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Ideas Have Consequences

The Impact of Law and Economics on American Justice

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Abstract

This paper provides a quantitative analysis of 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 – the program was popular across judges from all backgrounds, was regularly oversubscribed, and admitted judges on a first-come first-served basis – and results are robust to a variety of automatically selected covariates predicting the timing of attendance. 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/longer criminal sentences. The Manne program played a role in reinforcing the policy consequences of the law-and-economics movement via its influence on U.S. federal judges.

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

JEL codes: D7, K0, Z1

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1 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

[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.

we cannot rule out selection of judges into the timing of attendance, we take care to check for pre-trends in the outcome variable, and our results hold even when controlling for the small set of judicial characteristics, interacted with treatment and time, that predict the timing of attendance.

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 the use of economics language, not just among the attendees but also among peer circuit judges who did not attend but interacted with Manne-trained judges on case panels. 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. In the district courts, we find that Manne attendance is associated with harsher criminal penalties – that is, a higher likelihood of a prison sentence. Finally, we show that the difference in sentencing harshness between Manne and non-Manne judges is highest after the 2005 *Booker* decision, which gave more discretion to judges in sentencing.[2]

These results are important for the literature on judicial behavior, in particular on the old question of whether judges are legal formalists or political operators (Stephenson, 2009; Posner, 2008). 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

[2]With many instructors like Milton Friedman advocating against the drug war, it is notable that we find no increase in sentencing harshness for drug crimes.

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).

Beyond judicial behavior, the 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 closely 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. Like with finance training, economics ideas have an important scientific as well as normative component.[3] 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 discussed magnitudes and mechanisms. Section 7 concludes.

[3] 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). Fisman, Kariv, and Markovits (2009) find that law students exposed to an economics-trained professor behaved less pro-socially in lab experiments. Paredes, Paserman, and Pino (2020) find using Chilean data that majoring in economics is correlated with sexism expressed in survey measures. See also Ifcher and Zarghamee (2018).

2 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.

2.1 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).[4] 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).[5] On the other hand, many economists associ-

[4]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).

[5]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,

ated with the Chicago School also advocated for legalizing victimless crimes, such as recreational drug use and prostitution (e.g. Thornton, 2016).

2.2 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 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 who had previously run a similar course for law professors.[6] The institute was the the flagship program of the LEC. Substantial funding came from donations by pro-business foundations and corporations.[7]

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.[8] 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

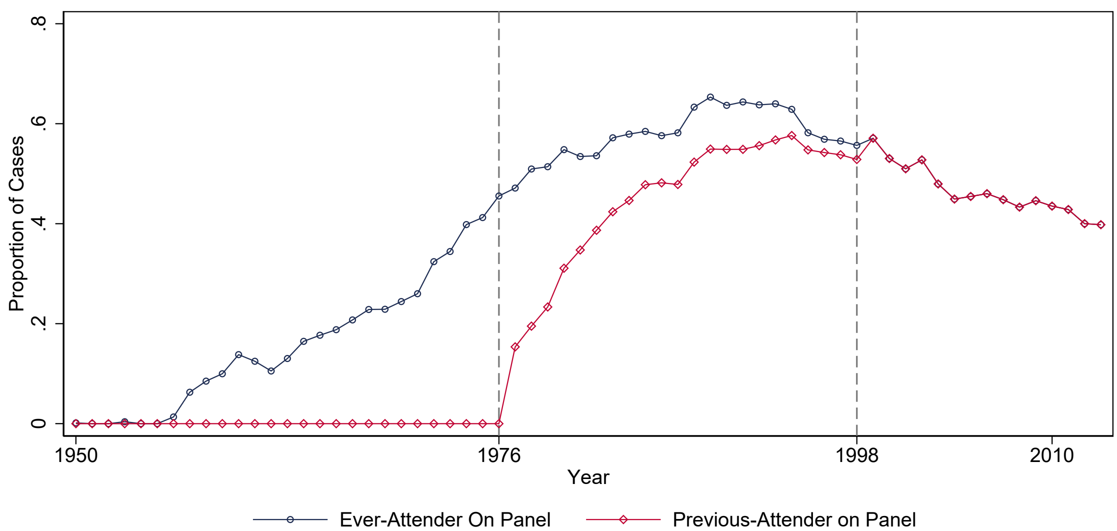
the insights from behavioral economics have led to a more nuanced view of how deterrence operates: e.g., swiftness, certainty, and fairness might deter crime more than the severity of punishment (Nagin 1998; Kleiman 2009; van Winden and Ash 2012).

[6]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).

[7]"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.

[8]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).

Figure 1: 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.

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.[9] Figure 1 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 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 Democrat appointees) who enthused about the program. The quotes testify to how much the judges appreciated the program, how

[9] 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.

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.[10] 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.[11]

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 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*."

[10] 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."

[11] 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).

2.3 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).[12] Moreover, judges are not just politicians (Choi, Gulati, and Posner, 2010; Ash and MacLeod, 2015). Judges from the same political party often dissent against each other, for example, showing the limits of the attitudinal model. Judicial independence arises because judges are highly skilled and highly 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 empirical question for us is whether economics ideas are persuasive for judges, and if so how.

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 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).[13]

[12] 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."

[13] Richard Epstein, a leading intellectual in early law and economics, has written: "Words are like

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.[14]

Finally, although criminal law was a central focus of law-and-economics scholarship and by Henry Manne himself (Gindis and Medema, 2022), it was not emphasized in the Manne curriculum. Hence, 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 harshness 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, we will look at measures of decision quality, such as citations and the probability of promotion to higher courts.

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the critical fortifications on a battlefield. You have to take them in order to win" (Epstein, 1995).

[14]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).

3 Data

This section describes our data sources and judicial outcome measures. Some additional information and summary statistics are reported in Appendix B.

3.1 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.[15]

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 Lexis Nexis.

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 cases as those involving labor or regulation.[16] Economics-related

[15]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.

[16]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.

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 2011 in two overlapping samples.[17] For the years 1992 through 2003 (used for the within-judge event study), there are approximately 1.03 million cases. For the years 1999 through 2011 (used for analyzing the effect of discretion provided in *Booker*), there are approximately 856,000 cases.

Federal Judge Biographies. We have biographical information on 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.[18]

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 exact years of attendance from annual reports obtained by FOIA requests and through correspondence with the Law and Economics Center at George Mason University.

3.2 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 phrases. 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 phrases used by Ellickson (2000) for the purposes of identifying law-and-economics articles in a law

[17] There are duplicates, so we present the analyses separately.
[18] See Appendix B for the enumerated list.

journal corpus. This index includes eleven words and phrases that are characteristic of the use of economic analysis in legal contexts.[19] One approach to measuring economics style would be to simply count these phrases in judicial opinions. However, these phrases 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 which 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

[19] 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.

of the embedding-based measure 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.3 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.

3.3 Judicial Decision Outcomes

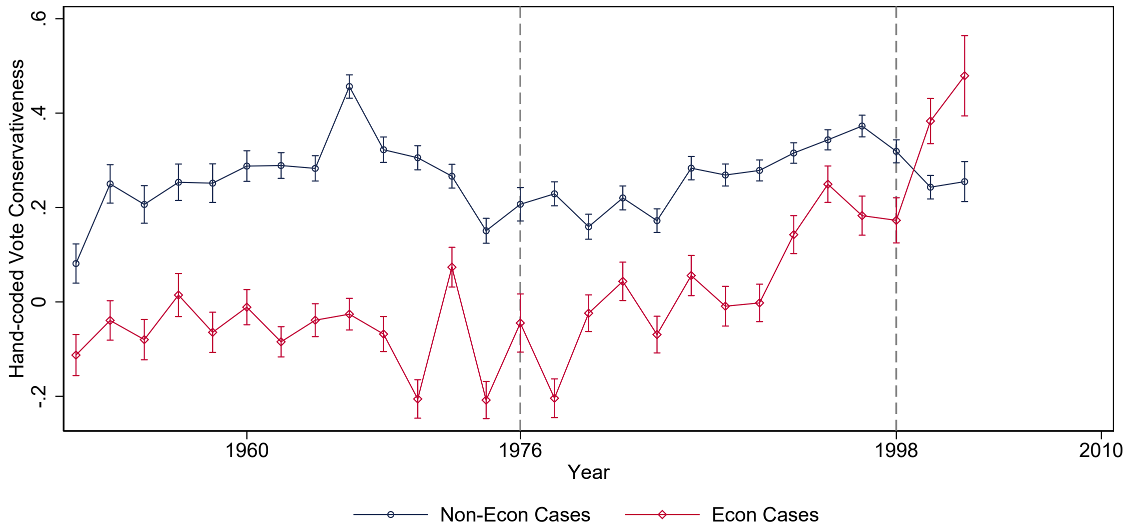
Labor and Environment Regulation. Our main outcome for circuit-court decisions is a machine-coded measure for voting against regulatory agencies. We look at regulatory cases where the government is a party to the case. In particular, we identify labor agencies as including the National Labor Relations 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). This is a 5% random sample of Circuit cases, available until the year 2002. 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. A downside of hand-coding is 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 the sampling was not done uniformly across courts and over time.

Figure 2 shows the trend in conservatism over time. It has increased since the

Figure 2: Increasingly Conservative Rulings in U.S. Federal Courts



Notes. Average conservative vote rate 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.

late 1970s, especially in economics cases (those on labor and regulation).

Antitrust. Next, we construct a new dataset of cases on antitrust. We start off with text-based searches to find a set of potential cases. We then have law students read the cases and see if a decision is made on a substantive antitrust issue. If so, we code it as in favor of stronger or weaker antitrust enforcement (generally, whether it is in favor of the regulatory agency or the claimant seeking relief). Our outcome measure is the rate at which these antitrust cases are decided against the claimant. More detail on this process is included in Appendix G.

Criminal Sentencing Decisions. We produce measures of sentencing harshness 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.[20] We

[20]The data contain information on prison sentences, probation sentences, fines, and the death penalty. We do not consider the death penalty, as it is rare in federal courts (just 71 cases). Probation sentences and monetary fines are much more frequent but still apply in only about 10% of the cases each. Monetary fines are mostly very small relative to prison sentences. The median non-zero monetary fine is $2,000, and the 90th percentile is $15,000. We thus ignore them as well,

drop life sentences and fines (relatively infrequent outcomes) and focus on prison sentence outcomes. We look at whether any prison was imposed, and the inverse hyperbolic sine of the imposed sentence in months. Results are qualitatively the same with log of sentence length (plus one).

4 Econometrics

We use a differences-in-differences design to estimate the effect of Manne attendance relative to colleague judges who have not yet attended the Manne program. This section provides information on the internal validity of the research design. Additional information is reported in Appendix C.

4.1 Identification

A major concern in an empirical analysis of the Manne program is endogenous selection into the program, both in terms of the type of judge and, within-judge, the timing of attendance, so that counterfactual outcomes are correlated with the timing of attendance. Put differently, we require parallel trends in our outcome measures. In Appendix C.3, 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 bumps in these outcomes after attendance in the attending cohorts, it is also clear that the groups are not on parallel trends in the full sample without controls. We adjust for these potential selection biases by controlling for fixed effects for court-year (taking out time variation and caseload variation) and judge (who enter and leave the sample over time).

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. To address this issue, our identification strategy leverages exogenous variation in short-run timing due to the first-come-first-serve rule. Up until the late 1980s, classes were oversubscribed and the judges applying later were bumped to subsequent sessions. Hence, opportunities were reduced for selection of specific types of judges to specific episodes of the course.

and focus exclusively on prison sentences.

That is, conditional on applying, the year of attendance is exogenous. Hence, other ever-attending judges who have not yet attended provide a good counterfactual for short-run effects of Manne attendance. Fortunately, most circuit judges (as opposed to district judges) in our sample attended during this early heyday period.

To evaluate the parallel trends assumption, Appendix Tables A.4 and A.5 assess differences across judges on observables, using all control variables as well as 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 2.2 above), many Democrats also attended and endorsed the program. Judges born in the 1910s are less likely to attend, as they are older, as are the ones born in the 1950s, who mostly joined the court after the Manne program's heyday.

In our dynamic panel design, selection concerns arise not from differences between attenders and never-attenders, but rather due to differences in timing of attendance. In Appendix Tables A.4 and A.5, Columns 3 and 4, we again see some differences in the Manne judges that attended earlier rather than later. Importantly, Republican affiliation (from nominating president) is not a statistically significant predictor for timing (and even dropped by elastic net in the circuit courts). Instead, the important predictors are mostly indicators for judge birth cohort, which is mechanically related to attendance timing due to the differences in when the judges were appointed.[21] These covariates are collinear with judge fixed effects, so they cannot be included directly in our regressions as controls using post double selection (Belloni, Chernozhukov, and Hansen, 2014). Instead, we will adjust for the elastic-net-selected characteristics that predict the timing of attendance, fully interacted with year fixed effects. For example, we allow judges born in the 1940s to have a different intercept in each year.[22] 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.

Besides endogenous timing of attendance, we are also concerned about endogenous selection of judges to cases. Fortunately, in our setting there is quasi-random assign-

[21] In addition, Appendix Table A.6 shows that the pre-1976 outcome means by judge (economics language, voting against regulatory agencies, or conservative economics vote) are not predictive of attendance or the timing of attendance.

[22] This approach is related to controlling for a generalized propensity score (e.g. Kluve et al., 2012).

ment of cases (see Appendix C.1). 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.[23] 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.13, A.24, and A.30). Further, our results are robust to controlling for case topics or charge fixed effects.

More specific to our setting, we would like to check whether Manne training has an effect on the types of cases that judges sit on or author. For the Circuits, 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. For the Districts, Appendix Table A.3 shows that Manne judges are not assigned to different types of criminal charges.

4.2 Choice of Control Group

In our preferred specification (see Section 4.3 below), we use two-way fixed-effects with only ever-attenders included in the control group. We condition on "ever attending" and use the variation in timing of attendance within that sample. Given the recent literature on difference-in-differences, however (e.g. Goodman-Bacon, 2018), this choice requires some additional justification. To summarize briefly, heterogene-

[23]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.

ity in treatment effects over time 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).

The standard approach to dealing with this problem is a stacked differences-in-differences design using never-treated units as a control group for each stacked treatment cohort (e.g. Callaway and Santanna, 2020; Ang, 2021). The problem in our case is that the never-attenders do not provide a good counterfactual; our identification assumption comes from exogenous timing conditional on attendance, a consequence of the first-come, first-served rule and oversubscription to the program. Besides being different on a number of observables, including political party (Appendix Table A.4), never-attenders are on a positively selected trend in the use of economics language in their opinions (see Appendix Figure A.10). These statistics reflect that law and economics was not only transmitted to judges by the Manne program; it was promoted in the legal academy through teaching and scholarship,[24] 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 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).

In our sample, never-attenders tend to come from younger cohorts, which are more likely to learn law-and-economics from law school and from other sources such as the Federalist Society. On top of that, there could be selective promotion of lower-court judges who were more economics-oriented (see Appendix Table A.23). Beyond judges, the exposure of law clerks to economics in their law school classes could have pushed economics language into the opinions of never-attenders.

Most importantly, our data show that there are significant spillovers in economics exposure to never-attenders (see Appendix C.4). In the sample of judges who have never attended, we regress our measure of economics language on the share of peer judges on the same court who have attended Manne. Appendix Table A.7 shows a large and significant positive spillover effect on economics language among never-

[24]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."

attenders, meaning they are exposed in part to treatment.

We therefore have only two comparisons being made in our differences-in-differences framework. Firstly, between early attenders and not-yet attenders; secondly, between late attenders and those that already attended. If the treatment effect is different (e.g. larger) for the early attenders than the late attenders, then this can induce negative weighting, depending on the size and share treated of each group. We confirm below that the later attenders do not seem to exhibit spillovers from early attenders in language, unlike the never-attenders. This could be due to differences in these judges' characteristics, in particular that the never-attenders are younger on average and therefore potentially more open to influence by peer judges, for example because they are less independent (e.g. Campbell and Wilcox, 2020).

Appendix C.5 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.8). 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. For completeness, regressions including never-attenders in the sample are described and reported in Appendix C.6.

4.3 Specification

Our outcome Y_{ijct} is a decision, vote, or text metric for case i by judge j in court (circuit or district) c during year t. For the differences-in-differences estimates, we estimate

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

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. 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).[25] The year before attendance ($k = -1$) is the excluded year from which coefficients are computed.

For both the differences-in-differences regressions and the event study, the judge fixed effects hold fixed any judge level time-invariant factors affecting the outcomes of interest. Next, the court-year interacted fixed effects hold constant any time-varying court-level factors. These fixed effects are important because cases are randomly assigned within a court-year block. So conditional on court-year fixed effects, judges face similar case portfolios to their peers.

To capture short-run effects conditional on Manne attendance, the baseline sample includes only judges within the event study window. In supporting results, we use broader samples as indicated below. To further hone in on the first-come-first-serve variation, we will also report results limiting to the early years of the Manne program where we know the courses were consistently oversubscribed.

The term for additional covariates, $\boldsymbol{X}_{ijct}$, is used in supporting specifications as follows. First, we have $\boldsymbol{X}_{ijct} = \lambda_t \boldsymbol{X}_j$, 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. If the results are robust to the inclusion of the elastic-net-selected controls interacted with year, that adds reassurance that there are not confounding judge-level factors driving the results. In robustness checks, we will add additional judge covariates to $\boldsymbol{X}_j$ that were not selected by elastic net, such as the party of the judge's appointing president.

Second, we would like to check for biases due to spillover effects of treated judges on their peers (see Appendix C.4). 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. As the peer share does not vary much within court-year, we set $\boldsymbol{X}_{ijct} = \alpha_j \bar{Z}_{ct}^{-j}$, allowing for a judge-specific effect of peer attendance.

Rounding out the specification, standard errors are clustered by judge. 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). In the district courts, we add additional exogenous covariates (month and

[25] We report results with shorter event study windows in the appendix.

day-of-the-week fixed effects) to improve efficiency.

5 Results

This section reports the empirical results on attending the Manne program on judge decisions. First we look at effects on the use of economics language in the circuit courts, then go on to circuit court decisions about regulatory agencies. Third, we look at results for criminal sentencing. Here, we report the main event study estimates. In Section 6 below, we summarize the magnitudes in Table 1. 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).

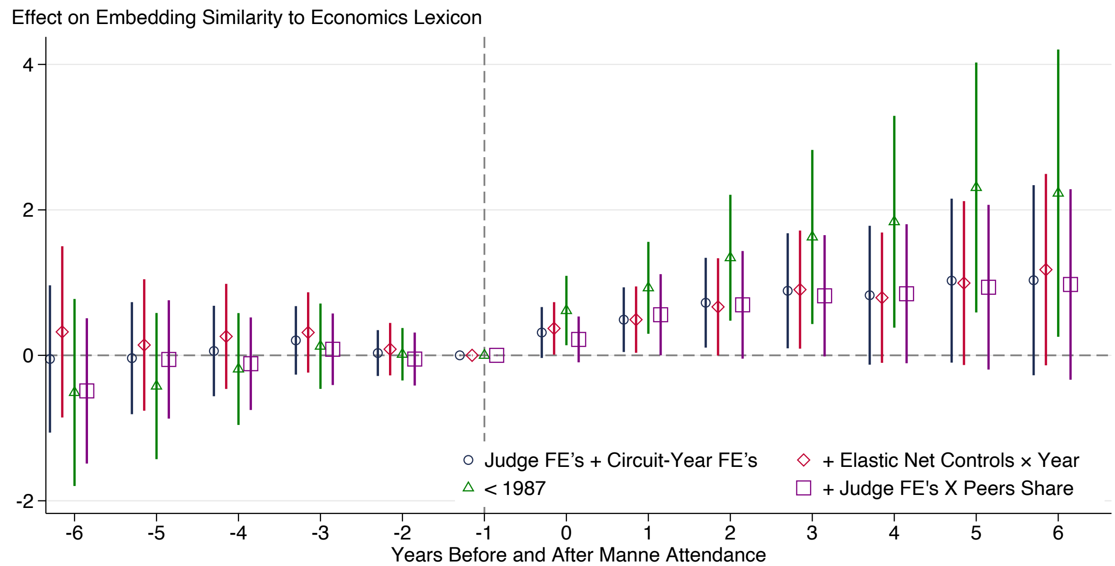
5.1 Effect of Economics Training on Judge Opinion Language

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 3.2 above. The sample includes majority-opinion authors and excludes non-author panel members.

