Ideas Have Consequences

The Impact of Law and Economics on American Justice

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Abstract

This paper 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.

Keywords: Judicial Decision-Making, Ideas, Law and Economics.

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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 paper 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 (Benabou, 2007), can potentially influence policy.

Law and economics comprises 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 to the legal communities in other countries, in the United States the influence of economics among law professors and judges is well-documented (Posner, 1987; Ellickson, 2000; Posner, 2008).

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 paper analyzes the effect of economics training on federal judges by linking records on Manne attendance (1976-1998) with a comprehensive dataset 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 differences-in-differences design, leveraging the fact that recruitment into the program was oversubscribed and on a first-come-first-serve basis, minimizing opportunities for selection in response to short-run changes in judge 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. While we cannot completely

[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, 2019). 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.

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, interacted with year fixed effects, that predict the timing of attendance, and provide a battery of auxiliary specification checks in Appendices.

The setting is relevant for economic policy because American 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 2007). Relatively few district court cases are appealed to the circuits, while fewer than one percent 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 towing 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, 2007), or else invariant judge characteristics such as political affiliation, average decision tendencies, campaign donation tendencies, or demographics (e.g. Cameron, 1993; Martin and Quinn, 2002; Epstein, Landes, and Posner, 2013; Ash, Chen, and Ornaghi, 2021; Bonica and Sen, 2021).

This paper 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, 2020). 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 paper is organized as follows. Section 2 gives background on the law and economics movement and the Manne program. Section 3 explains our various sources of data and measurement strategies. Section 4 describes our empirical approach. Section 5 reports the results, while Section 6 discusses their implications. Section 7 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 (e.g. 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.

[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 which 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 tradeoffs between redistribution and efficiency increased support for progressive taxes (see also Stantcheva, 2020).

II.1 Background

Three canonical examples from contracts, torts, and criminal law illustrate the potential impact 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 mid-century theories of sentencing according to either retribution on behalf of victims or rehabilitation of criminals (e.g. 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 (e.g. 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 to see the impacts 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 (2019), the Chicago-School-oriented law-and-economics movement was driven at least in party 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 (e.g. 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 also gained traction in criminal law through the promotion of deterrence theory, suggesting that severity of punishment can make up for low probabilities of detection (e.g. 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 (e.g. 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 (e.g. Thornton, 2016).

II.2 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, itself founded at the University of Miami in 1974, was the first academic research center devoted to law and

[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 (e.g. Bork, 1978).

[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 non-market 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: e.g., swiftness, certainty, and fairness might deter crime more than the severity of punishment (Nagin 1998; Kleiman 2009; van Winden and Ash 2012).

economics. LEC moved to Emory University in 1980, prior to 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 stay, and by inviting judges' family members to join. The organizers did not invite particular judges, and the admissions process was first-come-first-serve.[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-serve 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, forty percent 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 nineties, about half of cases were directly impacted

[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]"Big Corporations Bankroll Seminars For U.S. Judges," *Washington Post*, 20 Jan 1980, available at washingtonpost.com/archive/politics/1980/01/20/big-corporations-bankroll-seminars-for-us-judges/8385bf9f-1eb7-451a-8f3d-bdabb4648452/. See Appendix A for more background and documents related to the Manne Program.

[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).

[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.

by a Manne panelist.

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 were the lessons, 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 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 Appendix Figure A.1.

The annual reports also 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]

From the judges' perspective, the seminar made a lasting impression. Circuit Judge Paul Michel wrote that "[it] helped to provide a *principled basis* for deciding close cases," while 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*." Supreme Court Justice Ruth Bader Ginsburg wrote: "*the instruction was far*

[9]The former director Henry Butler (personal communication) 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 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). Follow-up courses were taught by other economists with a conservative reputation, including James Buchanan and Gary Becker (Butler, 1999).

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*."

II.3 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 non-legal 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 both 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 non-legal 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 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 for example strategic and collegial considerations with other judges and the broader policy and political currents

[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."

of the day (Posner, 2008). Further, clerks often contribute significantly to drafting of 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 impact 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, and 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 dataset of criminal sentencing decisions matched to the attending judges. Criminal law was a central focus of law-and-economics scholarship and by Henry Manne himself (Gindis and Medema, 2022), but it was not emphasized in the Manne curriculum. Hence, while 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 is providing 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,

[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).

[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, pp. 108).

we will 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 Appendix B.

III.1 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 the adjudication of 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 within 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 per year. Circuit court decisions comprise the vast majority of what law students are reading and what judges are applying.

Circuit Court Cases. Our key data set is the set of judicial decisions published by the United States Circuits of Appeal for the years 1970 through 2005. The cases come from Bloomberg Law and are cross-checked against other existing datasets, including the Songer Database, Federal Judicial Center's Administrator of Courts dataset, and information from LexisNexis.

The dataset 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

[14]The First through Eleventh Circuits preside over groups of 3-9 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.

cases as those involving labor or regulation.[15] Economics-related cases comprise about 30% of the dataset.

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 comes merged with judge identity for the years 1992 through 2003, with approximately 1.03 million cases.

Federal Judge Biographies. We have biographical information on federal circuit and district judges from the Federal Judicial Center. The dataset includes detailed information on judicial careers, party of appointing President, cohort/region of birth, and education.[16]

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. 2-3 day workshops). We supplemented this list with years of attendance from annual reports obtained by FOIA requests and through correspondence with the Law and Economics Center at George Mason University.[17]

III.2 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 the purposes of identifying law-and-economics articles in a law

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

[16] See 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.

journal corpus. This index includes eleven 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 fails to capture meaningful variation across opinions (see 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 locate 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 sparsity 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 Rehurek, Sojka et al. (2011). Previous work has shown that downstream measurements in social-science contexts are not that sensitive to these choices (Rodriguez and Spirling, 2021; Ash, Chen, and Ornaghi, 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. 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. Appendix Section 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. Appendix Figure A.8 shows the distribution of the embedding-based measure

[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.

and highlights that it is relatively normally distributed, contrasting with the sparsity of a count-based measure that requires exact matches to the lexicon.

For robustness, 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.3 Judicial Decision Outcomes

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

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. In particular, 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 then construct measures based on the voting of judges. We consider voting against the government in a regulation case as in line with a deregulatory policy objective.

Conservative Judicial Decisions. As a more general measure of conservatism, we have a hand-coded measure of decision direction from the Songer-Auburn database (e.g. 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 violation of 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. Further, 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).

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 appendix, we report supporting results with sentence length in months.

IV Econometrics

This section outlines our identification strategy. We use a differences-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 Appendix C.

Overview. The identification strategy is differences-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-serve variation. That is, we make comparisons of 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 Santanna (2020), 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 Appendix C.3, never-attenders are not a plausible control group because they are differentially selected on both levels and trends of pre-treatment 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 then catch up in terms of changes in a given outcome variable. Thus, using not-yet-attenders as a control group (rather than never-attenders) means that we cannot estimate long-term effects.

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

$$Y_{ijct} = \alpha_j + \alpha_{ct} + \gamma Z_{jt}^{\text{post}} + \boldsymbol{X}'_{ijct}\beta + \epsilon_{ijct} \tag{1}$$

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. $\boldsymbol{X}'_{ijct}\beta$ can include other covariates, as described below, while ϵ_{ijct} is an error term. For the event studies, we report the coefficients and confidence intervals produced from estimating

$$Y_{ijct} = \alpha_j + \alpha_{ct} + \sum_{k \in K} \gamma_k Z_{jt}^k + \boldsymbol{X}'_{ijct}\beta + \epsilon_{ijct} \tag{2}$$

where now we have indicators Z_{jt}^k, which correspond to the leads and lags of Manne attendance. The event study time window is $K = \{-W, -W+1, ..., -2, 0, 1, ..., W\}$, where W is the length of this event study window and the year before attendance

[19]For completeness, regressions including never-attenders in the sample are described and reported in Appendix C.7. The main results for effects on labor/environment cases and effects on criminal sentencing are robust to the inclusion of 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.

($k = -1$) is excluded.[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.

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. Further, to address the issue that some courts and years have more cases than others, we re-weight case observations such that judge-years count equally (Solon, Haider, and Wooldridge, 2015).

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). For the subset of courts where randomization has been questioned (Levy and Chilton, 2015), we can show robustness of our main results to dropping those courts (Appendix Figure A.12, A.25). Further, our results are robust to controlling for case topics or charge fixed effects.

More specific to our setting, we can check whether Manne training has an effect on the types of cases that judges sit on or author. 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.

Endogenous selection of judges into attendance and timing. The central concern is endogenous selection into the program. With judge fixed effects, the primary

[20] We have $W = 6$ for the circuit courts and $W = 5$ for the district courts (chosen for convenience, and since the district courts data are for a shorter time period). We report results with shorter event study windows in the appendix.

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 judge's 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 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.2 above), 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 nominating president) is not a statistically significant predictor for timing (and even dropped by elastic net in the circuit courts). The covariate balance within ever-attenders lends credence to the exogeous timing assumption, driven in part by the first-come-first-serve 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 both were more likely to attend, and more likely to attend earlier.

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 FE) 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 control for year-specific effects of all elastic-net-selected characteristics that predict the timing of attendance. Specifically, we set $\boldsymbol{X}_{ijct} = \lambda_t \boldsymbol{X}_j$, where $\boldsymbol{X}_j$ includes judge covariates, selected by elastic net as predictive of the timing of Manne attendance (see Appendix C.2), fully interacted with year fixed effects λ_t.[21]

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-serve rule was binding. Still, we cannot fully rule out endogenous timing of attendance along unobservable judge characteristics correlated with judicial outcomes.

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). Appendix C.6 presents diagnostics from De Chaisemartin and d'Haultfoeuille (2020) and Jakiela (2021) to show that negative weighting is only occurring for a small part of our sample, and further it does not appear that effect heterogeneity is a major concern (Appendix Table A.7). This combination of limited negative weighting and limited heterogeneity gives us confidence that our design is not vulnerable to mis-specification of the control groups, despite our lack of a clean set of never-treated judges. See Appendix C.3 for a discussion of this design choice.

SUTVA violations. Another concern is that judges are communicating among themselves, particularly within a circuit. As discussed above, judges serve on three-person panels on a variety of both economic and non-economic 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. Appendix C.5 shows there is little evidence of spillovers.

To address concerns about 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 impacts of peer share across judges, we set $\boldsymbol{X}_{ijct} = \alpha_j \bar{Z}_{ct}^{-j}$, allowing for a judge-specific effect of peer attendance.

[21] We also constructed averages of pre-treatment or pre-1976 outcome variables by judge as potential selection variables for elastic net. They were not selected.

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 secondly, go on to circuit court decisions about regulatory agencies. Third, we look at results for criminal sentencing. This section reports the main event study estimates. Section VI below summarizes the magnitudes (Tables I and II). Further supporting material and results are reported in Appendices D (economics language), E (regulatory decisions), F (conservative voting), G (antitrust), H (criminal sentencing), and I (additional supporting results).

V.1 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 Subsection III.2 above. The sample includes majority-opinion authors and excludes non-author 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), 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 year 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 includes peer Manne attendance shares interacted with 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.[22] Meanwhile, there are no significant effects in the pre-trend period. The

[22]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

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). Further, for all specifications, we run the test from Rambachan and Roth (2019) and rule out substantial non-linear pre-trends (Appendix Figure A.10).

Appendix Section D.2 reports an extensive set of supporting results and robustness checks on the use of economics language in judicial opinions. Complementary differences-in-differences regression results are reported in 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, 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 (Column 10-11). The long run effects (Columns 12-22), meanwhile, are generally not significant. Overall, the results suggest that Manne attendance increases the short-run use of economics-oriented language by about 0.3 standard deviations.

As described in 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 authoring judge (N =508,325). We then 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.

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 (Appendix Figure A.18; Appendix Table A.10).

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attendance.

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 non-economic 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 (Appendix Figure A.20), 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, or 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 increase – and if anything a decrease – in the use of statistical/quantitative language (Appendix Figure A.21). Hence, the effect on language seems to be more on the conceptual use of economics, rather than the use of econometric analysis.

V.2 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 agencies (especially the National Labor Relations Board and Department of Labor) and the Environmental Protection Agency. 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 above, we report a baseline specification (blue circles), with elastic net controls interacted with year (red diamonds), limiting to the early period (green triangles), and with judge-specific peer attendance share controls (purple squares). 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 in the 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 Appendix C.5 for additional discussion on potential peer spillovers). Appendix Figure A.22 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 year fixed effects, the pre-trend becomes insignificant, while our main effect remains highly significant. In addition, the pre-trend disappears, and the positive impact effect remains, when including judge-specific time trends. Finally, we run the test from Rambachan and Roth (2019) and can rule out substantial non-linear pre-trends in this outcome (Appendix Figure A.23). Still, we cannot fully rule out that these pre-trends indicate endogenous timing, in which judges are experiencing pre-existing shifts in a more conservative direction that then are reinforced by differential Manne attendance.

Appendix E provides additional results and robustness checks on the Labor/EPA analysis. The regression results for Equation (1) are reported in 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 (Column 12-22). The results are robust to inclusion of alternative controls, different samples, and different clustering. The results do not hold with un-weighted regressions where courts and periods with more cases are weighted more in the estimates (Columns 8, 19), but the baseline results hold with winsorized weights (Columns 9, 20). Overall, the results are consistent with a 15 percent 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 percent through 2002) where the ruling has been hand-coded as conservative or liberal by the Songer-Auburn Project. Appendix Figure A.28 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 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 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 Appendix G). With only 100 cases in the event study sample, we estimate mostly positive, but quite imprecise, event-study coefficients in the short run after Manne attendance (see Appendix Figure A.30). Appendix Table A.14 reports differences-in-differences regressions, and we observe mostly noisy and null estimates. The estimated coefficients are almost all positive, and a few are statistically significant. There are no statistically significant negative effects. While 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.3 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 analyzed above, 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.3 above).

Here we focus on the main sentence outcome of each district court case, conditional on conviction: whether a defendant is incarcerated or not. 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 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, while the second specification (red diamonds) adds elastic-net-selected judge characteristics (predicting time of attendance) interacted with year fixed effects.[23] For both specifications, we see a positive 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 yet not significant. In the years before attendance, we estimate zeroes.

Appendix H includes additional results and checks for criminal sentencing. Appendix Table A.15 reports the differences-in-differences estimates for how Manne attendance affected district judge sentencing (see also Table II below). 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 FE or charge FE, to the use of unweighted regressions, and to alternative clustering. However, we cannot rule out the presence of non-linear pre-trends, according to the test from Rambachan and Roth (2019) (Appendix Figure A.32). Using Poisson regressions, we show that the Manne program also increased severity through a longer sentence length (Table II, Appendix Figure A.31, Appendix Table A.16), although that estimate is more sensitive to specification than the one for the binary any-prison outcome.

Finally, in Appendix Table A.17, we explore heterogeneity in the Manne effect on sentencing severity for drug crimes. For non-drug crimes, the Manne program caused significant and robust increases in sentencing severity. For drug crimes, in contrast, the effects are much smaller and no longer statistically significant. This difference suggests that the Manne program, shaped by instructors like Milton Friedman who advocated for drug legalization, leads to weaker effects on sentencing severity for drug crimes. These crimes are perceived as victimless and, therefore, less deserving of harsher punishment.

[23] The coefficient plot has two fewer specifications than those above. First, we don't have the pre-1987 spec 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.

VI Discussion

VI.1 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 differences-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 heyday period.

The Manne program had a large effect on economics language in the early period – about half 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 0.3 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 0.024 and 0.059) are always smaller than for econ 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 percentage points (Column 1). The coefficient is statistically significant and about one-tenth of a standard deviation. That effect is even larger for non-drug crimes at 8 percentage points (Column 2), but smaller (4 percent) and not statistically significant for drug crimes (Column 3), consistent with the Manne program's views on drugs as victimless crimes. Meanwhile, the Poisson coefficient for sentence length (Column 4) implies that sentence lengths went up about 19.6% after attendance.

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 $\hat{\gamma} = \Delta y = .05$ (Table I Column 11), we can calculate a persuasion rate and compare it to 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 $y_0 = .46$, the mean outcome for the pre-attenders in economics cases, we have $p = 9$ percent. 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 percent), 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 percent).

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 0.05 and multiplying by 0.4 (the share of circuit judges who attended) renders a substantial fraction (0.02) of the overall 0.2 shift. Taken together, these numbers imply the Manne program could account for 10% of the rise in judicial conservatism.

VI.2 Broader Policy Influence of 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 the years 1976 through 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 SCOTUS appointees were Manne-trained. Supreme Court decisions are binding U.S. law, and so the ultimate influence of the Manne program on policy is likely larger than what we can measure with the design in this paper.

To understand the policy influence of Manne-trained judges across these 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 over 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 ratemaking bureau who had conspired to unfairly set rates for commerce between the U.S. 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.3 Why Economics?

Why might economics education have been so effective at persuading judges? Judges are generalists, called on to decide high-stakes decisions on a variety of 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 (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. In terms of career concerns, we find that Manne attendance by district judges appears to have increased the probability of promotion to higher appellate courts, at least for Republican nominating presidents (Appendix Table A.18).[24] But the perceived benefits were clearly bipartisan. From the records of judge testimonials, we know that the Manne program was attended and celebrated by many Democratically affiliated judges, including Ruth Bader Ginsburg (see Appendix A).

A number of other factors could have boosted both the desirability and impact of the Manne Program. We have seen from the archival documents that the Law and

[24] Forward citation rates to a judge's opinions, which reflect the usefulness of an opinion to future judges (e.g. Ash and MacLeod, 2021), do not increase after Manne attendance (Appendix Figure A.35). Further, the use of quantitative or statistical language actually seems to decrease relative to not-yet-attenders post-attendance (Appendix Table A.21).

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, Appendix Table A.18 shows that Manne-trained judges are in fact more likely 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 then influence decisions and language (Bonica et al., 2019).[25] While 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 impacts 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 policymaking (Seabright 1996; Besley and Coate 1997) and the importance of ideas versus institutions in determining policy (Romer 2002; Rodrik 2014). For example, the expansion

[25] 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 goes 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).

of economic regulation is one hallmark of the modern administrative state, yet the determinants of this sort of state power in American 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 both theory and evidence (Benabou, 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 paper.

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 policymakers and for future research.

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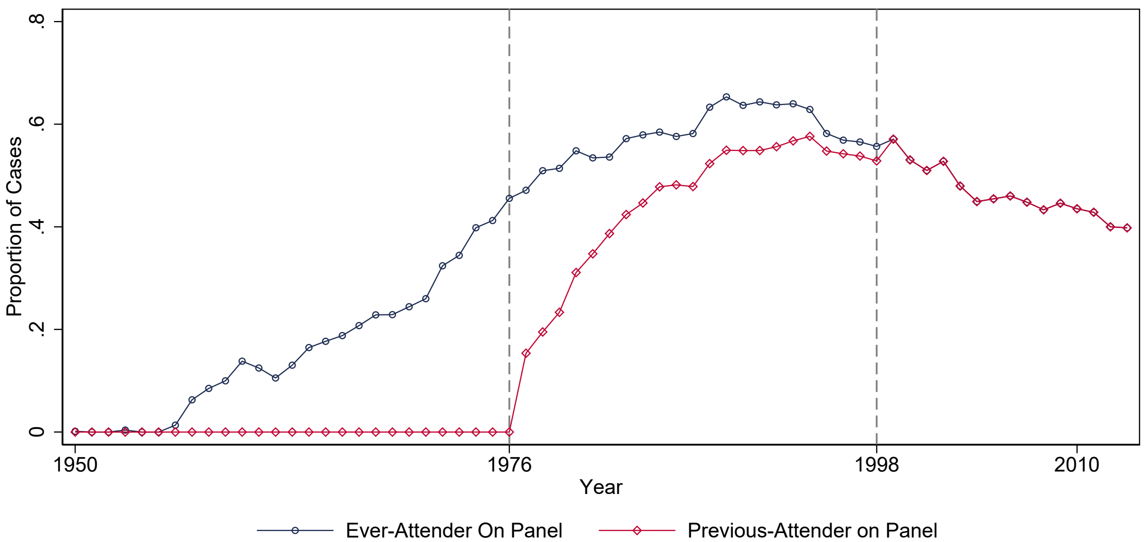
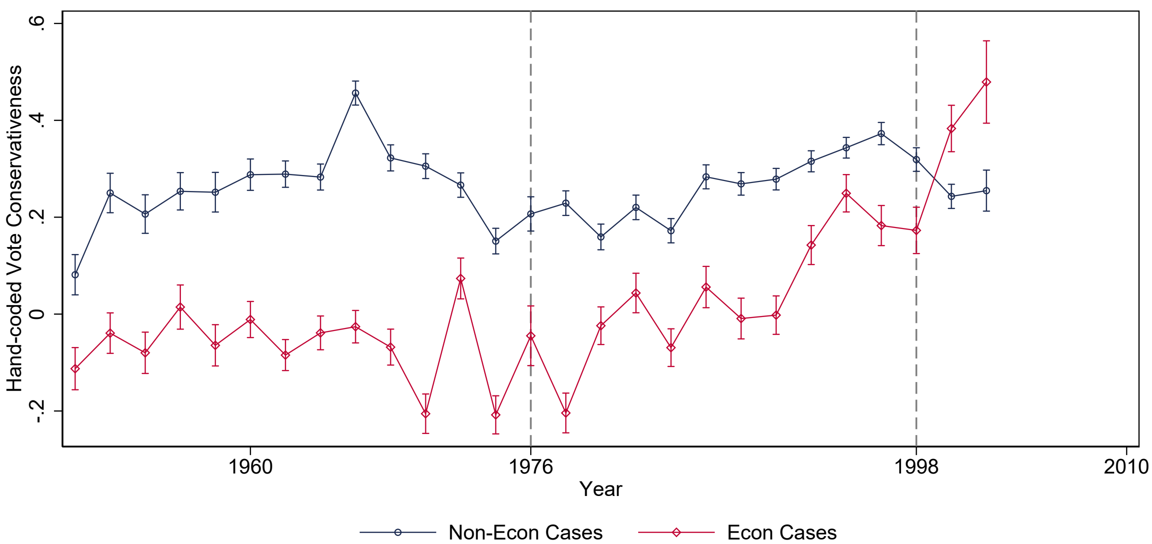
Figure I: Share of Cases with Manne Judge on Panel, 1950-2013



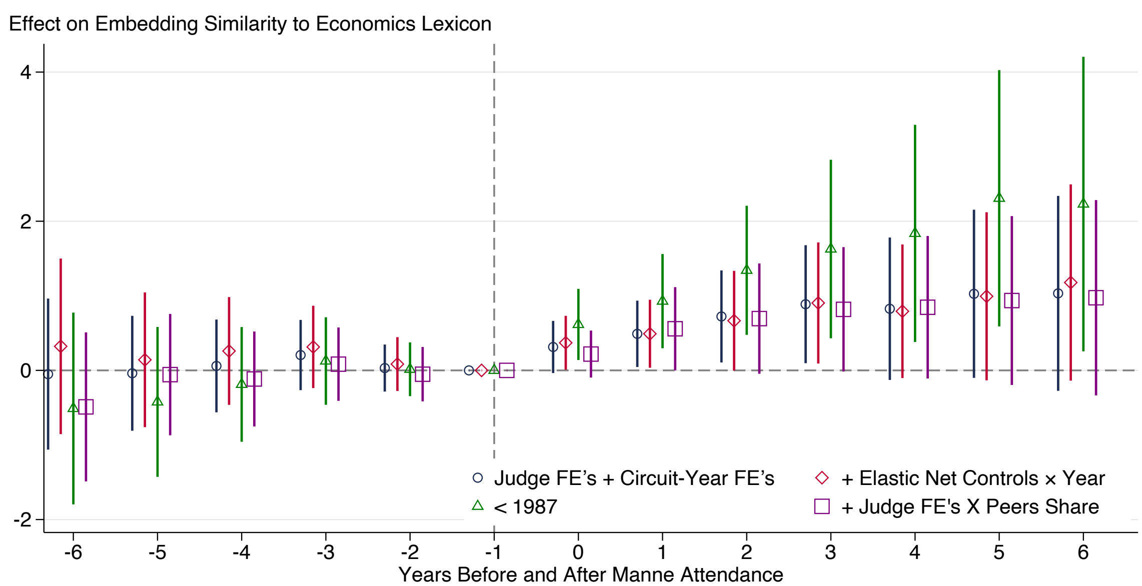
Notes. Share of cases with a Manne judge on the panel, plotted by year. Blue line gives judges who ever attended; red line gives judges who have already attended.

Figure II: Increasingly Conservative Rulings in U.S. Federal Courts



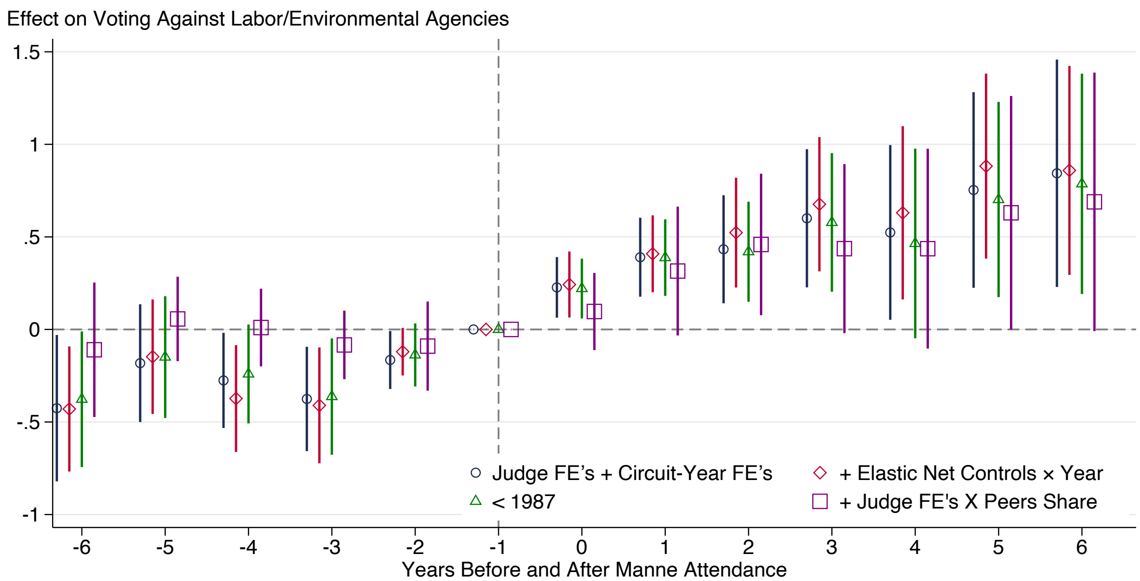
Notes. Average conservative vote rate in circuit courts using 5% hand-coded Songer Auburn data, plotted by year and separately by economics and non-economics cases. Error spikes give standard error of the mean. Data weighted to treat judge-years equally.

