 specification because the district court data do not go back that far. Second, we don't have the judge-specific peer-share controls because, unlike circuit judges, district judges work individually and do not sit on panels. Given the shorter time period for the district court data, we use a shorter event-study window (five years) and include all ever-attenders in the sample. Judge-year cells with few observations (bottom 1%) are dropped.

jump in the outcome in the year of and after attendance in the Manne program. In the two years after attendance, the effect is positive and significant. By the third and fourth year, it is still positive but not significant. In the years before attendance, we estimate zeroes.

[Online Appendix H](#) includes additional results and checks for criminal sentencing. [Online Appendix Table A.15](#) reports the difference-in-differences estimates for how Manne attendance affected district judge sentencing (see also [Table II](#)). We find evidence of stricter penalties by judges after attending Manne, even when using the full sample of judges including never-attenders in the control group. The results are robust to including party-year fixed effects or charge fixed effects, to the use of unweighted regressions, and to alternative clustering. However, we cannot rule out the presence of nonlinear pre-trends, according to the test from [Rambachan and Roth \(2023\)](#) ([Online Appendix Figure A.34](#)). In the [Online Appendix](#), we show that the Manne program's effect on criminal sentencing was mainly on the extensive margin, with little effect on overall sentence length ([Online Appendix Figure A.33](#), [Online Appendix Table A.16](#)).

VI. DISCUSSION

VI.A. *Quantitative Magnitudes*

This section provides some discussion of the evidence reported in [Section V](#), starting with a summary of the effect magnitudes. [Table I](#) reports the baseline difference-in-differences regression estimates for the circuit courts. We have results for economics language (columns (1)–(4)), voting against regulatory agencies (columns (5)–(8)), conservative voting in cases related to economics (columns (9)–(11)), and conservative voting in cases unrelated to economics (columns (12)–(16)). For each of the four outcomes, we report the short-run effects on attenders (limiting to the event-study window) and the long-run effects on attenders (including all years). We also report results for the pre-1987 hey-day period.

The Manne program had a large effect on economics language in the early period—about four-tenths of a standard deviation (columns (2) and (4)). When including all years, the effect is smaller (column (1)) and dies out in the long run (column (3)). The effects on voting against regulatory agencies are more

stable (columns (5)–(8)), with Manne attendees increasing their conservative vote rates about 16–17 percentage points—again about half a standard deviation. For conservative votes in economics cases, we estimate large positive coefficients; the effect of 0.27 in the short run is about two-thirds of a standard deviation and statistically significant. The estimates for the other specifications are somewhat imprecise, however. For conservative votes in non-economics cases (columns (13)–(16)), meanwhile, the coefficients (between 0.02 and 0.06) are always smaller than for economics cases, about one-tenth of a standard deviation and not statistically different from zero.

Table II reports the main regression results for the district courts. We analyze the any-prison-given outcome using OLS and the sentence-length outcome using a Poisson model. We see that after attendance, incarceration rates increased by about 6.2 percentage points in the short run (column (1)) or about 3.5 percentage points in the longer term (column (2)). Both coefficients are statistically significant and the short-term effect is about one-eighth of a standard deviation. Meanwhile, the Poisson coefficients for sentence length (columns (3) and (4)) suggest that sentence lengths did not change after attendance, reflecting that judges did not have much discretion over sentence lengths at this time due to strict sentencing guidelines.

These results suggest that the Manne program was effective at persuading judges. Focusing on conservative voting in economics cases and taking the most conservative estimate of $\hat{\gamma} = \Delta y = 0.043$ (Table I, column (11)), we can calculate a persuasion rate and compare it with other ideological or persuasion-based interventions (DellaVigna and Gentzkow 2010). Assuming that all attenders are exposed ($\Delta e = 1$), the persuasion rate for conservative voting is

$$p = 100 \times \frac{\Delta y}{\Delta e} \cdot \frac{1}{(1 - y_0)}.$$

Setting $y_0 = 0.46$, the mean outcome for the pre-attenders in economics cases, we have $p = 8\%$. This persuasion rate is not that different from other interventions that might influence policy beliefs, such as the Fox News effect estimated by DellaVigna and Kaplan (2007) for Republican voting ($p = 11.6\%$), or the effect estimated by Gerber, Karlan, and Bergan (2009) of a 10-week subscription to the *Washington Post* on Democratic vote share ($p = 19.5\%$).