Figure 3 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) include 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

Figure 3: 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 specification adding 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, with standard errors clustered by judge.

years after the program.[26] Meanwhile, there are no significant effects in the pre-trend period. The effect is notably larger when limiting to the early period (green triangles), reflecting that the effects on language are stronger in the early period (when law and economics is relatively new) and weaker in the later period (when law and economics is already relatively familiar). Further, for all specifications, we run the test from Rambachan and Roth (2019) and rule out substantial non-linear pre-trends (Appendix Figure A.11).

Appendix Section D.2 reports an extensive set of supporting results and robustness checks on the use of economics language use in judicial opinions. Complementary differences-in-differences regression results are reported in Appendix Table A.10 (see Table 1 Columns 1-4 for the baseline specifications). The main estimates looking at short-run effects 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

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

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.

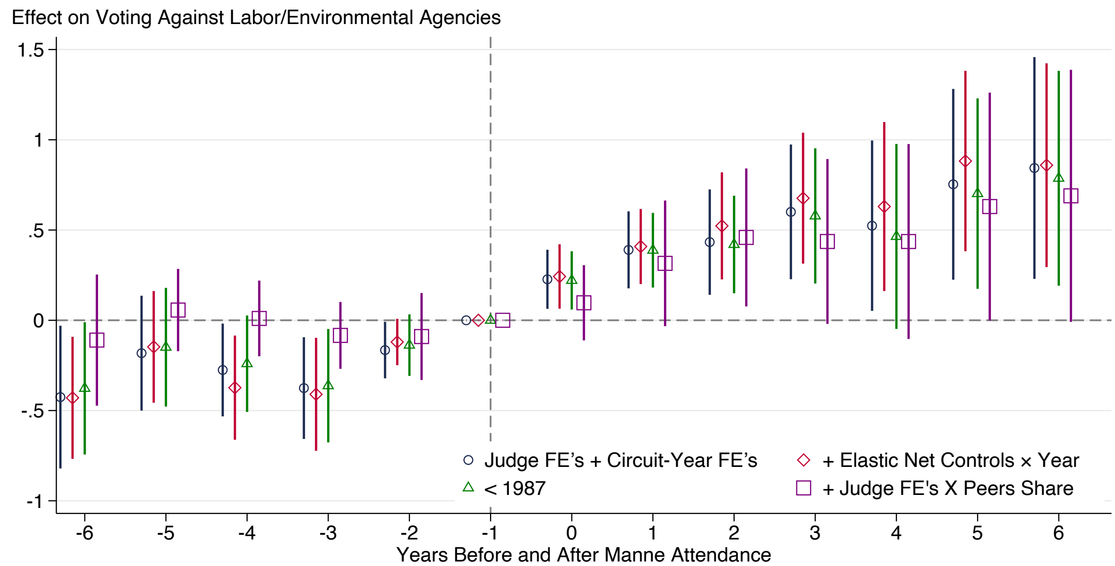
Appendix Section D.3 reports analogous results for the alternative measure of economics language using a supervised learning approach. That outcome is a machine prediction, based on the text of an opinion, of how similar it is to an opinion written on an economics topic. The results are consistent, with a statistically significant positive event-study effect from the Manne program (Appendix Figure A.18; Appendix Table A.11).

Appendix D.4 reports supporting results to help unpack the language result. 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 Table A.12), 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.20). Hence, the effect on language seems to be more on the conceptual use of economics, rather than the use of econometric analysis.

5.2 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 the Law-and-Economics movement specifically criticized: the labor agencies (especially the National Labor Relations Board and Department of Labor) and

Figure 4: 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. The baseline specification (blue circles) includes judge and circuit-year fixed effects. Additional specifications add 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, with standard errors clustered by judge.

the Environmental Protection Agency. Our outcome is whether a circuit judge votes against one of these agencies on appeal.

Figure 4 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 effect reflects 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, suggesting that they reflect peer effects from being exposed to earlier cohorts of Manne attenders in one's circuit or district. Appendix Figure A.21 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, upon the inclusion of 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.22). 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.13 (see Table 1 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.27 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 1 Columns 9-16 and Appendix Table A.14) 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).

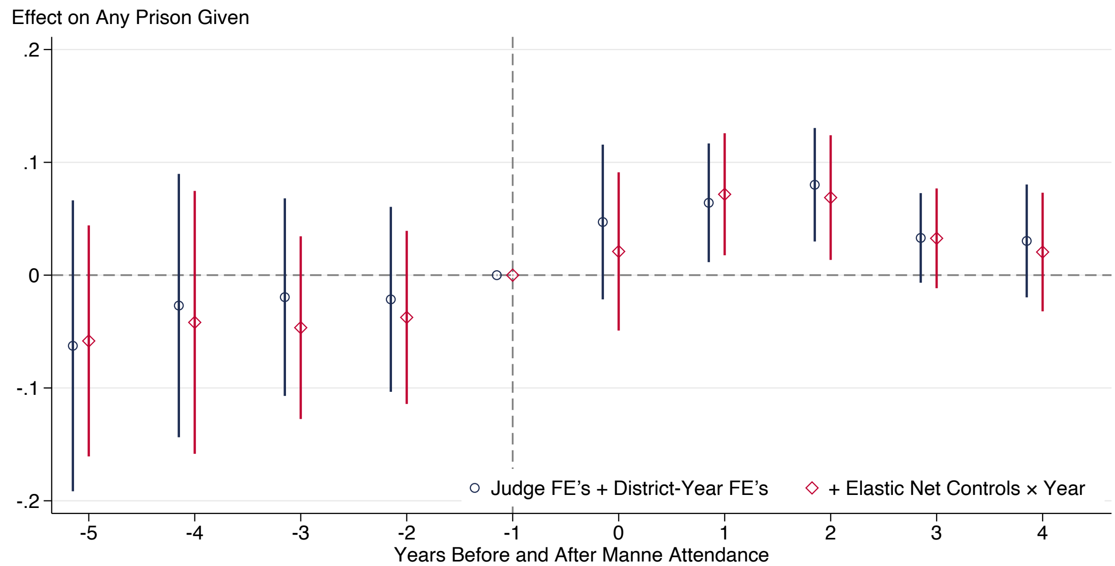
Now we consider the important category of antitrust law, a priority of the Chicago School and Henry Manne. Our outcome is a newly collected data point coded as voting against antitrust rights. Unfortunately, it is only available for a small number of cases (see Appendix G). With only 100 cases in the event study sample, we estimate positive, but quite imprecise, event-study coefficients in the short run after Manne attendance (see Appendix Figure A.33). Appendix Table A.16 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.

5.3 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 harsher penalties to increase deterrence, or a reduction in penalties given their social costs, or fines rather than jail (see Section 2.3 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

Figure 5: 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.

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 5. 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.[27] For both specifications, we see a positive jump in the outcome in the year 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. First, Appendix Figure A.34 shows the event study for IHS prison length, which despite sentencing guidelines still shows a positive effect, although not quite significant at

[27] 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.

the 5% level. Appendix Tables A.17 and A.18 report the differences-in-differences estimates for how Manne attendance affected district judge sentencing (see Table 1 Columns 17-20 for the baseline specs). We find evidence of harsher penalties on both any-prison and IHS sentence length, even when using the full sample of judges including never-attenders in the control group. The results are robust to include 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.35).

As mentioned, the event-study effects of Manne on sentence length are less robust than effects on any-prison because in the 1992-2003 period, district judges faced strict sentencing mandates. Those sentencing mandates were loosened by a 2005 Supreme Court Case, *United States v. Booker,* which gave judges more discretion in sentencing and allowed them to deviate from the guidelines. Next, we ask whether this increase in discretion was associated with differences in sentencing between Manne judges and their non-Manne colleagues.

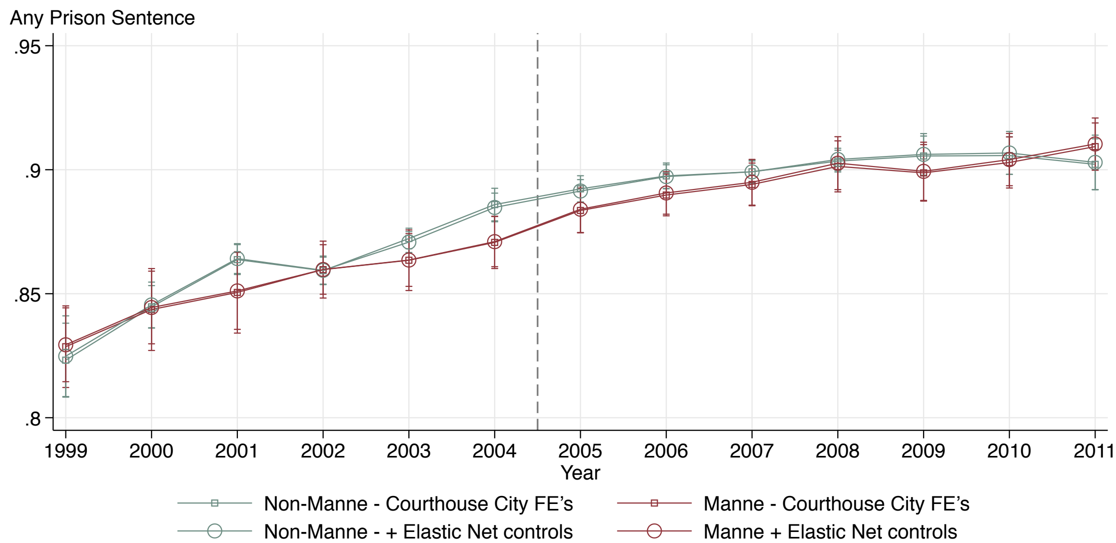
Formally, we model the crime sentencing outcomes (any-prison, and IHS prison length) as

$$Y_{ijct} = \alpha_c + \gamma_\alpha \alpha_t + \gamma_Z Z_j \alpha_t + X'_{ijct}\beta + \epsilon_{ijct} \tag{3}$$

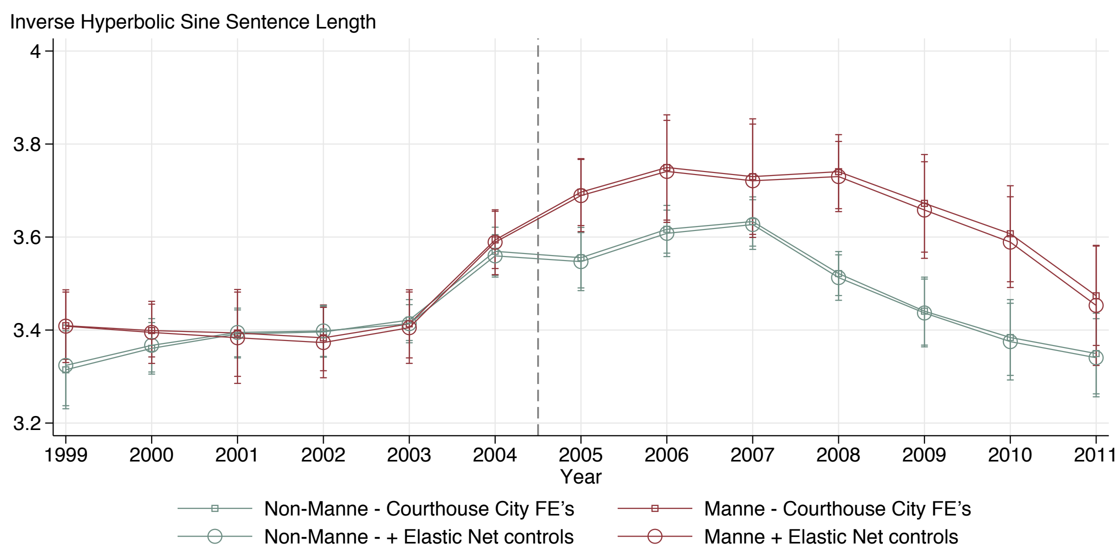
where α_c is a courthouse fixed effect and X_{ijct} includes case-level and judge-level covariates. We take advantage of more detailed data in the later period, adding fixed effects for month, day-of-the-week, crime category, and investigating agency. At the judge level, we have elastic-net-selected judge characteristics – where the variables are selected to predict a dummy variable for Manne attendance (rather than to predict the timing of attendance, as done above). With α_t representing year effects and Z_j equaling one for judges who attended the Manne program, we have that $\hat{\gamma}_\alpha$ contains the annual averages of the outcome (residualized on other covariates) for non-Manne judges, while $\hat{\gamma}_z$ contains the annual differences for Manne judges relative to non-Manne judges.

To visualize the estimates from Equation (3), we use marginal effect estimates to produce linear predictions for the outcomes by year and separately for Manne and non-Manne judges. Figure 6 reports these linear predictions for any-prison (panel a) and IHS sentence length (panel b). For each outcome, we have the predictions from specifications with and without elastic-net-selected controls (although for most regressors they are not distinguishable). At the extensive margin of receiving any

Figure 6: Effect of Manne Program on Sentencing under Higher Discretion



(a) Any Prison Given



(b) IHS Prison Sentence Length

Notes. Margins plots for differences between Manne and non-Manne judges in sentencing outcomes over time. Panel (a): indicator variable for any prison given; Panel (b): inverse hyperbolic sine of the sentence length (in months). Regressions include fixed effects for courthouse, month, day-of-the-week, crime category, and investigating agency. Series with circles include elastic net selected controls. Spikes give 95% confidence intervals, with standard errors clustered by court.

prison time (panel A), we can see that there is no difference between Manne and non-Manne judges, before or after the *Booker* decision. For sentence length (panel B), however, there is a divergence between Manne and non-Manne judges starting only in the wake of *Booker*. The difference persists over the subsequent six years and barely changes when controlling for the elastic-net covariates. There is no sign of a difference beforehand, meanwhile.

Complementary regression estimates are reported in Appendix Table A.19, where we include a full set of courthouse fixed effects as well as calendar fixed effects for day-of-week and year-month. We see that there is no difference in sentencing harshness in the cross-section before *Booker* (second row). After *Booker* (third row), there is no Manne effect on sentencing at the extensive margin (Column 1). For length of sentencing (Column 2), there is a significant positive divergence for Manne judges relative to their non-Manne colleagues, consistent with Figure 6. The estimated effect translates to roughly 10 months in prison.[28] In Columns 5 and 6, we show that The differential effect of Manne under *Booker* discretion is focused on non-drug crimes; there is no effect on drug crimes.[29]

6 Discussion

This section provides some discussion of the evidence reported in Section 5, starting with a summary of the effect magnitudes. Table 1 reports the baseline differences-in-differences regression estimates. For the Circuit Courts (Columns 1-16), 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). Further,

[28]These effect sizes are slightly larger than previously estimated differentials for black defendants relative to comparable white defendants arrested for the same crimes. For example, Rehavi and Starr (2014) find that black defendants receive ten percent longer sentences than comparable white defendants for the same crimes. These results add to the findings in Yang (2014) that disparities are associated with judge demographic characteristics, with Democratic and female judges being more likely to exercise enhanced discretion after *Booker*.

[29]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." Appendix Table A.20 shows the *Booker* results when dropping a selection of crime types. The Manne-*Booker* interaction is largest when dropping drug crimes.

Table 1: Regression Estimates for Effect of Manne Program on Attenders

	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
	Any Prison Given				*IHS Sentence Length*			
	(17)		(18)		(19)		(20)	
Post Manne	0.061		0.049		0.240		0.198	
	(0.028)		(0.020)		(0.137)		(0.089)	
N	70784		260516		70528		259600	
Court-Year / 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 case outcomes, indicated by column headers. Specifications are the same as detailed in the associated regression tables for each outcome. Columns 1-16 are in the Circuit Courts; Columns 17-20 are in the District Courts. 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.

we report results for the whole time period and for the pre-1987 heyday period. In the District Courts (Columns 17-20), we have results for the any-prison-given outcome, as well as the inverse hyperbolic sine of sentence length. We have results for the short run and long run, but not pre-1987 since the District Court data does not go back that far.

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 is 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. That estimates for the other specifications are somewhat imprecise, however. For conservative votes in non-economics cases (Columns 13-16), meanwhile, there are consistently small coefficients (between .024 and 059), about one-tenth of a standard deviation and not statistically different from zero.

In the district courts, we see that after attendance, incarceration rates increased by 5-6 percent. Those coefficients are statistically significant and about one-tenth of a standard deviation. The effect on IHS sentence length, about .2-.24, is also statistically significant and about one-fifth of a standard deviation.

Given the stakes of the associated outcomes, these magnitudes indicate economically meaningful impacts and speak to the power of economics ideas. Perhaps the most similar evidence to ours is Azgad-Tromer and Talley (2017), who show that a comparable financial economics training course influenced the asset pricing decisions of energy regulators. Yet even brief exposure to economics can have an effect. 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). Related findings from the effect of partisan media on voters come from DellaVigna and Kaplan (2007), Gerber, Karlan, and Bergan (2009), and Martin and Yurukoglu (2017).

A variety of mechanisms from the literature on political influence can help explain how the Manne Program had such a major impact. Was the Manne program just a lobbying vehicle, where judges heard a cloaked version of a general business interest?

Or was it a partisan, ideologically persuasive curriculum, akin to a sophisticated version of partisan media? Or was it more of a human capital investment, providing objective tools for analysis? While the precise psychological and social mechanisms by which the program influenced judges are not distinguishable by us, we can rule out the simplest explanations, indicating a somewhat complex relationship of economics to judicial decision making.

A first possibility is that the Manne program consists of lobbying judges by interested business parties (Grossman and Helpman, 2001; Teles, 2012; Bertrand et al., 2021). In that case, like other lobbying efforts the program combines quid-pro-quo with information provision that could nudge decisions for businesses, especially in domains relevant to the funders. Consistent with this view, we find that judges become more conservative in decisions related to business. After attendance, they tend to disfavor regulatory actions, which might have cut into company bottom lines through environmental cleanup and supporting stronger labor unions.

A second, related, possibility is that the Manne program is a partisan, pro-Republican initiative, designed to shift judges into supporting Republican policy priorities (Hovenkamp and Scott Morton, 2019). The aforementioned pro-business shift would fit comfortably into the Republican policy platform of the time. Moreover, the effect on criminal decisions is more consistent with partisan ideology, rather than lobbying. The businesses funding the Manne program would likely not care much about how the judges decide on criminal cases, yet Republicans are conservative on crime and would encourage harsher sentencing. The results on conservative dissents against Democrat appointees also suggests a partisan effect (Appendix Table A.21). A pivotal role for ideology in the decision shifts would be consistent with Blinder and Krueger (2004), who find that ideology is more important than economics knowledge in determining policy opinions.