Figure III: Effect of Manne Program on Economics Language



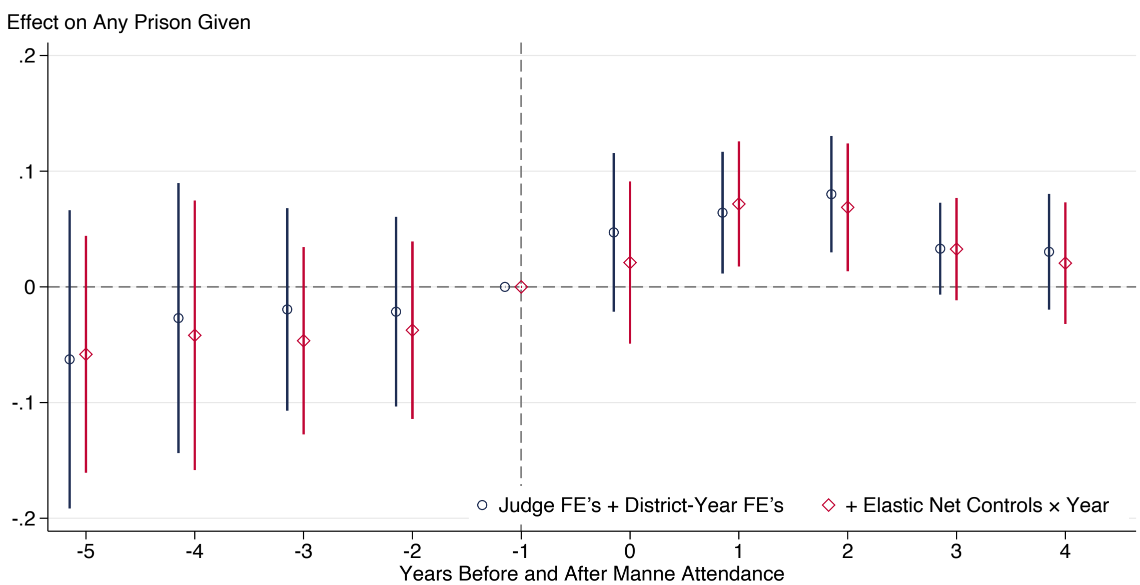
Notes. Event study effect of Manne attendance on Word Embedding Similarity to Law-and-Economics Lexicon (from Ellickson, 2000). Sample is limited to case authors. Regressions include judge and circuit-year fixed effects (blue circles), with additional specifications adding elastic-net-selected controls interacted with year 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.

Figure IV: Effect of Manne Program on Votes Against Labor/Environmental Agencies



Notes. Event study effects on voting against government agency on labor and environmental issues, relative to 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 year 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.

Figure V: Effect of Manne Program on Giving a Prison Sentence



Notes. Event study effect of Manne attendance on criminal sentencing outcomes in district courts, 1992-2003. Outcome is any prison given. Regressions include judge and district-year fixed effects (blue circles), plus elastic-net-selected controls interacted with year fixed effects (red diamonds). Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals.

Table I: Regression Estimates: Effect of Manne Program in Circuit Courts

	Economics Language				*Voting Against Regulators*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Manne	0.355	0.474	0.026	0.465	0.157	0.165	0.172	0.168
	(0.131)	(0.183)	(0.096)	(0.160)	(0.067)	(0.073)	(0.048)	(0.065)
N	5267	3191	10215	4085	2639	2068	4192	2564
	Conservative Vote (Econ Case)				*Conservative Vote (Non-Econ Case)*			
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Post Manne	0.304	0.18	0.051	0.104	0.059	0.024	0.028	0.056
	(0.130)	(0.118)	(0.070)	(0.071)	(0.074)	(0.091)	(0.048)	(0.090)
N	800	579	1543	759	2401	1527	4788	1945
Court-Year FE	X	X	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X	X	X
Sample	Short Run		Long Run		Short Run		Long Run	
Pre-1987		X		X		X		X

Notes. Summary of 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. "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) clustered at judge level, and observations weighted to treat judge-years equally.

Table II: Regression Estimates: Effect of Manne Program in District Courts

	Any Prison Given			*Sentence Length*
	(1)	(2)	(3)	(4)
Post Manne	0.061	0.0804	0.0467	0.179
	(0.028)	(0.0328)	(0.0518)	(0.092)
N	70784	41038	29737	70448
Crime Type	All	Non-Drug	Drug	All
Court-Year FE	X	X	X	X
Judge FE	X	X	X	X

Notes. Summary of estimated effects of Manne training on district court outcomes, indicated by column headers. Specifications are the same as detailed in the associated regression tables for each outcome:. Columns 1-3 are OLS, while Column 4 is a Poisson model. Regressions include court-year and judge, fixed effects. Regression is done on the event-study sample. "Drug" indicates the sample is limited to crimes from USC Title 21; "Non-Drug" indicates the sample is all other crimes. Standard errors (in parentheses) clustered at judge level.

Ideas Have Consequences:
The Impact of Law and Economics on American Justice

Online Appendix

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Appendix Table of Contents

A More Background on Manne Program

The public perception of the Manne Program was a beach on the south of Miami for a few weeks funded by large corporate donors. A *Washington Post* reporter writes:

> 105 corporate contributors are almost always before a federal judge somewhere, often in antitrust, regulatory, or affirmative-action cases... probably all federal judges face some possibility [of having a contributor as litigant].[26]

The perception put forward by the program from its annual reports is a collection of photographs of judges diligently taking notes and receiving reading assignments. In contrast to the *Washington Post*, a *New York Times* reporter writes:

> For three weeks, 19 Federal judges from around the country took a grueling, six-day-a-week course in economics.. With classes starting at 9 A.M. and sometimes ending at 10 P.M. or later, the judges received the equivalent of a full semester at the college level. ... From the beginning, the judges, some of them 60 years or over, behaved like students, deferring to their teachers.[27]

While the courses were later shortened from three weeks, they were never shorter than two weeks.

Next, a few notes about the content of the curriculum. Henry Manne (who taught some of the lectures) articulated the view that insider trading was economically efficient. He writes: "It is ironic that the word 'profit' has become a swear word, since profit is the only decent measure of the real public benefit provided by business." Another instructor, Professor Goetz, defended "'Unequal' Punishment for 'Equal' Crime," arguing that discrimination in punishment can be economically efficient. In more recent years, the annual reports include instructors with known conservative stances on immigration (George Borjas), crime (James Q. Wilson), and family law (Jennifer Roback Morse, founder of the ant-LGBT Ruth Institute).

In a *Fortune* magazine article (May 21, 1979), instructor quotes indicate the ideas offered by the economics instructors. Alchian said, "I'm trying to change your view of

[26]"Big Corporations Bankroll Seminars For U.S. Judges," *Washington Post*, 20 Jan 1980. The list of donors included Abbott Laboratories, Alcoa, Amoco, Bristol-Myers, Campbell Soup, Chase Manhattan Bank, Chevron, du Pont, Kodak, Exxon, Ford Motor Company, General Electric, General Motors, Gerber Baby Foods, Getty Oil, Hoffmann-La Roche, Eli Lilly, Merrill Lynch, Mobil, Pennzoil, Pfizer, Procter & Gamble, Raytheon, Schering-Plough, Sears Roebuck, Shell, Southwestern Bell, Sun Company, Texaco, Unilever, Union Oil, Upjohn, US Steel, Winn-Dixie, Xerox, among many others.

[27]"19 U.S. Judges Study Economics to Help Them in Work on Bench"

Figure A.1: Manne Program: Sample Agenda

LEC ECONOMICS INSTITUTE FOR FEDERAL JUDGES
Westward Look Resort, Tucson, AZ
Sunday, March 3 to Saturday, March 16, 1991

PROGRAM AGENDA

SUNDAY, MARCH 3	
7:00 p.m.	Reception – LEG Hospitality Suite
7:45 p.m.	Dinner – Board Room
MONDAY, MARCH 4	
8:30 - 12:00 Noon	CLASS # 1 - Alchian
Topic:	Competition, Demand, Exchange
Assignment:	A&A., Chapters 1, 2 and 3
Recommend:	Alchian, additional materials
	Alchian, "Uncertainty, Evolution, and Economic Theory"
TUESDAY, MARCH 5	
8:30 - 12:00 Noon	CLASS # 2 - Alchian
Topic:	Prices and Markets, Information Costs
Assignment:	A&A, Chapters 4 and 5
WEDNESDAY, MARCH 6	
8:30 - 12:00 Noon	CLASS # 3 – Alchian
Topic:	Capital Values, Future Yields, Interest
Assignment:	A&A., Chapter 6
Recommended:	Alchian, "Words: Musical or Meaningful?"
THURSDAY, MARCH 7	
8:30 - 12:00 Noon	CLASS # 4 - Alchian
Topic:	Production
Assignment:	A&A, Chapters 7 and 8
Recommended:	Alchian and Demsetz, "Production, Information Costs, and Economic Organization"
1:00 - 4:30 p.m.	CLASS # 5 - Butler
Topic:	The Modern Corporation
Assignment:	A&A, Chapter 9
	Butler, "The Contractual Theory of the Corporation"
Recommended:	Alchian, "Corpation Management and Property Rights"
	Fama and Jensen, "Separation of Ownership and Control"
	Manne, "Our Two Corporation Systems: Law and Economics"
FRIDAY, MARCH 8	
8:30 - 12:00 Noon	CLASS # 6 - Goetz
Topic:	Price Takers, Price Searchers
Assignment:	A&A., Chapters 10 and 11
7:45 - 9:15 p.m.	Panel: all available instructors
SATURDAY, MARCH 9	
8:30 - 12:00 Noon	CLASS # 7 - Goetz
Topic:	Competitive and Monopoly Makers
Assignment:	A&A., Chapters 11 (cont'd), 12 and 13
Recommended:	Goetz, pp. 441-447 (Second-Best Theory)
MONDAY, MARCH 11	
8:30 - 12:00 Noon	CLASS # 8 - Alchian
Topic:	Pricing and Employment
Assignment:	A&A., Chapters 14 and 15
7:45 - 9:15 p.m.	SPECIAL SESSION – Hoffman
TUESDAY, MARCH 12	
8:30 - 12:00 Noon	CLASS # 9 - Ashenfelter
Topic:	Statistical Inference
Assignment:	Paulos, Innumeracy, Chapters 1 and 2
WEDNESDAY, MARCH 13	
8:30 - 12:00 Noon	CLASS # 10 - Ashenfelter
Topic:	Econometrics
Assignment:	Paulos, Innumeracy, Chapter 5
1:00 - 4:30 Noon	CLASS # 11 - Goetz
Topic:	Evolving Property Rights and Competition
Assignment:	Demsetz, "Toward a Theory of Property Rights"
	Caves, "Vertical Restraints as Integration by Contract: Evidence and Policy Implications
THURSDAY, MARCH 14	
8:30 - 12:00 Noon	CLASS # 12 - Samuelson
Topic:	Stochastic Processes
Assignments:	Brealey, pp. 1-87
	Samuelson, additional materials
	Samuelson, "Challenge to Judgement"
	Sharpe and Murphy, "Second Thoughts About the Efficient Market"
	Samuelson, Chapter 24 (appendix)
Recommended:	Black, "Yes, Virginia, There is Hope"
7:45 - 9:15 p.m.	PANEL: Alchian, Ashenfelter, Butler, Manne, Goetz, Samuelson
Topic:	Intractable Questions in Economics: Wealth Distribution; Original Entitlements; Valuation Theory; Normative Implications of Positive Theory
FRIDAY, MARCH 15	
8:30 - 12:00 Noon	CLASS #13 - Samuelson
Topic:	Economics and Comparative Advantage
Assignment:	Samuelson, "International Trade for a Rich Country"
	Samuelson & Nordhaus, Chapters 38, 39, 40, *especially Chapter 38*
Recommended:	Samuelson, "To Protect Manufacturing?"
SATURDAY, MARCH 16	
8:30 - 12:00 Noon	CLASS #14 - Goetz
Topic:	Law and Economics
Assignment:	Goetz, pp.
	– 49-68 (Nuisance)
	– 166-176 Prejudgment Interest)
	– 375-391 (Costs and Damages)

Notes. Sample Agenda, including readings and course schedule, for the 1991 Economics Institute for Federal Judges ("Manne Program"). Obtained from Butler (1999) Appendix A.

the world, to show you that what you thought was bad really may not be." Klein and Demsetz gave the received views on antitrust ("price discrimination, which encourages production, is good") and the judge as social planner ("the consumer who is supposed to benefit .. isn't represented; he isn't there in front of you with his lawyer"). On damages and deterrence, Demsetz said: "[an agent is] not likely to be caught, [so] the threat of simple damages may not be a tough enough deterrent." He also discussed the moral hazard associated with tort liability: "The plaintiffs may wait a long time before they complain, because they want damages to pile up." On environmental law, Alchian stated: "Give me a capsule that will magically clean all the air in Los Angeles ... Beg me to crush it. ... I won't crush the capsule. Because, if I do, poor blacks will have to pay $20 a month more for land rental... [T]he black in Watts, already used to living with bad air, loses his discount for doing that."

Butler (1999) includes quotations about the judges' reaction to the program. Butler wrote that academic attention to the role of economics in law

> could actually be the most lasting contribution of the judges' program to the development of law and economics . . . As I always told the judges in my session-closing remarks, 'If you are doing your job right, *there really should not be many different results in your cases*. But you will have a better understanding of the law because of the insights economics offers, and that will help you be better judges."' (p. 321, emphasis added).

So at least in principle, the program was billed as a non-partisan tool to help judges understand their decisions.

On the other hand, the promotional materials emphasized concrete impacts. Even early on, LEC was aware of how the program would influence judicial outputs. The 1982 LEC annual report writes:

> For those interested in the impact of our programs, one sentence out of a recent letter from a distinguished U.S. Court of Appeals judge says it all. "In reviewing the cases I have sat upon in the last six months, I thought you might be interested to know that in fully 50 percent of them a portion of the case or the whole case turned on an issue I felt I was better able to decide because of my opportunity to study in your program". Who could ask for stronger testimony?

A few choice quotes from judges illustrate that the program plausibly had an impact on its participants:

District Judge Robert Carter: "*I regard myself as a social progressive* and all the economists in attendance, from my perspective, had Neanderthal views on race and social policy. The basic lesson I learned . . . is that social good comes at a price, a social and economic cost. I had never thought that through before being exposed to Henry's teachings. . . . [It] has *led me to measure the cost of the social good being furthered against the gain to be achieved*."

District Judge Anthony Alaimo: "There is a wide area of decision entrusted to us where the result can go either way, depending on how we view the evidence. *That area* is *called 'judicial discretion.'* This is the area that is *most affected by these seminars* . . . as a result of what I have learned at these seminars, *I have become a much better judge*."

District Judge Thomas Griesa: "Henry and his LEC colleagues were of a *conservative persuasion*. . . . the class wanted to express our gratitude on the final day. The person who rose to speak was Judge Hall from West Virginia, who was from the Fourth Circuit. *Without doubt he was a Democrat going back to New Deal days. He was fervent in his appreciation*."

Supreme Court Justice Ruth Bader Ginsburg: "Cheers to Henry, innovator and dean nonpareil. As a student in two of his seminars, I can affirm that 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."

Circuit Judge Paul R. Michel: "The courses I attended helped to provide a principled basis for deciding close cases."

Circuit Judge Grady Jolly: "As a new judge, a principal concern for me was that I develop reasoned criteria for deciding cases. While each judge must wrestle with what that criteria should be, I found Henry's courses helped to provide me with a sound theoretical and rational structure for my decisions. . . [I]n many cases, one need look no further than the letter of the law. However, in those cases where the law is not clear, there is, consciously or unconsciously, a proclivity to resolve the case in favor of the party with whom you most identify or sympathize. To avoid succumbing to

> this pattern, it is essential to understand the economic and social impact of one's decision. . . [T]he courses gave to me a greater understanding of the potential effects and foreseeable impact of imposing a duty or liability on a particular party in a case. And with that understanding came an appreciation of the broader impact that my decisions could have on other similarly situated parties. In sum, the courses I attended helped to provide a principled basis for deciding close cases."

The programs were intense. According to District Judge Robert Doumar,

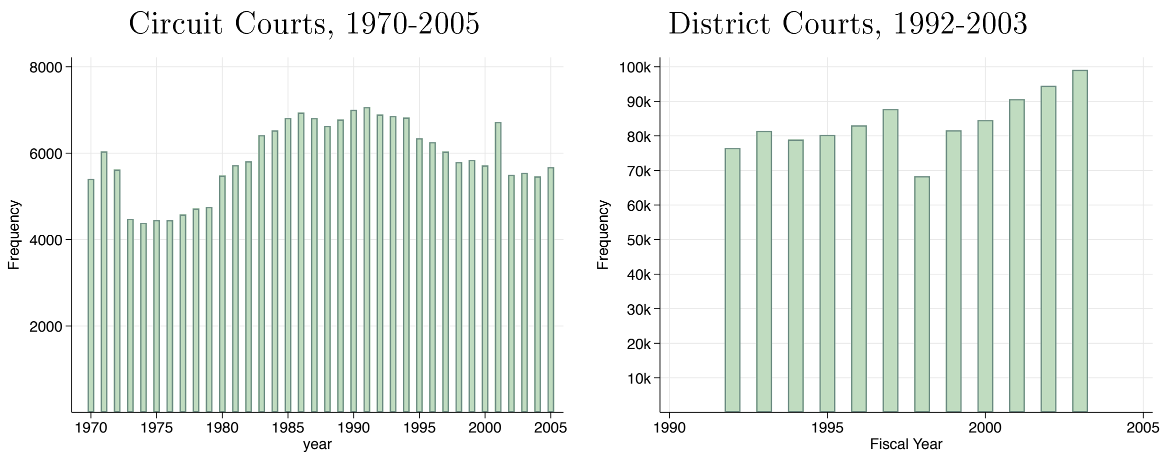
> Henry always chose places for classes that embodied the principles of economic success. One need only to look out the window to see it all around. One's eyes never wandered far as the teachers were always the epitome of expertise. However, Henry, as truly economic, made it clear that he expected one not to participate in the abundance that surrounded them until all the classes were over and done with.

Similarly, District Judge Thomas J. Curran remarked:

> Frankly, I did not expect such a concentrated agenda. I don't believe I have ever attended a seminar that involved such intensive study and discussion. My wife, who accompanied me, commented, "I don't see any more of you here than I do at home." Another compliment came from one of my fellow judges who said, "I can't believe how much I have learned, but I'm glad I didn't have to take this course in college."

B Data

Figure A.2: Number of Cases by Year



Notes. Number of case observations in the circuit courts (left panel) and district courts (right panel) in main analysis samples.

Table A.1: Distribution of Circuit Court Case Topics

Songer Topic	Freq.	Percent	Detailed Topic (partial list)	Freq.	Percent
Regulation	127168	20.23	Criminal Law	160807	25.58
Due Process	161522	25.69	Civil Procedure	120163	19.11
Criminal Appeal	161179	25.64	Administrative Law	33209	5.28
Miscellaneous	94515	15.03	Constitutional Law	23998	3.82
Civil Rights	47431	7.54	Appellate Procedure	22674	3.61
Labor	32424	5.16	Habeas Corpus	20342	3.24
First Amendment	3629	0.58	Civil Rights	20341	3.24
Privacy	826	0.13	Bankruptcy Law	17477	2.78
Total	1,120,227	100.0	... [86 additional topics]		

Includes cases from 1970-2005 in U.S. Circuit Courts.

Figure A.2 shows the number of cases in the main analysis samples for the circuit courts and district courts. From the Songer Database we have a set of high-level case topics, with the tabulation reported in Appendix Table A.1. A substantial portion are related to criminal law (20%) and our two economics topics: regulation (20%) and labor (5%). From Bloomberg we have a set of topics coded by Bloomberg staff attorneys (right side).

Table A.2: Summary Statistics on Outcomes

Variable	Mean	S.D.	N
Circuit Courts			
Embedding Similarity to Economics	.2615	1	494109
Conservatives Votes Econ	.5147	.4443	7029
Conservative Votes Non-Econ	.6314	.4431	21063
Votes against Labor/EPA	.8661	.3404	19744
Votes in Favor of Lax Antitrust	.6924	.4615	2689
District Courts			
Any Prison Given	.4415	.496	1008378
Log 1 + Sentence Length (Years)	1.554	1.899	1005547

We have judge biographical characteristics from the Appeals Court Attribute Data,[28] Federal Judicial Center, and previous data collection.[29] These data help control for other shifters of ideology. We constructed dummy indicators for whether the judge was female, non-white, black, Jewish, catholic, protestant, evangelical, mainline, non-religiously affiliated, whether the judge obtained a BA from within the state, attended a public university for college, had a graduate law degree (LLM or SJD), had any prior government experience, was a former magistrate judge, former bankruptcy judge, former law professor, former deputy or assistant district/county/city attorney, former Assistant U.S. Attorney, former U.S. Attorney, former Attorney-General, former Solicitor-General, former state high court judge, former state lower court judge, formerly in the state house, formerly in state senate, formerly in the U.S. House of Representatives, formerly a U.S. Senator, formerly in private practice, former mayor, former local/municipal court judge, formerly worked in the Solicitor-General's office, former governor, former District/County/City Attorney, former Congressional counsel, formerly in city council, born in the 1910s, 1920s, 1930s, 1940s, or 1950s, whether government (Congress and president) was unified or divided at the time of appointment, and whether judge and appointing president were of the same or different political parties.

[28] http://www.cas.sc.edu/poli/juri/attributes.html

[29] Missing data was filled in by searching transcripts of Congressional confirmation hearings and other official or news publications on Lexis.

C Additional Identification and Specification Checks

C.1 Checks on Selection into Different Case Types

This section presents background and checks on randomization of judges to cases. This randomness has been used in a growing set of economics papers (Kling 2006; Maestas, Mullen, and Strand 2013; Belloni et al. 2012; Dahl, Kostøl, and Mogstad 2014; Mueller-Smith 2015). In Circuit Courts, almost all cases are randomly assigned to a panel of three judges. In District Courts, cases are randomly assigned to judges within the same courthouse. In the circuit panels, one judge among the three is chosen to author the opinion. Authorship is determined by the most senior judge on the case (in terms of years on the court), or the chief judge. When there is a dissent on the panel, the senior judge in the majority assigns the opinion.

Previous work has assessed judge randomization through interviews of courts and orthogonality checks on observables. For example, Sunstein et al. (2006) code 19 characteristics determined by the lower court for a sample of gender-discrimination cases and find that case characteristics are uncorrelated with judicial panel composition.[30] However, Levy and Chilton (2015) take a more rigorous approach and find nonrandom assignment for four circuits (2nd, 8th, 9th, and D.C.). The approach in Levy and Chilton requires data on the case calendars, which they obtained for the years 2008-2013. Unfortunately that data are not available for most of our time period (1970-2005), so we cannot check directly for nonrandomness using the Levy-Chilton method. Still, we show that our main results hold when limiting to the circuits for which they found randomness (Appendix Figure A.12, A.25). Further, our results are robust to controlling for case topics or charge fixed effects.

In more detail: The process for Circuit Courts in recent years is as follows. Two to three weeks before oral argument, a computer randomly assigns available judges to a case, including visiting judges. The algorithm ensures that judges are not sitting together repeatedly, and ensures that senior judges have fewer cases. Judges can occasionally recuse themselves. On appeal after remand, the same panel reviews a case. There are exceptions to randomization for rare specialized cases such as those involving the death penalty. We assume that any deviations from randomness are independent

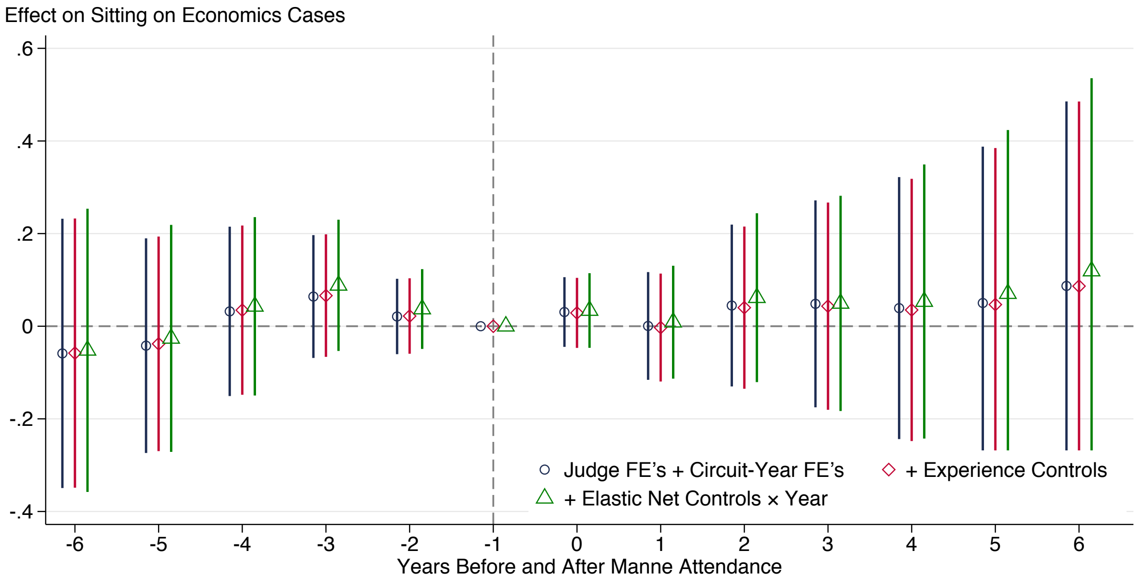
[30] See also Chen and Sethi (2011) and Boyd, Epstein, and Martin (2010). Previous work has examined whether the sequence of judges assigned to cases in each Circuit Court mimics a random process. They find, for example, that the string of judges assigned to cases is statistically indistinguishable from a random string.

of our main effects, and show below that treated judges do not get different types of cases.

