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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, issue more conservative decisions in economics-related cases, rule against regulatory agencies more often, favor more lax enforcement in antitrust cases, and impose more/longer criminal sentences. The law-and- economics movement had policy consequences via its influence on U.S. federal judges.

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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 policymakers' policy decisions. 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 judiciary. This approach emphasizes cost-benefit criteria, freedom of contract, criminal deterrence, 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, 1987a; Ellickson, 2000; Posner, 2008a).

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, by the early 1990s more than half the working federal judges had attended this intensive two-week training camp. The Manne program was controversial even in its early years, not least because it was funded by prominent business and conservative foundations (Butler, 1999). We estimate the effect of attendance on judge decision making, exploiting both quasi-random assignment of cases to judges and the staggered attendance of judges in this program over its two decades of operation.

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 2007a). 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

[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.

almost all circuit court decisions are final.

Our dataset includes the list of judges in each cohort of the Manne program, 1976-1998, with about twenty judges in each cohort. For each circuit judge, we have the portfolio of published decisions. For each district judge, we have detailed information on his/her criminal sentencing decisions. For each judge on circuit courts and district courts, we have detailed biographical information. The case data include rich metadata including the associated legal topic. In the circuit courts, we have the digitized written opinions for use in text analysis.

We estimate the impact on decisions and language in a differences-in-differences framework. Judge fixed effects control for many time-invariant characteristics of judges that may influence case outcomes, such as appointing party, education, and previous career experience. We use circuit-by-year fixed effects to control for court- and case-level factors and ensure that treated judges are not selecting into particular types of cases. Manne program records indicate that recruitment was oversubscribed and on a first-come-first-serve basis, minimizing opportunities for selection in response to short-run changes in judge beliefs/attitudes. Consistent with exogenous timing, we show that a wide set of judge biographical variables (e.g. party of nominating president) are not predictive of the timing of attendance, even as they predict attendance. Moreover, 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 do predict the timing of attendance.

To measure the influence of law and economics on legal thinking, we first look at how it shaped legal writing of judicial opinions. Besides showing how judges reason to a decision, the published writings are independently important because they can be cited and quoted in future legal decisions. Specifically, we compute a word-embedding-based measure, borrowed from machine translation (Mikolov et al., 2013; Arora et al., 2016), between written opinions and a lexicon of law-and-economics terminology. By using word embeddings rather than word counts, we recover the subtler and more conceptual legal associations with economics. We find that judges significantly increase their use of economics language after attending the Manne program, relative to judges who attend later.

Next, we look at how the Manne program influenced decisions in policy-relevant appellate cases. Using a sample of cases hand-coded for ideological direction (see, e.g., Haire et al. 2003) we find that, post Manne attendance, judges vote for conservative verdicts in economics-relevant cases (but not in non-economics cases). Further, using a set of machine-coded decisions, 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. Using newly collected data on antitrust decisions, we also find some evidence that post-Manne judges are more likely to vote against antitrust protections, although this result is more sensitive to specification than the others.

Moving to the district courts, we analyze the impact on criminal sentencing (which is handled by district judges rather than appeals judges). We find that Manne attendance is associated with harsher criminal penalties – whether a defendant is given any prison and the length of prison sentences imposed – consistent with an emphasis on severe punishment for guilty offenders favored by deterrence theory. We show that the difference in sentencing harshness between Manne and non-Manne judges is highest after the 2005 *Booker* decision gave more discretion to judges in sentencing. 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.

Taken together, these results are consistent with a large and significant impact of law and economics – as delivered by the Manne program – on the federal judiciary. In short, the injection of economic ideas into legal thinking via the Manne program had consequences for judicial policy making. In Section 6.1, we contextualize this impact by comparing it to other studies on partisan influence. Our estimated persuasion rates are slightly larger than the partisan media interventions that have been studied before and are closest to the change in Democratic governor vote share induced by a 10-week subscription to the Washington Post (Gerber et al. 2009).