But the qualitative record on the structure and content of the Manne program speaks against the simple lobbying or partisan stories. As shown in the sample agenda from 1991 (Appendix Figure A.1), the reading material and lectures consisted of introductory economics, applications to legal issues, some statistics and econometrics, and a handful of more normative seminars. There was little discussion of individual industries or businesses, let alone advocacy in favor of particular interests. Overall, the curriculum was only indirectly related to business or policy. Even the normative discussion on the wealth distribution (page 3 of the agenda) was ideologically balanced by the inclusion of Paul Samuelson on the panel. Many Democratically affiliated judges attended the Manne program and celebrated it.[30] For example, liberal D.C. Circuit Judge (and later Supreme Court Justice) Ruth Bader Ginsburg attended the Manne program, while her conservative colleague (also on the D.C. Circuit and subsequently promoted to the Supreme Court) Antonin Scalia did not. Further, if it were just partisanship, we would expect to see an increase in conservatism in social issues as well as economics issues. Yet in the hand-coded conservative-vote results, we do not see an increase in conservatism on social issues in the circuit courts.

A third possible interpretation is that the observed effects on Manne attendees are those ostensibly intended – that is, they are the result of judges learning economics. Notwithstanding the Chicago-School orientation of the instructors, the contents of the courses were not outside the bounds of mainstream 1970s economics. The program provided a bundle of economics ideas plus some tools of economic analysis. Consistent with that, Manne judges adopt the language of economic reasoning; after all, judges could find other reasons besides economic analysis to change their decisions to favor business litigants or push partisan priorities (Posner, 2008). Economics would support lower regulation, if the pre-existing regulation levels were suboptimally high. A Becker (1968) incentives approach to crime would support harsher sentencing to deter crime, holding the current detection probability constant. Each of these decision shifts could result from honest application of economics ideas, rather than lobbying or persuasion, especially in light of the diversity among economists in policy preferences (Fuchs, Krueger, and Poterba, 1997). Still, all of the main effects observed are in a clearly conservative direction.

Beyond the policy ideas, the Manne program was designed to build human capital, including economic analysis and statistics.[31] On an optimistic interpretation, the Manne program provided information about the economic costs and benefits of various decisions, improving the rationale and direction of economic judgments – that is, it might work like the program in Azgad-Tromer and Talley (2017), where finance training for utility regulators helped them set prices in line with asset pricing theory. If the previous legal decision-making was inefficient, then the results could be explained by the Manne program teaching judges to make more efficient decisions. The attending judges could then draw on this training over many years, with the overall

[30]See Butler (1999) and letters excerpted in Appendix A.

[31]According to Manne (1993): "Not only do these courses introduce judges to the basics of price theory, economic notions of cost, and the theory of the firm, but they also introduce many judges for the first time to the basics of accounting, statistics and finance."

quality of judicial decision-making going up.

Some previously reported evidence for a human capital component of the Manne program is Baye and Wright (2011), who find that Manne-trained judges are less likely to be reversed in antitrust cases. In this respect, the Manne program could have protected judicial opinions from appeal by giving judges literacy in economic analysis, while favoring particular outcomes. We find, in addition, that attendance of district judges appears to have increased the probability of promotion to higher appellate courts (Appendix Table A.23). Those promotions are driven by Republican appellate nominees (Columns 4 and 5), however, so the effect may be due to a partisan affinity between Republican administrations and the conservative economic jurisprudence promoted by the Manne program, rather than due to improved judge ability. Meanwhile, 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 Table A.22). Finally, the use of quantitative or statistical language actually decreases relative to not-yet-attenders post-attendance (Appendix Table A.20), suggesting that the attendees are not becoming more numerate afterward. Overall, this evidence suggests that the Manne program's ideas, rather than the analytical tools, were most impactful on the attending judges.

While we cannot distinguish whether attending judges changed preferences, skills, or beliefs (or all of these), our evidence, along with the judges' appreciation letters, suggests that the persuasion was effective. Why might economics education be so effective at persuading judges? One possibility is that price theory functioned as a "narrative" that amplified the information given (Schwartzstein and Sunderam, 2021). But even with rational judges, an insight from Gentzkow and Kamenica (2011) is that the Manne program could effectively persuade judges even if they recognize the program's conservative slant. In this framework, the economics 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 agent 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.

The social and group aspects likely added to the program's suasive impact. Knowing that other judges understand the language of economics would encourage attendees to use such language, as this could reduce the probability that other (economics-exposed) judges would overturn a decision (Gennaioli and Shleifer, 2007; Baye and Wright, 2011). The program may have had a lasting effect on the policy preferences of judges by altering their access to information, social identity, and social networks – even after the program was over. We have seen from the archival documents that the Law and Economics Center frequently followed up with judges by mailing them material and inviting them to subsequent events and workshops. The Manne program may have helped establish links between judges and the broader set of conservative legal networks, such as the Federalist Society. 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).[32] The multiple gift-exchange features of the initial Economics Institute – an upscale venue, often on the beach, catered meals, with family members accompanying the judges at no cost – could have easily established a reciprocal relationship. Finally, student judges may overweight the information provided during the Manne program due to attention biases, information processing costs, or motivated beliefs (Benabou 2007). These social and psychological factors could be explored further in future work.

7 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 harsher 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-

[32] 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).

economics approaches, which applied the simplest price theory arguments. A training course for judges based on more recent generations of law-and-economics scholarship would be quite different, as the field has become more open to behavioral factors and much more empirical. Still, nothing in the Manne program was outside the bounds of the economics discipline. Normative assessment of these policy shifts likely depends on one's views about the efficiency of the law and economics interpretations of various legal rules, and the cogency of prior legal thinking.

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

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].[33]

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.[34]

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

[33] "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.

[34] "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.

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 how normative the economics instructors tended to be. Alchian said, "I'm trying to change your view of 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."

Some notable letters commented on the policy impact. The following quotes summarize how the program changed their approach to judging. First, District Judge Robert Carter, a self-identified progressive, comments on how the program made him think in terms of costs and benefits:

> I attended the first of the law and economics programs Henry organized for federal judges and what was learned was so worthwhile that I attended two additional programs-this despite the fact that 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, however, would have been forthcoming whatever the social outlook of the economist and that 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. While my views have not changed, the exposure to the thinking and teaching of the economists in these programs has led me to measure the cost of the social good being furthered against the gain to be achieved. I suppose what was learned amounts to social responsibility and required me to choose my priorities with greater care than before.

District Judge Anthony A. Alaimo discusses the potential scope of impact outside of traditional economic topics, but to areas of "judicial discretion" more broadly:

> While we are circumscribed by the parameters of existing statutes, regulations and case law, 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 on economics conducted under Dr. Manne's direction. I have attended his seminars during the past ten years and am eager to testify to their value. Indeed, I feel that, as a result of what I have learned at these seminars, I have become a much better judge, hopefully rendering more valuable and salutary decisions to this society.

Finally, District Judge Thomas P. Griesa comments on the impact on non-conservatives:

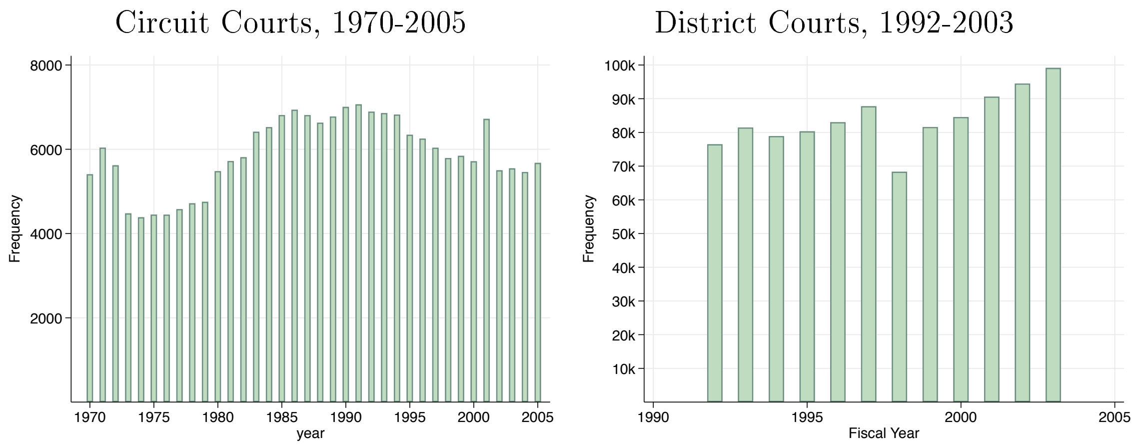
> There has been a feeling in some quarters that Henry and his LEC colleagues were of a conservative persuasion. I am not inclined to deny

> that. However, what has been taught has been professional economics of the highest and most sophisticated caliber. In any event, people of all stripes have attended and greatly benefited. I recall my first course when 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 of the LEC course.

These quotes qualitatively buttress the quantitative results in the paper: judges clearly found the program important for their thinking on legal questions.

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,[35] Federal Judicial Center, and previous data collection.[36] 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.

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

[36] 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

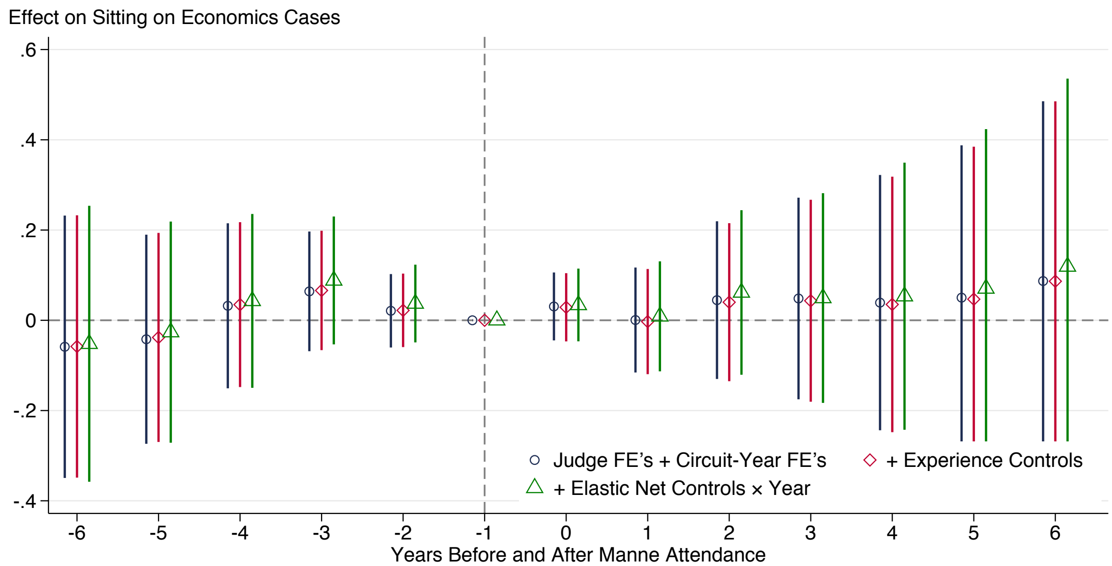
C.1 Checks on Selection into Different Case Types

Federal judges are randomly assigned 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). 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 of our main effects, and show below that treated judges do not get different types of cases.

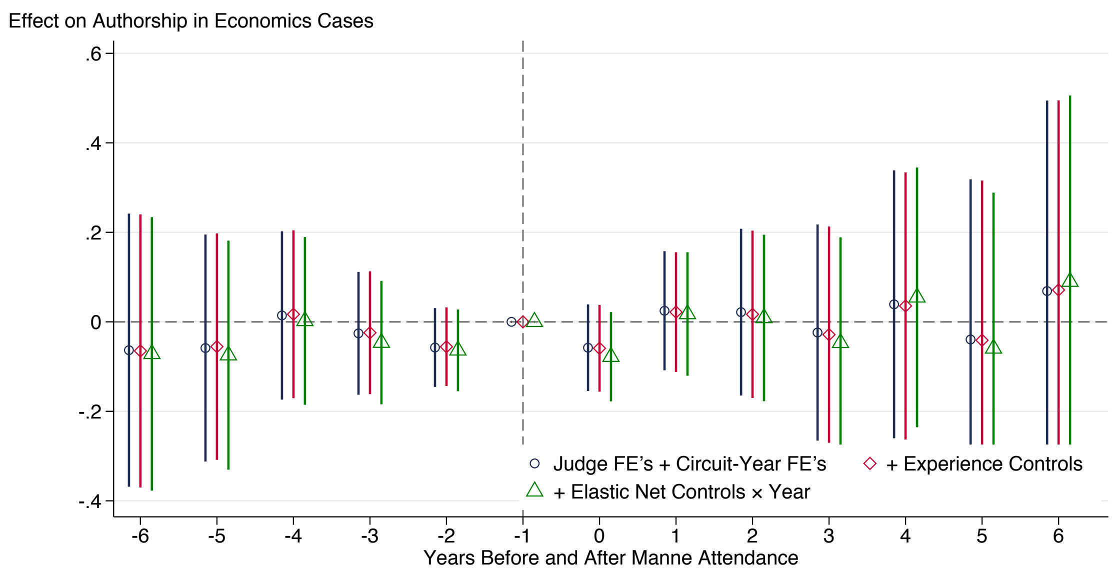
Appendix Figure A.3 shows that randomness does not appear to be violated in the context of Manne judges and the proportion of cases they sit on related to economics topics. In addition, they do not selectively author more economics cases.

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: Manne District Judges Don't See Different Types of Crimes

	Econ Training				
	(1)	(2)	(3)	(4)	(5)
Crime Type	-0.00545	0.0148	-0.00362	0.00319	-0.000646
	(0.0157)	(0.0441)	(0.0107)	(0.00898)	(0.00939)
Crime Type *	0.0127	-0.0132	-0.00621	-0.00825	-0.00691
Booker ($\geq$2005)	(0.0127)	(0.0445)	(0.0160)	(0.0147)	(0.0142)
N	930448	930448	930448	930448	930448
adj. R-sq	0.245	0.245	0.245	0.245	0.245
Courthouse and Calendar FE	Y	Y	Y	Y	Y
Crime Type	Drug	Immigration	Fraud	Weapon	Other

Effect of Manne Econ Training on the type of cases taken by district court judges.

For the district courts, Appendix Table A.3 presents an omnibus check for endogenous settlement or selection of cases by judges. It shows that economics judges are not systematically appearing on certain types of crimes before or after *Booker*.

C.2 Balance Checks on Manne Attendance

We report our balance checks in Appendix Tables A.4 (for circuit judges) and A.5 (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.4: Covariate Balance, Circuit Court Judges

	Ever Attend		Attendance Year			Ever Attend		Attendance Year	
	(1)	(2)	(3)	(4)		(1 cont.)	(2 cont.)	(3 cont.)	(4 cont.)
Republican	0.0640**		-0.0427		District Atty	-0.0294		-0.936	
	(0.0179)		(2.491)			(0.0332)		(0.860)	
Unified Appoint	-0.0251		-0.277		City Council	-0.0689		-1.420	
	(0.0194)		(2.488)			(0.0571)		(2.091)	
Cross-Party App.	-0.0548		-0.282		Cty Comm	-0.0346	-0.0387	1.739	1.390
	(0.0391)		(1.203)			(0.0495)	(0.0484)	(1.523)	(1.429)
State Senator	0.127		-0.712		Assit U.S. Atty	0.0153		-0.383	
	(0.0708)		(1.170)			(0.0261)		(0.656)	
State Lower Ct	-0.0326		0.311		Atty General	0.0842		-1.590*	
	(0.0242)		(0.593)			(0.210)		(0.807)	
State Supr Court	0.0153	0.00448	0.902	0.860	Asst Dist Atty	0.00676		-0.893	
	(0.0423)	(0.0423)	(1.015)	(0.973)		(0.0287)		(0.684)	
State House	-0.0381		1.235		Any Govt	0.0396		-0.128	
	(0.0463)		(1.051)			(0.0250)		(0.994)	
Solicitor General	-0.235**		0		Black	0.0511		0.711	
	(0.0838)		(.)			(0.0399)		(0.994)	
Solici Gen. Office	0.0765		3.243		Born 1910s	0.0977**	0.0673*	-2.881	-2.878**
	(0.124)		(2.338)			(0.0276)	(0.0289)	(2.869)	(1.076)
State Atty Gen.	-0.0305	-0.0261	-0.518	-1.219	Born 1920s	0.270**	0.255**	0.873	0.599
	(0.0374)	(0.0367)	(0.982)	(0.882)		(0.0314)	(0.0325)	(2.897)	(1.130)
Private Practice	-0.0951**		0.291		Born 1930s	0.219**	0.209**	4.399	4.416**
	(0.0332)		(1.067)			(0.0315)	(0.0328)	(2.936)	(1.175)
Mayor	0.0597		-2.548*		Born 1940s	0.0731*	0.0604*	9.082**	9.051**
	(0.124)		(1.289)			(0.0285)	(0.0287)	(2.896)	(1.182)
Local Court	0.0706	0.0664	0.726	0.684	Born 1950s	-0.0383	-0.0470	12.18**	11.67**
	(0.0385)	(0.0371)	(0.780)	(0.754)		(0.0275)	(0.0274)	(3.016)	(1.688)
U.S. House	-0.185**		5.796**		Bnktcy Judge	-0.0657		-2.434	
	(0.0525)		(1.696)			(0.0805)		(1.971)	
Governor	0.0318		-6.012**		Magistrate	-0.0878*		0.523	
	(0.113)		(1.026)			(0.0368)		(1.368)	
All Variables	X		X			X		X	
Post Elastic Net		X		X			X		X
N	699	699	379	379		699	699	379	379
adj. R-sq	0.124	0.129	0.464	0.497		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.

Table A.5: 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.

Table A.6: Pre-1976 Outcomes do not Predict Attendance

	Ever Attend		Year of Attendance	
	(1)	(2)	(3)	(4)
Pre-1976 Mean				
Econ Language	-0.00977	-0.00799	0.749	0.745
	(0.0658)	(0.0665)	(0.737)	(0.743)
Ruling Against Labor/EPA	0.0664	0.0870	0.807	0.953
	(0.144)	(0.149)	(1.865)	(2.062)
Conservative Economic Vote	0.00528	0.00112	2.392	2.337
	(0.149)	(0.155)	(2.217)	(2.177)
Circuit FE	X	X	X	X
Post E-Net X		X		X
N	1777	1777	379	379
adj. R-sq	0.108	0.110	0.464	0.497

Notes. Regression of Manne training on pre-1976 outcome means by judge. Robust standard errors clustered at the judge level in parentheses. $*p < 0.05, **p < .01$. Data collapsed by judge.