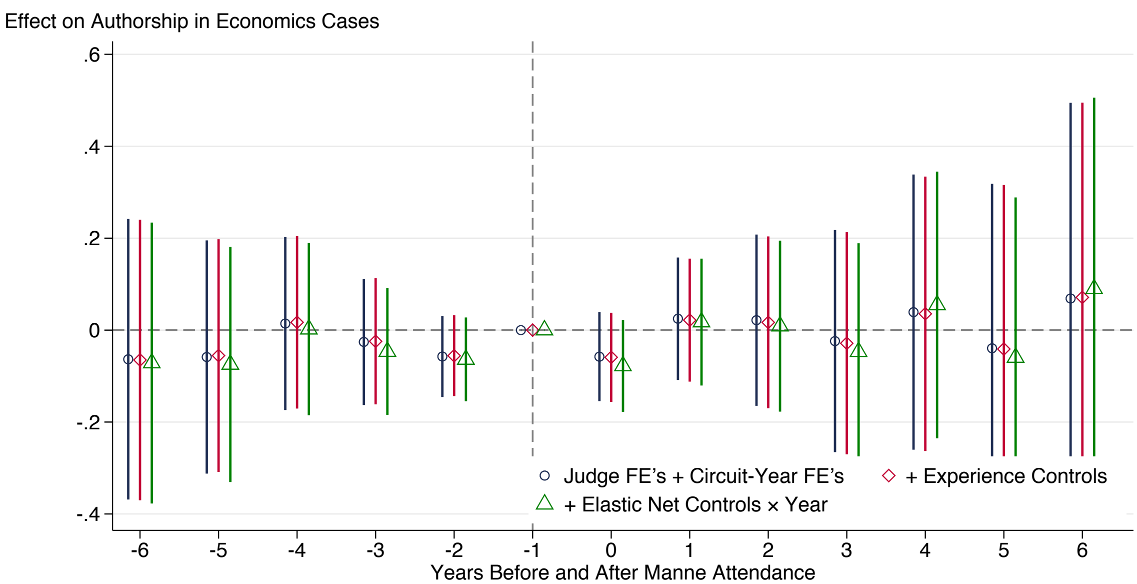
Appendix Figure A.3 shows that randomness does not appear to be violated in the context of Manne judges and the proportion of cases they sit on related to economics topics. In addition, they do not selectively author more economics cases.

Figure A.3: Manne Program has no Effect on Assignment to Economics Cases

(a) Probability of Sitting on Economics Cases



(b) Probability of Authoring Economics Cases



Notes. Event study effect of Manne attendance on working on economics cases. Panel (a): Probability of sitting on economics-related cases. Panel (b):Probability of authoring economics cases. Regressions include judge and circuit-year fixed effects (blue circles), with additional specifications adding quadratic in judge years on court (red diamonds), plus elastic-net-selected controls interacted with year fixed effects (green triangles). Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals, with standard errors clustered by judge.

Table A.3: Covariate Balance, Circuit Court Judges

	Ever Attend		Attendance Year	
	(1)	(2)	(3)	(4)
Republican	0.0640**		-0.0427	
	(0.0179)		(2.491)	
Unified Appoint	-0.0251		-0.277	
	(0.0194)		(2.488)	
Cross-Party App.	-0.0548		-0.282	
	(0.0391)		(1.203)	
State Senator	0.127		-0.712	
	(0.0708)		(1.170)	
State Lower Ct	-0.0326		0.311	
	(0.0242)		(0.593)	
State Supr Court	0.0153	0.00448	0.902	0.860
	(0.0423)	(0.0423)	(1.015)	(0.973)
State House	-0.0381		1.235	
	(0.0463)		(1.051)	
Solicitor General	-0.235**		0	
	(0.0838)		(.)	
Solici Gen. Office	0.0765		3.243	
	(0.124)		(2.338)	
State Atty Gen.	-0.0305	-0.0261	-0.518	-1.219
	(0.0374)	(0.0367)	(0.982)	(0.882)
Private Practice	-0.0951**		0.291	
	(0.0332)		(1.067)	
Mayor	0.0597		-2.548*	
	(0.124)		(1.289)	
Local Court	0.0706	0.0664	0.726	0.684
	(0.0385)	(0.0371)	(0.780)	(0.754)
U.S. House	-0.185**		5.796**	
	(0.0525)		(1.696)	
Governor	0.0318		-6.012**	
	(0.113)		(1.026)	
All Variables	X		X	
Post Elastic Net		X		X
N	699	699	379	379
adj. R-sq	0.124	0.129	0.464	0.497

	Ever Attend		Attendance Year	
	(1 cont.)	(2 cont.)	(3 cont.)	(4 cont.)
District Atty	-0.0294		-0.936	
	(0.0332)		(0.860)	
City Council	-0.0689		-1.420	
	(0.0571)		(2.091)	
Cty Comm	-0.0346	-0.0387	1.739	1.390
	(0.0495)	(0.0484)	(1.523)	(1.429)
Assit U.S. Atty	0.0153		-0.383	
	(0.0261)		(0.656)	
Atty General	0.0842		-1.590*	
	(0.210)		(0.807)	
Asst Dist Atty	0.00676		-0.893	
	(0.0287)		(0.684)	
Any Govt	0.0396		-0.128	
	(0.0250)		(0.994)	
Black	0.0511		0.711	
	(0.0399)		(0.994)	
Born 1910s	0.0977**	0.0673*	-2.881	-2.878**
	(0.0276)	(0.0289)	(2.869)	(1.076)
Born 1920s	0.270**	0.255**	0.873	0.599
	(0.0314)	(0.0325)	(2.897)	(1.130)
Born 1930s	0.219**	0.209**	4.399	4.416**
	(0.0315)	(0.0328)	(2.936)	(1.175)
Born 1940s	0.0731*	0.0604*	9.082**	9.051**
	(0.0285)	(0.0287)	(2.896)	(1.182)
Born 1950s	-0.0383	-0.0470	12.18**	11.67**
	(0.0275)	(0.0274)	(3.016)	(1.688)
Bnktcy Judge	-0.0657		-2.434	
	(0.0805)		(1.971)	
Magistrate	-0.0878*		0.523	
	(0.0368)		(1.368)	
	X		X	
		X		X
	699	699	379	379
	0.124	0.129	0.464	0.497

Notes. Regression of Manne training on all covariates (1) and (3) and elastic-net-selected covariates (2) and (4). Robust standard errors clustered at the judge level in parentheses. $*p < 0.05, **p < .01$. Data collapsed by judge. A variable that mentions a position means the judge had prior experience in that position. Codebook for variables available in online appendix.

C.2 Balance Checks on Manne Attendance

We report our main balance checks on judge characteristics and in Appendix Tables A.3 (for circuit judges) and A.4 (for district judges). Columns 1 and 3 include all control variables. Columns 2 and 4 include those selected by elastic net with regularization parameters chosen by cross-validation. Especially, Manne judges are more likely to be Republican appointees, and more likely to be from earlier judicial cohorts. However, Republican-appointee is not correlated with the timing of attendance. Cohorts are unsurprisingly predictive of the timing of attendance.

Table A.5 provides complementary regressions assessing differences in the outcomes according to attendance. These are cross-sectional regressions at the judge×case level, as in the main text. The outcome is as indicated, with the embedding-based measure of Economics Language in Columns 1-4, and conservative Labor/EPA decision in Columns 5-8.

In Columns 1, 2, 5, and 6, the sample includes never-attenders and not-yet-attenders – i.e., Manne judges but in the years before attendance at the course. The regressions

Table A.4: Covariate Balance, District Court Judges

	Ever Attend		Year of Attendance			Ever Attend		Year of Attendance	
	(1)	(2)	(3)	(4)		(1 cont.)	(2 cont.)	(3 cont.)	(4 cont.)
Unified Appoint	-0.0200	-0.0197	-3.711	-3.690	District Atty	-0.0179		-0.347	
	(0.0105)	(0.0105)	(2.805)	(2.790)		(0.0176)		(0.818)	
Cross-Party Appt	-0.0369	-0.0353	-0.820	-0.893	City Council	-0.0643	-0.0627	-1.969	-0.0103
	(0.0302)	(0.0302)	(1.112)	(1.094)		(0.0470)	(0.0490)	(2.427)	(2.689)
Republican	0.0539**	0.0537**	-3.862	-3.894	Cty Comm	-0.0327	-0.0316	1.982	1.726
	(0.00962)	(0.00962)	(2.808)	(2.791)		(0.0340)	(0.0339)	(1.371)	(1.368)
State Senator	0.0316	0.0282	-1.215	-1.342	Asst U.S. Atty	0.0309	0.0336	-0.0345	0.0562
	(0.0309)	(0.0309)	(1.224)	(1.192)		(0.0185)	(0.0185)	(0.613)	(0.614)
State Lower Ct	-0.0168	-0.0159	0.293	0.303	Atty General	0.0810	0.0408	-1.607*	-1.656*
	(0.0160)	(0.0159)	(0.557)	(0.550)		(0.128)	(0.129)	(0.756)	(0.744)
State Sup Court	0.00852	0.00927	0.633	0.584	Asst Dist Atty	-0.00218	-0.00554	-0.636	-0.856
	(0.0249)	(0.0247)	(0.930)	(0.912)		(0.0200)	(0.0199)	(0.659)	(0.639)
State House	-0.0272	-0.0316	1.289	1.244	Any Govt	0.0463**	0.0430**	-0.295	-0.268
	(0.0215)	(0.0213)	(0.949)	(0.955)		(0.0165)	(0.0162)	(0.899)	(0.904)
Solicit Gen Off.	-0.144*		0		Black	0.0512	0.0522	0.255	0.263
	(0.0676)		(.)			(0.0298)	(0.0298)	(1.060)	(1.053)
Solicitor Gen.	0.0632		3.548		Born 1910s	0.146**	0.151**	-5.938	-5.912
	(0.106)		(2.249)			(0.0171)	(0.0173)	(4.022)	(4.020)
U.S. Senator	-0.0530	-0.0518	0	0	Born 1920s	0.344**	0.349**	-2.121	-2.140
	(0.0278)	(0.0270)	(.)	(.)		(0.0248)	(0.0247)	(4.044)	(4.041)
State Atty Gen.	-0.00128		-0.962		Born 1930s	0.289**	0.297**	1.791	1.791
	(0.0239)		(0.928)			(0.0253)	(0.0252)	(4.047)	(4.046)
Priv. Practice	0.00217	0.000786	-0.867	-0.774	Born 1940s	0.120**	0.127**	6.015	6.026
	(0.0241)	(0.0240)	(1.065)	(1.043)		(0.0179)	(0.0178)	(4.058)	(4.055)
Mayor	0.0390	0.0319	-1.304	-0.576	Born 1950s	0.0137	0.0208	8.376*	8.414*
	(0.0486)	(0.0488)	(1.472)	(1.345)		(0.0119)	(0.0114)	(4.257)	(4.247)
Local Court	0.0336	0.0326	0.162	0.152	Bnktcy Judge	-0.0332	-0.0314	-0.861	-0.761
	(0.0254)	(0.0254)	(0.756)	(0.747)		(0.0592)	(0.0591)	(2.530)	(2.512)
U.S. House	-0.0736**		4.494*		Magistrate	-0.0665**	-0.0656**	0.727	0.704
	(0.0198)		(1.806)			(0.0248)	(0.0247)	(1.362)	(1.373)
Governor	0.00120	0.00142	-5.695**	-4.247*					
	(0.0501)	(0.0479)	(0.955)	(1.945)					
All Variables	X		X			X		X	
Post Elastic Net		X		X			X		X
N	2226	2276	350	350		2226	2276	350	350
adj. R-sq	0.113	0.117	0.457	0.468		0.113	0.117	0.457	0.468

Notes. Regression of Manne training on all covariates (1) and (3) and elastic-net-selected covariates (2) and (4). Robust standard errors clustered at the judge level in parentheses. $*p < 0.05, **p < .01$. Data collapsed by judge. A variable that mentions a position means the judge had prior experience in that position.

include circuit×year FE, but not judge FE, so they are cross-sectional regressions comparing pre-Manne judges to never-Manne judges on the same court at the same time. We can see that these groups of judges are quite different from each other on both outcomes. Those differences hold with or without controls for other judge characteristics. That result calls into question the use of never-attenders in the control group, as these differences suggest they do not provide a valid comparison with parallel trends.

In Columns 3, 4, 7, and 8, the sample is limited to not-yet-attenders. Again the regressions are cross-sectional, with the dependent variable being the judge's attendance year. So these regressions measure differences in outcomes between future-attending judges on the same court at the same time who attended in different years. Here, there is no significant difference, with or without controls. That provides additional support for the use of other ever-attending judges as a comparison group in the main regressions.

Table A.5: Balance on Outcomes for Not-Yet-Attenders

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Economics Language				Labor/EPA Decision			
Ever-Attender	0.0926**	0.0944**			-0.0433*	-0.0417*		
	(0.0348)	(0.0347)			(0.0208)	(0.0209)		
Attendance Year			0.00008	0.00164			0.00204	0.0048
			(0.0099)	(0.0100)			(0.0057)	(0.0059)
Republican		-0.00900		-0.0910		0.00445		-0.0120
		(0.0192)		(0.0815)		(0.00951)		(0.0571)
Born in 1910s		-0.0286		-0.0990		-0.00403		0.0414
		(0.0231)		(0.118)		(0.0117)		(0.0489)
Born in 1920s		-0.0449+		-0.0815		-0.0357*		-0.0346
		(0.0239)		(0.0821)		(0.0158)		(0.0513)
Born in 1930s		-0.0913**		-0.131		-0.0185		-0.0903
		(0.0346)		(0.0981)		(0.0192)		(0.0747)
Born in 1940s		-0.0555+		0.157		-0.0140		0.0882
		(0.0330)		(0.156)		(0.0186)		(0.154)
Born in 1950s		-0.0110		0.407**		-0.0177		-0.0499
		(0.0561)		(0.155)		(0.0233)		(0.0997)
Circuit-Year FE	X	X	X	X	X	X	X	X
Never-Attenders	X	X			X	X		
Not-Yet-Attenders	X	X	X	X	X	X	X	X
N	136001	136001	9862	9862	17195	17195	1757	1757
R-sq	0.057	0.058	0.194	0.198	0.188	0.189	0.375	0.379

Notes. Regression of indicated outcome (embedding similarity to economics or conservative labor/EPA decision) on indicator for "ever attended Manne" ("Ever-Attender") and a linear variable for the attendance year, as indicated. All specifications exclude post-attending judges. Columns 3, 4, 7, 8 include only future-attending judges. Standard errors clustered at the judge level in parentheses. $*p < 0.05, **p < .01$.

C.3 Assessment of Never-Attenders as Potential Control Group

This section explains our choice of control group, and in particular the exclusion of never-attending judges. First, we show that never-attenders are on different trends in the outcome variables. In Appendix C.4, we report a set of time series in the main outcome variables (economics language, voting against regulatory agencies, criminal sentencing) separated out by Manne attendance and non-Manne attendance. While we can see post-attendance bumps in these outcomes for attenders, we can also see that attenders and never-attenders are not on parallel trends.

Additional results in this direction are shown in Appendix Table A.5. As discussed in Appendix C.2, we can see that the main outcomes (economics language and labor/environmental decisions) are significantly different when comparing never-attenders and not-yet-attenders. However, there are no differences in these outcomes across cohorts within the set of not-yet-attenders.

In particular, economics language is significantly higher at baseline among the not-yet-attenders (Appendix Table A.5 Columns 1 and 2), compared to the never-attenders. This is indicative of a pre-existing use and interest in economics reasoning, that could be confounded with treatment timing, that one would worry about if using the never-attenders in the control group. For example, if the never-attenders are at a lower baseline compared to their contemporaneous colleagues, they might also be more susceptible to adopting economics ideas from other sources outside the Manne program. That calls into question their usefulness as a clean control group.

A mechanism for heterogeneous exposure of never-attenders outside of Manne could be due to never-attenders coming from younger cohorts, for example. On top of that, there could be selective promotion of lower-court judges who were more economics-oriented. Beyond judges, law clerks could have been exposed to economics in their law school classes.

Indeed, law and economics was not only transmitted to judges by the Manne program. It was promoted in the legal academy through teaching and scholarship,[31] by other organizations such as the Federalist Society and its predecessors (Riehl, 2007), as well as in the popular discourse (Posner, 1987; Hovenkamp and Scott Morton, 2019). A

[31] For example, the first edition of the monograph *Economic Analysis of Law*, Posner (1972), was published in 1972. In his history of the Manne Program, Butler (1999) highlights the "pervasive influence of economics on legal education." He writes: "Some of the younger judges might have had Law & Economics courses while in law school and thus do not feel the need to attend the judicial programs."

notable example of non-Manne economics exposure is D.C. Circuit Judge (and subsequent Supreme Court Justice) Antonin Scalia, who never attended the Manne program yet notably relied on economic reasoning to evaluate car safety standards in *Center for Auto Safety v. Peck*, 751 F.2d 1336 (D.C. Cir. 1985) (Viscusi, 1987).

C.4 Outcome Trends for Attenders and Never-Attenders

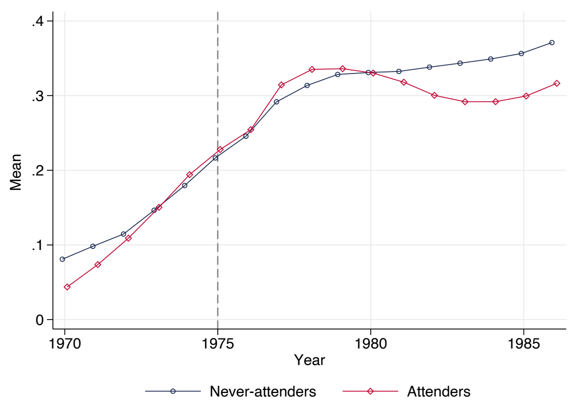
We produced a set of figures showing the trends in the key outcomes by Manne cohort, compared to never attenders. Figures A.4 and A.5 plot each outcome (language and labor/EPA, respectively) by Manne program attendance, aggregating the cohorts of attendance in 4 groups (1976-1980, 1981-1987, 1988-1992, and 1993-1997). Note that there were no Manne cohorts in 1983 or 1985, hence the second group includes 1981-1987. Figure A.6 reports a corresponding figure for the criminal sentencing outcomes, limited to the 1993-1997 cohorts because of data availability.

The series were produced as follows. Within a range of 6 years before and after the first and last cohort in the group, we plot the outcome for the attenders and the non attenders, with observations weighted by judge-year as in the main text. To reduce noise in the outcomes, point values are a rolling smoothed average over 5 years. The smoothing has a cut-off before and after the first attendance year of the group, so years nearer to the cut-off have less smoothing and can be noisier.

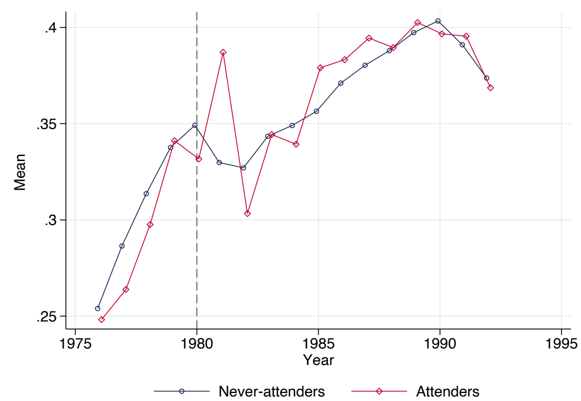
The series are somewhat noisy even after the mild smoothing. And there are big differences in pre-trends between attenders and never-attenders in the unadjusted data points. Still, overall, we see evidence for a post-attendance increase in associated outcomes for the Manne attendees (in red), relative to the never-attenders (in blue).

Figure A.4: Trends in Economics Language, by Manne Attendance Cohorts

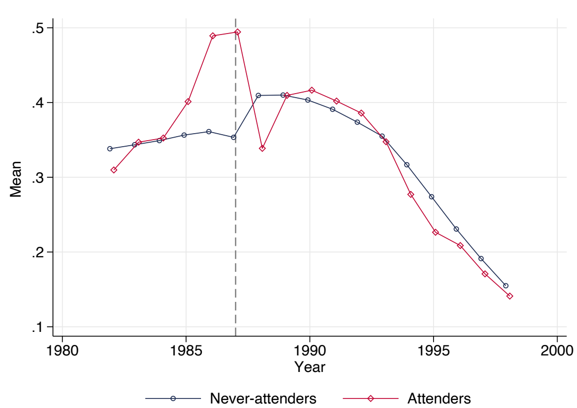
(a) 1976-1980 cohorts



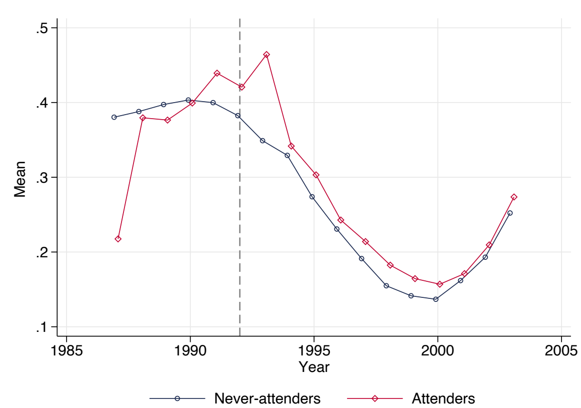
(b) 1981-1987 cohorts



(c) 1988-1992 cohorts



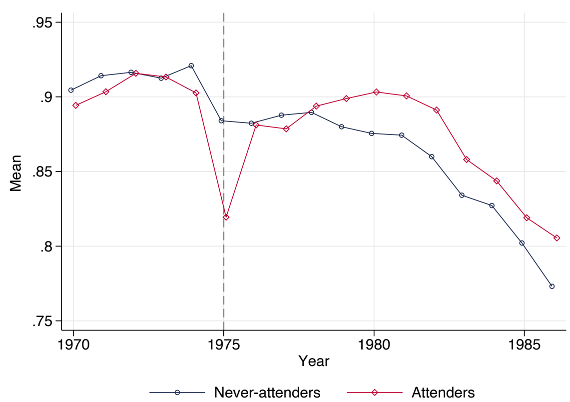
(d) 1993-1997 cohorts



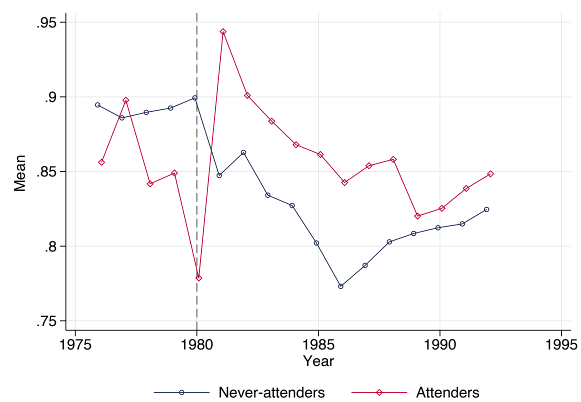
Notes. Average Economics Language over time, for Manne and non-Manne judges, separately by four cohort groups as indicated. Plotted values give smoothed rolling averages. Vertical dashed line at year before first cohort in group.

Figure A.5: Trends in Ruling Against Labor/EPA, by Manne Attendance Cohorts

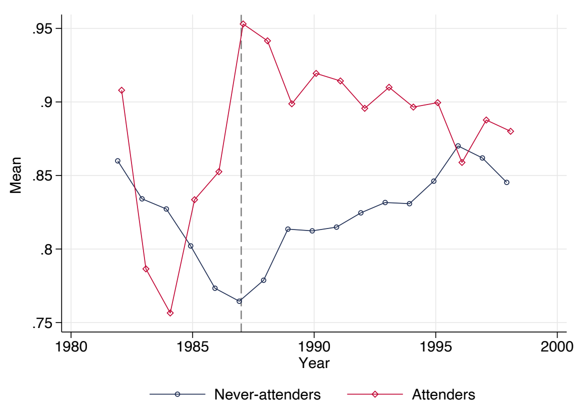
(a) 1976-1980 cohorts



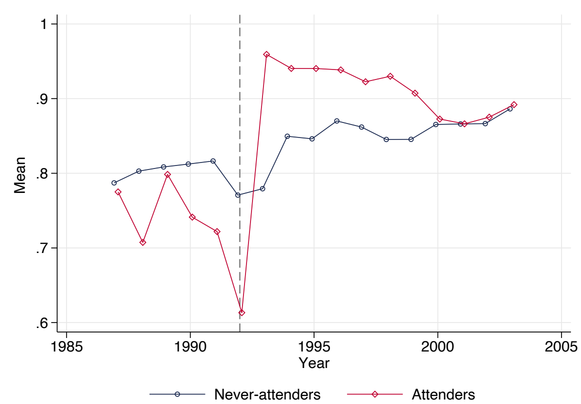
(b) 1981-1987 cohorts



(c) 1988-1992 cohorts

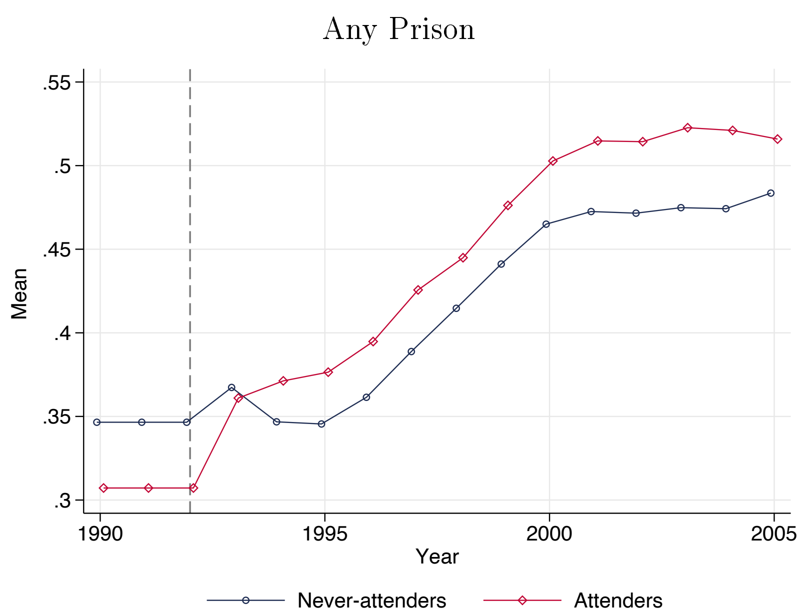


(d) 1993-1997 cohorts



Notes. Average of Ruling against Labor/EPA over time, for Manne and non-Manne judges, separately by four cohort groups as indicated. Plotted values give smoothed rolling averages. Vertical dashed line at year before first cohort in group.