To put this another way: from the mid-1970s to the early 2000s, the Songer database documents an increase of 0.2 in the likelihood to vote conservative rather than liberal. Taking the Manne coefficient of 0.043 and multiplying by 0.4 (the share of circuit judges who attended) renders a substantial fraction (0.017) of the overall 0.2 shift. Taken together, these numbers imply the Manne program could account for 9% of the rise in judicial conservatism.

VI.B. *Broader Policy Influence of the Manne Program*

Our econometric strategy only identifies the effects of Manne attendance on the ever-attending circuit judges, but the policy effects of the program went far beyond this group. For starters, over 1976–2005, Manne-trained circuit court judges sat on panels and voted on 84,286 circuit court decisions. Of these, the Manne-trained judges authored the lead opinion in 28,720 cases. The authored opinions have been cited as precedent over 300,000 times by subsequent circuit court cases, and their arguments have been widely read by judges, lawyers, and law students. In the same time frame, 129 lead opinions by Manne-trained judges were appealed and then affirmed by the Supreme Court, becoming binding precedent on all circuits. Meanwhile, there were 61 circuit cases where the Supreme Court agreed with a dissenting Manne judge and reversed a circuit decision. Starting in the latter years of the Manne program (after 1988) and until the nomination of current Chief Justice John Roberts in 2004, two out of the four Supreme Court appointees were Manne-trained. Supreme Court decisions are binding U.S. law, so the ultimate influence of the Manne program on policy is likely larger than what we can measure with the design in this article.

To understand the policy influence of Manne-trained judges across tens of thousands of cases, consider the following specific example. In *Matter of Bell Petroleum Services, Inc.* (5th. Cir. 1993), the EPA sought to hold local producers liable for costs of building an alternative water supply after groundwater contamination. Manne-trained judge E. Grady Jolly rejected the EPA's decision to implement an alternative water supply, calling it "arbitrary and capricious." In particular, Jolly objected to the EPA's regulatory overreach and justified the ruling as a deterrent against the EPA's "unrestrained spending discretion." *Bell* is one of the most influential federal cases on the environment,

cited more than 200 times by courts inside and outside the Fifth Circuit.

A second example, *Square D Co. v. Niagara Frontier* (2d. Cir. 1985), demonstrates the influence of economics ideas in antitrust. In that case, claimant shipping companies sued carriers and a rate-making bureau who had conspired to unfairly set rates for commerce between the United States and Canada. Manne graduate Henry J. Friendly refused to allow special punitive damages designed to deter antitrust violations, agreeing with the defendants that standard compensatory damages provided a sufficient deterrent. On appeal, Friendly's ruling was affirmed by the Supreme Court, becoming final binding precedent on all U.S. judges.

Precedents like these exert influence far beyond the direct applications of each case. Besides judges following precedents, they influence legislators and regulators who have to write statutes and rules in the shadow of the law. Law students read these precedents and the arguments can be reused far in the future. Finally and not least, the economics institute for federal judges was just one of many judge training programs introduced by Manne and the Law and Economics Center for the federal courts, state courts, and international courts (Butler 1999).

VI.C. *Why Economics?*

Why might economics education have been so effective at persuading judges? Judges are generalists, called on to decide high-stakes decisions on various policy issues. In turn, economics is a general framework, and the Manne instructors taught economics as a general way of thinking, rather than as a set of specific lessons by legal domain. That is reflected in the sample agenda (Online Appendix Figure A.1), showing the general coverage of introductory economics, law and economics (e.g., property rights, corporations), some statistics, and a handful of more normative seminars. The Manne curriculum did not simply advocate decisions that favor a given business constituency or specific partisan direction. The judges, as skeptical professionals, would have easily seen through clearly biased course material.

Instead, bringing in the principles of economics, delivered by credited economists with academic reputations to defend, might boost the persuasive impact of potentially biased material. In the framework from Gentzkow and Kamenica (2011), the Manne

program's curriculum corresponds to a signal structure with commitment—regardless of the true state, the instructor is bound (perhaps by academic or scientific norms) to reveal the results of the policy analysis. In the relevant example from [Gentzkow and Kamenica \(2011\)](#), the principal will choose either an informative signal or none at all. Thus, even if the judge knows the economist is biased for a particular outcome, the economist can still influence the judge to vote in the preferred direction some of the time, and the shift can happen precisely because the economist is committed to revealing the signal generated by the economic analysis. Economics, as a rigorous social science that ties the hands of practitioners, becomes more powerful than other idioms as a tool for guiding the decisions of sophisticated agents.