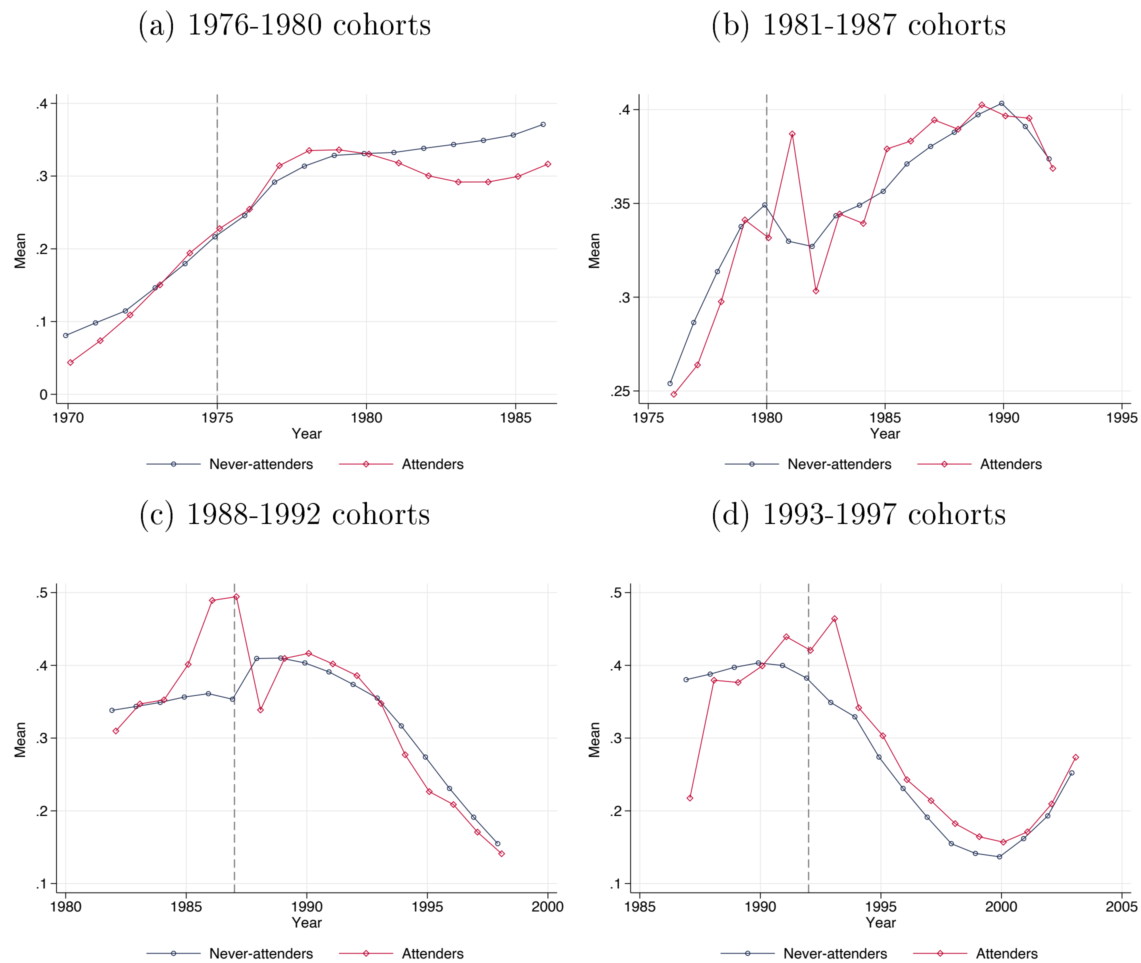
C.3 Outcome Trends for Attenders and Never-Attenders

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).

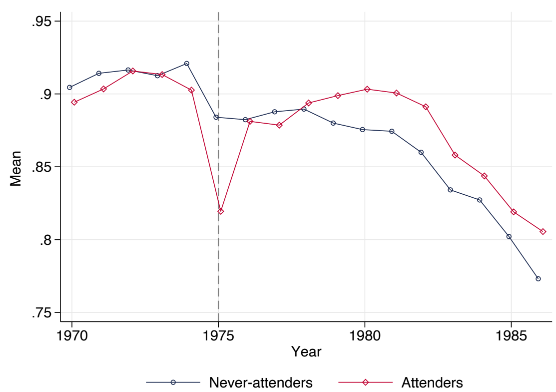
Figure A.4: Trends in Economics Language, by Manne Attendance Cohorts



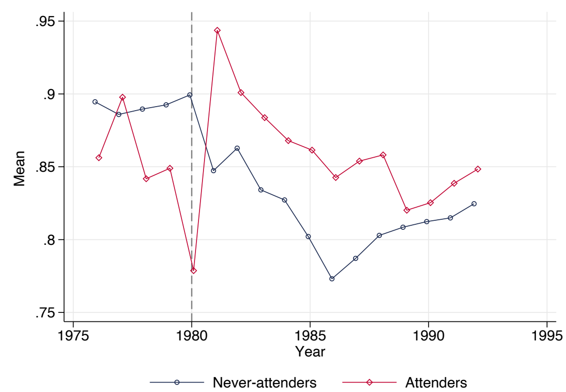
Notes. Average Economics Language over time, for Manne and non-Manne judges, separately by four cohort groups as indicated. Plotted values give smoothed rolling averages. Vertical dashed line at year before first cohort in group.

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

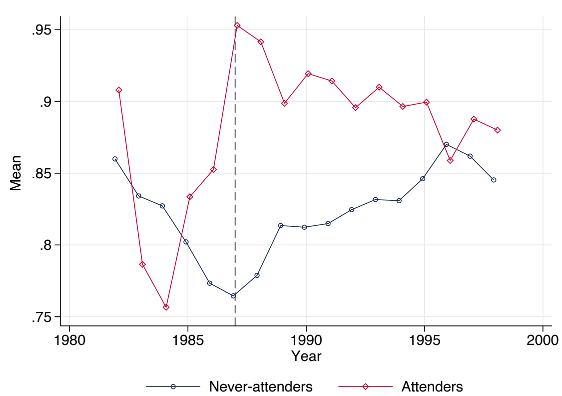
(a) 1976-1980 cohorts



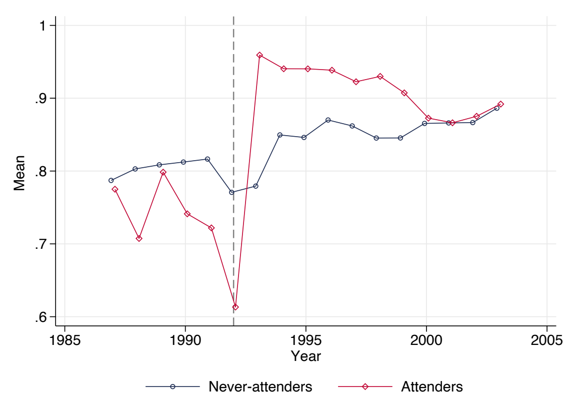
(b) 1981-1987 cohorts



(c) 1988-1992 cohorts



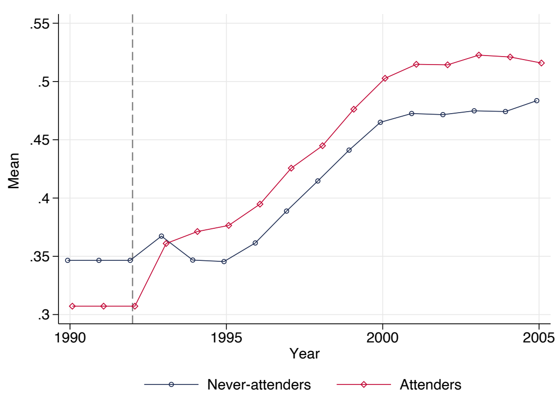
(d) 1993-1997 cohorts



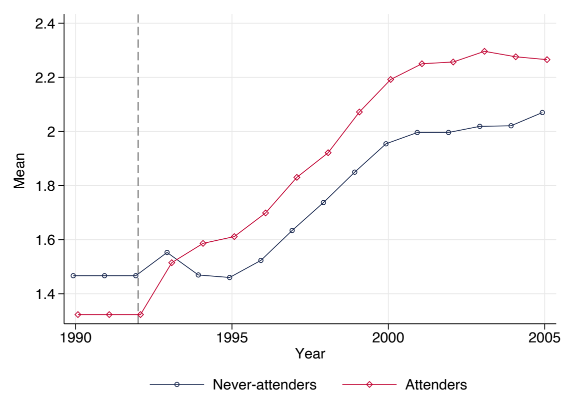
Notes. Average of Ruling against Labor/EPA over time, for Manne and non-Manne judges, separately by four cohort groups as indicated. Plotted values give smoothed rolling averages. Vertical dashed line at year before first cohort in group.

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

(a) Any Prison



(b) IHS Sentence Length



Notes. Average criminal sentencing (any prison and sentence length respectively) 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.4 Peer Effects on Never Attenders

Table A.7: Peer Effects of Manne Attendance

	(1)	(2)	(3)	(4)	(5)
	Effect on Economics Language				
Peer Attendance	0.324**	-0.143	0.261*	0.368**	0.181*
	(4.80)	(-0.60)	(2.41)	(2.84)	(2.09)
Circuit FE	X	X	X	X	X
Year FE	X	X	X	X	X
Judge FE					X
Never-Attenders	X		X	X	X
Pre-Attenders		X			
Authors Only			X		
Econ Case				X	
N	379599	28375	126114	99157	379467
R-sq	0.032	0.036	0.032	0.031	0.085

Notes. Regression of embedding similarity to economics on the share of peer judges who have had Manne training. Sample limited to never-attenders or pre-attenders, as indicated. Authors Only, Econ Case, and the year constraints also correspond to sample restrictions. Standard errors clustered at the judge level in parentheses. $*p < 0.05, **p < .01$.

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. Substantively, this is interesting as it could substantiate the further spread of economics ideas beyond the direct effects on attendees. Econometrically, it is important because if we use never-attenders as a control group, then peer effects would contaminate the control group and induce bias.

To test for peer effects, we estimate

$$Y_{ijct} = \alpha_c + \alpha_t + \gamma \bar{Z}_{ct}^{-j} + \epsilon_{ijct} \tag{4}$$

where as in the main text, Y_{ijct} is a decision, vote, or text metric for case i by judge j in court (circuit or district) c during year t. Further, we include court fixed effects α_c, 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.

Appendix Table A.7 reports effects of peer attendance share on economics language. For never attenders (Columns 1 and 3-5), we see a positive and statistically significant effect, which holds when including circuit fixed effects or judge fixed effects. Meanwhile, there is no spillover effect for attenders before they have attended (Column 2). These results indicate significant spillovers on never-attenders, suggesting caution when using these never-attending judges as a control group in empirical analysis.

One 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.15, A.26, A.32, and A.37.

C.5 Negative-Weighting Issues from Staggered Treatment Timing

A recent line of papers, starting with Goodman-Bacon (2018), 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. 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.8 Panel A. We can see that for almost all treated units ("LATEs"), the weights are positive.

Next, we apply the complementary diagnostic by Jakiela (2021), focusing on the

Table A.8: 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	0.0518	0.0626	0.329	-0.00207
Treatment Status	(0.153)	(0.372)	(0.211)	(0.00302)
Share Neg. Resids	0.330	0.280	0.310	0.360
Heterogeneity $\times$ 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).

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.8 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.6 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, becuase they are on different trends and due to peer spillovers. Hence, these results should not be interpreted causally but they provide a comparison for the main results.s

Table A.9 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.9: 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

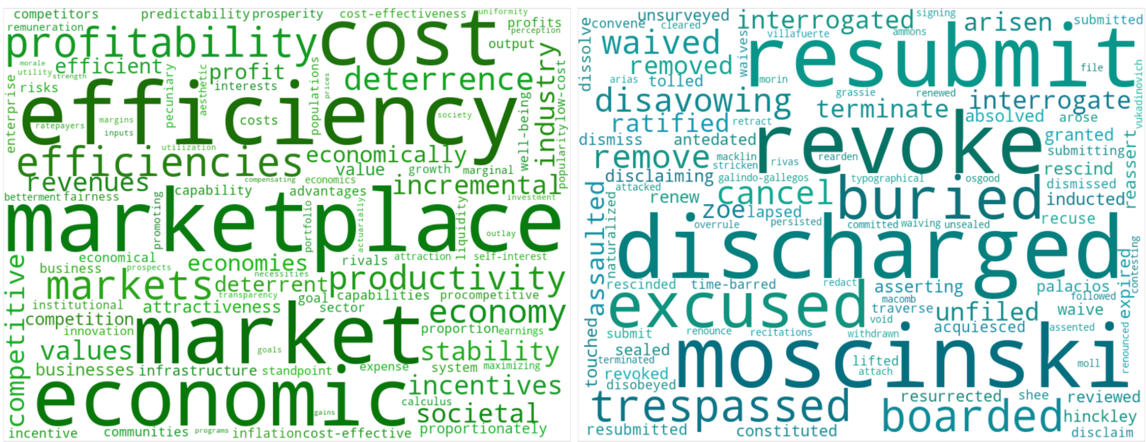
Notes. Estimated effects of Manne training in the full sample of judges where never-attenders are included. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means limiting to years 1986 and earlier. Party 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 non-regulated 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.

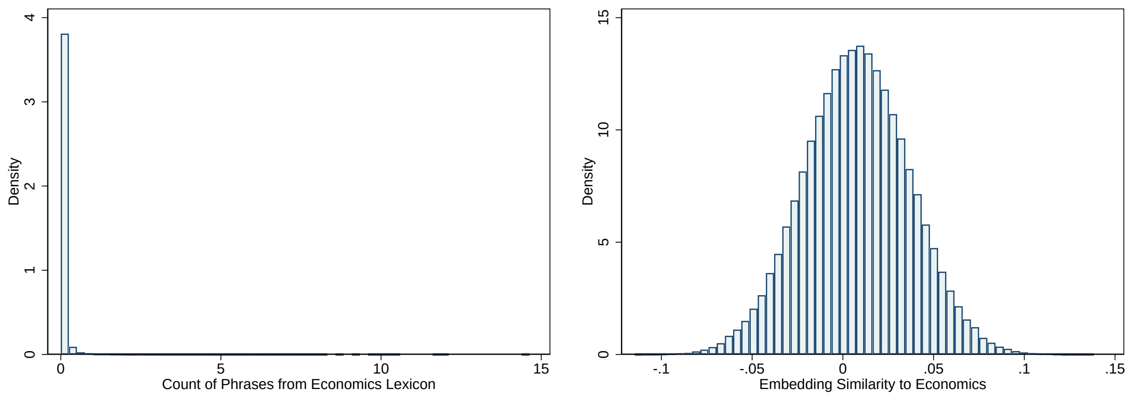
5. In an oligopolistic market characterized by few producers, price leadership occurs when firms engage in interdependent pricing, setting their prices at a profit-maximizing, supracompetitive level by recognizing their shared economic interests with respect to price and output decisions.

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.

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).

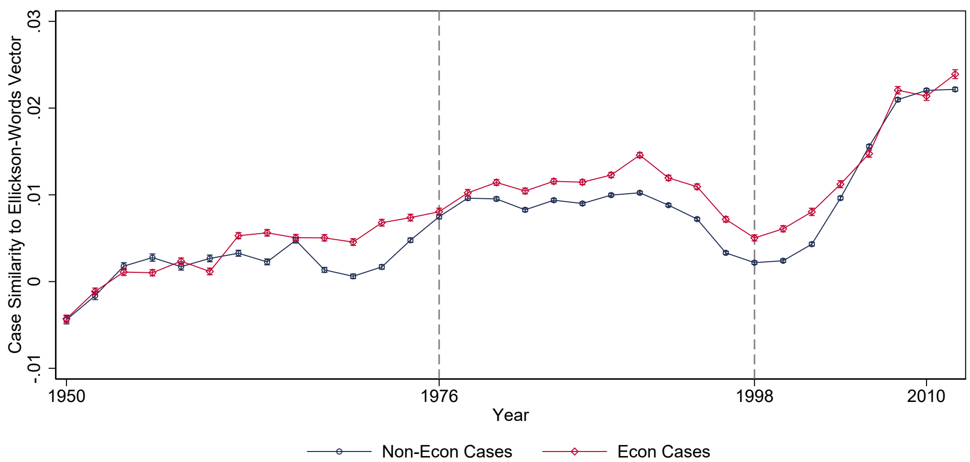
9. Reducing the number of interchanges and reducing the average length of haul have no economic significance in themselves, though both might reduce average transit time, which would be a benefit to shippers and hence a genuine efficiency gain

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.

Intuitively, 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.

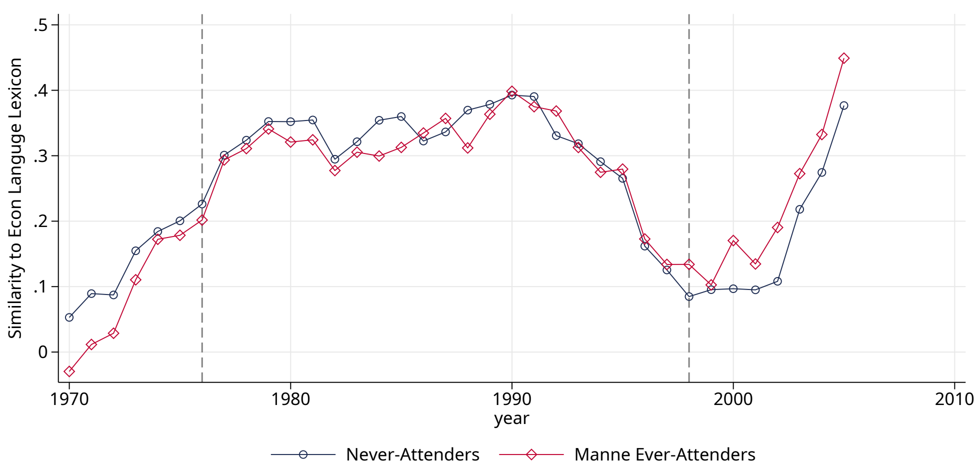
Figure A.9 shows the trend in the average case similarity to the law-econ dimension since 1950. We see that economics cases tend to score more highly, as expected. In addition, the use of economics language has been increasing over time. Figure A.10 shows the trends separately by circuit judges who attended Manne (in red) versus those who never attended (in blue). At the beginning of the sample, the Manne judges were actually negatively selected in terms of economics language. However, by the late years in the period, Manne judges were using more economics language on average. To see how such trends varied across Manne cohorts, see Figure A.4.

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.

Figure A.10: Trends in Economics Language, by Manne Attendance



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.

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 a problem for our empirical analysis, however, 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. So the shift in the language measure is due to the use of economics language.

D.2 Robustness Checks on Economics Language Results

Table A.10: 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 3.2. 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.10 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. You 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).