Figure A.6: Trends in Criminal Sentencing by Manne Attendance, 1993-1997 cohorts



Notes. Average criminal sentencing (any prison) over time, for Manne and non-Manne judges, for the 1993-1997 cohorts. Plotted values give smoothed rolling averages. Vertical dashed line at year before first cohort in group.

C.5 Peer Spillovers in Economics

A methodological issue, as well as a substantively interesting question, is whether attendance at Manne program has impacts on other non-attending judges on a court, through peer effects. Such spillovers are unlikely in the district courts, where judges work individually in separate chambers and do not collaborate with other judges. In the circuit courts, however, judges work on rotating three-judge panels, where they interact and decide together, sometimes concurring or dissenting with each others' judgments. Further, circuit decisions are binding precedent on future judges in the court and they should be followed, cited, and are often quoted directly. These are direct mechanisms for sharing of ideas from the Manne program and the associated textual indicators. This is most important for our purposes because if could contaminate control-group judges and result in a violation of the Stable Unit Treatment Values Assumption (SUTVA).

This appendix provides exploration of the issue of peer spillovers in economics ideas from the Manne program. Anecdotally, we found a striking potential example of peer effects in the Second Circuit's *Northeastern Telephone v. American Telephone and Telegraph* (1981). The author of the opinion, Irving Kaufman, had not attended Manne, but one of his co-panelists, Charles Brieant, had just attended in the 1979 cohort. Perhaps influenced by Brieant, Crawford came up with some striking passages:

> Although the term "predatory pricing" lacks a precise economic meaning. . . courts and commentators have generally defined predation as "the deliberate sacrifice of present revenues for the purpose of driving rivals out of the market and then recouping the losses through higher profits earned in the absence of competition." . . . Detailed economic analysis of this behavior is of comparatively recent vintage, gaining wide recognition only in 1975, with the publication of Areeda & Turner's incisive article. . . This approach involves a comparison of a monopolist's prices and expenditures, and necessarily entails an understanding of the various economic costs that confront a firm. These expenses fall into two rough categories variable costs, those which fluctuate with a firm's output, and fixed costs, those which are independent of output. Variable costs typically include such items as materials, fuel, labor, maintenance, licensing fees, and depreciation occasioned by use. The sum of all variable costs divided by output yields average variable cost. Fixed costs generally include management expenses, interest on bonded debt, the rate of return necessary to attract and maintain equity

investment, irreducible overhead, and depreciation occasioned by obsolescence. The sum of the firm's fixed and variable costs divided by output equals average cost. By definition, average cost exceeds average variable cost at all levels of output. . .

Marginal cost, unlike the categories just defined, cannot be determined using data generated by conventional accounting methods; it is an economist's construction. It is traditionally defined as "the increment to total cost that results from producing an additional increment of output." . . . In most industries, marginal cost is low at low levels of output. It may decline slightly as output increases, but soon reaches a minimum, and then increases continuously with further increases in production. Thus, at low output levels, it is less than either average variable cost or average cost. At high levels, it is greater than either. . .

Adopting marginal cost as the proper test of predatory pricing is consistent with the pro-competitive thrust of the Sherman Act. When the price of a dominant firm's product equals the product's marginal costs, "only less efficient firms will suffer larger losses per unit of output; more efficient firms will be losing less or even operating profitably." . . . Marginal cost pricing thus fosters competition on the basis of relative efficiency. Establishing a pricing floor above marginal cost would encourage underutilization of productive resources and would provide a price "umbrella" under which less efficient firms could hide from the stresses and storms of competition. Moreover, marginal cost pricing maximizes short-run consumer welfare, since when price equals marginal cost, consumers are willing to pay the expense incurred in producing the last unit of output. At prices above marginal cost, per contra, output is restricted, and consumers are deprived of products the value of which exceed their costs of production.

Passages like these exemplify the peer language adoption we are interested in testing for.

To test for peer effects, we estimate

$$Y_{ijct} = \alpha_j + \alpha_t + \gamma \bar{Z}_{ct}^{-j} + \epsilon_{ijct} \tag{3}$$

where as in the main text, Y_{ijct} is the outcome – i.e. economics language and labor/EPA decision – for case i by judge j in court (circuit or district) c during year t. Further, we include judge fixed effects α_j and time fixed effects α_t. The new term $\bar{Z}_{ct}^{-j}$ is the share of other judges (weighted by caseload) on court c at time t (besides j) who have attended the Manne program. Again, standard errors are clustered by judge.

Table A.6: Peer Effects of Manne Attendance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Effect on Economics Language						Labor/EPA Decision		
Peer Attendance	0.0166	-0.0721	0.128	-0.0801	-0.224	-0.0310	-0.0258	-0.143	0.169
	(0.118)	(0.185)	(0.118)	(0.171)	(0.267)	(0.431)	(0.109)	(0.120)	(0.223)
Court FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X	X	X	X
Econ Case		X		X		X	-	-	-
All Judges	X	X					X		
Never-Attenders			X	X				X	
Attenders					X	X			X
N	164193	42695	125818	32467	38375	10228	19330	15106	4224
R-sq	0.138	0.198	0.140	0.194	0.137	0.217	0.278	0.269	0.341

Notes. Regression of embedding similarity to economics (columns 1-6) and labor/EPA (columns 7-9) on the share of peer judges (other judges on the same court) who have had Manne training. Columns 1-6 include opinion authors only. Never-Attenders, Attenders, Is Author, and Econ Case: Sample limited, as indicated. Standard errors clustered at the judge level in parentheses. $*p < 0.05, **p < .01$.

Appendix Table A.6 reports effects of peer attendance share on economics language (columns 1-6) and labor/EPA decisions (columns 7-9). Overall, there is little evidence of spillovers.

Another issue raised by these spillovers is that error residuals may be correlated across cases within circuit-year. To allow for such correlation, we ran our main regressions with two-way clustering of standard errors by judge and court-year. The resulting confidence intervals are similar, as shown in Appendix Figures A.14, A.27, and A.34.

We also experimented with peer spillovers within the same three-judge panel. We did not find evidence of peer effects in that context either.

C.6 Negative-Weighting Issues from Staggered Treatment Timing

A recent line of papers, starting with Goodman-Bacon (2021), have identified problems with differences-in-differences estimates using two-way fixed effects when there is variation in timing across treated units. These papers have shown that heterogeneity in treatment effects plus differential timing of treatment – where units treated in the past are used as controls – can result in some event study estimates being biased by negative weighting (Jakiela, 2021). Since we have multiple treatments over time, for each Manne attendance cohort, this is a potential problem in our context.

These papers have produced a number of approaches for addressing this problem. However, the standard stacked diff-in-diff approaches do not map directly into our setting. We do not have a standard panel dataset, with each treated unit (a judge) having a single observation in each time period (a year). Our data is at the case level, and judges could have multiple cases, one case, or no cases (in a given outcome class) in a given year. We must include circuit-year fixed effects to obtain block randomization of judges to cases, so we cannot aggregate up to the judge-year level. Further, there is major imbalance in the panel, where judges are regularly entering and leaving over time.

Most importantly, the off-the-shelf estimators use never-treated units as the comparison group. As discussed above, given the different trends and spillovers for never-attenders, the never-treated judges in our context do not provide a clean control group. One variant of Callaway and Santanna (2020) uses only future-attenders, and not already-attenders, but that does not give us enough statistical power. Thus, the off-the shelf estimators would not work well in our context.

Our first approach to the problem is to diagnose the severity of the negative-weights problem. De Chaisemartin and d'Haultfoeuille (2020) provide a method to do so. In the paper, they show that the TWFE estimator can be decomposed as a weighted average of several ATEs, that might be heterogeneous across groups or periods. If the control group is treated in consecutive periods, then "the treatment effect at the second period gets differenced out by the DID", generating negative weights that might cause the TWFE to be negative even if all ATEs are positive. We used their provided Stata package, `twowayfeweights`, to diagnose the presence of negative weights in our baseline TWFE regressions. These statistics are reported in Table A.7 Panel A. We can see that for almost all treated units ("LATEs"), the weights are positive.

Table A.7: Diagnostics for Negative Weights in Staggered Treatment Timing

A. Diagnostic from De Chaisemartin and d'Haultfoeuille (2020)

	(1) LATEs with Positive weights	(2) LATEs with Negative weights	(3) LATEs with Positive weights	(4) LATEs with Negative weights
Outcome	6 Years Window		Full Sample	
Labor/EPA Conservative	56	1	57	0
Conservative Econ Vote	21	1	21	0
Conservative Non-Econ Vote	44	0	44	0
Embedding Similarity	157	1	158	0

B. Diagnostic from Jakiela (2021)

	(1) Labor/EPA Conservative	(2) Conservative Econ Vote	(3) Conservative Non-Econ Vote	(4) Embedding Similarity
Heterogeneity by Treatment Status	0.0518	0.0626	0.329	-0.00207
	(0.153)	(0.372)	(0.211)	(0.00302)
Share Neg. Resids	0.330	0.280	0.310	0.360
Heterogeneity × Share Neg Resids	0.017	0.017	0.1	-0.0007
DD Coeff.	0.15	0.3	0.05	0.01

Panel A: Number of local average treatment effects (LATEs, or treated units) with positive weights, versus those with negative weights, using the diagnostic method proposed by De Chaisemartin and d'Haultfoeuille (2020). Panel B: estimates for heterogeneity by treatment status and the share of negative residuals by outcome, using the diagnostic from Jakiela (2021).

Next, we apply the complementary diagnostic by Jakiela (2021), focusing on the event-study sample. First, we check for negative weights by looking at the distribution of residualized treatment indicators – that is, after partialling out circuit-year and judge fixed effects. Since $\hat{\gamma} = \sum_i \frac{Y_i Z_i}{Z_i^2}$, if Z_i is negative then some observations are weighted negatively. We regress the residualized outcomes on a residualized treatment indicator (i.e. partialling out circuit-year and judge FE). Table A.7 Panel B shows that the correlation between the residuals within pre-Manne observations is similar to the correlation within the post-Manne observations, suggesting that there is not much heterogeneity by duration of treatment. The upper bound on the bias from negative weighting implied by these estimates is proportionally small compared to the estimates reported in the main text. Overall, as discussed in Jakiela (2021), relying on the standard two-way fixed-effects estimates is justified given that the standard adjustment procedures, such as Callaway and Santanna (2020), may provide noisier estimates. That is important in our setting, as those estimators rely on never-treated units as a control group, and our never-treated judges do not provide a clean comparison in light of different trends and peer spillovers.

C.7 Results using Never-Attenders in Control Group

This section reports our main regression results with the full sample of judges. That means that never-attenders (never-treated judges) are included in the comparison group. As discussed, these judges do not provide a good counterfactual for the treated judges because they are on different trends. Hence, these results should not be interpreted causally but they provide a comparison for the main results.

Table A.8 reports differences-in-differences estimates from Eq. 1 using the full sample of judges. Each Panel A-E reports the results for a different outcome measure, as indicated. The specifications are indicated at the bottom of the table and include the baseline (circuit-year and judge fixed effects), baseline but limiting to the pre-1987 period, including party-year interacted fixed effects, including elastic net selected judge covariates interacted with year fixed effects, baseline but limiting to court-years with below-median peer share, adding peer share controls interacted with judge fixed effects, case topic fixed effects, baseline with no weighting, baseline with winsorized weights, robust standard errors rather than clustering, and two-way clustering by judge and circuit-year.

For the last specification/column, we adopted the approach from Callaway and Santanna (2020) and Ang (2021) to correct for staggered treatment timing. For each attendance cohort, we estimated the difference-in-difference specification for the effect of Manne attendance on the outcome. As a control group, we include ever-attenders that attended more than six years in the future or more than six years in the past (and therefore not changing treatment status in this window). We then averaged these cohort-level estimates to produce adjusted estimates for the overall effect, weighted by the number of cases in each cohort.

These regressions including the full sample generate mostly null estimates for the treatment effect of Manne attendance. The exception is the Labor/EPA outcome, where we find consistently positive and statistically significant estimates.

Table A.8: DD Regression Results Including Never-Attenders in Control Group

	A. Ellickson Embedding Similarity to Economics Language											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post Manne	-0.062	0.051	-0.064	-0.043	0.073	-0.063	-0.079	0.016	-0.049	-0.062	-0.062	0.144
	(0.066)	(0.084)	(0.066)	(0.065)	(0.101)	(0.156)	(0.062)	(0.032)	(0.062)	(0.054)	(0.072)	(0.127)
N	42694	19196	42694	42694	20493	42694	42691	42694	42694	42694	42694	.
	B. ML-Predicted Similarity to Economics											
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Post Manne	0.05	0.049	0.051	0.068+	0.033	0.148*	0.070*	0.013	0.042	0.05	0.05	.10
	(0.038)	(0.062)	(0.038)	(0.039)	(0.070)	(0.073)	(0.029)	(0.017)	(0.033)	(0.037)	(0.039)	(.082)
N	93185	36036	93185	93185	44874	93185	93063	93185	93185	93185	93185	.
	C. Labor/EPA Voting Against Regulatory Agencies											
	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)	(33)	(34)	(35)	(36)
Post Manne	0.104**	0.137**	0.102**	0.100**	0.115+	0.078	0.100**	0.017	0.082**	0.104**	0.104**	0.143*
	(0.031)	(0.046)	(0.031)	(0.030)	(0.066)	(0.079)	(0.031)	(0.014)	(0.025)	(0.028)	(0.038)	(0.065)
N	19330	12344	19330	19330	12607	19330	19010	19330	19330	19330	19330	.
	D. Hand-Coded Conservative Votes (Economics Cases)											
	(37)	(38)	(39)	(40)	(41)	(42)	(43)	(44)	(45)	(46)	(47)	(48)
Post Manne	-0.049	0.053	-0.054	-0.007	-0.012	-0.088	-0.056	-0.014	-0.042	-0.049	-0.049	.026
	(0.050)	(0.058)	(0.048)	(0.050)	(0.105)	(0.090)	(0.050)	(0.029)	(0.051)	(0.043)	(0.070)	(.116)
N	6664	3609	6664	6664	3437	6664	6664	6664	6664	6664	6664	.
	E. Hand-Coded Conservative Votes (Non-Economics Cases)											
	(49)	(50)	(51)	(52)	(53)	(54)	(55)	(56)	(57)	(58)	(59)	(60)
Post Manne	-0.008	0.05	-0.014	-0.004	-0.009	-0.108	-0.007	-0.031	0.009	-0.008	-0.008	.008
	(0.039)	(0.048)	(0.039)	(0.036)	(0.057)	(0.071)	(0.036)	(0.021)	(0.036)	(0.032)	(0.044)	(.071)
N	20557	9781	20557	20557	10312	20557	20364	20557	20557	20557	20557	.
Circuit-Year / Judge FE	X	X	X	X	X	X	X	X	X	X	X	X
Pre-1987		X										
Party × Year FE			X									
E-net × Year FE				X								
Low Peer Share					X							
Judge FE X Peer Share						X						
Case Topic FE							X					
No Weighting								X				
Winsorized Weights									X			
Robust SE										X		
Two-Way Cluster SE's											X	
Callaway-Santanna												X

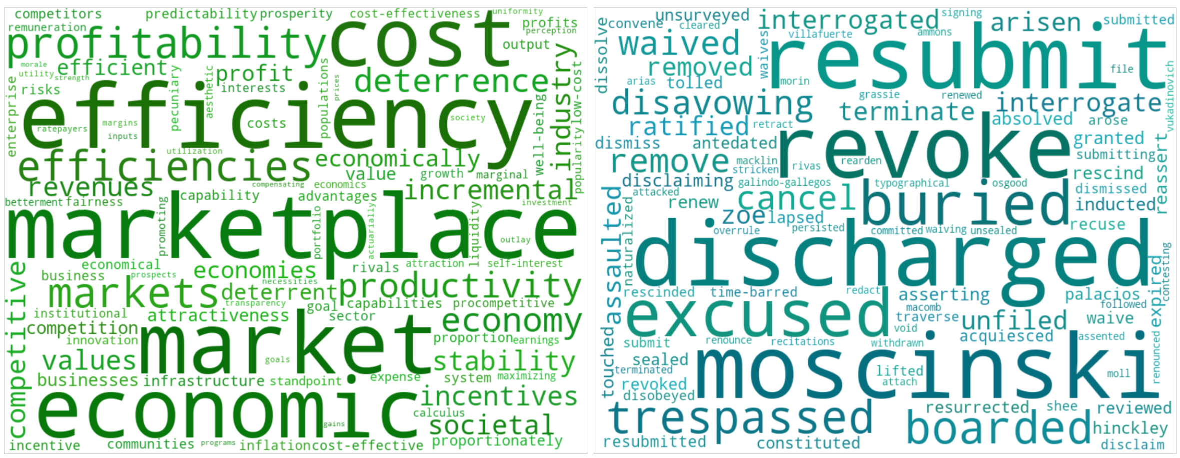
Notes. Estimated effects of Manne training in the full sample of judges where never-attenders are included. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means limiting to years 1986 and earlier. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Case Topic FE is fixed effect for case topic. Low Peer Share only includes circuit-years where the share of peer Manne attendees is below median. Judge FE X Peer Share means the share of a judge's peers who have attended, interacted with judge FE. No Weighting means observations are not weighted. Winsorized weights means regression weights are winsorized at 99%. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and circuit-year. Callaway-Santanna means stacked DD across all cohorts, as described in the text. Panels are by outcome, as indicated. $+p < .1, *p < 0.05, **p < .01$.

D Additional Material on Economics Language

D.1 Embedding Similarity to Ellickson Lexicon

Figure A.7: Words Correlated with Law-and-Economics Lexicon Dimension

(a) Positively Associated Words **(b) Negatively Associated Words**



Notes. Panel (a): The set of words that have the highest cosine similarity to the average word vector for Ellickson phrases in the word embedding space; Panel (b): Words that have the lowest (most negative) cosine similarity to this vector.

Figure A.7 shows the set of words driving our word embedding dimension for law and economics. We can see clearly economics-related language, such as efficiency and markets. The negatively associated words are very different, and don't involve economics at all. The words are mostly related to procedure. "Moscinski" is the name of a defendant in a 1997 free speech case.

How does this language look in context? To get at this question, we sampled approximately 80,000 sentences from the corpus and produced the Ellickson economics similarity metric at the sentence level. Here are the ten sentences ranking highest on this metric (with mild editing, and excluding two short sentences):

1. It explained that "the policy allows increased direct access to transportation markets, imposes upon LDCs the need to discipline costs to maintain customers, allows pipelines to compete for markets served inefficiently, provides leverage to parties seeking to obtain services priced efficiently, and assures the benefits of competition to all market participants."

2. Applying the principle that cost burdens should be matched with service benefits, the commission includes in the rate base only property that it considers "necessary to the efficient conduct

of a utility's business, presently or within a reasonable period." The commission has considerable discretion to determine the appropriate time, in advance of property going into service, at which it first becomes "necessary to the efficient conduct of a utility's business"; it may distinguish among various types of expenditures upon the basis of any relevant concern, including its concern with the differing incentives it has invoked in the cases of PUC-LT and PHFU.

3. In connection with its abandonment of structural separation, the FCC established numerous nonstructural safeguards to reduce the danger of cross-subsidization and anti-competitive action by the BOCs, including: 1) adoption of the principle of full allocation of costs across services, rejecting the view that unregulated activities should bear only the incremental or marginal costs they cause, joint cost order; requiring that the additional costs of upgrading or replacing facilities primarily for the benefit of unregulated services be excluded from the regulated accounts; adoption of specific allocation rules requiring that a carrier charge nonregulated activity at the tariff rate for any tariffed services it uses; requiring allocation of costs directly to the relevant activity where possible, and otherwise assigning costs on the basis of a formula related to the allocation of other costs and expenses; adoption of rules governing transactions between affiliates; imposition of comparably efficient interconnection and open network architecture requirements.

4. In short, the District Court failed to make the kind of factual determinations necessary to render the appellees' efficiency defense sufficiently concrete to offset the FTC's prima facie showing.

5. In an oligopolistic market characterized by few producers, price leadership occurs when firms engage in interdependent pricing, setting their prices at a profit-maximizing, supracompetitive level by recognizing their shared economic interests with respect to price and output decisions.

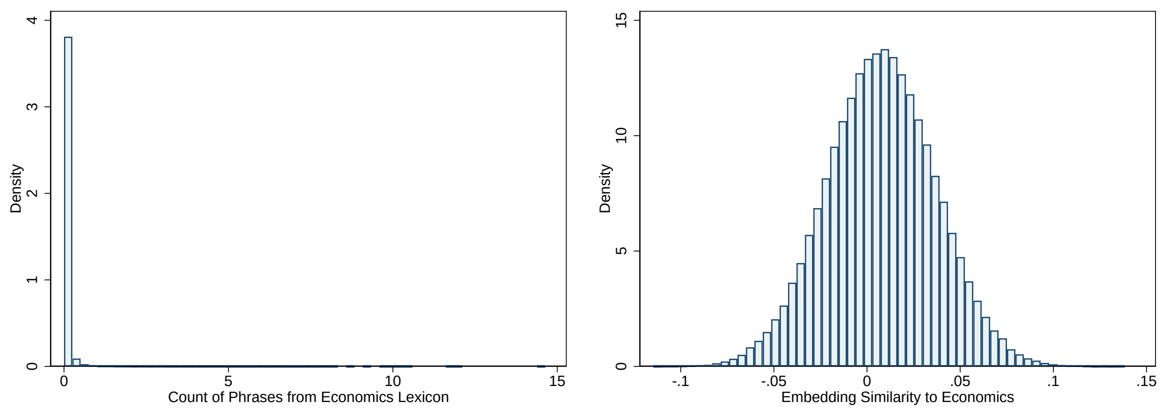
6. The commission should require Conrail to present evidence on the impact of the cancellations on Conrail outbound traffic, to submit additional evidence on the relative efficiency of the individual closed and open through routes as distinct from the relative efficiency of the closed and open routes in the aggregate, and to give the petitioners a reasonable opportunity to analyze the computer tapes and programs underlying the study.

7. In other words, the inquiry of whether a still-employed claimant is totally disabled should be guided by a pragmatic test measuring whether his health has been sacrificed sufficiently to require monetary compensation.

8. As the commission recognized, however, a regulator can realistically seek to achieve "second best" efficiency: the set of prices that allows the firm to recover its total costs while minimizing adverse effects on consumer surplus -- the difference between the price of a good and what consumers would be willing to pay for that good.

9. Reducing the number of interchanges and reducing the average length of haul have no economic significance in themselves, though both might reduce average transit time, which would be a benefit to shippers and hence a genuine efficiency gain

Figure A.8: Distributions of Count-Based and Embedding-Based Econ Language Measures



Notes. Histograms by case of the number of words in a case from the Ellickson lexicon (left graph), vs the embedding-based economics language similarity measure (right graph).

10. While the two most common methods of quantifying antitrust damages are the "before and after" and "yardstick" measures of lost profits, this court has defined the two methods as follows: the before and after theory compares the plaintiff's profit record prior to the violation with that subsequent to it.

Interestingly, these sentences are using not just economics language but many are doing economics reasoning. Consistent with measuring law-and-economics legal reasoning, Sentences #6 and #9 (and many others in the set of most economics-oriented sentences) were written by Circuit Judge Richard Posner, a well-known law-and-economics proponent.

Figure A.9 shows the trend in the average case similarity to the law-econ dimension since 1950. We see that economics cases tend to score more highly, as expected. In addition, the use of economics language has been increasing over time.

In regard to these trends, it is important to note that changes in economics language are driven in part by changes in the topics covered in appealed cases. The measure pulls in correlated factual and doctrinal text features. Changes in the economic content of appeals is not an identification problem, as we condition out circuit-year effects and have random assignment of cases. As discussed further in Appendix C.1, we know that Manne attendance is not affecting the cases that judges review or author.

Figure A.9: Trends in Economics Language, by Econ and Non-Econ Cases

Notes. Average embedding similarity to Ellickson law-and-economics lexicon, plotted by biennium and separately by economics cases (regulation and labor) and other cases. Error spikes give standard error of the mean. Data weighted to treat judge-years equally.