These points help explain the supply side of the “why economics” question—that is, why the organizers and supporters of the Law and Economics Center would set up the Manne Program. But what about the demand side? What did the judges gain by attending? It could have provided tools to make their opinions more persuasive, consistent with the [Baye and Wright \(2011\)](#) result that Manne-trained judges are less likely to be reversed in antitrust cases.²⁵ For the judges themselves, there were clear perceived benefits. From the records of testimonials, we know that the Manne program was attended and celebrated by many Democratic-affiliated judges, including Ruth Bader Ginsburg (see [Online Appendix A](#)).

A number of other factors could have boosted the desirability and impact of the Manne Program. We have seen from the archival documents that the Law and Economics Center frequently followed up with judges by mailing them material and inviting them to subsequent events and workshops. The Manne program may have helped establish links between judges and the broader set of conservative legal networks, such as the Federalist Society. Consistent with that notion, [Online Appendix Table A.17](#) shows that Manne-trained judges are in fact more likely

25. In [Online Appendix Table A.17](#), we show that Manne attendance by district judges did not appear to increase the probability of promotion to higher appellate courts. Forward citation rates to a judge's opinions, which reflect the usefulness of an opinion to future judges ([Ash and MacLeod 2021](#)), do not increase after Manne attendance ([Online Appendix Figure A.38](#)). Further, the use of quantitative or statistical language does not change relative to not-yet-attenders post-attendance ([Online Appendix Figure A.22](#)).

to become members of the Federalist Society, but only among Republican-appointed judges. Finally, the establishment of ties between judges and economics-minded law professors could have helped judges hire clerks with a more conservative or more economics-oriented outlook, which would influence decisions and language (Bonica et al. 2019).²⁶ Although these features are not special to economics, they might have been complementary in encouraging the program's impact.

VII. CONCLUSION

The U.S. law and economics movement shifted legal outcomes in U.S. courts. After economics training, judges used economic analysis in their written opinions, rendered conservative rulings related to regulation, and imposed tougher criminal sentences. When ideas move from economics into law, there are important policy consequences.

In the case of the Manne program, notwithstanding efforts for balance (Butler 1999), the effects of economics ideas were in a conservative policy direction. This is perhaps unsurprising, given the Manne program's emphasis on 1970s law and economics approaches, which applied the simplest price theory arguments. A training course for judges based on more recent generations of law and economics scholarship would be quite different, as the field has become more open to behavioral factors and much more empirical. Still, nothing in the Manne program was outside the bounds of the economics discipline. Normative assessment of these policy shifts likely depends on one's views about the efficiency of the law and economics interpretations of various legal rules and the cogency of prior legal thinking.

This work adds to the literature exploring constitutional constraints on policy making (Seabright 1996; Besley and Coate 1997) and the importance of ideas versus institutions in determining policy (Romer 2002; Rodrik 2014). For example, the expansion of economic regulation is one hallmark of the modern

26. Using data on law clerks from Bonica et al. (2019), we tried to check for systematic differences among clerks for Manne judges. The data only go back to 1995, however, limiting what analysis could be done. We did find that judges who had ever attended Manne were more likely than never-attenders to recruit clerks from George Mason Law School (the headquarters of the Law and Economics Center).

administrative state, yet the determinants of this sort of state power in U.S. society are not well understood (Hamburger 2014). The role of ideas or ideology, as opposed to interest-based lobbying or partisanship, are relatively unexplored by economists in terms of theory and evidence (Bénabou 2007). Yet intellectual commitments—such as a judge’s nonpartisan commitment to a strict interpretation of the Constitution—are frequently invoked in legal discourse. Quantifying the role for legal schools of thought, such as law and economics, is a key contribution of this study.

The results on the Manne Program invite broader questions on the role of training and education programs for judges and other public officials. Are such effects replicable by other programs? What is the proper role of economists and other social scientists in participating in such programs? Should there be more limitations or greater disclosure requirements? Did the Manne Program’s financial donors get a return on their investment? Are other schools of legal thinking (e.g., originalism or critical legal studies) similarly influential for judicial decision-making? These are important questions for policy makers and for future research.

SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

DATA AVAILABILITY

The data underlying this article are available in the Harvard Dataverse, <https://doi.org/10.7910/DVN/XATYFX> (Ash, Chen, and Naidu 2025).

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