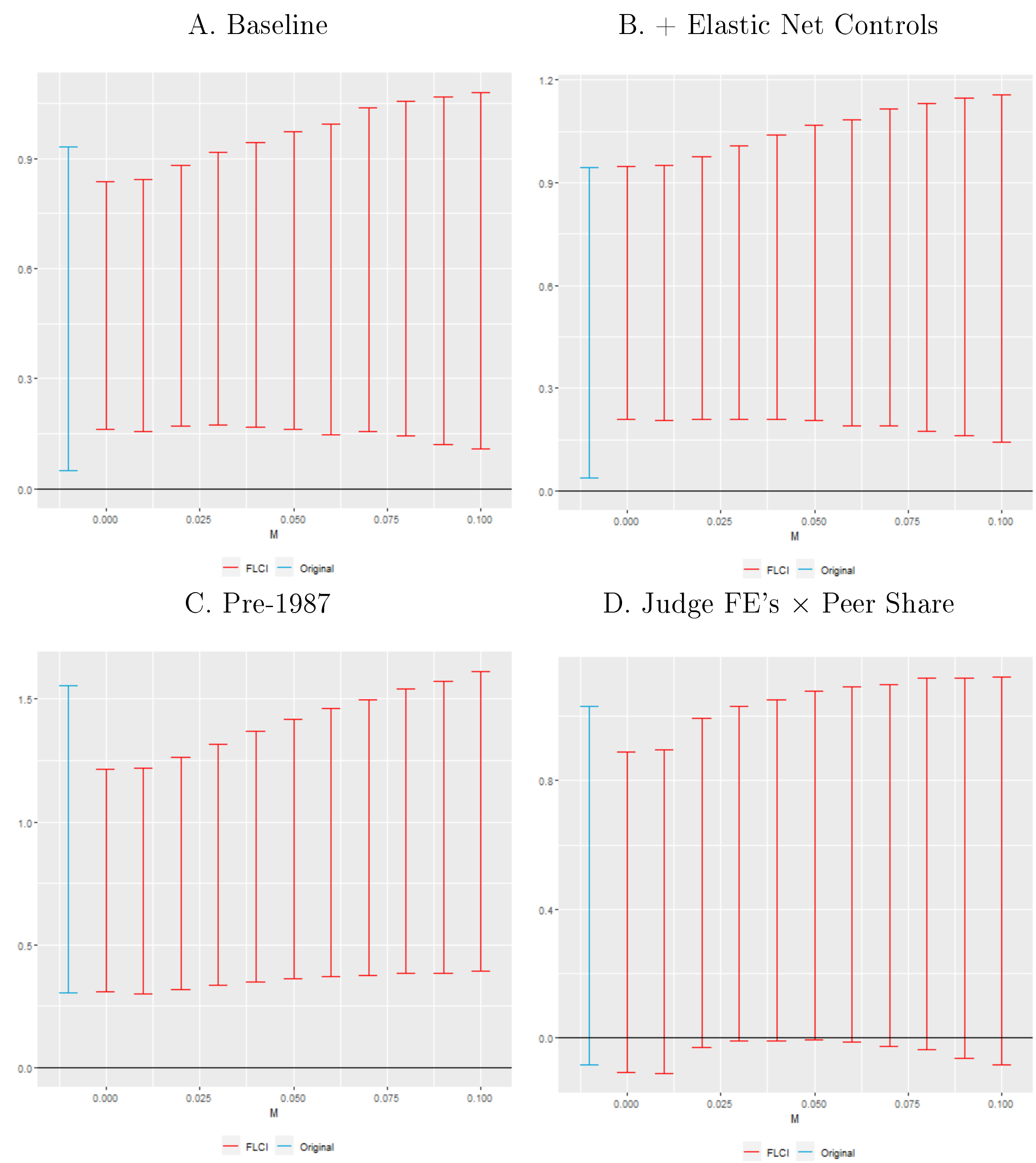
Next, we assess robustness to additional controls. The main short-run results are robust to controlling for party of a judge's appointing president, interacted with year fixed effects (Column 3), for the elastic-net-selected controls (predicting timing of attendance) interacted with year fixed effects (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.

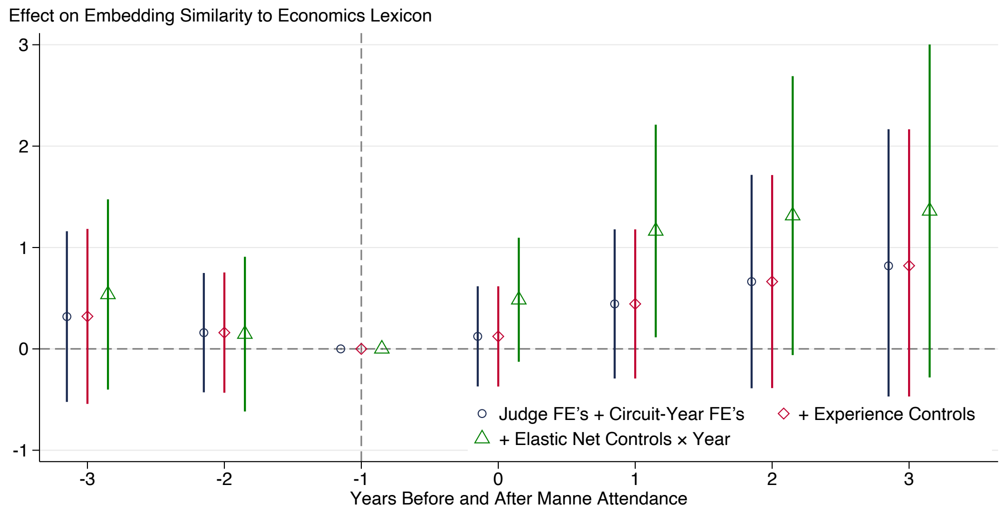
Moving back to the event study, we run the test from Rambachan and Roth (2019) to check for non-linear pre-trends. Appendix Figure A.11 shows that there is no sign of non-linear pre-trends according to that test. Further, we can show that the effects on economics language are not driven just by selective 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.12, the estimates are noisy and short-lived, yet overall consistent with our main results.

Figure A.11: Ellickson Econ Language: Pre-Trend Sensitivity Analysis



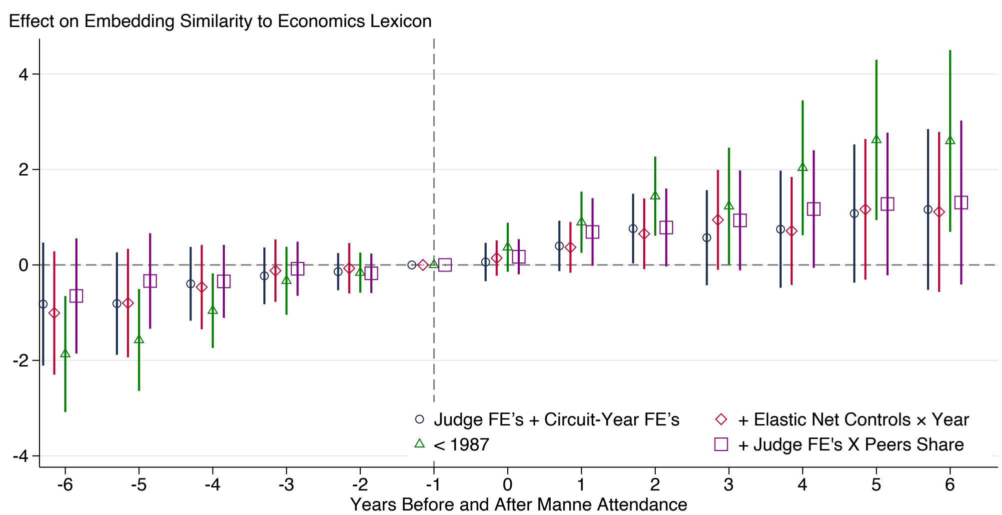
Notes. Sensitivity graphs for violation of the parallel trends assumption, applying the method from Rambachan and Roth (2019); see also Ang (2021). Outcome is Ellickson Embedding Similarity to Economics. The axis-crossing value of of $\bar{M}$ indicates that the significant treatment effect of Manne 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.12: Econ Language: Balanced Panel with Shorter Window



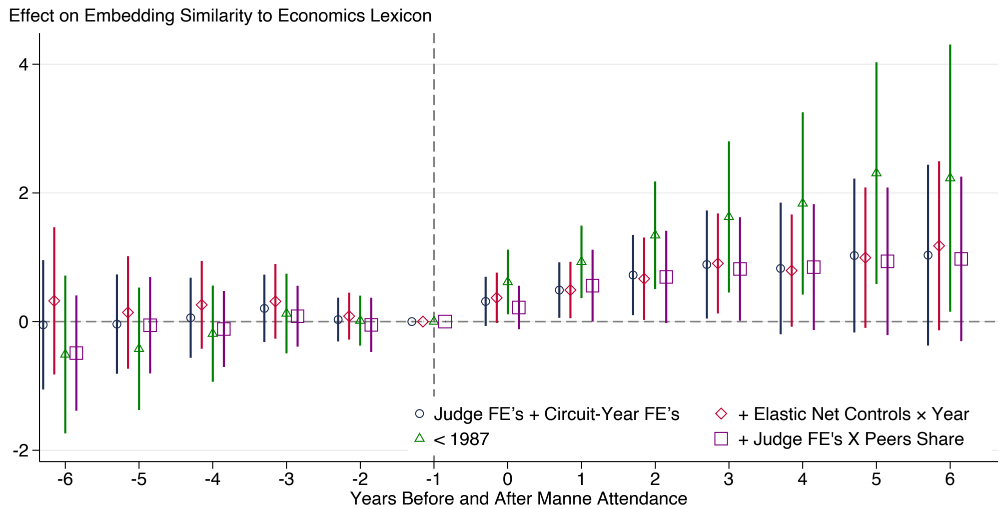
Notes. Event study regressions with balanced panels of judges, for three years of lags and leads, for Embedding Similarity to Economics. For other details see notes in the associated main-text exhibits.

Figure A.13: Ellickson Event Study: Dropping 2nd, 8th, 9th, and D.C. Circuits



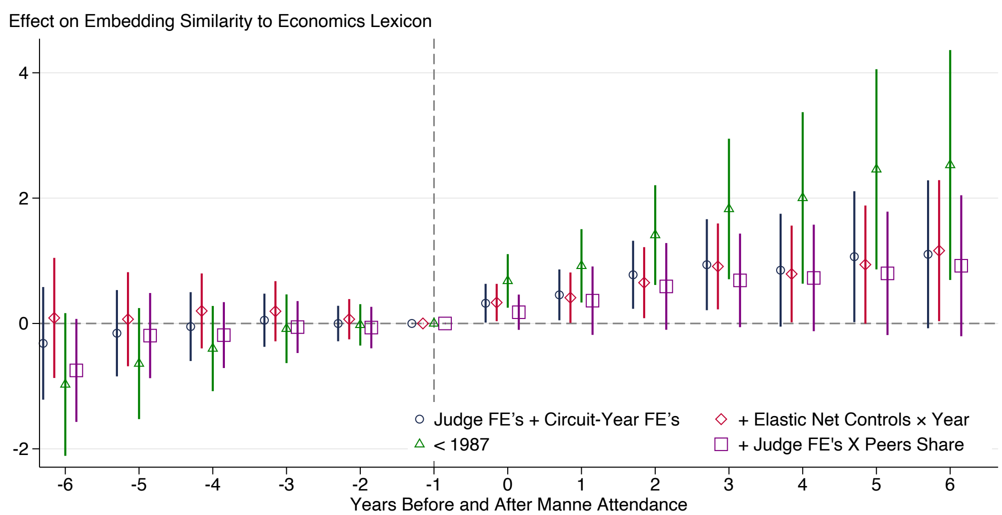
Notes. Main event study results for the circuit courts (from Figure 3) but dropping those circuits for which Levy and Chilton (2015) find nonrandom assignment in their calendar dataset from the years 2008-2013 (2nd, 8th, 9th, and D.C. Circuits). Outcome is Economics Language. For other details see notes in the associated main-text exhibits.

Figure A.15: 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.

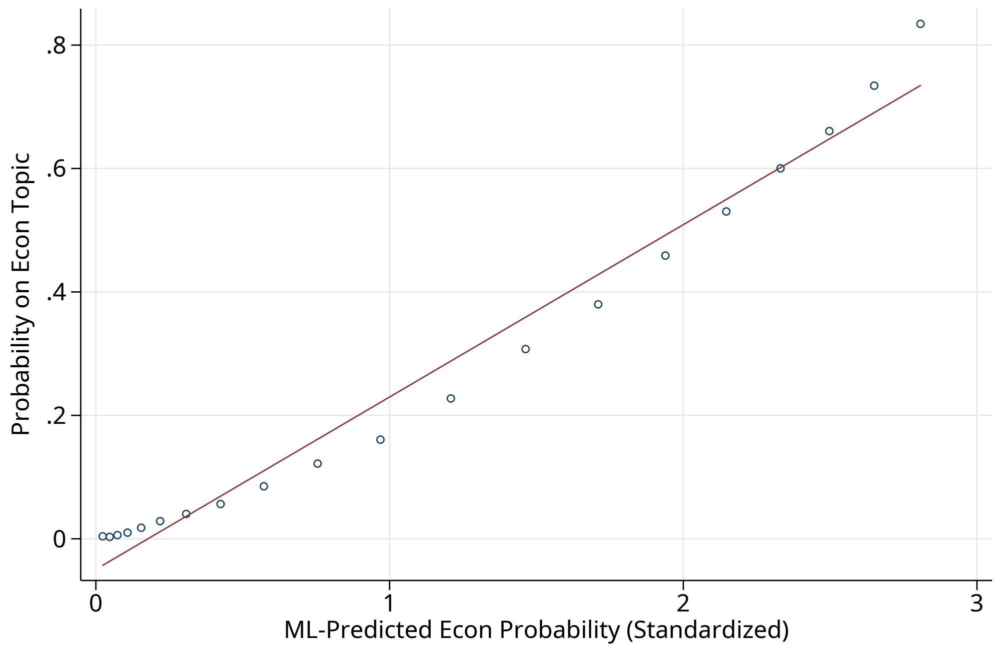
Figure A.14: Ellickson Event Study with Legal Topic Fixed Effects



Notes. Main event study results for the circuit courts (from Figure 3) 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 Text-Predicted Similarity to Economics Topics

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

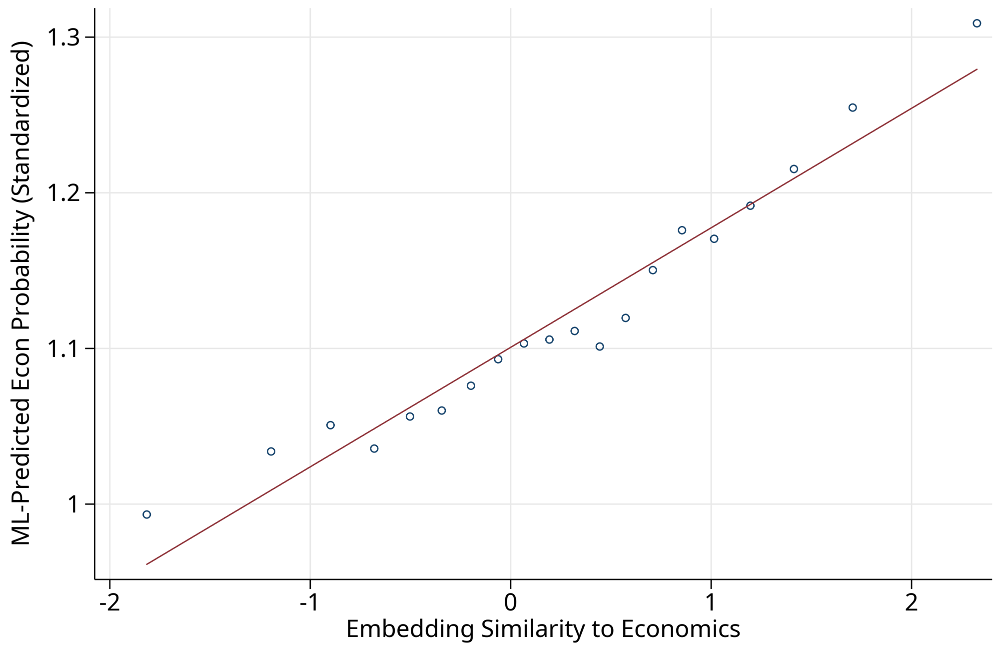


Notes. Binscatter of L2 logistic prediction for y = text-predicted economics case, in held out test sample. Horizontal axis is the predicted probability that a case is on an economics topic. The vertical axis is the true rate by bins of the prediction.

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.

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, 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.

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

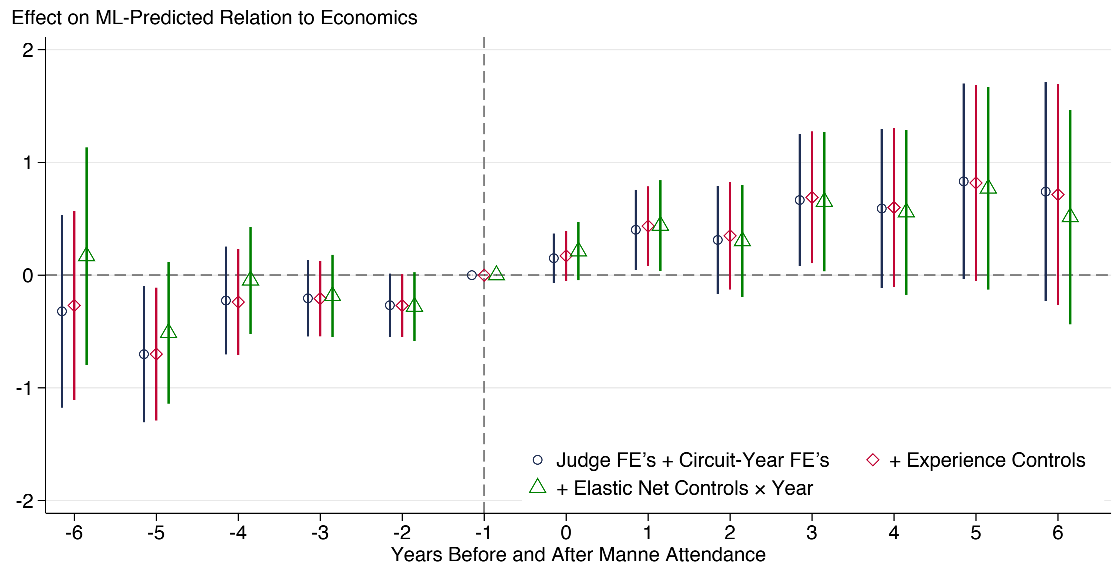


Notes. Binscatter of a case's embedding similarity to the Ellickson Law-and-Economics lexicon, against the predicted probability that a case is concerning economics topics.

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



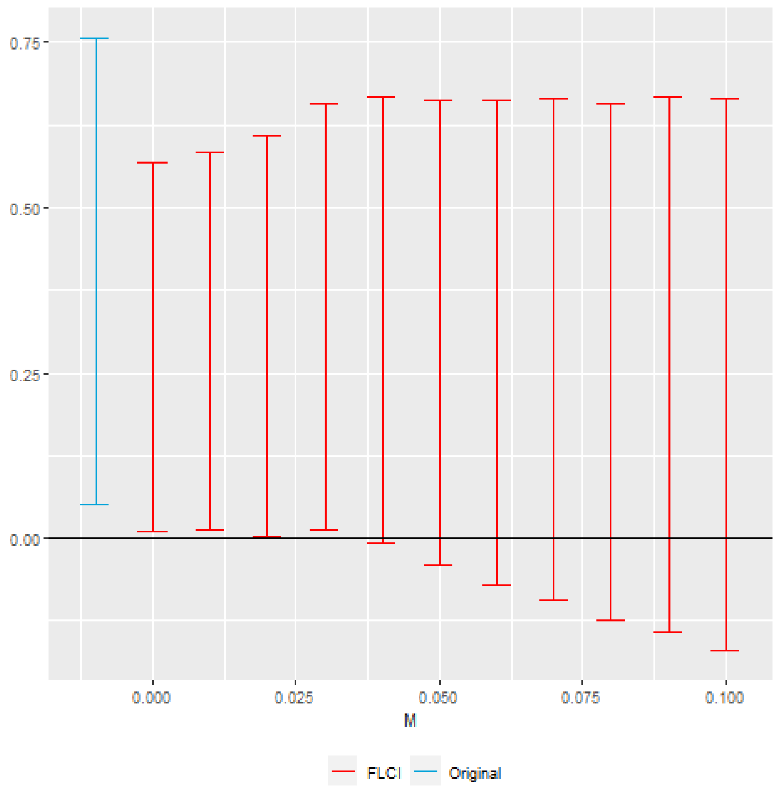
Notes. Event study effect of Manne attendance on text-based predicted probability that case is on an economics topic (regulation or labor). Sample is limited to case authors. Regressions include judge and circuit-year fixed effects (blue circles), with additional specifications adding quadratic in judge 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.