D.2 Robustness Checks on Economics Language Results

Table A.9: Regression Estimates: Effect of Manne Program on Economics Language

					A. Short-Run Effects on Attenders						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post Manne	0.355**	0.474*	0.346*	0.320*	0.784**	0.335	0.301*	0.099	0.322*	0.355**	0.355*
	(0.131)	(0.183)	(0.141)	(0.159)	(0.254)	(0.270)	(0.118)	(0.087)	(0.126)	(0.115)	(0.145)
N (Opinions)	5267	3191	5267	5267	2702	5267	5265	5267	5267	5267	5267
					B. Long-Run Effects on Attenders						
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Post Manne	0.026	0.465**	0.014	0.11	0.515*	-0.009	0.015	0.009	0.015	0.026	0.026
	(0.096)	(0.160)	(0.099)	(0.105)	(0.197)	(0.189)	(0.089)	(0.043)	(0.087)	(0.081)	(0.107)
N (Opinions)	10215	4085	10215	10215	4121	10215	10215	10215	10215	10215	10215
Circuit-Year / Judge FE	X	X	X	X	X	X	X	X	X	X	X
Pre-1987		X									
Party × Year FE			X								
E-net × Year FE				X							
Low Peer Share					X						
Judge FE X Peer Share						X					
Case Topic FE							X				
No Weighting								X			
Winsorized Weights									X		
Robust SE										X	
Two-Way Cluster SE's											X

Notes. Estimated effects of Manne training on embedding similarity of an economics case to the law-and-economics lexicon, described in Subsection III.2. Sample is limited to case opinion authors. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means limiting to years 1986 and earlier. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Case Topic FE is fixed effect for case topic. Low Peer Share only includes circuit-years where the share of peer Manne attendees is below median. Judge FE X Peer Share means the share of a judge's peers who have attended, interacted with judge FE. No Weighting means observations are not weighted. Winsorized weights means regression weights are winsorized at 99%. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and circuit-year. Panel A includes the event-study sample. Panel B includes ever-attenders for all years. $+p < .1, *p < 0.05, **p < .01$.

Appendix Table A.9 report the effects of Manne attendance using differences-in-differences regressions. We estimate $\hat{\gamma}$ from Equation (1) with the text measure as the outcome. In Panel A (Columns 1-11), we limit to the event study sample (only Manne attendees, and only six years before and after attendance). Panel B (Columns 2-22) includes Manne attendees but for all years of their career (between 1970 and 2005), so it measures more long-term treatment effects. Columns 1/12 have the baseline specification with circuit-year fixed effects and judge fixed effects. One can already see there, as is the case in most of the specifications, that the short-run effects are positive and significant, while the long-run effects are small and not significant. So the rest of this discussion focuses on the short-run effects.

Column 2 limits to the pre-1987 sample, and the effect is larger and more significant. This means that the effect of Manne on language was strongest in the early period when law and economics was less familiar. The effect is weaker in the latter period when economics ideas had become common in law schools and the courts. Consistent with this, we see in Column 5 that the effect is even larger and more significant when limiting to the courts and years with below-median peer share (below 15% attenders).

Next, we assess robustness to additional controls. The main short-run results are robust to controlling for party of a judge's appointing president, interacted with year fixed effects (Column 3), for the elastic-net-selected controls (predicting timing of attendance) interacted with year fixed effects (Column 4), for the share of peers who are Manne attendees, interacted with judge fixed effects (Column 6), or for case topic, i.e. fixed effects for the 94 detailed legal areas (Column 7).

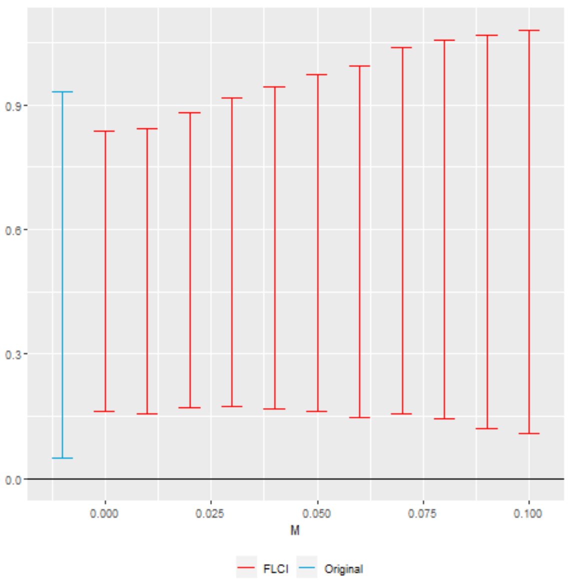
Columns 8 and 9 test robustness to weighting. The effect of Manne on economics language is not robust to using unweighted regressions (Column 8), where courts and years with more cases are weighted more. In the case of language, this null is somewhat mechanical. There is an increasing caseload over time, which works to down-weight the early period where the language effects are concentrated (e.g. Column 2). Column 9 shows that the baseline results are robust to winsorizing the weights, meaning that the effects are not an artifact of court-years with low caseloads.

Finally, Columns 10 and 11 show that statistical inference is not sensitive to how standard errors are constructed. The precision of the estimates is similar without clustering (Column 10), or with two-way clustering by judge and court-year.

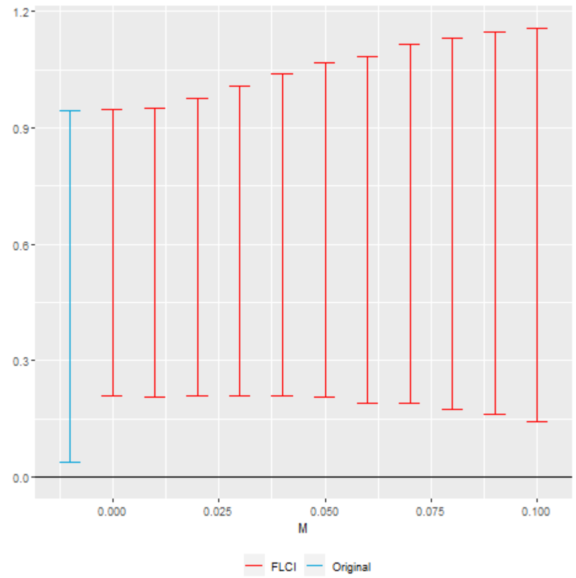
Moving back to the event study, we run the test from Rambachan and Roth (2019) to check for non-linear pre-trends. Appendix Figure A.10 shows that there is no major sign of non-linear pre-trends according to that test. Further, we can show that the effects on economics language are not driven just by selective attrition. We produced event-study estimates for a balanced sample of judges, for a shorter time window (three years before and after). As shown in Appendix Figure A.11, the estimates are noisy and short-lived, yet overall consistent with our main results.

Figure A.10: Ellickson Econ Language: Pre-Trend Sensitivity Analysis

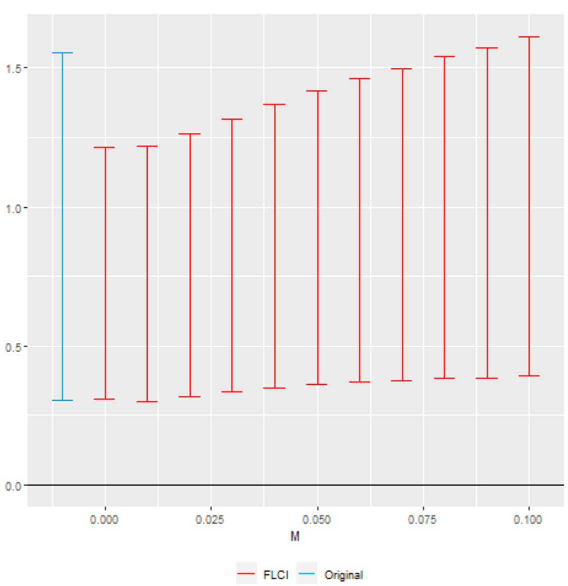
A. Baseline



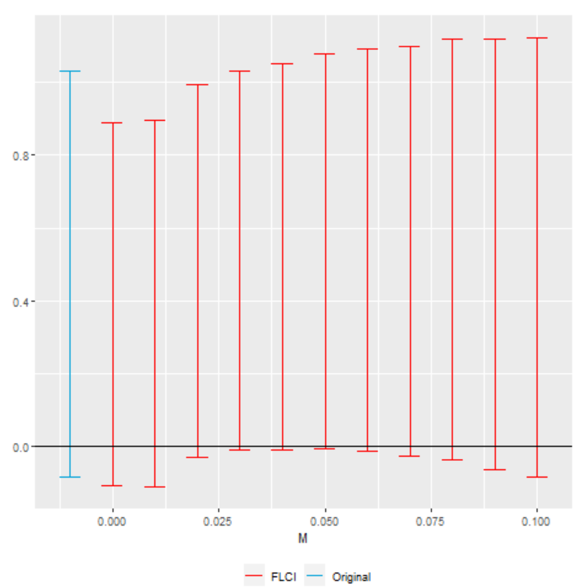
B. + Elastic Net Controls



C. Pre-1987

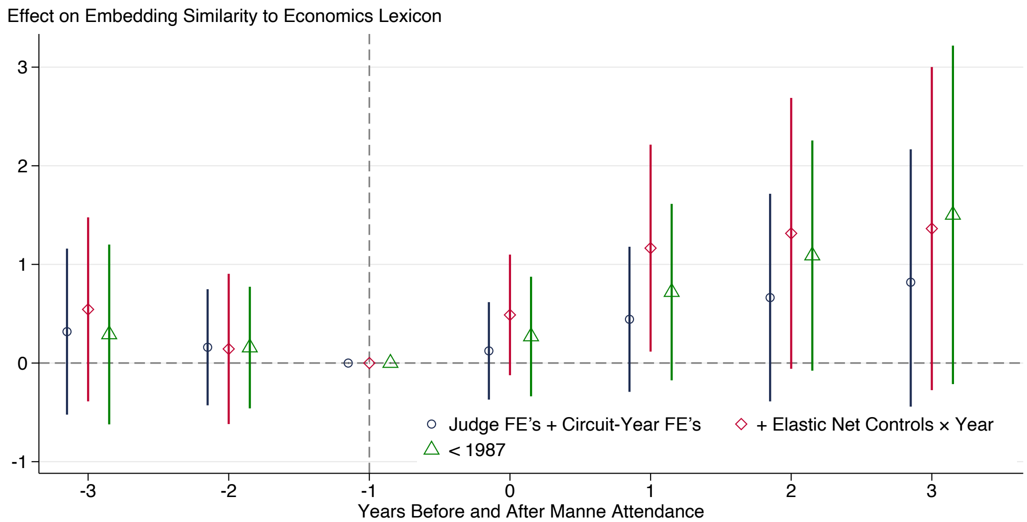


D. Judge FE's × Peer Share



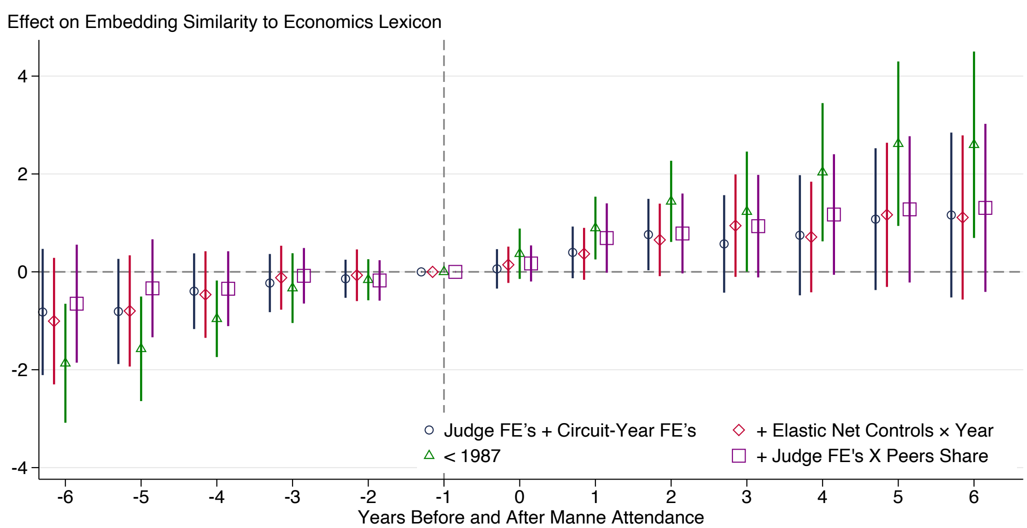
Notes. Sensitivity graphs for violation of the parallel trends assumption, applying the method from Rambachan and Roth (2019); see also Ang (2021). Outcome is Ellickson Embedding Similarity to Economics. The axis-crossing value of of $\bar{M}$ indicates that the significant treatment effect of Manne attendance (at 95% confidence) is robust to allowing for a non-linearity in the differential trend in the post-treatment period that is about M times the maximum observed non-linearity in the pre-treatment period.

Figure A.11: Econ Language: Balanced Panel with Shorter Window



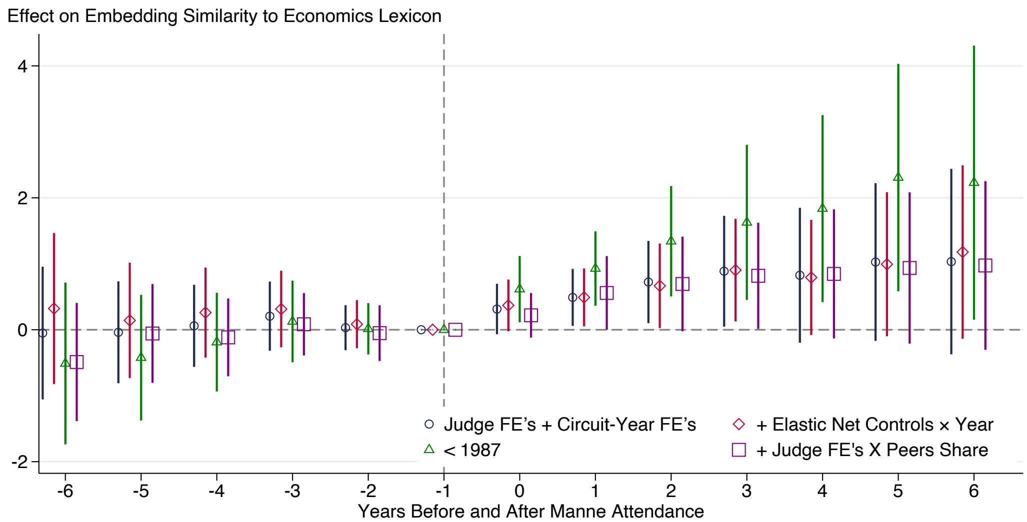
Notes. Event study regressions with balanced panels of judges, for three years of lags and leads, for Embedding Similarity to Economics. For other details see notes in the associated main-text exhibits. The spec with peer share controls is dropped, as the confidence intervals are very large with the reduced sample.

Figure A.12: Ellickson Event Study: Dropping 2nd, 8th, 9th, and D.C. Circuits



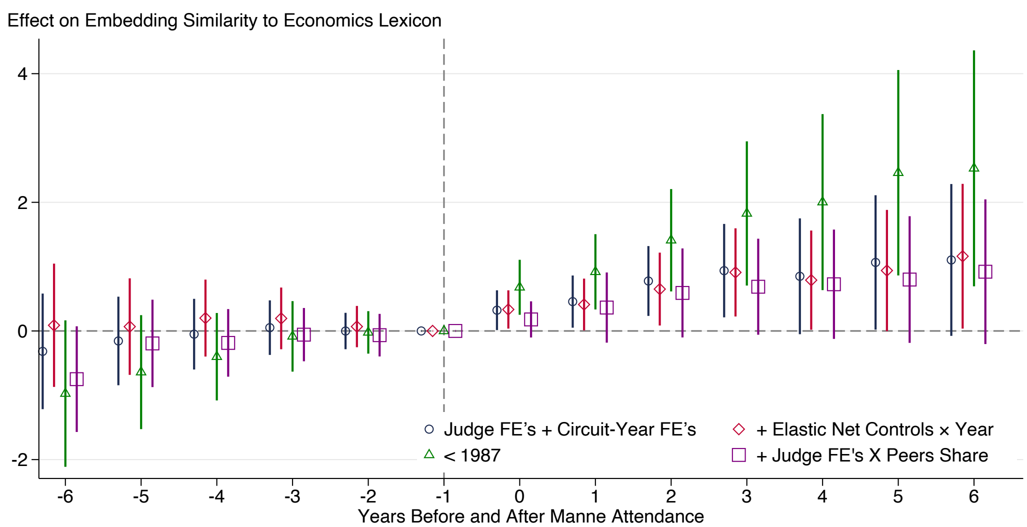
Notes. Main event study results for the circuit courts (from Figure III) but dropping those circuits for which Levy and Chilton (2015) find nonrandom assignment in their calendar dataset from the years 2008-2013 (2nd, 8th, 9th, and D.C. Circuits). Outcome is Economics Language. For other details see notes in the associated main-text exhibits.

Figure A.14: Econ Language Event Study, Two-Way Clustering



Notes. Main event study results for embedding measure of econ language with two-way clustering of standard errors by judge and court-year. For other details see notes in the associated main-text exhibit.

Figure A.13: Ellickson Event Study with Legal Topic Fixed Effects



Notes. Main event study results for the circuit courts (from Figure III) but including fixed effects for 94 detailed legal topics. Outcome is Economics Language. For other details see notes in the associated main-text exhibits.

D.3 Economics Language in District Court Opinions

As an additional robustness check on our main analysis of economics language in the circuits, we produce similar measures in the district courts. In civil cases, district court judges also often write opinions explaining their decisions. We put together a corpus of all published district court opinions from 1970 through 2005 using data from LexisNexis. We matched the opinions to our judges database based on the author. We then produced the same embedding-based measure of economics language for each opinion. The resulting dataset has an economics language score and matched authoring judge for 508,325 opinions from 1970 through 2005.

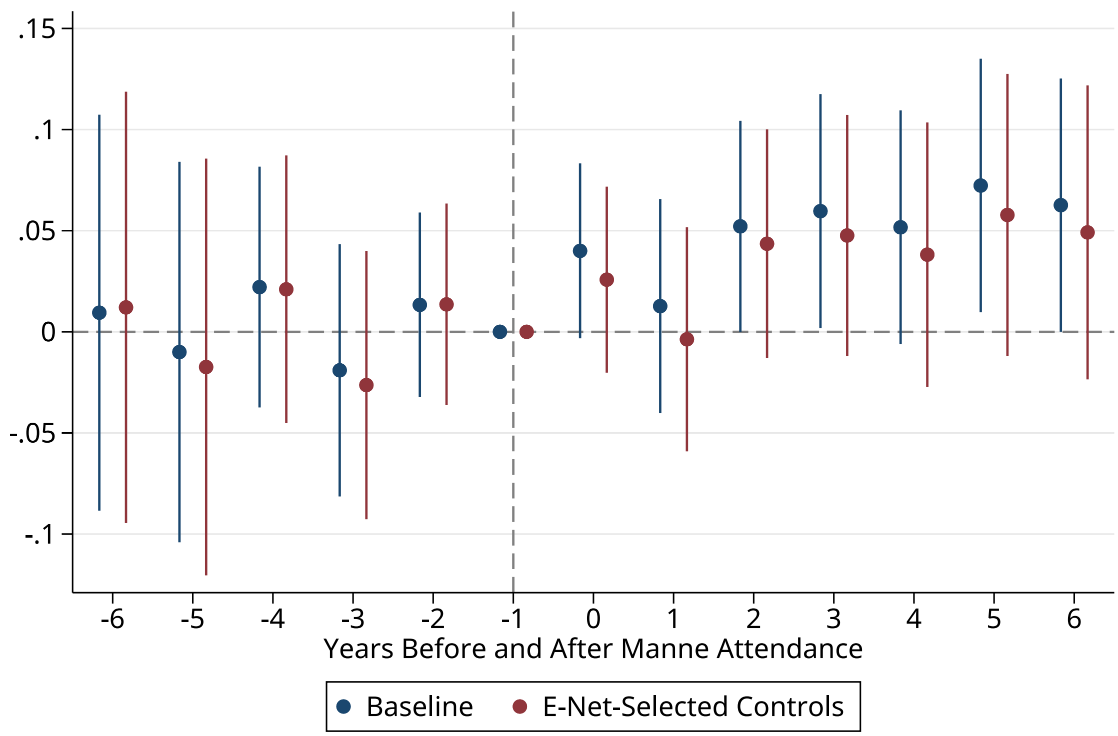
The district court opinion data are not directly comparable to the circuit court opinion data. First, opinions can be written at multiple stages of the case process. For example, there could be a ruling on summary judgment at the beginning of the case, a ruling on admissibility evidence, and/or a ruling on the final judgment. Further, writing a published opinion is optional at all of these stages (including the final ruling), and often (actually, most of the time), the district judge will not publish an opinion. Next, unlike circuit court judges, district court judges operate alone, and do not work on panels with other judges. Finally, we do not have systematic case topic metadata so we cannot identify "economics cases" the way we do with the circuit courts.

We run event study regressions for the effect of Manne attendance on the embedding-based similarity of a judge's opinions to the economics lexicon (standardized to mean zero and variance one). We use a panel event study design with ever-attenders in the control group. Judge fixed effects and court-year fixed effects are absorbed, with additional specifications adding elastic-net-selected controls interacted with year fixed effects.

The event study results for economics language in the district courts are reported in Figure A.15. We report results for the baseline with judge and court-year FE (blue), and an additional series with elastic-net-selected controls interacted with year (red). We see a statistical increase in the use of economics language among district court judges after attendance at the Manne program. The results are qualitatively similar, yet not statistically significant, when including the elastic-net-selected judge characteristics.

While these estimates are not as robust as with the circuit courts, overall these results for the district courts add additional evidence for the effect of the Manne program in shifting the language – and expressed judicial reasoning – used by the attending judging. Like the Circuit Courts, the results are not robust to including never-attenders

Figure A.15: Econ Language Event Study in District Courts

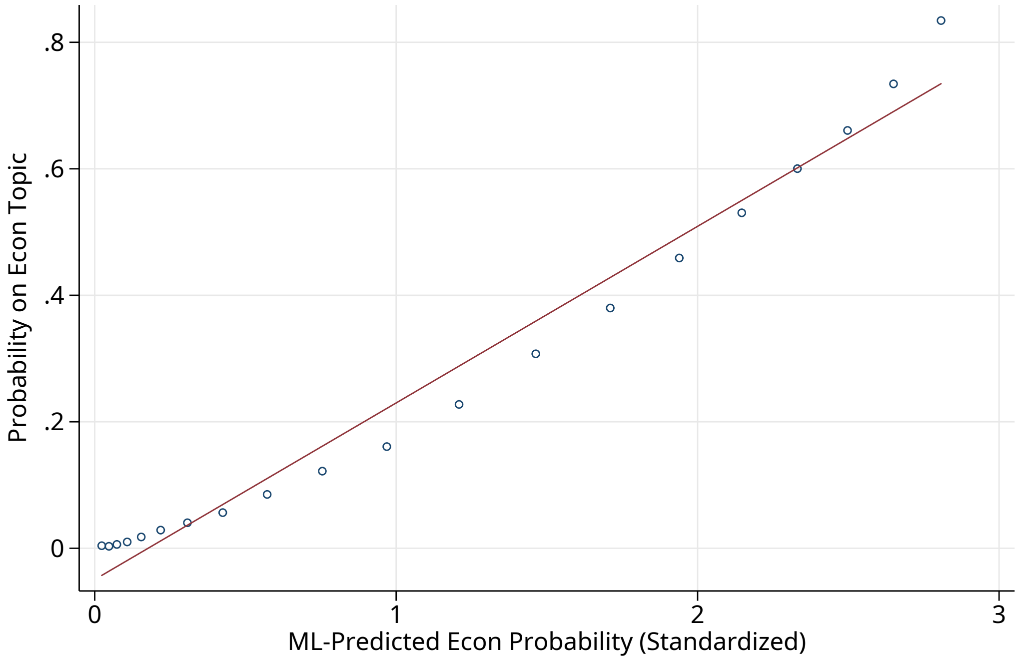


Notes. Event study results for embedding measure of econ language in the district court opinions. Regressions include judge FE and district-year FE (blue), with the second series (red) including elastic-net-selected controls for judge characteristics, interacted with year fixed effects. Sample limited to ever-attenders. Standard errors clustered by judge.

in the control group. We do not use weighting in these regressions to adjust for caseload size, given that caseload varies endogenously for the district courts. The results are noisier, but qualitatively similar, with weighting to adjust for caseload size.

D.4 Text-Predicted Similarity to Economics Topics

Figure A.16: Calibration Plot for Predicted Econ-Related Case

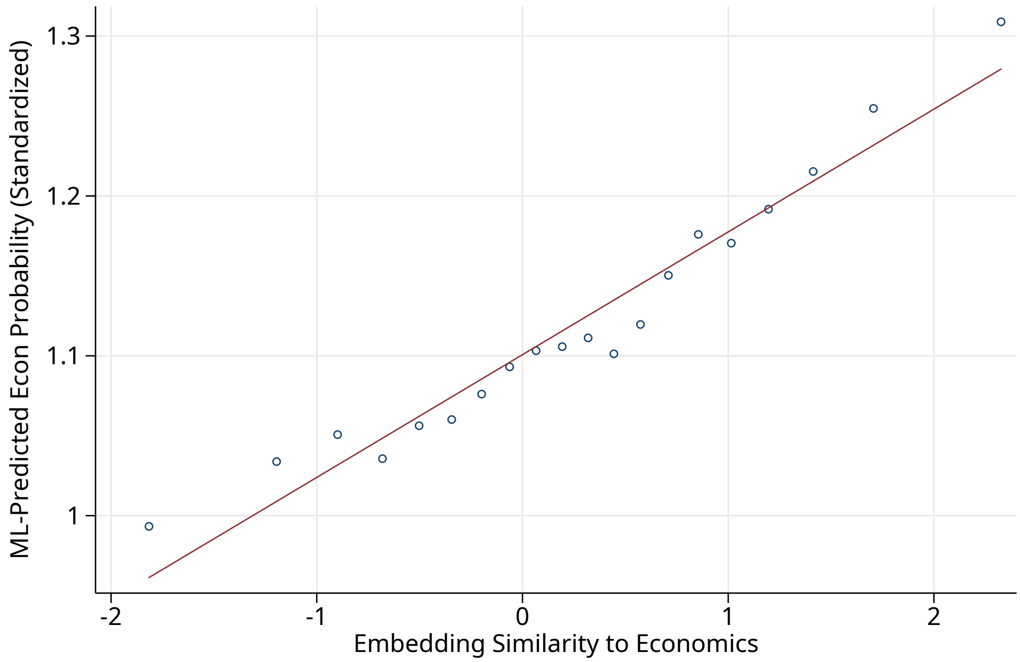


Notes. Binscatter of L2 logistic prediction for y = text-predicted economics case, in held out test sample. Horizontal axis is the predicted probability that a case is on an economics topic. The vertical axis is the true rate by bins of the prediction.