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.11 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

Baseline



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.11: 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.4 Additional Text-Data Results

Table A.12: Effect of Manne Program on Related Text Measures

	Similarity to Law Journals			*Citations to Bill of Rights*		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Manne	0.000123	0.000442	0.000478	0.00381	0.00276	0.00221
	(0.00567)	(0.00570)	(0.00561)	(0.00257)	(0.00258)	(0.00233)
N (Opinions)	18475	18475	18475	18475	18475	18475
Circuit-Year FE	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X
Experience Vars		X	X		X	X
Party × Year FE			X			X
E-net × Year FE			X			X

Notes. Estimated effects of Manne training on case text similarity to law journals (Columns 1-3) and citations to bill of rights amendments (Columns 4-6). Sample is limited to case opinion authors; short-run effects on attenders within 6 years of attendance. Standard errors clustered at the judge level in parentheses. $+p < .1, *p < 0.05, **p < .01$. Observations are weighted to treat judge-years equally.

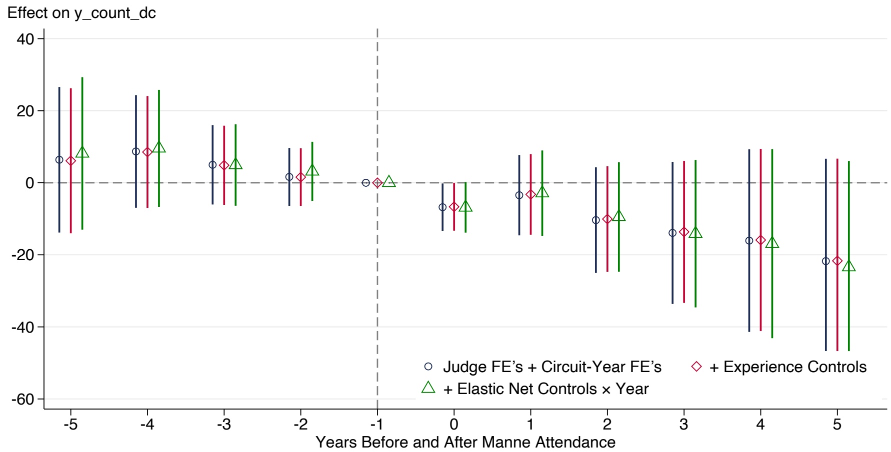
This section reports results for some additional measures of ideology and conservatism constructed from the text of the judicial opinions. First, we check whether our language measure is picking up more academic language, rather than economics language. The idea is that the Manne program worked by exposing 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 Table A.12), 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.20).

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.[37] We find no effect on this outcome (Appendix Table A.12). We tried other measures

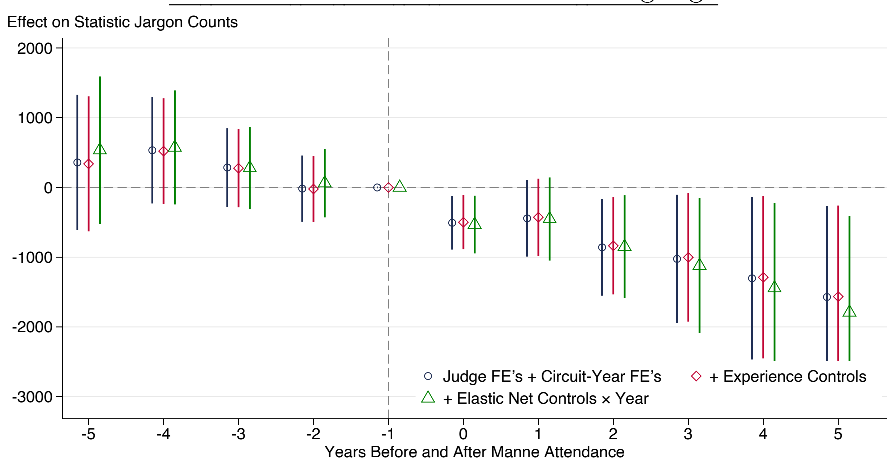
[37] We use frequency of citations to the Bill of Rights amendments for this outcome. A preferred measure of constitutional conservatism would have been Federalist Society membership, but this is not, to our knowledge, publicly available.

Figure A.20: 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"). 95% confidence intervals constructed using standard errors clustered at the judge level. Observations are weighted to adjust for varying caseloads across courts and years.

of constitutionalist reasoning, such as citations directly to the Constitution's articles, with similar zero effects.

E Additional Results on Regulatory Decisions

Table A.13: 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.13. 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).

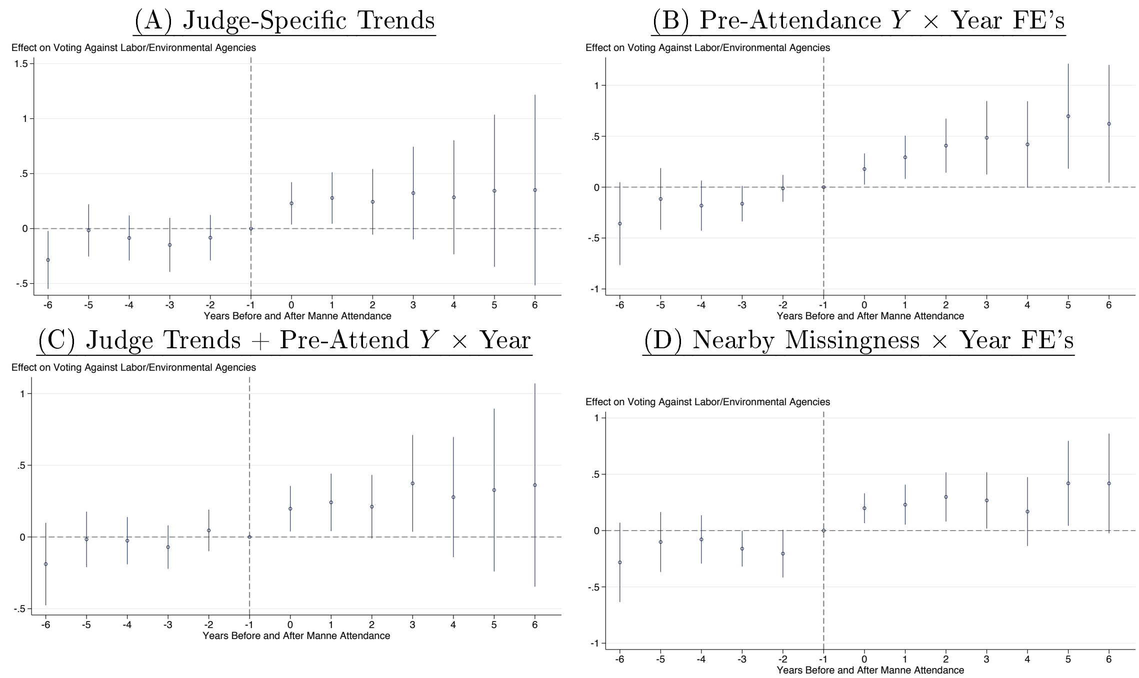
Overall, there are positive and statistically significant effects of Manne attendance on voting against labor/environmental agencies. In the long run, the estimate is robustly significant and stable across the inclusion of controls (Columns 14, 15, 16, 18). In the short run, the coefficient is stable 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).

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 having a larger effect in the early years when relatively fewer peers have already attended, when the caseload was lower. 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.9 Panel C, the coefficients have a similar positive magnitude and mostly significant across specifications. The exceptions are adjusting for judge-specific peer share (Column 30) and not weighting (Column 32).

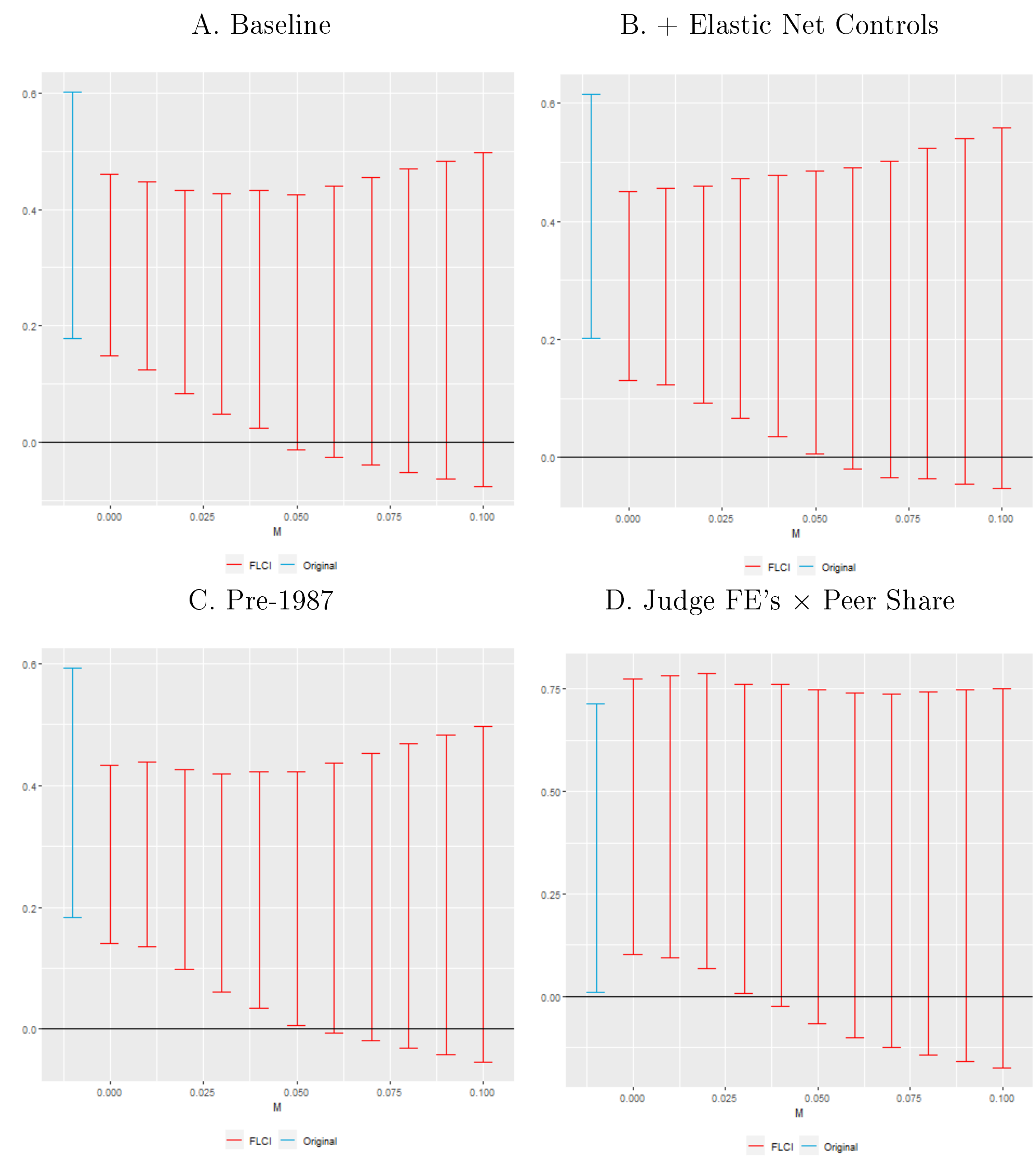
Figure A.21: Event Study for Labor/Environmental, Alternative Specifications



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.

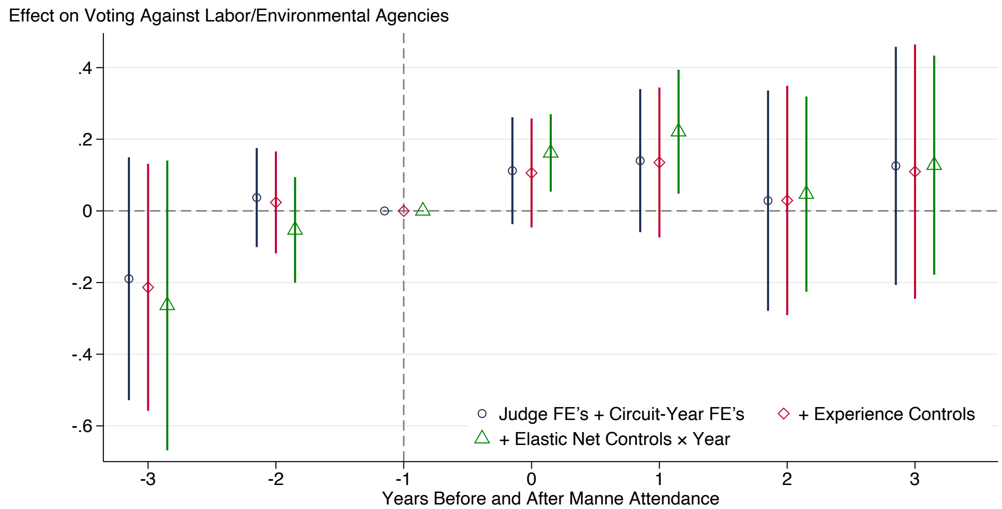
Now we revisit the event study regressions. Figure A.21 reports a number of alternative specifications which eliminate any sign of a pre-trend for the Manne effect on regulatory agencies. Panel A shows the event-study effect for labor-EPA cases with judge trends. Panel B alternatively includes the average 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 4. Further, we run the test from Rambachan and Roth (2019) in Appendix Figure A.22. 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.22: 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.

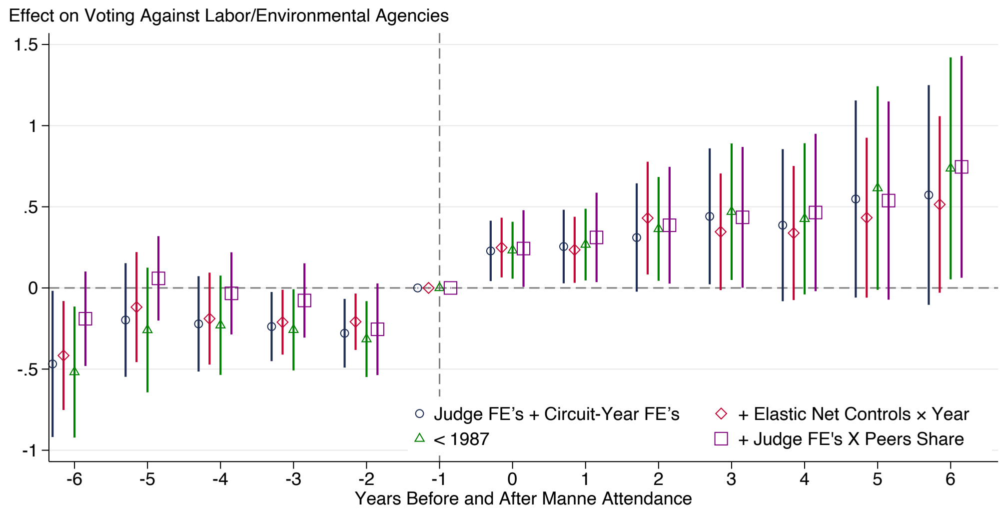
Figure A.23: Labor/EPA: Balanced Panel with Shorter Window



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.

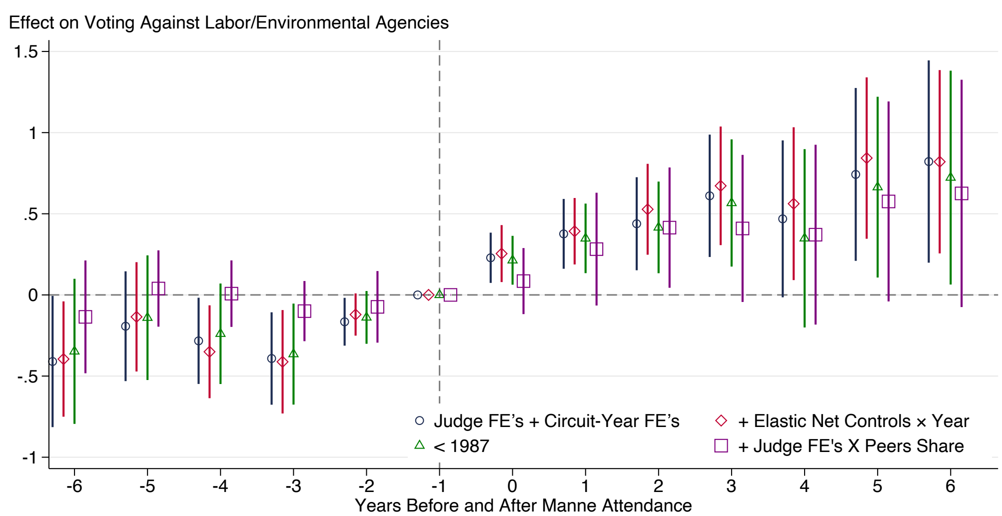
Next, to try to assess whether the event-study results are driven by selective attrition, we produced the event-study results for a balanced sample of judges with a shorter time window (three years before and after). Appendix Figure A.23 shows that the estimates are still positive but noisy.

Figure A.24: Labor/EPA Event Study: Dropping 2nd, 8th, 9th, and D.C. Circuits



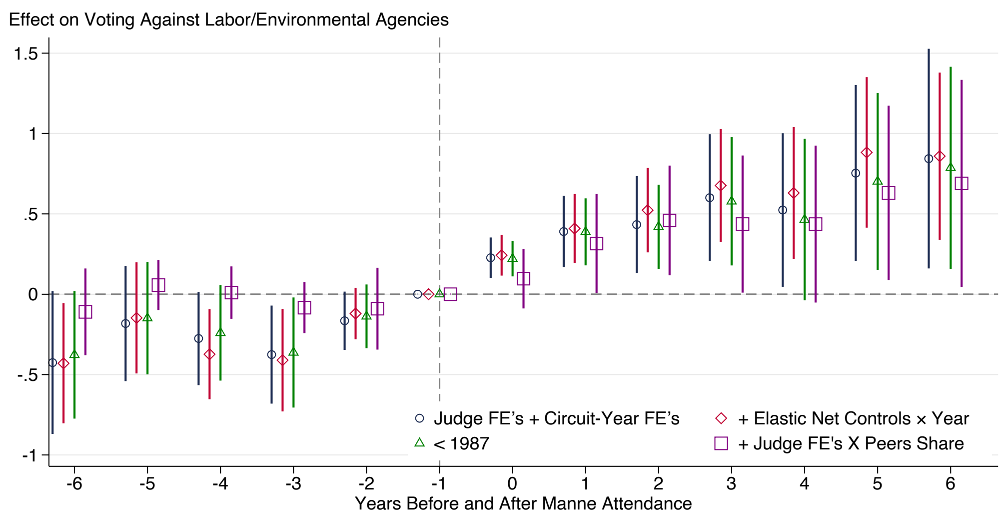
Notes. Main event study results for the circuit courts (from Figure 4) but dropping those circuits for which Levy and Chilton (2015) find nonrandom assignment in their calendar dataset from the years 2008-2013 (2nd, 8th, 9th, and D.C. Circuits). Outcome is Voting against Labor/Environmental Agencies. For other details see notes in the associated main-text exhibits.

Figure A.25: Labor/EPA Event Study, with Legal Topic Fixed Effects



Notes. Main event study results for the circuit courts (from Figure 4) 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.

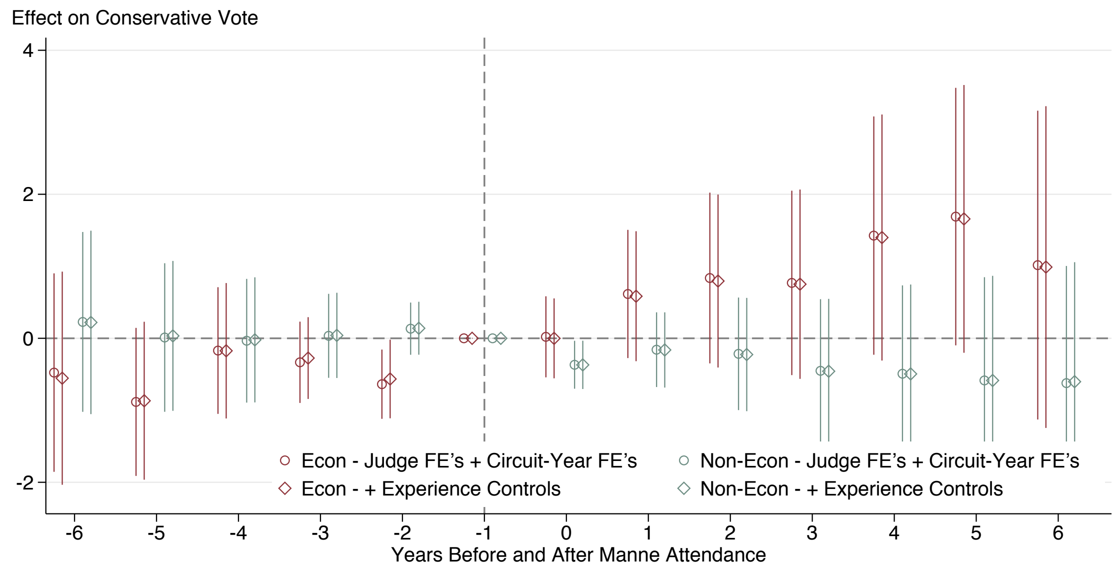
Figure A.26: Labor/EPA Event Study, Two-Way Clustering



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.27: Effect of Manne Program on Conservative Voting

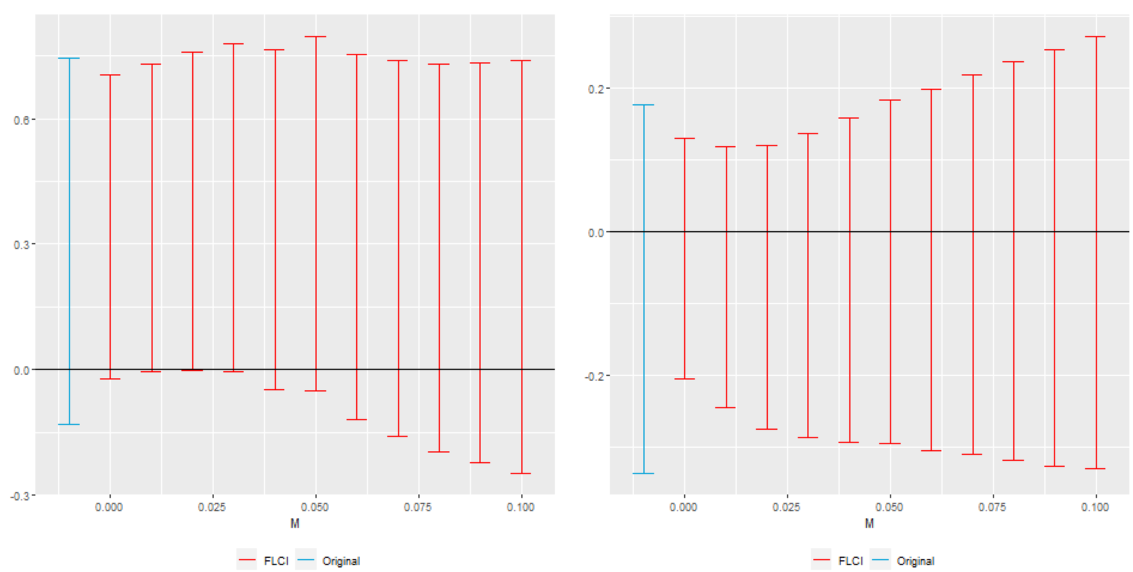


Notes. Event study effect on conservative vote in economics cases (regulation and labor; in red) and non-economics cases (in teal). Baseline specification (left dot in pair) includes judge and circuit-year fixed effects. Second specification (right dot in pair) includes controls for judge experience. 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.27. 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.28, 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. We do not include a specification with elastic net controls interacted with year because with a small (5%) sample of cases, we could not identify all interactions, leads, and lags for both economics and non-economics cases. For the same reason, we cannot undertake some of the other robustness checks for this outcome, such as estimates with a balanced sample.

Figure A.28: 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.14: 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.14 and A.15. In economics cases, we see evidence of positive effects (Appendix Table A.14). 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

Table A.15: 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

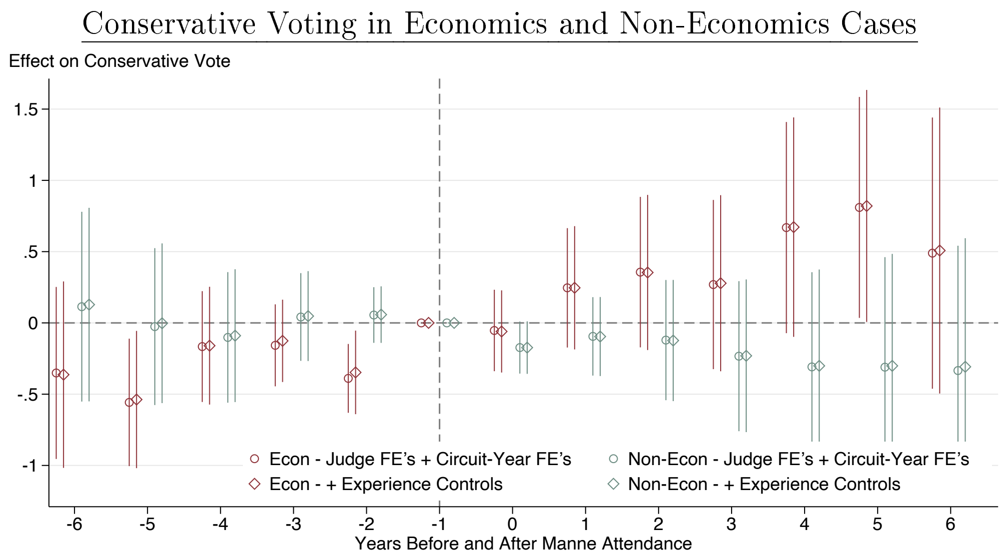
Notes. Estimated effects of Manne training on conservative voting in non-economics cases, hand-coded by Songer-Auburn for 5% of cases 1970 to 2002. Except where indicated, standard errors (in parentheses) clustered at judge level, and observations weighted to treat judge-years equally. Pre-1987 means 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. $+p < .1, *p < 0.05, **p < .01$.

(Column 5). Finally, the short-run results are robust to different specification choices for weighting (Columns 8-9) or clustering (Column 10-11).

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.

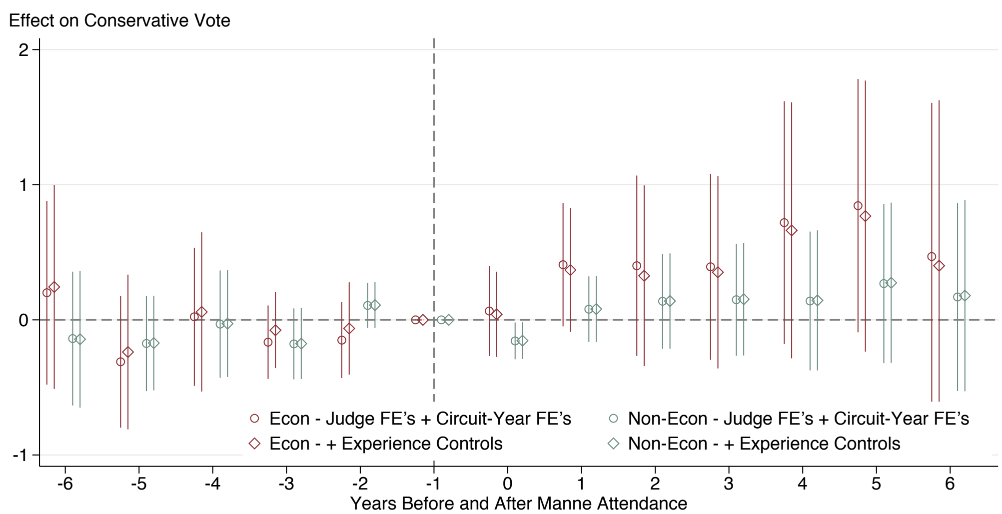
Looking to the non-economics cases in Table A.15, we see consistently null results with mixed sign. We are confident in saying there is no effect of the Manne program on ideological voting in non-economics cases in the circuit courts.

Figure A.29: Conservative Voting Results for Oversubscribed Period



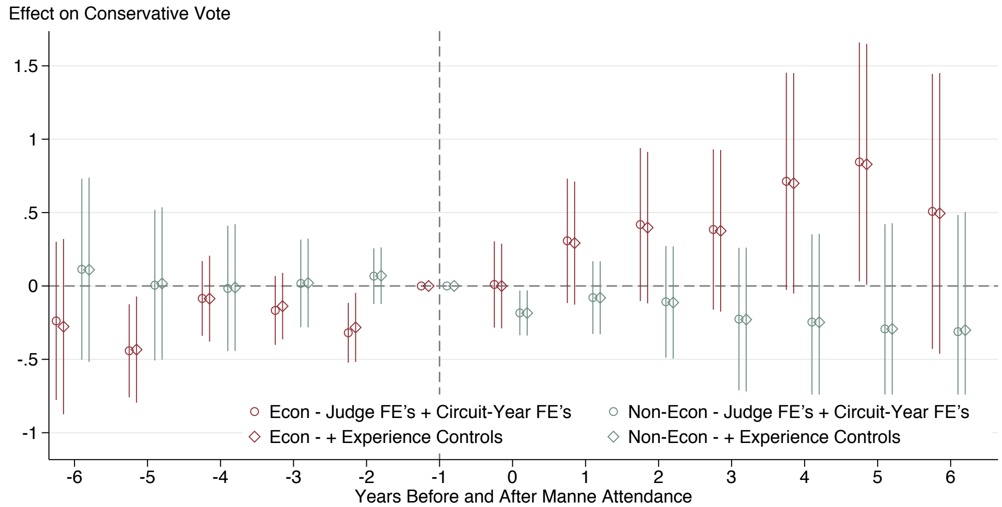
Notes. Main event-study results for conservative voting, limited to the heyday period when the Manne program was oversubscribed on a first-come-first-serve basis (pre-1987).

Figure A.30: Conservative Vote Event Study: Dropping 2nd, 8th, 9th, and D.C. Circuits



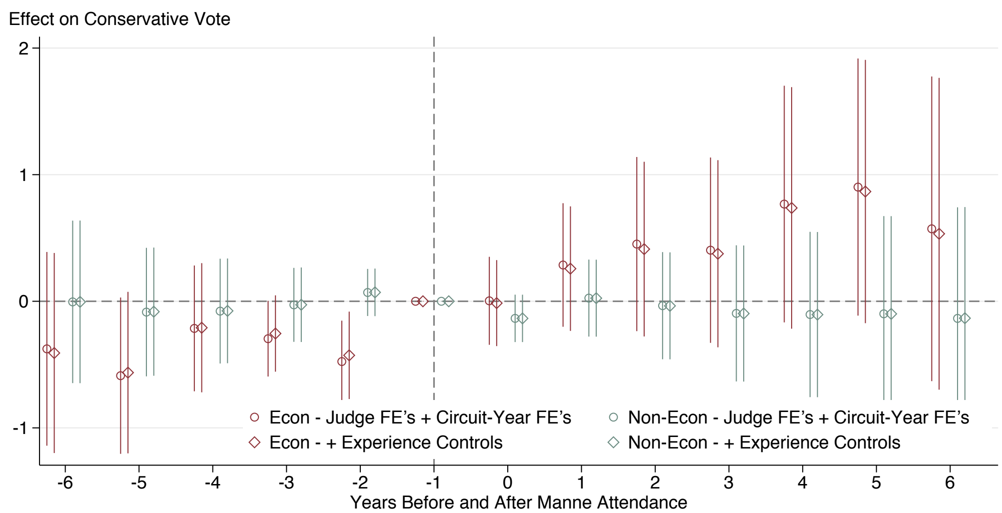
Notes. Main event study results for the circuit courts (from Figure A.27) but dropping those circuits for which Levy and Chilton (2015) find nonrandom assignment in their calendar dataset from the years 2008-2013 (2nd, 8th, 9th, and D.C. Circuits). Outcome is Conservative Vote in Econ and Non-Econ Cases. For other details see notes in the associated main-text exhibits.

Figure A.32: Conservative Vote Event Study, Two-Way Clustering



Notes. Main event study results for conservative vote 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.31: Conservative Vote Event Study with Legal Topic Fixed Effects



Notes. Main event study results for the circuit courts (from Figure A.27) but including fixed effects for 94 detailed legal topics. Outcome is Conservative Vote in Econ and Non-Econ Cases. For other details see notes in the associated main-text exhibits.

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 against the opposing party 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 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.

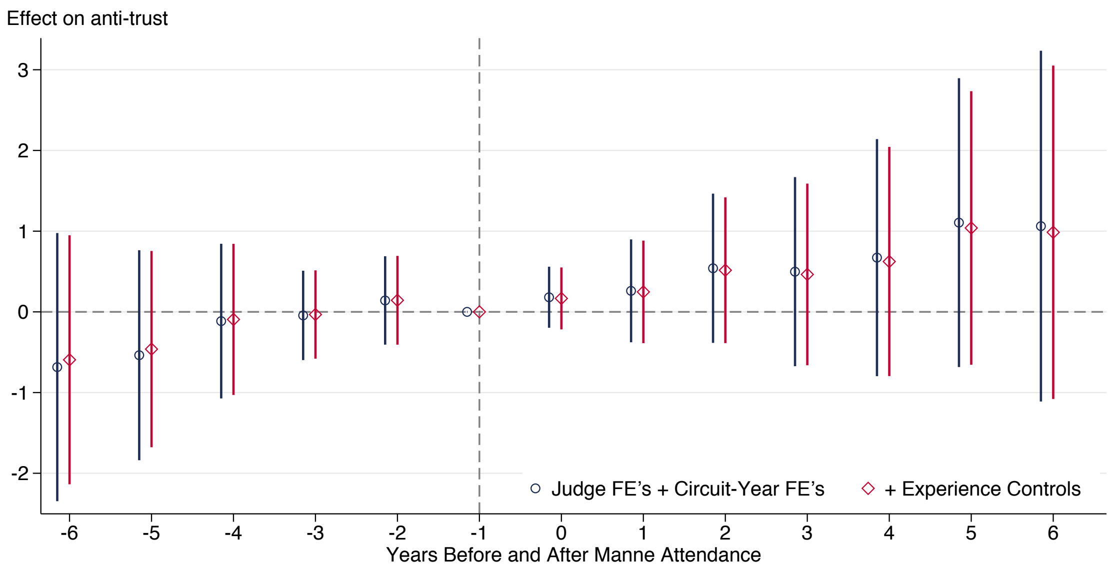
G.2 Results

The event study estimates for antitrust are reported in Appendix Figure A.33. As mentioned in the text, we could not identify all the lags and leads with the inclusion of elastic net controls interacted with year. So that specification is excluded. The specification with missing dummies in the years around attendance, interacted with year (Panel B), shows a positive and significant effect on antitrust conservatism, relative to trend.

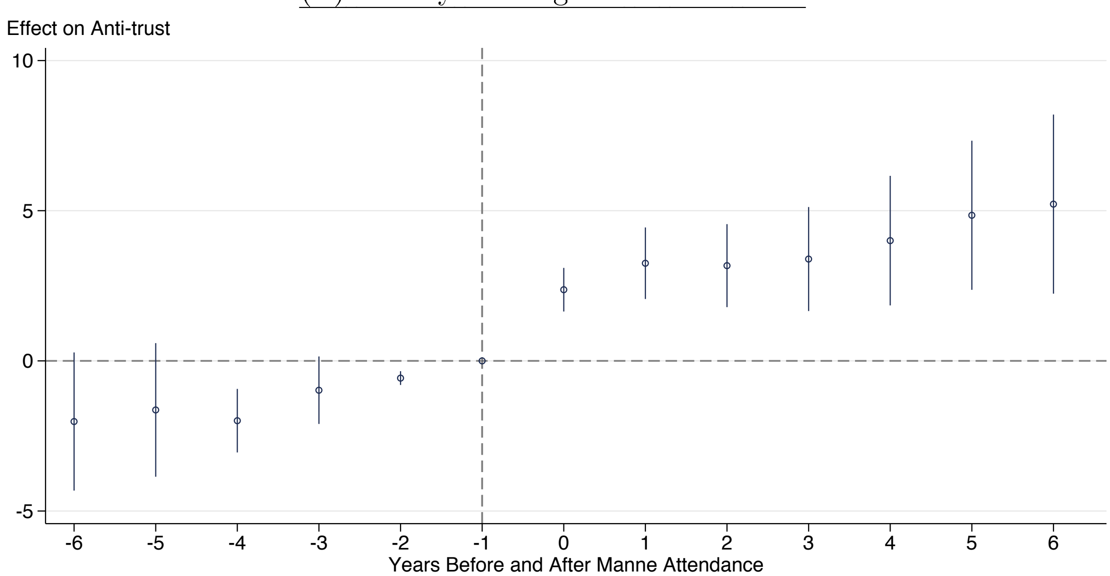
The set of regression results are reported in Table A.16. In the short run (Columns 1-11), there are mixed negative and positive coefficients, reflecting the small sample of cases. We get much more stable estimates in the long run (Columns 12-22), and all of the coefficients are positive. The coefficients are statistically significant in a few of the specifications, but mostly noisy and imprecise.

Figure A.33: Effect of Manne Program on Antitrust Decisions

(A) Baseline Specifications



(B) Nearby Missingness × Year FE's



Notes. Event study effects on voting against antitrust claimants, relative to year before attendance at Manne economics training. In Panel A, the baseline specification (blue circles) includes judge and circuit-year fixed effects. Additional specifications add experience controls (red diamonds). Panel B 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.