We produced a second measure of economics language using supervised learning on corpus metadata. For each case in our corpus, we have labels for whether it is an economics-related case (regulation or labor). We take this label (economics case) as an outcome and predict it based on the text features of the case. For the text features, we used the Arora, Liang, and Ma (2016) document embeddings for each case – i.e., the average of the word embeddings for each word in a case, with inverse frequency weighting to down-weight common words.

For the machine learning model, we use an L2-penalized logistic regression (ridge penalty, with $L_2 = .004$ selected to maximize fit in held-out data). The model can predict this label with 81% accuracy in a held-out test set. Figure A.16 visualizes how well our prediction model replicates the probability that a case is about economics. We can see that cases that are more likely to be econ-related based on the prediction model,

Figure A.17: Econ Embedding Similarity Correlated with Text-Predicted Econ



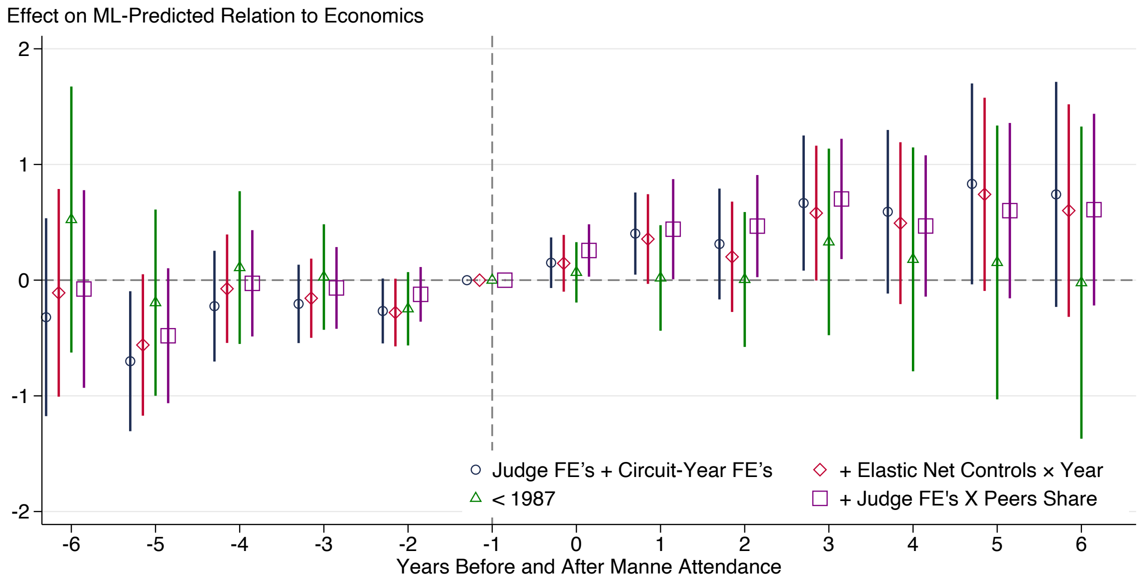
Notes. Binscatter of a case's embedding similarity to the Ellickson Law-and-Economics lexicon, against the predicted probability that a case is concerning economics topics.

are also more likely to be so in the held-out test data. This shows that the machine learning model is not over-fitting the data and replicating the label.

We then apply the trained model to the full corpus to form the text-predicted probability that a case is on an economics topic. This prediction then provides a scale of economics jurisprudence, inasmuch as even non-economics-related cases are treated using economics language. For this reason, in our preferred specification we only include non-economics-related cases in analyzing this outcome.

Figure A.17 shows that the two measures of economics style are correlated. This relationship is highly statistically significant ($\beta = .077, p < .0001$). The $R^2 = .01$ is quite low, however, so the variables are measuring different dimensions of language.

Figure A.18: Effect of Manne Program on Alternative Economics Language Measure

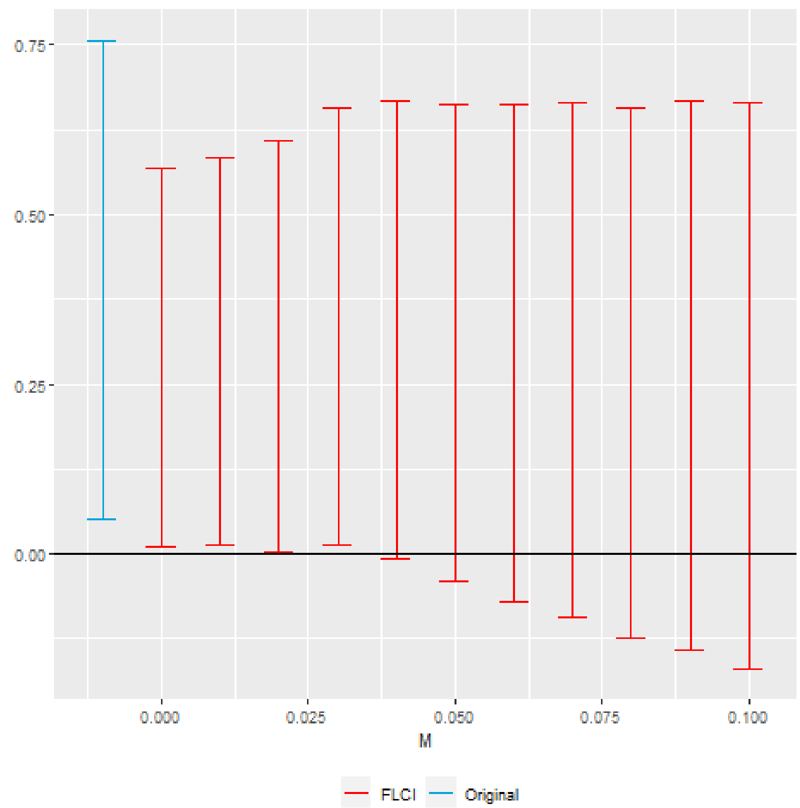


Notes. Event study effect of Manne attendance on text-based predicted probability that case is on an economics topic (regulation or labor). Sample is limited to case authors. Regressions include judge and circuit-year fixed effects (blue circles), with additional specifications adding elastic net controls (red diamonds), limiting to the pre-1987 period(green triangles), and including peer share controls interacted with judge (purple squares). Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals, with standard errors clustered by judge.

Figure A.18 reports the event study for the machine learning measure. The effect is significant even five years later. There is no significant pre-trend. Figure A.19 reports the "honest parallel trends" test from Rambachan and Roth (2019) and shows there are not substantial non-linear pre-trends relative to the years after treatment.

Appendix Table A.10 reports the associated differences-in-differences estimates. Again, there is a positive effect of Manne attendance on the use of economics language, which is not quite significant in the short run (Columns 1-11). The effect is robustly significant for the long-run (Column 12-22), even without weighting (Column 19), except when limiting to the low-peer-share sample (Column 16). The estimated effect is about 12 percent of a standard deviation. Overall, these results providing supporting evidence on the increasing use of economics language after judges attend the Manne program.

Figure A.19: ML-Based Econ Similarity: Pre-Trend Sensitivity Analysis



Notes. Sensitivity graphs for violation of the parallel trends assumption, applying the method from Rambachan and Roth (2019); see also Ang (2021). Outcome is ML-Predicted Similarity to Economics. The axis-crossing value of of $\bar{M}$ indicates that the significant treatment effect of Manne attendance (at 95% confidence) is robust to allowing for a non-linearity in the differential trend in the post-treatment period that is about M times the maximum observed non-linearity in the pre-treatment period.

Table A.10: Effect of Manne Program on Alternative Economics Language Measure

	A. Short-Run Effects on Attenders										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post Manne	0.147	0.085	0.124	0.123	-0.055	0.145	0.162*	0.057	0.129	0.147	0.147
	(0.106)	(0.129)	(0.104)	(0.118)	(0.174)	(0.152)	(0.070)	(0.054)	(0.096)	(0.100)	(0.131)
N (Opinions)	9946	5463	9946	9946	5206	9946	9929	9946	9946	9946	9946
	B. Long-Run Effects on Attenders										
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Post Manne	0.113*	0.156+	0.115*	0.139*	-0.019	0.227*	0.099*	0.056*	0.103*	0.113*	0.113*
	(0.049)	(0.087)	(0.051)	(0.058)	(0.114)	(0.105)	(0.041)	(0.025)	(0.046)	(0.052)	(0.055)
N (Opinions)	20174	7213	20174	20174	8285	20174	20147	20174	20174	20174	20174
Circuit-Year / Judge FE	X	X	X	X	X	X	X	X	X	X	X
Pre-1987		X									
Party × Year FE			X								
E-net × Year FE				X							
Low Peer Share					X						
Judge FE X Peer Share						X					
Case Topic FE							X				
No Weighting								X			
Winsorized Weights									X		
Robust SE										X	
Two-Way Cluster SE's											X

Notes. Estimated effects of Manne training on alternative ML-based measure of economics case to the law-and-economics lexicon. Sample is limited to case opinion authors. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means limiting to years 1986 and earlier. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Case Topic FE is fixed effect for case topic. Low Peer Share only includes circuit-years where the share of peer Manne attendees is below median. Judge FE X Peer Share means the share of a judge's peers who have attended, interacted with judge FE. No Weighting means observations are not weighted. winsorized weights means regression weights are winsorized at 99%. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and circuit-year. Panel A includes the event-study sample. Panel B includes ever-attenders for all years. $+p < .1, *p < 0.05, **p < .01$.

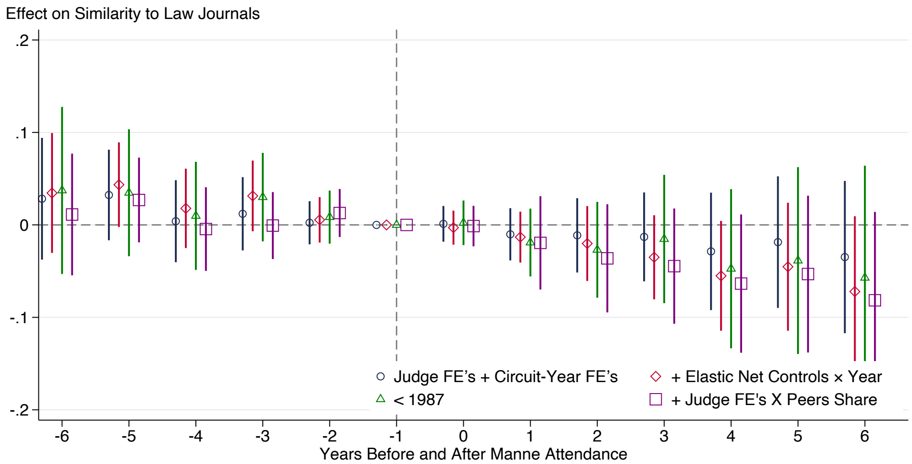
D.5 Additional Text-Data Results

This section reports results for some additional measures of ideology and conservatism constructed from the text of the judicial opinions. First, we check whether our language measure is picking up more academic language, rather than economics language. The idea is that the Manne program worked by exposing judges to a more academic approach to law, rather than a more economic approach. To check for this, we produce a measure of non-economic academic language – similarity to a corpus of law journal articles published in recent decades. We find no effect of Manne attendance on a scholarly style (Appendix Figure A.20 Panel A), consistent with an economics approach mattering more than an academic approach. Similarly, we show that there is no increase (and perhaps a decrease) in the use of quantitative or statistical language (Appendix Figure A.21).

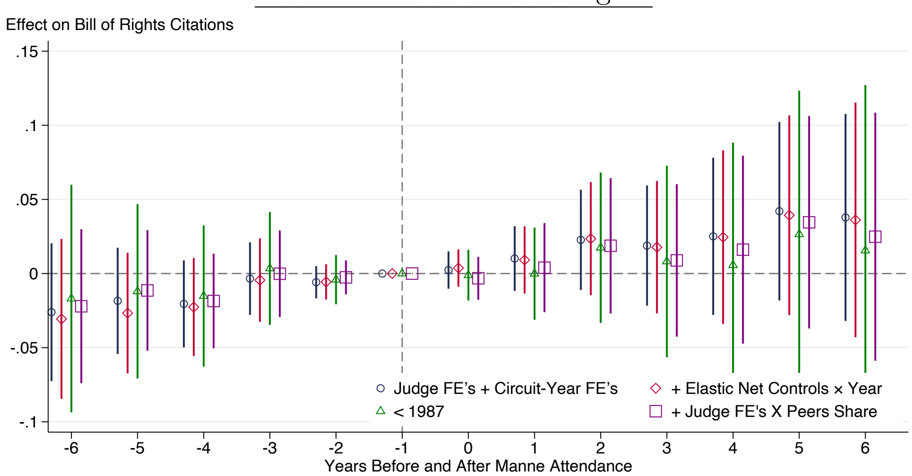
Second, we ask whether the Manne program shifted concerns with core constitutional questions, a traditional focus of conservative legal theory (Berger 1977). We produce a measure of constitutional reasoning using the citation choices of judges. We use frequency of citations to the Bill of Rights amendments for this outcome and find no effect (Appendix Figure A.20 Panel B). We tried other measures of constitutionalist reasoning, such as citations directly to the Constitution's articles, with similar zero effects.

Figure A.20: Effect of Manne Program on Alternative Legal Language Measures

A. Similarity to Law Journals



B. Citations to Bill of Rights

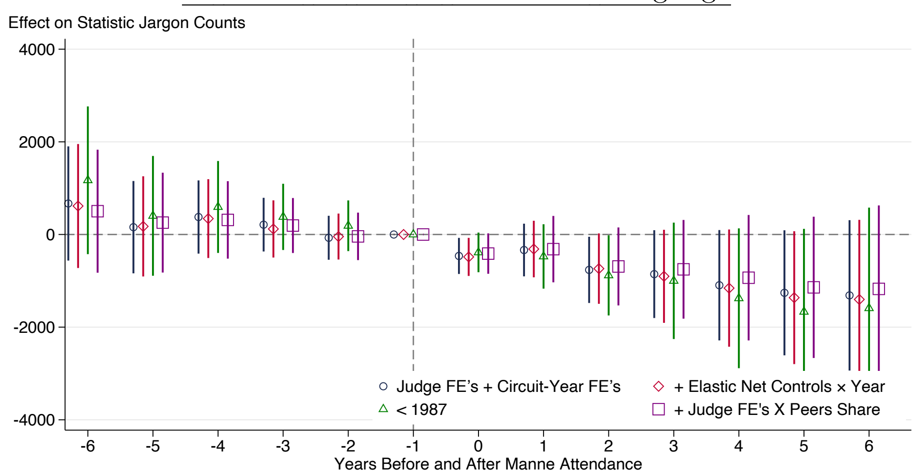


Notes. Estimated effect of Manne training on legal language measures. Panel A: case text similarity to law journals. Panel B: citations to key bill of rights amendments. Specifications are the same as other event studies: baseline, e-net controls, pre-1987, peer share controls. 95% confidence intervals constructed using standard errors clustered at the judge level. Sample is limited to case opinion authors and economics cases. Observations are weighted to adjust for varying caseloads across courts and years.

Figure A.21: Effect of Manne Program on Use of Quantitative/Statistical Language

A. Effect on Use of Quantitative Language

B. Effect on Use of Statistical Language



Notes. Estimated effect of Manne training on language. Panel A: effect on quantitative language, using a Lexicon from LIWC. Panel B: Effect on statistics-related language (statistic*, econometrics, median, "standard deviation", "standard error"). Specifications are the same as other event studies: baseline, e-net controls, pre-1987, peer share controls. 95% confidence intervals constructed using standard errors clustered at the judge level. Observations are weighted to adjust for varying caseloads across courts and years.

E Additional Results on Regulatory Decisions

Table A.11: Regression Results: Voting Against Labor/Environmental Agencies

			A. Short-Run Effects on Attenders								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post Manne	0.157*	0.165*	0.172*	0.143	0.294*	0.231+	0.161*	-0.008	0.115+	0.157**	0.157*
	(0.067)	(0.073)	(0.073)	(0.088)	(0.133)	(0.138)	(0.073)	(0.025)	(0.058)	(0.060)	(0.064)
N (Votes)	2639	2068	2639	2639	1663	2639	2593	2639	2639	2639	2639
			B. Long-Run Effects on Attenders								
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Post Manne	0.172**	0.168*	0.169**	0.147**	0.257*	0.267**	0.169**	0.024	0.139**	0.172**	0.172**
	(0.048)	(0.065)	(0.050)	(0.052)	(0.108)	(0.088)	(0.049)	(0.022)	(0.043)	(0.042)	(0.050)
N (Votes)	4192	2564	4192	4192	2294	4192	4125	4192	4192	4192	4192
Circuit-Year / Judge FE	X	X	X	X	X	X	X	X	X	X	X
Pre-1987		X									
Party × Year FE			X								
E-net × Year FE				X							
Low Peer Share					X						
Judge FE × Peer Share						X					
Case Topic FE							X				
No Weighting								X			
Winsorized Weights									X		
Robust SE										X	
Two-Way Cluster SE's											X

Notes. Estimated effects of Manne training on voting against regulatory agencies. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means limiting to years 1986 and earlier. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Case Topic FE is fixed effect for case topic. Low Peer Share only includes circuit-years where the share of peer Manne attendees is below median. Judge FE X Peer Share means the share of a judge's peers who have attended, interacted with judge FE. No Weighting means observations are not weighted. Winsorized weights means regression weights are winsorized at 99%. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and circuit-year. Panel A includes the event-study sample. Panel B includes ever-attenders for all years. $+p < .1, *p < 0.05, **p < .01$.

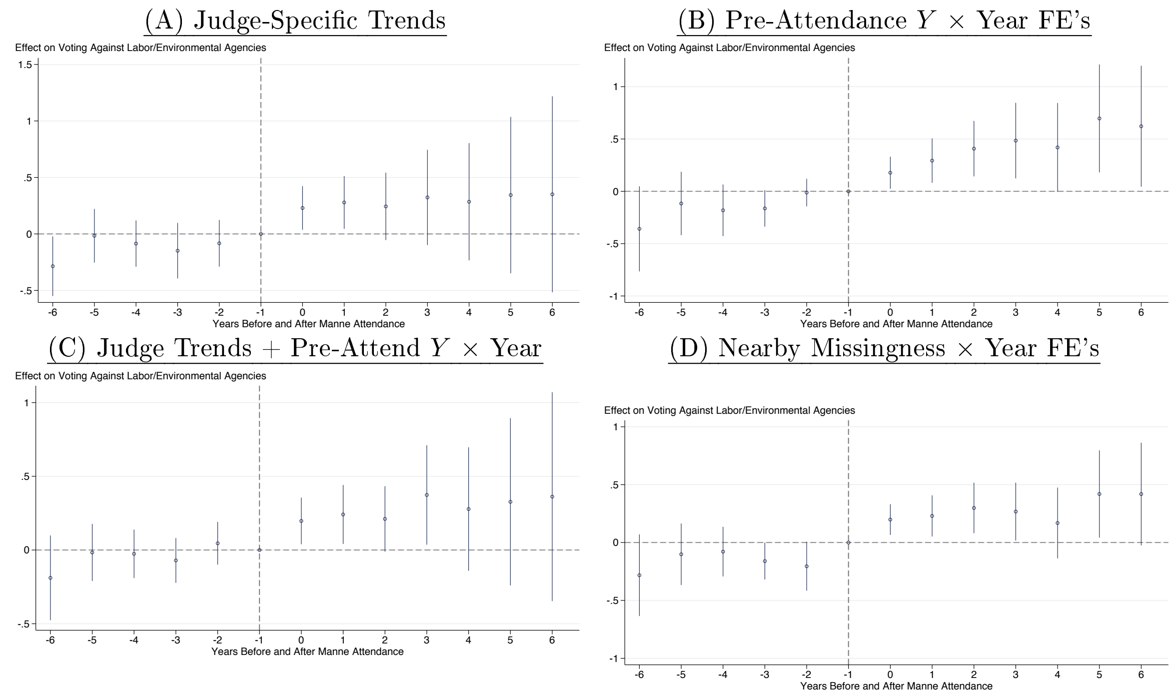
The regression results for Equation (1) with the regulatory-agencies outcome are reported in Appendix Table A.11. The specifications are the same as those outlined in the discussion of the economics language result above. We report results in the short run (within six years) in Panel A (Columns 1-11) and in the long run (all years) in Panel B (Columns 12-22).

Overall, there are positive and statistically significant effects of Manne attendance on voting against labor/environmental agencies. In the long run, the estimate is robustly significant and stable across the inclusion of controls (Columns 14, 15, 16, 18). In the short run, the coefficient is stable across control specs (Columns 3, 4, 6, 7) but not quite significant with elastic net controls (Column 4). The effect is substantially larger when limiting to the courts and years with below median (under 15%) share of post-Manne judges (Columns 5, 16). That larger effect could be related to what was observed in the main event studies, that the results are robust, without pre-trends, when adjusting for peer share.

As with the economics-language outcome, the results for labor/EPA are not robust to regressions without weights – that is, when courts and years with more cases are weighted more. This is due in part to the larger effect in the early years when the caseload was lower, and a smaller effect in the later years. The winsorized-weights specification is still positive and significant, however (Columns 9, 20), meaning that the results are not driven by outlier judge-years with few cases. Finally, results are robust to alternative standard errors (Columns 10, 11, 21, 22).

It is also worth noting that, unlike the other outcomes, the regression results with labor/EPA also hold in the full sample of judges including never-attenders in the control group. As shown in Appendix Table A.8 Panel C, the coefficients have a similar positive magnitude and are mostly significant across specifications. The exceptions are adjusting for judge-specific peer share (Column 30) and not weighting (Column 32).

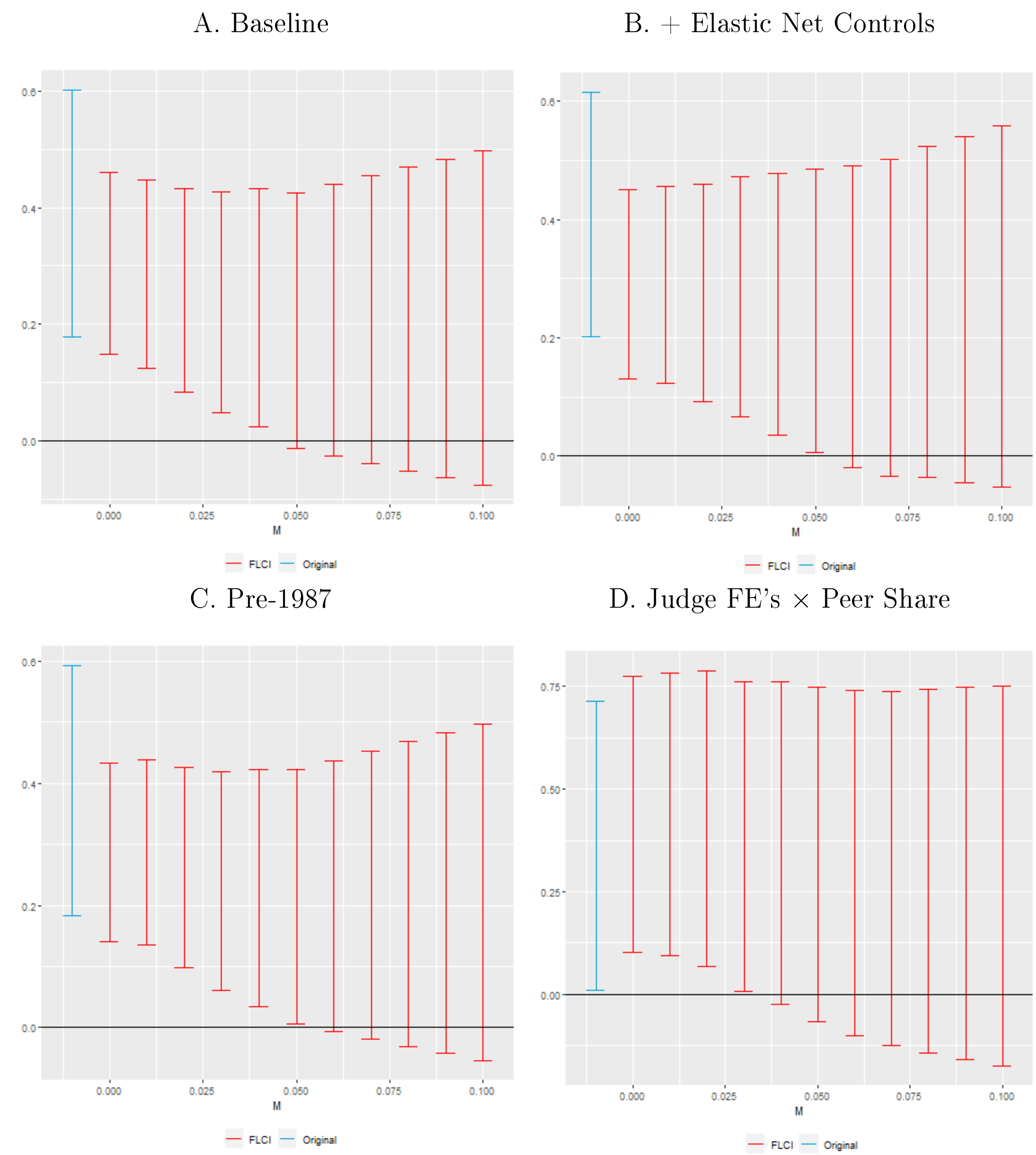
Figure A.22: Event Study for Labor/Environmental, Alternative Specifications



Notes. Event study effects on voting against government agency on labor and environmental issues, relative to year before attendance at Manne economics training. All panels include judge fixed effects and circuit-year fixed effects. Panel A includes judge-specific trends. Panel B includes the average for the outcome in the three years before attendance, interacted with year. Panel C includes both the trends and the pre-attendance variables interacted with year. Panel D includes indicators for whether a labor-EPA case is present in two years before/after the attend year, interacted with year. Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals, with standard errors clustered by judge.