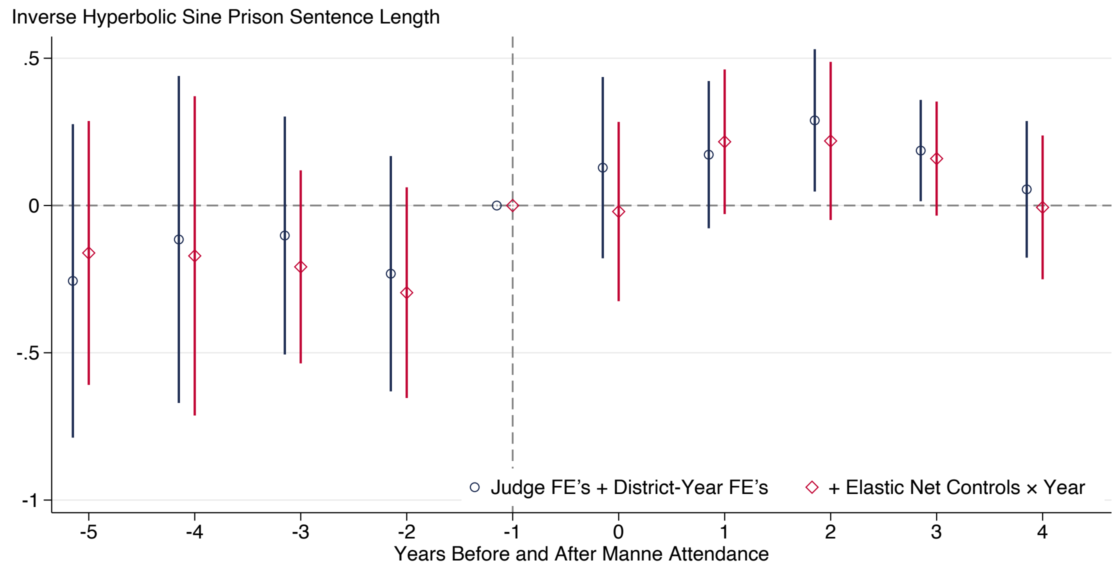
Table A.16: Regression Results: Conservative Voting in Antitrust Cases

	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.34: Effect of Manne Program on IHS Prison Sentence Length

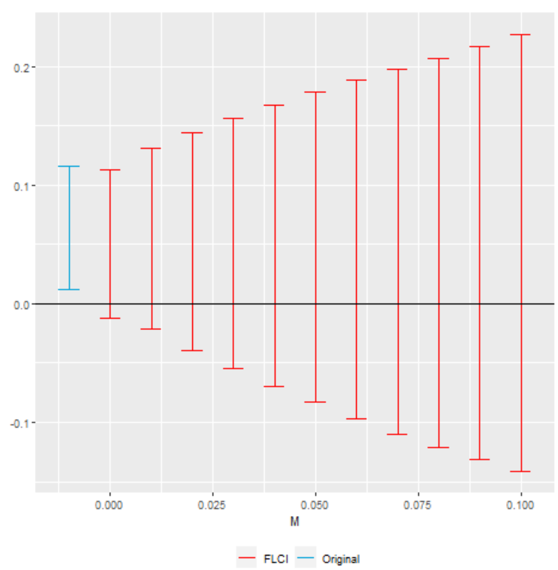


Notes. Event study effect of Manne attendance on criminal sentencing outcomes in district courts, 1992-2003.Outcome is inverse hyperbolic sine of prison sentence in days (plus one, to allow for zeros). Regressions include judge and district-year fixed effects (blue circles), plus elastic-net-selected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

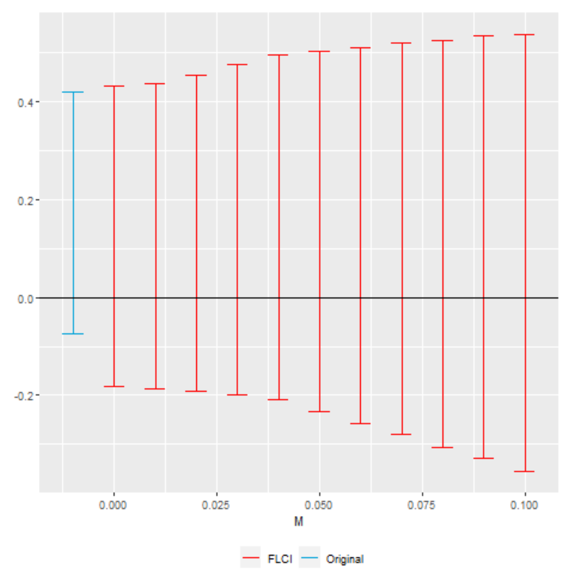
Figure A.34 shows the event study for the inverse hyperbolic sine (IHS) of sentence length. Given sentencing guidelines, it is not surprising that there is mostly a non-significant effect on sentence length. Appendix Figure A.35 shows the Rambachan and Roth (2019) test for non-linear pre-trends for both criminal sentencing outcomes. The sentence length outcome (Panel B) is not significant when averaging across the event study coefficients. The any-prison effect (Panel A) is significant but not robust to the non-linear trends test.

Figure A.35: Criminal Sentencing: Pre-Trend Sensitivity Analysis

A. Any Prison



B. IHS Sentence Length



Notes. Sensitivity graphs for violation of the parallel trends assumption, applying the method from Rambachan and Roth (2019); see also Ang (2021). Outcome is Criminal Sentencing, as indicated. 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.17: 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
Circuit-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 embedding. 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. $+p < .1, *p < 0.05, **p < .01$.

The full set of regression results for criminal sentencing are reported in Appendix Tables A.17 (any prison) and A.18 (IHS sentence length). We report results with three samples. 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). This makes sense to do in the district courts because judges work independently,

Table A.18: Regression Results: Effect of Manne Program on IHS Sentence Length

	A. Short-Run Effects on Attenders							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Manne	0.240+	0.214	0.386**	0.217	0.277**	0.240+	0.24	0.24
	(0.137)	(0.145)	(0.140)	(0.141)	(0.097)	(0.137)	(0.149)	(0.156)
N (Sentences)	70528	70528	70528	70368	70528	70528	70528	70528

	B. Long-Run Effects on Attenders							
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Post Manne	0.198*	0.194*	0.168+	0.173*	0.118*	0.198*	0.198**	0.198*
	(0.089)	(0.091)	(0.091)	(0.086)	(0.054)	(0.089)	(0.074)	(0.094)
N (Sentences)	259600	259600	259600	259333	259600	259600	259600	259600

	C. Long-Run Effects, Including Never-Attenders							
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Post Manne	0.174*	0.152+	0.158+	0.170*	0.085*	0.174*	0.174**	0.174*
	(0.087)	(0.087)	(0.084)	(0.081)	(0.042)	(0.087)	(0.062)	(0.087)
N (Sentences)	1003989	1003989	1003989	1003425	1003989	1003989	1003989	1003989
Circuit-Year / Judge FE	X	X	X	X	X	X	X	X
Party $\times$ Year FE		X						
E-net $\times$ 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 – IHS sentence length. 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. $+p < .1, *p < 0.05, **p < .01$.

rather than in panels like the circuits, so we don't expect the same spillovers to never-attenders. For these same reasons (data availability and no peer effects), we also don't report the specifications limiting to the early period, or the specification adding controls for peer share.

The specifications we do report are summarized as follows, for each of the three samples. 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).

Starting with the results for the binary outcome of any prison given (Appendix Table A.17), we can see that 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. And unlike the circuits, there is no issue of peer spillovers to the comparison group of never-attenders.

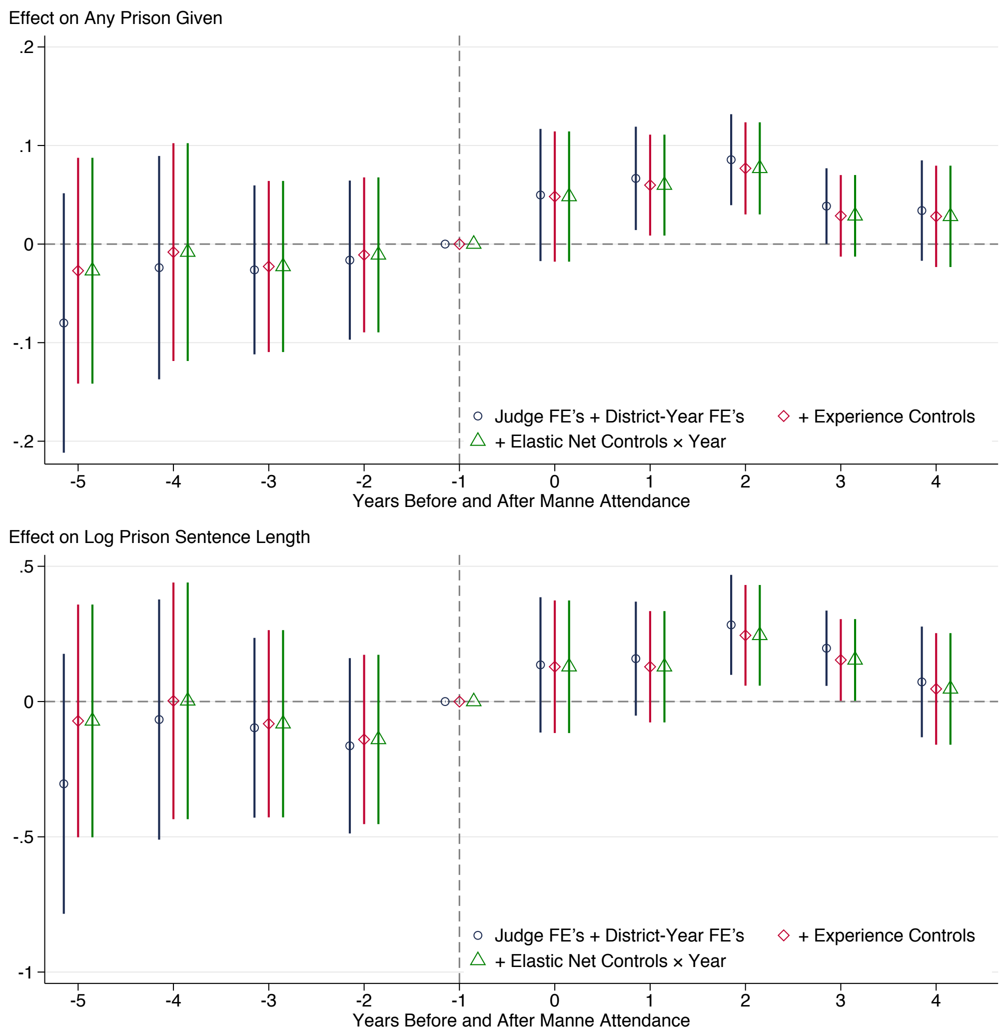
Appendix Table A.18 reports corresponding estimates for the sentence length outcome, specified as the inverse hyperbolic sine of the sentence in months, including zeroes. Here, we might not expect much of an effect given mandatory sentencing guidelines that constrain judge discretion. But still, we see some evidence of a positive effect. In the short run (Columns 1-8), the coefficient is positive and stable, but not significant with party-year controls (Column 2), charge fixed effects (Column 4) or alternative construction of standard errors (Columns 7, 8). In the long run (Columns 9-16) and in the all-judges sample (Columns 17-24), however, all estimates are positive and statistically significant, ranging from about 15 to 20 percent. Again, these results support the view that Manne attendance increased harshness of criminal sentencing.

Table A.19: Effect of Manne Judges on Criminal Sentencing, Pre- and Post-*Booker*

	Any Prison	*IHS Sentence Length*				
	(1)	(2)	(3)	(4)	(5)	(6)
Booker (≥2005)	0.0350**	0.0681	-0.0815	0.105*	0.126**	0.187*
	(0.00504)	(0.0601)	(0.0528)	(0.0485)	(0.0366)	(0.0760)
Econ Training	-0.00141	-0.0319	-0.0287		0.0306	-0.0609
	(0.00725)	(0.0417)	(0.0388)		(0.0339)	(0.0556)
Econ Training *	0.00887	0.154*	0.129*	0.117*	-0.0470	0.196**
Booker (≥2005)	(0.00621)	(0.0599)	(0.0570)	(0.0500)	(0.0447)	(0.0733)
N	882543	882543	781362	882940	307660	574857
adj. R-sq	0.033	0.054	0.113	0.063	0.127	0.050
Sample	All	All	Sentence > 0	All	Drug	Non-Drug
Court FE	X	X	X	X	X	X
Calendar FE	X	X	X	X	X	X
Judge FE				X		

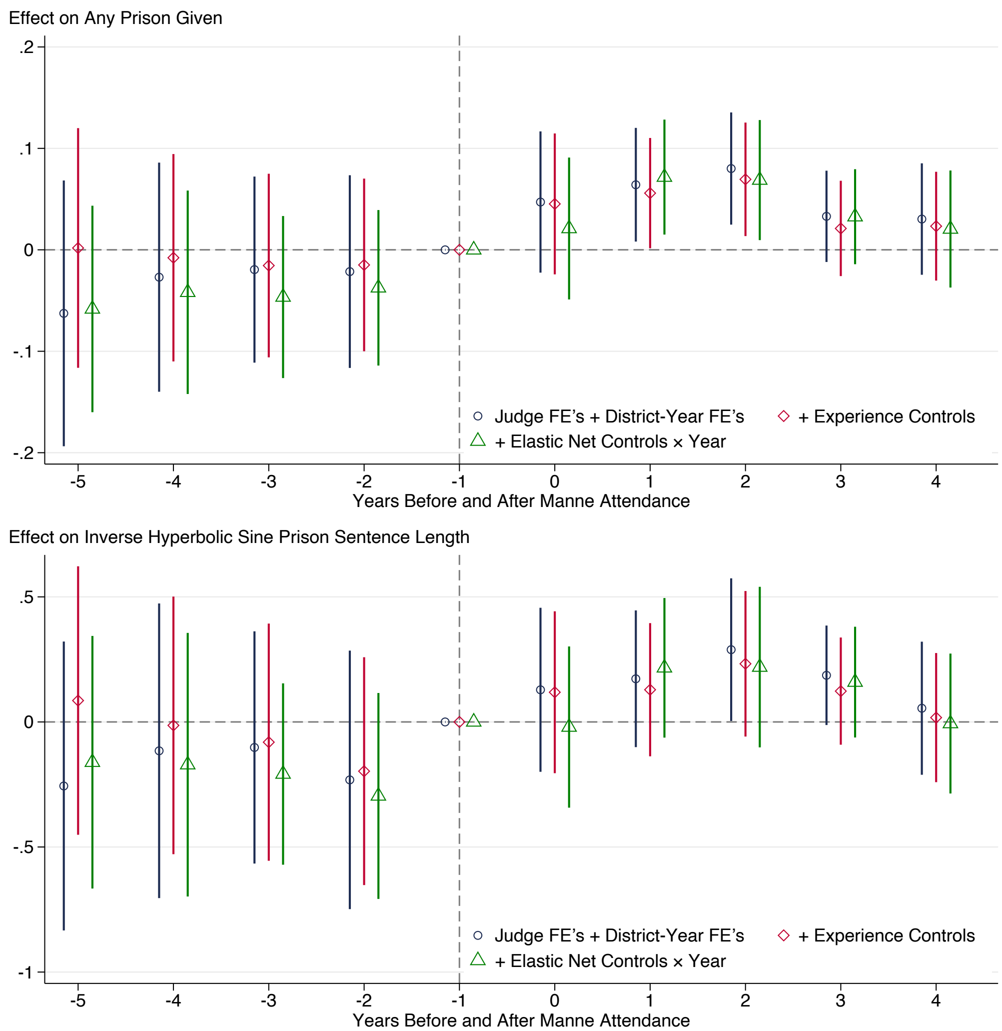
Notes. Estimates for impact of *Booker,* Manne economics training, and their interaction on sentencing outcomes. Calendar FE includes day-of-week and year-month. Standard errors clustered by district in parentheses. $+p < .1, *p < 0.05, **p < .01$. Results are similar with fully interacted Republican-appointee dummies.

Figure A.36: District Event Studies with Crime Charge Fixed Effects



Notes. Main event study results for the district courts (from Figure 5) but including fixed effects for crime type (345 categories). Outcomes are Any Prison Given and Log Sentence Length. For other details see notes in the associated main-text exhibit.

Figure A.37: District Event Studies, Two-Way Clustering



Notes. Main event study results for the district courts (from Figure 5) with two-way clustering of standard errors by judge and court-year. Outcomes are Any Prison Given and Log Sentence Length. For other details see notes in the associated main-text exhibit.

Table A.20: Effect of Manne Judges on Criminal Sentencing, by Crime Type

	IHS Sentence Length				
	(1)	(2)	(3)	(4)	(5)
Econ Training	-0.0752	-0.0114	-0.0339	-0.0335	-0.0424
	(0.0860)	(0.0378)	(0.0629)	(0.0654)	(0.0586)
Booker (≥2005)	0.240*	0.338**	-0.0477	0.0486	-0.0741
	(0.102)	(0.0324)	(0.0862)	(0.0880)	(0.0816)
Econ Training *	0.245*	0.0443	0.219*	0.183*	0.198*
Booker (≥2005)	(0.101)	(0.0410)	(0.0907)	(0.0913)	(0.0870)
N	574857	654533	745856	794685	760219
adj. R-sq	0.042	0.045	0.044	0.039	0.052
Drop Crime	Drug	Immigration	Fraud	Weapon	Other
Courthouse FE	X	X	X	X	X
Courthouse Calendar FE	X	X	X	X	X

Notes. Estimates for impact of *Booker*, Manne economics training, and their interaction on sentencing outcomes. Each column drops a crime type, indicated by Drop Crime row. Standard errors clustered by district in parentheses. $+p < .1, *p < 0.05, **p < .01$.

I Additional Supporting Results

Table A.21: Effect of Manne Program on Additional Conservatism Measures

	Cites Reagan/Bush Nominee			*Conservative Dissent*		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Manne	-0.00177	-0.00164	0.000311	0.0953**	0.0956*	0.0855*
	(0.00571)	(0.00593)	(0.00606)	(0.0362)	(0.0368)	(0.0368)
N (Cases)	58474	58474	58474	1605	1605	1605
Circuit-Year FE	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X
Experience Vars		X	X		X	X
Party × Year FE			X			X
E-net × Year FE			X			X

Notes. Estimated effects of Manne training on citations to circuit judges nominated by Reagan and Bush (Columns 1-3) and the "conservative dissent" measure: dissenting against a Democrat-authored ruling. For the latter, sample is limited to dissenting votes. Sample includes event study window. Standard errors clustered at the judge level in parentheses. $+p < .1, *p < 0.05, **p < .01$. Observations are weighted to treat judge-years equally.

We produce some additional measures of conservative legal reasoning. In Appendix Table A.21, 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.

Besides citations, another relevant choice made by circuit judges is when to dissent. We produced a measure of "conservative dissent" as the rate at which judges dissent against majority opinions written by Democrats. We show in Columns 4-6 that there is a positive effect on this measure.

Table A.22 shows the effect of economics training on how often a judge is cited by future circuit cases. We show results for all citations, and also limit based on other circuits (where a citation would be persuasive precedent). There is no effect.

Table A.22: Effect of Manne Program on Forward Citations to Opinions

	Total Citations			*Outside Citations*		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Manne	-0.0170	-0.0104	0.00157	-0.0220	-0.0188	-0.00822
	(0.0489)	(0.0499)	(0.0504)	(0.0467)	(0.0476)	(0.0484)
N (Opinions)	64153	64153	64153	64153	64153	64153
Event Study	X	X	X	X	X	X
Circuit-Year FE	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X
Experience Vars		X	X		X	X
Party $\times$ Year FE			X			X
E-net $\times$ Year FE			X			X

Notes. Estimated effects of Manne training on citations to a judges opinions from circuit court cases. Total means all circuits; Outside means other circuits. Standard errors clustered at the judge level in parentheses. $+p < .1, *p < 0.05, **p < .01$. Observations are weighted to treat judge-years equally.

Table A.23: Effect of Manne Program on Promotion of District Judges to Circuit

	Promoted to Circuit				
	(1)	(2)	(3)	(4)	(5)
Manne Judge	0.0838**	0.0588*	0.0482+	0.0901*	0.0272
	(0.0262)	(0.0284)	(0.0278)	(0.0408)	(0.0411)
N (Judges)	1426	1419	1419	774	637
Sample	All	All	All	Republican	Democrat
Court FE	X	X	X	X	X
Start-Year FE		X	X	X	X
Bio Covariates			X		

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

Table A.23 shows 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). Democrat presidents do not selectively promote Manne judges.