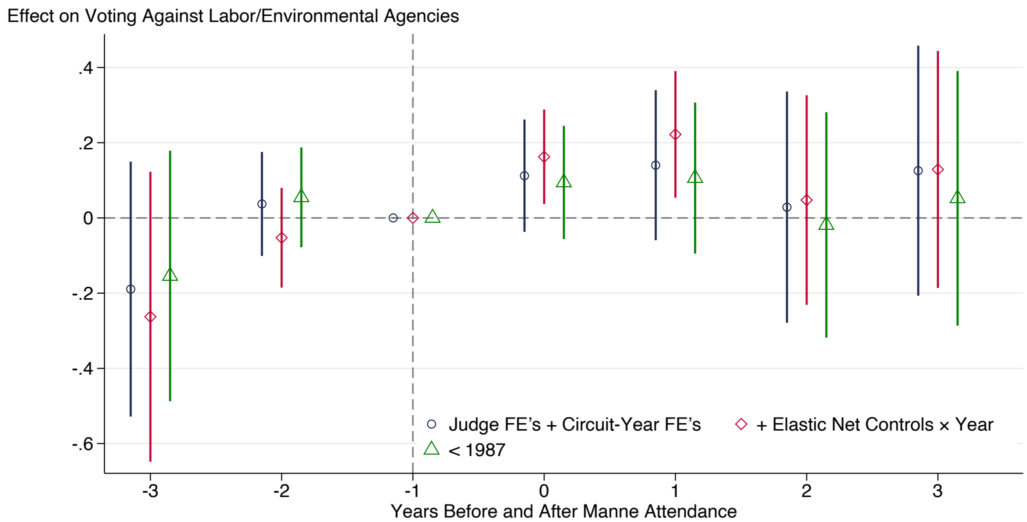
Now we revisit the event study regressions for Labor/EPA. Figure A.22 reports a number of alternative specifications which eliminate any sign of a pre-trend for the Manne effect on regulatory agencies. Panel A shows the event-study effect for labor-EPA cases with judge-specific linear trends. Panel B alternatively includes the average outcome (labor/EPA rulings) for the three years prior to attendance, interacted with year fixed effects. Panel C includes both. Panel D alternatively adds dummies for whether a judge has a labor/EPA case in the years around attendance, interacted with year fixed effects. All of these alternative specifications eliminate the pre-trend observed in Figure IV. Further, we run the test from Rambachan and Roth (2019) in Appendix Figure A.23. The event-study effects are significant under the test, but it requires some assumptions of relatively low non-linearity in the post-period.

Figure A.23: Labor/EPA: Pre-Trend Sensitivity Analysis



Notes. Sensitivity graphs for violation of the parallel trends assumption, applying the method from Rambachan and Roth (2019); see also Ang (2021). Outcome is Voting Against Labor/Environmental Agencies. The axis-crossing value of of $\bar{M}$ indicates that the significant treatment effect of Manne attendance (at 95% confidence) is robust to allowing for a non-linearity in the differential trend in the post-treatment period that is about M times the maximum observed non-linearity in the pre-treatment period.

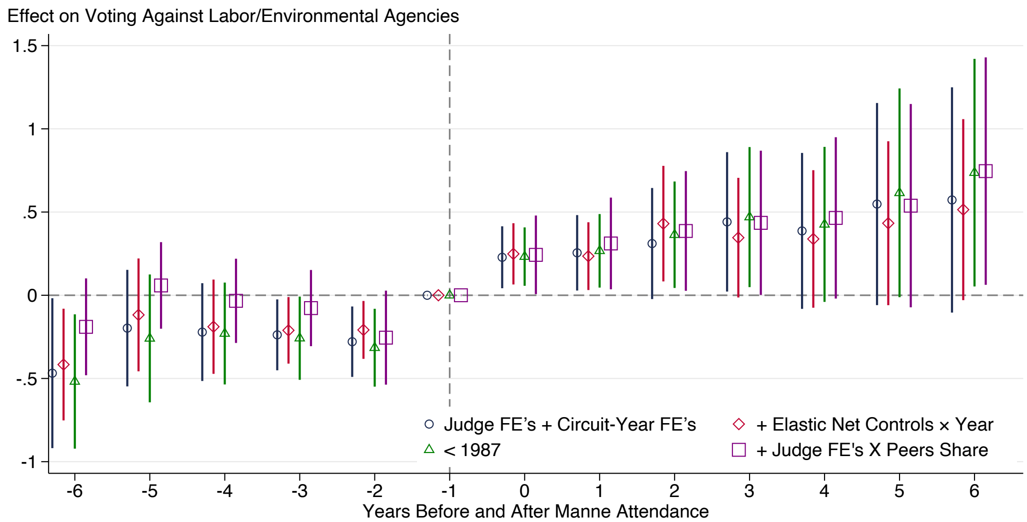
Figure A.24: Labor/EPA: Balanced Panel with Shorter Window



Notes. Event study regressions with balanced panels of judges, for three years of lags and leads, for Labor/EPA Regulatory Vote. For other details see notes in the associated main-text exhibits. The spec with peer share controls is dropped, as the confidence intervals are very large with the reduced sample.

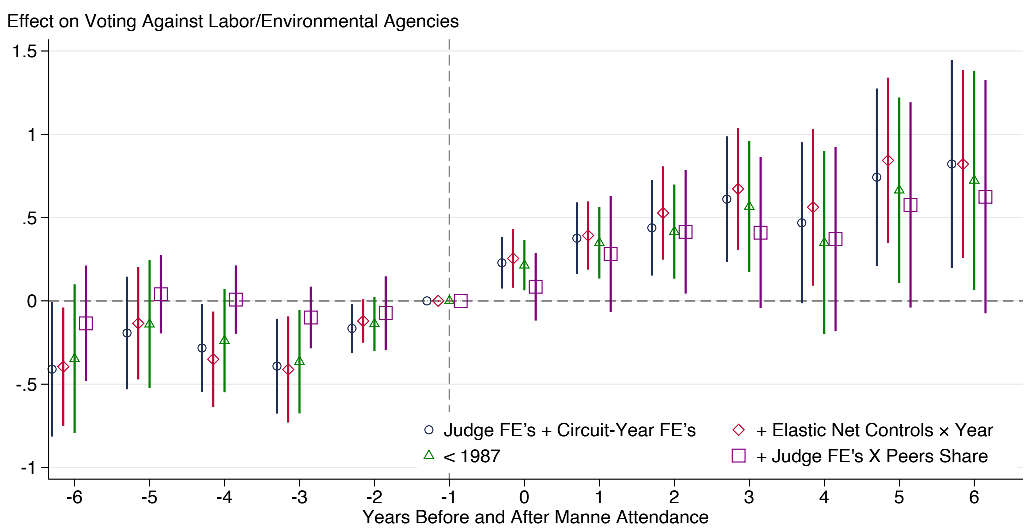
Next, to try to assess whether the event-study results are driven by selective attrition, we produced the event-study results for a balanced sample of judges with a shorter time window (three years before and after). Appendix Figure A.24 shows that the estimates are still positive but noisy.

Figure A.25: Labor/EPA Event Study: Dropping 2nd, 8th, 9th, and D.C. Circuits



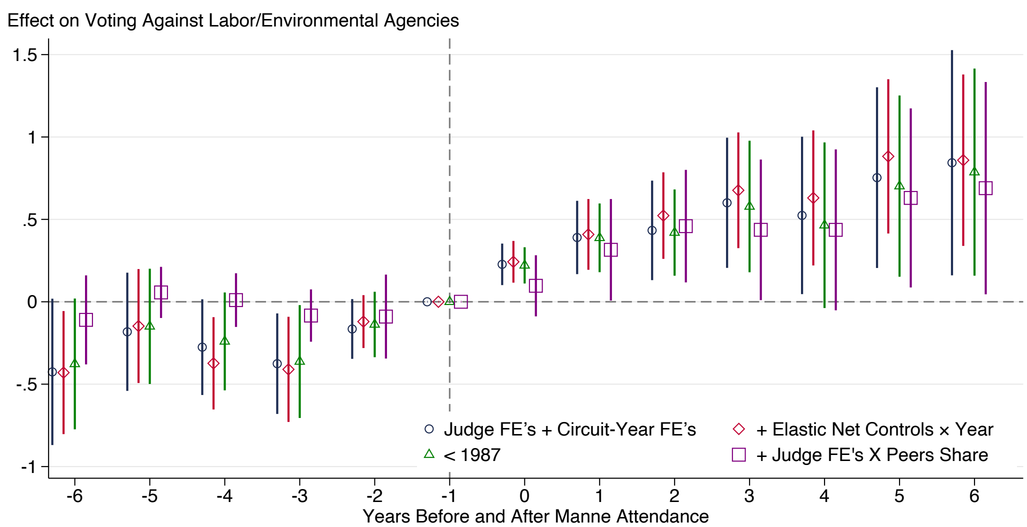
Notes. Main event study results for the circuit courts (from Figure IV) but dropping those circuits for which Levy and Chilton (2015) find nonrandom assignment in their calendar dataset from the years 2008-2013 (2nd, 8th, 9th, and D.C. Circuits). Outcome is Voting against Labor/Environmental Agencies. For other details see notes in the associated main-text exhibits.

Figure A.26: Labor/EPA Event Study, with Legal Topic Fixed Effects



Notes. Main event study results for the circuit courts (from Figure IV) but including fixed effects for 94 detailed legal topics. Outcome is Voting against Labor/Environmental Agencies. For other details see notes in the associated main-text exhibits.

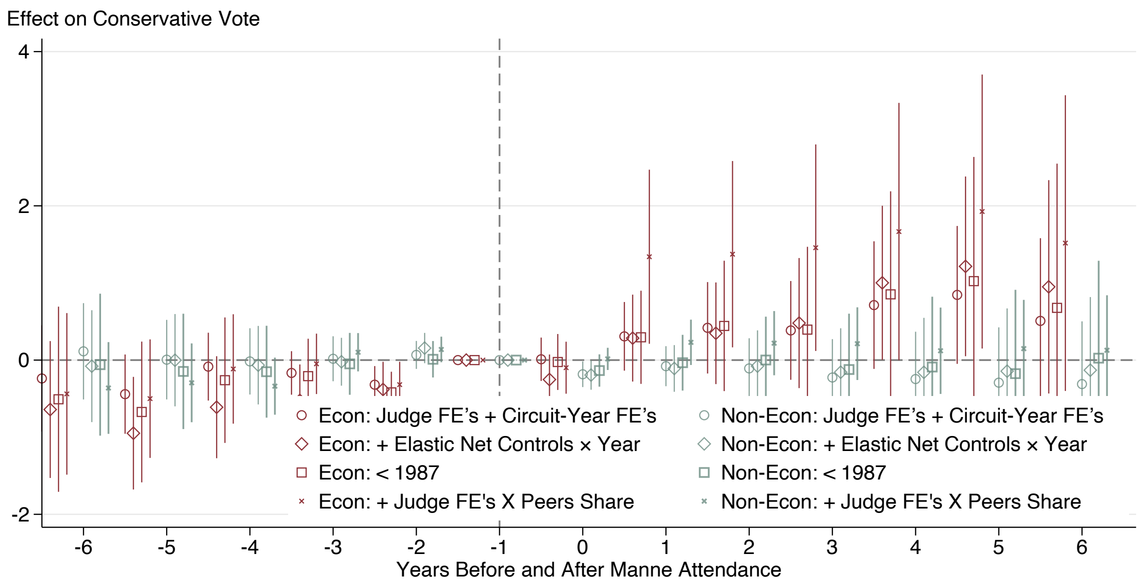
Figure A.27: Labor/EPA Event Study, Two-Way Clustering



Notes. Main event study results for labor/EPA with two-way clustering of standard errors by judge and court-year. For other details see notes in the associated main-text exhibit.

F Additional Results on Conservative Decisions

Figure A.28: Effect of Manne Program on Conservative Voting



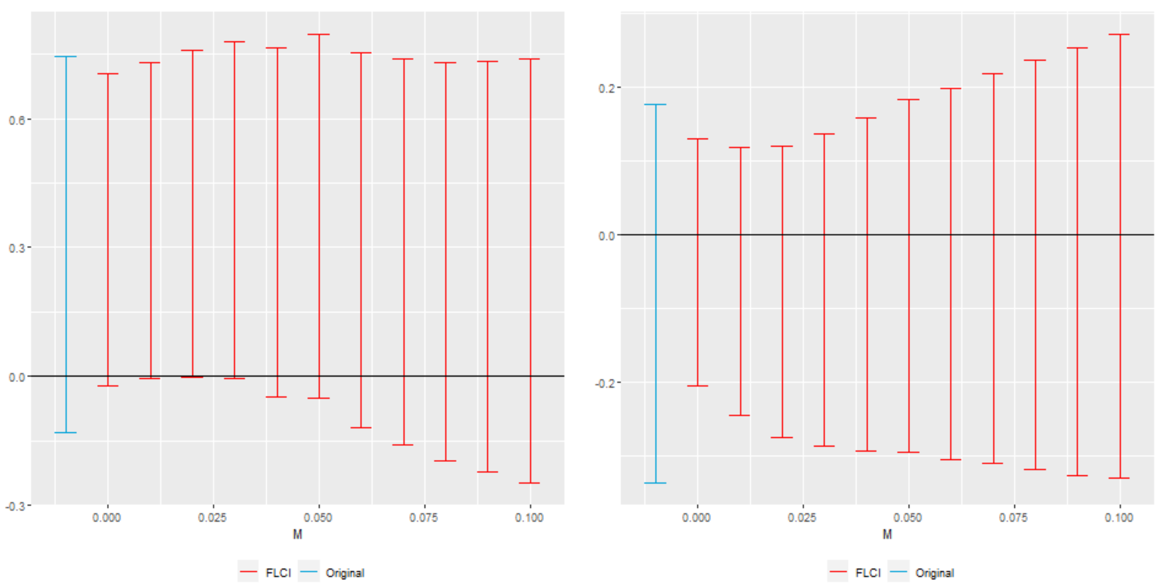
Notes. Event study effect on conservative vote in economics cases (regulation and labor; in red) and non-economics cases (in teal). Baseline specification (left dot in pair) includes judge and circuit-year fixed effects. Second specification (right dot in pair) includes elastic net selected controls interacted with year. Third specification (square) is baseline limited to pre-1987 years. Fourth specification (star) include peer share controls interacted with judge fixed effects. Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals, with standard errors clustered by judge.

The event study results for conservative voting are reported in Figure A.28. While the results are somewhat noisy, there is evidence of an increase in conservative voting for economics cases, but not for non-economics cases. There is a sign of a pre-trend, however. To assess the importance of this pre-trend, we applied the statistical test from Rambachan and Roth (2019). As shown in Figure A.29, we can rule out major non-linear pre-trends for conservative voting in economics cases, but as with the labor/EPA outcome, it depends on the parameter assumptions.

Figure A.29: Conservative Vote: Pre-Trend Sensitivity Analysis

A. Economics Cases

B. Non-Economics Cases



Notes. Sensitivity graphs for violation of the parallel trends assumption, applying the method from Rambachan and Roth (2019); see also Ang (2021). Outcome is Conservative Voting in Economics Cases (Panel A) and Non-Economics Cases (Panel B). The axis-crossing value of of $\bar{M}$ indicates that the significant treatment effect of Manne attendance (at 95% confidence) is robust to allowing for a non-linearity in the differential trend in the post-treatment period that is about M times the maximum observed non-linearity in the pre-treatment period.

Table A.12: Regression Results: Conservative Voting in Economics Cases

			A. Short-Run Effects on Attenders								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post Manne	0.304*	0.18	0.276*	0.25	0.391	1.177**	0.320*	0.225+	0.304*	0.304*	0.304*
	(0.130)	(0.118)	(0.123)	(0.172)	(0.289)	(0.352)	(0.138)	(0.117)	(0.130)	(0.132)	(0.131)
N (Votes)	800	579	800	800	424	800	792	800	800	800	800
			B. Long-Run Effects on Attenders								
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Post Manne	0.051	0.104	0.008	0.081	0.403*	0.002	0.032	0.047	0.05	0.051	0.051
	(0.070)	(0.071)	(0.077)	(0.078)	(0.178)	(0.181)	(0.073)	(0.056)	(0.070)	(0.066)	(0.073)
N (Votes)	1543	759	1543	1543	629	1543	1540	1543	1543	1543	1543
Circuit-Year / Judge FE	X	X	X	X	X	X	X	X	X	X	X
Pre-1987		X									
Party × Year FE			X								
E-net × Year FE				X							
Low Peer Share					X						
Judge FE × Peer Share						X					
Case Topic FE							X				
No Weighting								X			
Winsorized Weights									X		
Robust SE										X	
Two-Way Cluster SE's											X

Notes. Estimated effects of Manne training on conservative voting in economics cases, hand-coded by Songer-Auburn for 5% of cases 1970 to 2002. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means limiting to years 1986 and earlier. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Case Topic FE is fixed effect for case topic. Low Peer Share only includes circuit-years where the share of peer Manne attendees is below median. Judge FE X Peer Share means the share of a judge's peers who have attended, interacted with judge FE. No Weighting means observations are not weighted. Winsorized weights means regression weights are winsorized at 99%. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and circuit-year. Panel A includes the event-study sample. Panel B includes ever-attenders for all years. $+p < .1, *p < 0.05, **p < .01$.

The regression estimates for Equation (1) for conservative voting in economics cases (regulation and labor) and non-economics cases (everything else) are reported in Appendix Tables A.12 and A.13. In economics cases, we see evidence of positive effects (Appendix Table A.12). As before, we report results in the short run (Columns 1-11) and long run (Columns 12-22). In the short run, there are consistently positive estimates that are quite large in magnitude (at least 0.18 on a binary scale). The effect is robustly significant to inclusion of party-year controls (Column 3) or case topic controls (Column 7). The coefficient is stable, but noisier and not significant, with elastic-net-selected controls (Column 4). With judge-specific peer share controls (Column 6), the coefficient is unrealistically large, but this appears to be a multi-collinearity problem due to the small sample and large number of controls in that specification. Also due to the small sample, we cannot get precise estimates if we shrink it further by limiting to the early period (Column 2) or low-peer-share sample (Column 5). Finally, the short-run results are robust to different specification choices for weighting (Columns 8-9) or clustering (Column 10-11).

Table A.13: Regression Results: Conservative Voting in Non-Economics Cases

	A. Short-Run Effects on Attenders										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post Manne	0.059	0.024	-0.027	-0.01	-0.043	0.099	0.072	0.051	0.062	0.059	0.059
	(0.074)	(0.091)	(0.073)	(0.089)	(0.154)	(0.192)	(0.083)	(0.043)	(0.074)	(0.062)	(0.081)
N (Votes)	2401	1527	2401	2401	1311	2401	2384	2401	2401	2401	2401

	B. Long-Run Effects on Attenders										
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Post Manne	0.028	0.056	-0.012	0.007	0.039	-0.043	0.037	-0.024	0.033	0.028	0.028
	(0.048)	(0.090)	(0.046)	(0.054)	(0.100)	(0.095)	(0.049)	(0.032)	(0.047)	(0.043)	(0.055)
N (Votes)	4788	1945	4788	4788	1995	4788	4750	4788	4788	4788	4788
Circuit-Year / Judge FE	X	X	X	X	X	X	X	X	X	X	X
Pre-1987		X									
Party × Year FE			X								
E-net × Year FE				X							
Low Peer Share					X						
Judge FE × Peer Share						X					
Case Topic FE							X				
No Weighting								X			
Winsorized Weights									X		
Robust SE										X	
Two-Way Cluster SE's											X

Notes. Estimated effects of Manne training on conservative voting in non-economics cases, hand-coded by Songer-Auburn for 5% of cases 1970 to 2002. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means limiting to years 1986 and earlier. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Case Topic FE is fixed effect for case topic. Low Peer Share only includes circuit-years where the share of peer Manne attendees is below median. Judge FE X Peer Share means the share of a judge's peers who have attended, interacted with judge FE. No Weighting means observations are not weighted. Winsorized weights means regression weights are winsorized at 99%. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and circuit-year. Panel A includes the event-study sample. Panel B includes ever-attenders for all years. $+p < .1, *p < 0.05, **p < .01$.

The long run estimates, in Columns 12-22, all generate positive coefficients. But they are smaller in magnitude and not statistically significant, except for the low peer share sample which is significant (Column 16). Overall, the effect on economics cases for post-attenders, relative to not-yet attenders, is short run.

Looking to the non-economics cases in Table A.13, we see consistently null results with mixed sign. We are confident in saying there is no effect of the Manne program on ideological voting in non-economics cases in the circuit courts.

G Antitrust Analysis

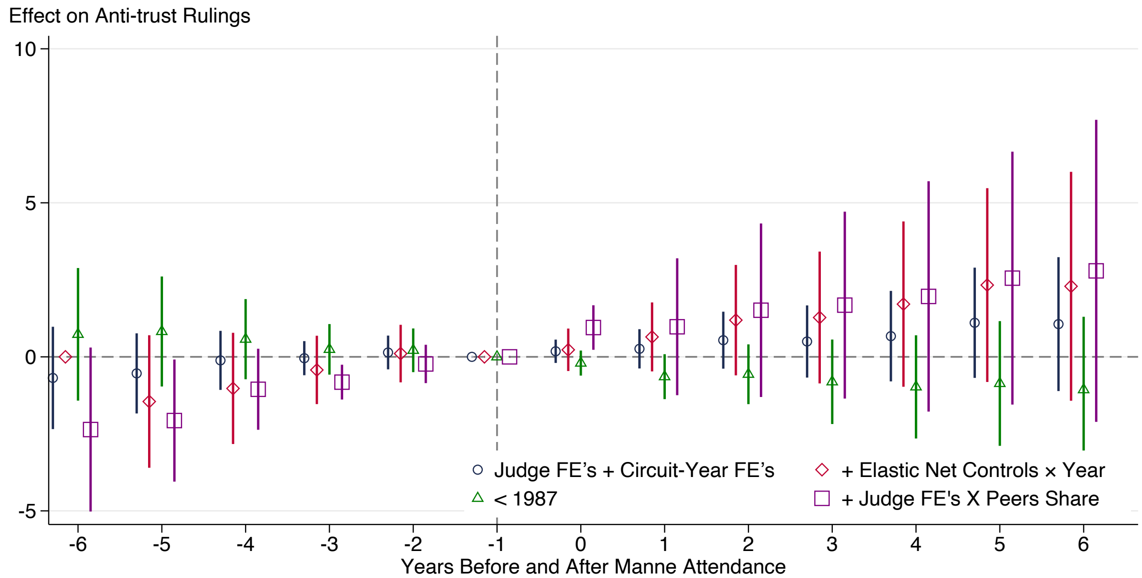
G.1 Data Collection

Antitrust cases were collected and annotated in three ways. We had two sources for previous annotations. First, the Songer-Auburn dataset has a handful of antitrust cases (5% sample) annotated as liberal or conservative, following a rubric similar to ours (we verified this by re-annotating some of these cases). Second, we have another sample of cases matched to information from the Federal Judicial Center's Administrator of Courts dataset. Some of these cases have "Antitrust" labeled as the nature of suit, so a ruling against the plaintiff in these cases indicated a conservative direction.

Third, we used a legal search engine to identify an additional sample of cases, based on the search terms in Baye and Wright (2011). Each case was first analyzed for its antitrust content. To be included in our data set, a decision needed to involve an action or claim by at least one party that asserted a violation of state or federal antitrust law. Some decisions that do not directly address substantive antitrust questions were included if they rule on procedural issues in favor of parties seeking antitrust enforcement or asserting antitrust claims, both because these rulings may be indicative of judges' larger views of antitrust law and because such procedural or arguably procedural questions can bear on parties' ability to assert antitrust claims successfully. Decisions that did not address a party's antitrust claim through either a procedural or substantive ruling, such as cases that merely analogize to antitrust jurisprudence or that otherwise contain relevant search terms but do not impact an antitrust claim, were removed from our set.

Next, we assigned each ruling a number based on whether it offered a party asserting an antitrust claim a favorable decision. If a ruling was favorable to the antitrust-asserting party on any grounds, we assigned that ruling a "1"; if not, it received a "0". Our favorability analysis focused on the margin, looking to the disposition of the case in the appellate court relative to its status after the lower court's ruling. For example, if a private plaintiff asserted an antitrust claim against another market participant and had its suit dismissed in federal district court at the summary judgment stage, an appellate decision reversing dismissal and remanding the case would be assigned a 1 even if the ruling did not address the relevant antitrust issues on their merits. If a government agency won an injunction preventing a merger in lower court—a favorable outcome for the antitrust-asserting party—and had that lower court ruling affirmed on appeal, the

Figure A.30: Effect of Manne Program on Antitrust Decisions



Notes. Event study effects on voting against antitrust claimants, relative to year before attendance at Manne economics training. The baseline specification (blue circles) includes judge and circuit-year fixed effects. Additional specifications add elastic net controls (red diamonds), limit to pre-1987 (green triangles), and add peer share controls by judge (purple squares). Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals, with standard errors clustered by judge.

appellate decision would also receive a 1. Some of the rulings in our set involved a favorable disposition with respect to some claims and an unfavorable disposition with respect to others. As long as a ruling was at least partly favorable for an asserted antitrust claim, we assigned it a 1.

G.2 Results

The event study estimates for antitrust are reported in Appendix Figure A.30. The specifications are otherwise the same as in the main text. As can be seen, the coefficients are sensitive to specification and suggest no effect. The set of regression results are reported in Table A.14. In the short run (Columns 1-11), there are mixed negative and positive coefficients, reflecting the small sample of cases. We get much more stable estimates in the long run (Columns 12-22), and all of the coefficients are positive. The coefficients are statistically significant in a few of the specifications, but mostly noisy and imprecise.

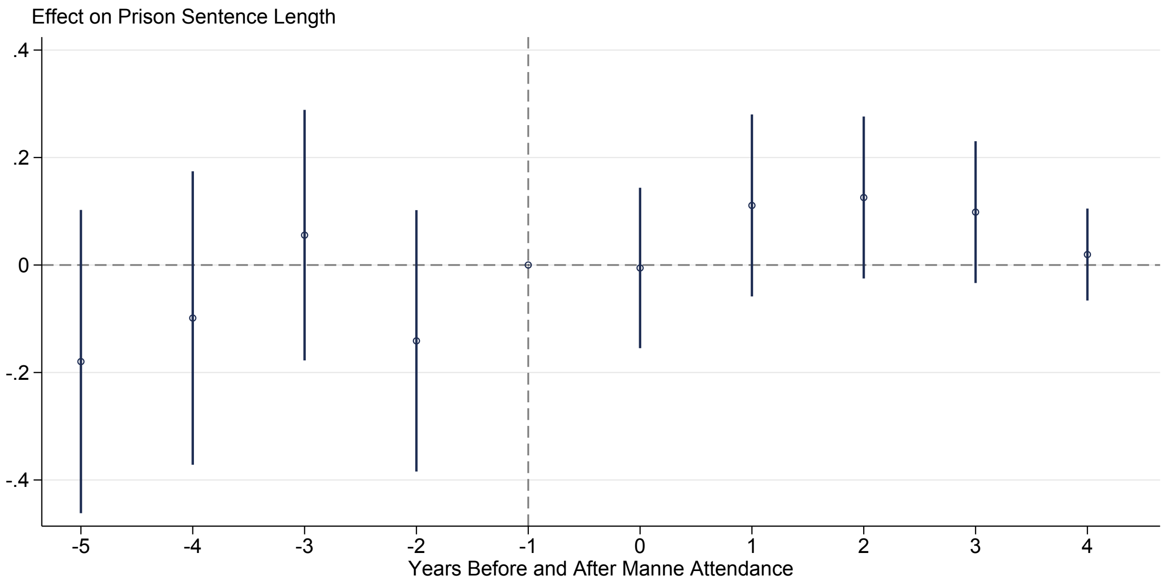
Table A.14: Regression Results: Conservative Voting in Antitrust Cases

				A. Short-Run Effects on Attenders							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post Manne	0.013	-0.195	-0.141	0.079	0.376*	0.318	0.075	0.066	0.013	0.013	0.013
	(0.154)	(0.190)	(0.198)	(0.183)	(0.158)	(0.434)	(0.170)	(0.136)	(0.154)	(0.203)	(0.150)
N (Votes)	313	239	313	313	158	313	295	313	313	313	313
				B. Long-Run Effects on Attenders							
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
Post Manne	0.119	0.043	0.078	0.338**	0.124	0.412+	0.088	0.106	0.119	0.119	0.119
	(0.087)	(0.145)	(0.085)	(0.113)	(0.149)	(0.211)	(0.070)	(0.078)	(0.087)	(0.108)	(0.081)
N (Votes)	623	299	623	623	214	623	614	623	623	623	623
Circuit-Year / Judge FE	X	X	X	X	X	X	X	X	X	X	X
Pre-1987		X									
Party × Year FE			X								
E-net × Year FE				X							
Low Peer Share					X						
Judge FE × Peer Share						X					
Case Topic FE							X				
No Weighting								X			
Winsorized Weights									X		
Robust SE										X	
Two-Way Cluster SE's											X

Notes. Estimated effects of Manne training on conservative voting in antitrust cases, hand-coded by the authors. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means limiting to years 1986 and earlier. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Case Topic FE is fixed effect for case topic. Low Peer Share only includes circuit-years where the share of peer Manne attendees is below median. Judge FE X Peer Share means the share of a judge's peers who have attended, interacted with judge FE. No Weighting means observations are not weighted. Winsorized weights means regression weights are winsorized at 99%. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and circuit-year. Panel A includes the event-study sample. Panel B includes ever-attenders for all years. $+p < .1, *p < 0.05, **p < .01$.

H Additional Results on Criminal Sentencing

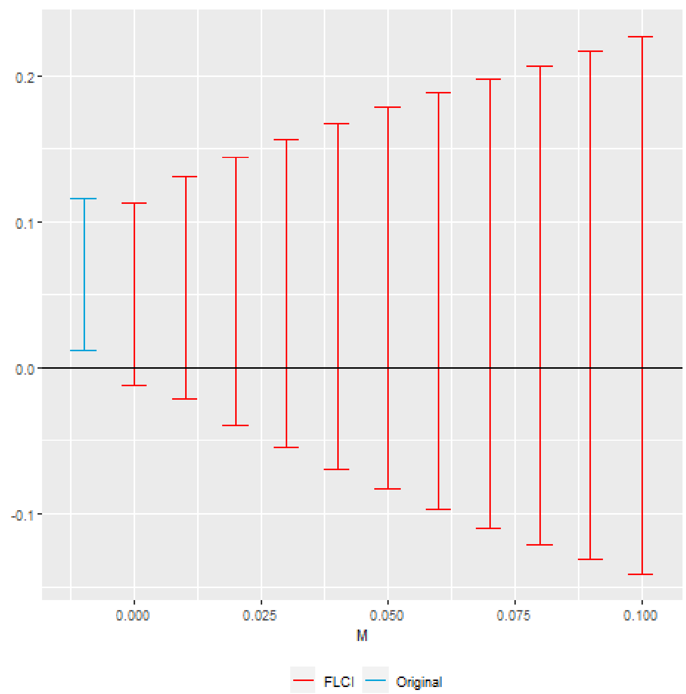
Figure A.31: Effect of Manne Program on Prison Sentence Length



Notes. Event study effect of Manne attendance on criminal sentencing outcomes in district courts, 1992-2003. Coefficients come from a Poisson regression where the outcome is the prison sentence in months. Regression includes judge and district-year fixed effects. Observations are not weighted. Error spikes give 95% confidence intervals.

Figure A.31 shows the event study for sentence length, using a Poisson regression. Given sentencing guidelines, it is not surprising that there is mostly a non-significant effect on sentence length. Appendix Figure A.32 shows the Rambachan and Roth (2019) test for non-linear pre-trends for the any-sentence outcome. The any-prison effect is significant but not robust to the non-linear trends test.

Figure A.32: Criminal Sentencing: Pre-Trend Sensitivity Analysis



Notes. Sensitivity graphs for violation of the parallel trends assumption, applying the method from Rambachan and Roth (2019); see also Ang (2021). Outcome is Any Prison. The axis-crossing value of of $\bar{M}$ indicates that the significant treatment effect of Manne attendance (at 95% confidence) is robust to allowing for a non-linearity in the differential trend in the post-treatment period that is about M times the maximum observed non-linearity in the pre-treatment period.

Table A.15: Regression Results: Effect of Manne Program on Any Prison Given

	A. Short-Run Effects on Attenders							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Manne	0.061*	0.057+	0.088**	0.058*	0.066**	0.061*	0.061+	0.061*
	(0.028)	(0.030)	(0.030)	(0.028)	(0.018)	(0.028)	(0.032)	(0.030)
N (Sentences)	70784	70784	70784	70624	70784	70784	70784	70784
	B. Long-Run Effects on Attenders							
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Post Manne	0.049*	0.050*	0.040*	0.041*	0.032**	0.049*	0.049**	0.049*
	(0.020)	(0.020)	(0.020)	(0.019)	(0.011)	(0.020)	(0.016)	(0.021)
N (Sentences)	260516	260516	260516	260250	260516	260516	260516	260516
	C. Long-Run Effects, Including Never-Attenders							
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Post Manne	0.044*	0.039*	0.040*	0.041*	0.019*	0.044*	0.044**	0.044*
	(0.019)	(0.019)	(0.019)	(0.018)	(0.009)	(0.019)	(0.014)	(0.019)
N (Sentences)	1006820	1006820	1006820	1006256	1006820	1006820	1006820	1006820
Court-Year / Judge FE	X	X	X	X	X	X	X	X
Party × Year FE		X						
E-net × Year FE			X					
Charge FE				X				
No Weighting					X			
Winsorized Weights						X		
Robust SE							X	
Two-Way Cluster SE's								X

Notes. Estimated effects of Manne training on criminal sentencing – any prison given. Includes years 1992 through 2003. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Charge FE is fixed effect for main criminal charge. No Weighting means observations are not weighted. Winsorized weights means regression weights are winsorized at 99%. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and court-year. Panel A includes the event-study sample. Panel B includes ever-attenders for all years. Panel C includes all judges. $+p < .1, *p < 0.05, **p < .01$.

Regression results for giving a prison sentence are reported in Appendix Table A.15. First, we report results for short-run effects on attenders, five years before/after attending (Panel A, Columns 1-8). Second, we report long-run effects on attenders including all years (Panel B, Columns 9-16). Given the shorter period of the District Court dataset (1991-2003), however, we don't expect these samples to diverge as much as in the Circuit Court dataset (1970-2005). Third, we include the same specifications with all judges, including never-attenders (Panel C, 17-24). Besides the baseline with judge fixed effects and district-year fixed effects (Columns 1, 9, 17), we include judge party affiliation (from appointing president) interacted with year (Columns 2, 10, 18), elastic-net-selected controls (predictive of attendance timing) interacted with year (Columns 3, 11, 19), and criminal charge fixed effects (Columns 4, 12, 20). We report unweighted regressions (Columns 5, 13, 21) and those with winsorized weights (Columns 6, 14, 22).

Finally, we test robustness with robust standard errors (no clustering, Columns 7, 15, 23) and two-way clustering by judge and court-year (Columns 8, 16, 24). Due to data availability, we do not report the specification limiting to the early period (as done with the circuits). We also exclude the spec that controls for peer share, since in the district courts judges work independently, rather than in panels like the circuits.

As seen in Appendix Table A.15, all 24 estimated effects are positive and statistically significant. In the short run, prison is given 6 percentage points more of the time after Manne attendance compared to later-attenders; in the long run, 5 percentage points. When never-attenders are included in the control group, the estimate is very similar at 4 percentage points. So the effect of the Manne program on the incarceration rate of defendants, conditional on conviction, is quite robust.

Appendix Table A.16 reports results from Poisson regressions for the sentence length outcome in months, including zeroes. Here, we might not expect much of an effect given mandatory sentencing guidelines that constrain judge discretion. The specifications are the same as that with the any-prison outcome, except that the specifications with alternative weighting are dropped. With the poisson regression estimator we use, weighted regressions are not possible. So all regressions are not weighted.

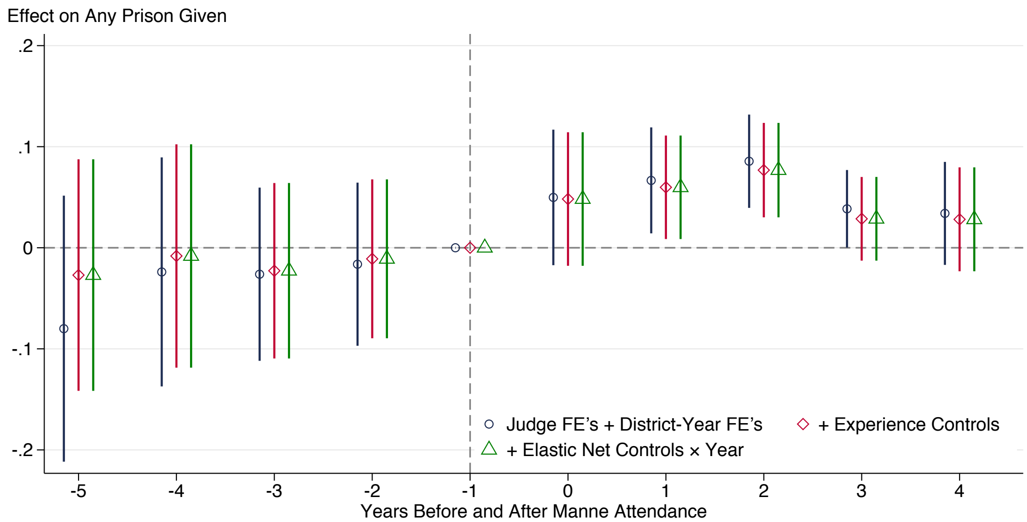
With sentencing, we see evidence of a positive effect. In the short run (Columns 1-6), the coefficient is positive, stable, and significant. In the long run (Columns 7-12) and in the all-judges sample (Columns 13-18), estimates are all positive, yet smaller in magnitude, somewhat sensitive to specification, and not always statistically significant. Still, overall, these results support the view that Manne attendance increased severity of criminal sentencing.

Table A.16: Regression Results: Effect of Manne Program on Sentence Length

	A. Short-Run Effects on Attenders					
	(1)	(2)	(3)	(4)	(5)	(6)
Post Manne	0.179+	0.171+	0.238**	0.168*	0.179**	0.179+
	(0.092)	(0.098)	(0.090)	(0.086)	(0.066)	(0.104)
N (Sentences)	70448	70448	70448	69691	70448	70448
	B. Long-Run Effects on Attenders					
	(7)	(8)	(9)	(10)	(11)	(12)
Post Manne	0.073	0.071	0.114*	0.088*	0.073*	0.073
	(0.047)	(0.047)	(0.049)	(0.039)	(0.030)	(0.055)
N (Sentences)	258970	258970	258970	257251	258970	258970
	C. Long-Run Effects, Including Never-Attenders					
	(13)	(14)	(15)	(16)	(17)	(18)
Post Manne	0.082+	0.081*	0.044	0.089*	0.082***	0.082+
	(0.042)	(0.039)	(0.048)	(0.035)	(0.025)	(0.046)
N (Sentences)	1002510	1002510	1002510	1000113	1002510	1002510
Court-Year / Judge FE	X	X	X	X	X	X
Party × Year FE		X				
E-net × Year FE			X			
Charge FE				X		
Robust SE					X	
Two-Way Cluster SE's						X

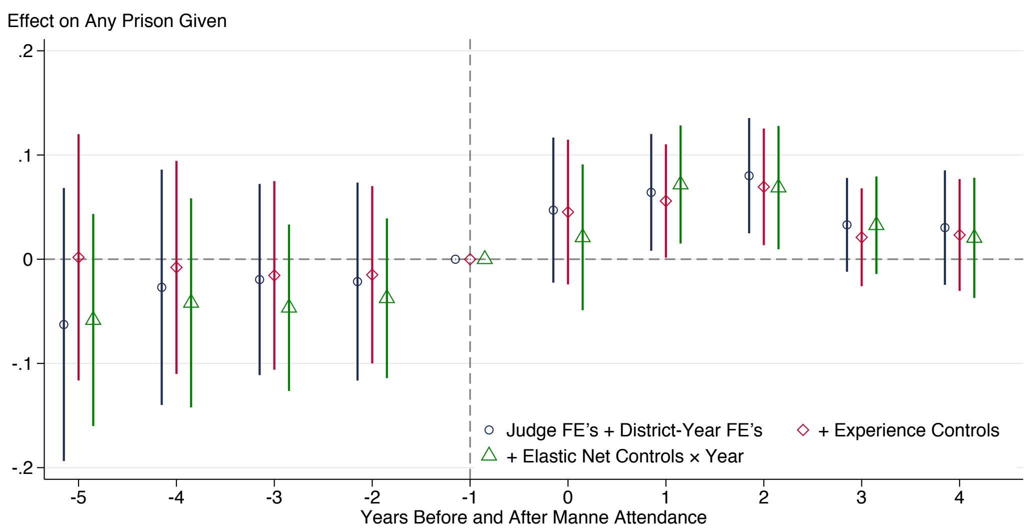
Notes. Estimated effects of Manne training on criminal sentencing – sentence length in months – using poisson regressions. Includes years 1992 through 2003. Except where indicated, standard errors (in parentheses) clustered at judge level. Observations are not weighted. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. Charge FE is fixed effect for main criminal charge. Robust SE means no clustering, and Two-Way Clustering means clustering by both judge and court-year. Panel A includes the event-study sample. Panel B includes ever-attenders for all years. Panel C includes all judges. $+p < .1, *p < 0.05, **p < .01$.

Figure A.33: Event Study Effect on Any Prison, Crime Charge Fixed Effects



Notes. Main event study results for the district courts (from Figure V) but including fixed effects for crime type (345 categories). Outcome is Any Prison Given. Baseline includes judge FE's and court-year FEs (blue circles). Second spec (red diamonds) includes a polynomial in judge experience. Third spec (green triangles) includes elastic-net-selected controls, interacted with year.

Figure A.34: Event Study Effect on Any Prison, Two-Way Clustering



Notes. Main event study results for the district courts (from Figure V) with two-way clustering of standard errors by judge and court-year. Outcome is Any Prison Given. Baseline includes judge FE's and court-year FEs (blue circles). Second spec (red diamonds) includes a polynomial in judge experience. Third spec (green triangles) includes elastic-net-selected controls, interacted with year.

Next we look for heterogeneity in the Manne effect on sentencing severity by the

Table A.17: Effect of Manne on Prison Given, by Drug and Non-Drug

	Effect on Any Prison Given							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Manne	0.080*	0.091**	0.104**	0.057*	0.047	0.013	0.054	0.051+
	(0.033)	(0.032)	(0.037)	(0.025)	(0.052)	(0.054)	(0.049)	(0.026)
N (Sentences)	41038	41038	41038	156338	29737	29737	29737	104152
Crime Type	Non-Drug				Drug			
Court-Year FE	X	X	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X	X	X
Party × Year FE		X				X		
E-net × Year FE			X				X	
Short Run DD	X	X	X		X	X	X	
Long Run DD				X				X

Notes. Estimated effects of Manne training on criminal sentencing – any prison given – separately for Drug Crimes and Non-Drug Crimes. "Drug" indicates the sample is limited to crimes from USC Title 21; "Non-Drug" indicates the sample is all other crimes. Regressions include court-year and judge, fixed effects. Standard errors (in parentheses) clustered at judge level. Observations weighted to treat judge-years equally. Party X Year FE means appointing party of judge, interacted with year FE. E-net X Year FE refers to elastic-net selected controls for predicting timing of Manne attendance, interacted with year FE. "Short Run DD" includes the event-study sample. "Long Run DD" includes ever-attenders for all years. $+p < .1, *p < 0.05, **p < .01$.

type of crime, focusing on drugs versus non-drugs. Some of the Manne instructors, including most notably Milton Friedman, were known for advocating the legalization of drug use as it is a victimless crime. According to Butler (1999), "Friedman always started [his Manne lectures] on legalization of recreational drugs."

For this analysis, "Drug Crime" means that the lead charge is from USC Title 21, which regulates food and drugs. Table A.17 reports the diff-in-diff results separately for non-drug crimes (Columns 1-4) and drug crimes (Columns 5-8). First, in Columns 1-4 we see large, positive, and statistically significant effects on sentencing for non-drug crimes. Comparing to Table A.15, the coefficients are larger in magnitude and more robustly significant across specifications, including the addition of party (Column 2) or elastic net controls (Column 3) interacted with year, or including the long-run DD sample of ever-attenders across all years (Column 4).

In Columns 5-8, in contrast, we see a very different result for drug crimes. In the short run (Columns 5-7), the estimates are much smaller, close to zero, and not statistically significant. In the long run, the estimate is marginally significant but still smaller in magnitude.

Overall, this evidence shows that the Manne effect on sentencing is weaker for drug crimes. That is consistent with the content of the Manne program, for example Milton Friedman's view in favor of legalizing drugs. Because it is a victimless crime, on this view, it is not worth deterring with harsher punishment.

I Additional Supporting Results

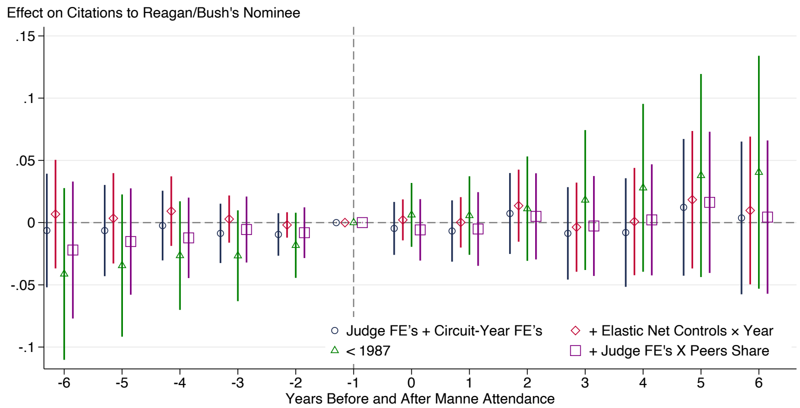
We produce some additional supporting results from the citation network. In Appendix Figure A.35 Panel A, we look at the citations choices of judges. In particular, we ask whether after Manne attendance judges tend to cite opinions written by circuit court judges nominated by Ronald Reagan or George H.W. Bush. There is no effect on this measure. Next, Panels B and C shows the effect of economics training on how often a judge is cited by future circuit cases, where we separate out the effects by economics and non-economics cases. The effects are noisy, suggesting a null result, or perhaps a decrease in citations.

Table A.18 Columns 1 through 5 show the effect of Manne training on being elevated from a district judgeship to a circuit judgeship. District judges who attended Manne are more likely than their court colleagues to be promoted. The effect is robust to starting-year fixed effects and judge biographical controls. Interestingly, we can see that the effect is concentrated totally among Republican presidents (Column 4). Democratic presidents do not selectively promote Manne judges.

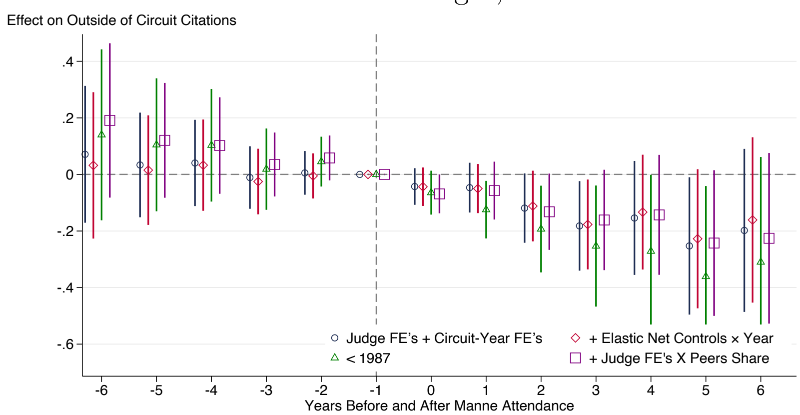
Next, we followed the approach in Scherer and Miller (2009) and built a dataset on federal judge membership in the Federalist Society (Fed Soc), a legal organization associated with conservative causes and Originalist jurisprudence. We found evidence of 170 judges being Fed Soc members. Table A.18 Columns 6 and 7 show the OLS effect of Manne training on joining Fed Soc. Manne judges are more likely to join Fed Soc (Column 6). This is concentrated among Republican judges (Column 7), as few Democratic-appointed judges joined Fed Soc.

Figure A.35: Effect of Manne Program on Citation Measures

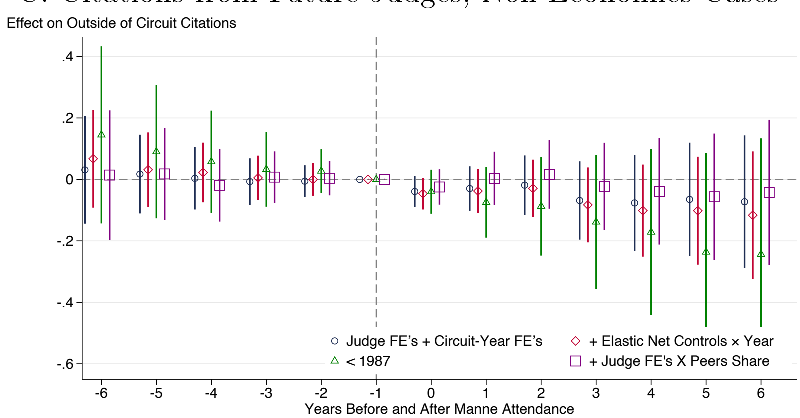
A. Citations to Reagan/Bush Appointees



B. Citations from Future Judges, Economics Cases



C. Citations from Future Judges, Non-Economics Cases



Notes. Main event study results for the circuit courts with outcomes measured from citations data. Panel A: Citations to Reagan/Bush Appointees; Panel B: Citations from Future Judges in Economics Cases; Panel C: Citations from Future Judges in Non-Economics Cases. Citation counts are from judges in other circuits (persuasive precedent). For other details see notes in the associated main-text exhibits.

Table A.18: Effect of Manne Program on Promotion and Fed Soc Membership

	Promoted to Circuit					*Joined Fed Soc*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Manne Judge	0.0838**	0.0588*	0.0482+	0.0901*	0.0272	0.0347+	0.0874**	-0.0356+
	(0.0262)	(0.0284)	(0.0278)	(0.0408)	(0.0411)	(0.0188)	(0.0294)	(0.0201)
N (Judges)	1426	1419	1419	774	637	999	542	435
Sample	All	All	All	Republican	Democrat	All	Republican	Democrat
Court FE	X	X	X	X	X	X	X	X
Start-Year FE		X	X	X	X	X	X	X
Bio Covariates			X			X	X	X

Notes. Estimated effects of Manne training on probability to be promoted to the circuit court from a district judgeship (Columns 1-5) or joining the Federalist Society (Columns 6 and 7). Bio covariates include party and birth decade. "Republican" and "Democrat" indicate party of promoting president. Standard errors clustered at the judge level in parentheses. $+p < .1, *p < 0.05, **p < .01$.

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