How did the Manne program change judge decisions? Section 6.2 provides a discussion and interpretation of the evidence. On the one hand, a course in economics provides a set of tools and principles for understanding the welfare impacts of decisions (Posner, 2014). On the other hand, the program had a recognized pro-business conservative slant and could have worked via ideological persuasion (DellaVigna and Gentzkow, 2010). We don't have sufficient qualitative or quantitative evidence to rule out either mechanism, so perhaps both are at work.

Either way, 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, 2008b). If judges are formalists following the law as written, the program would have no effect. Similarly, if judges are politicians towing the party line, the program would still have no effect. Neither of these prototypical models can explain the evidence. Instead, our results show a shift in the judge-specific component of decision-making, holding law and political affiliation constant. On this particular point, the best previous evidence was Bonica et al. (2019), who show in the context of the U.S. Supreme Court that changes in the ideology of selected clerks sometimes shift a justice's votes. Beyond that, the literature has largely attended to legal rules determining outcomes (Kornhauser, 1992; Gennaioli and Shleifer, 2007b), 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 et al., 2013; Ash et al., 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 et al., 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.[2] Our evidence suggests that there is room for policy analysis to influence judicial decision-making.

A more targeted literature has focused on economics education, and how that influences normative beliefs and social preferences. 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).[3] Economics professors are less ideologically liberal and

[2]Similarly, Hjort et al. (2019) 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 et al. (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.

[3]An influential working paper by Fisman et al. (2009) found that law students exposed to an economics-trained professor behaved less pro-socially in lab experiments 1 and 3 years later. Bleemer and Mehta (2020) find using a regression discontinuity that economics majors tend to earn higher wages by working in higher-paying industries. Paredes et al. (2020) find using Chilean data that majoring in economics is correlated with sexism expressed in survey measures. See also Ifcher and Zarghamee (2018).

less likely to be registered Democrats (or contribute to Democratic candidates) than professors in the other social sciences (Jelveh et al. 2018). Our paper builds on these papers, as well as others that are more qualitative (e.g. Hirschman and Berman, 2014), by looking at the effect on established professionals (judges), and by looking at high-stakes decisions in real-world courtrooms. Our findings are consistent with an intensive, immersive course in economics changing a judge's understanding of the law and legal rules.

Unlike the previously examples of ideas influencing attitudes and policies, judges write extensive judicial opinions documenting their reasoning (Posner, 1995). We examine that reasoning directly using text analysis. In this respect, our paper contributes methodologically to the literature on text as data (Gentzkow et al. 2017). A complementary analysis by Jelveh et al. (2018) uses text to classify economics articles as conservative or liberal, finding (for example) that *Journal of Law and Economics* consistently ranks as right-wing. Related work on polarization of congressional speech includes Jensen et al. (2012), Ash et al. (2017), and Gentzkow et al. (2019). These papers use supervised learning to measure partisanship, while we use text embeddings to measure the influence of economics reasoning. An advantage of the judicial context is that judges have limited control over their caseload, which holds the topic of discussion constant (unlike Congress, where speakers can choose what they talk about).[4]

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.

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, 1987b). First, we provide

[4]Papers that use text methods to analyze (non-economics) dimensions of judicial reasoning include Carlson et al. (2015), Ash and Chen (2019), and Ash et al. (2021). Most recently, Cao (2021) provides a cross-sectional analysis comparing a judge's use of economics terms with voting on antitrust cases. Bertrand et al. (2021) analyze corporate influence on federal rulemaking using a document similarity approach.

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, 1972b). 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, 1968a), 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, 1987a). 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.

What is the heart of law and economics? This intellectual community and movement has advanced the application of economic principles to jurisprudence and prioritized 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).[5] 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, 1968b). 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).[6] On the other hand, many economists associated with the Chicago School also advocated for legalizing victimless crimes, such as recreational drug use and prostitution (e.g. Thornton, 2016).

[5]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).

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

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.[7] The institute was the the flagship program of the LEC. Substantial funding came from donations by pro-business foundations and corporations.[8]

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.[9] This means, importantly, that there was no selection of particular judges for attendance on the side of the program organizers.

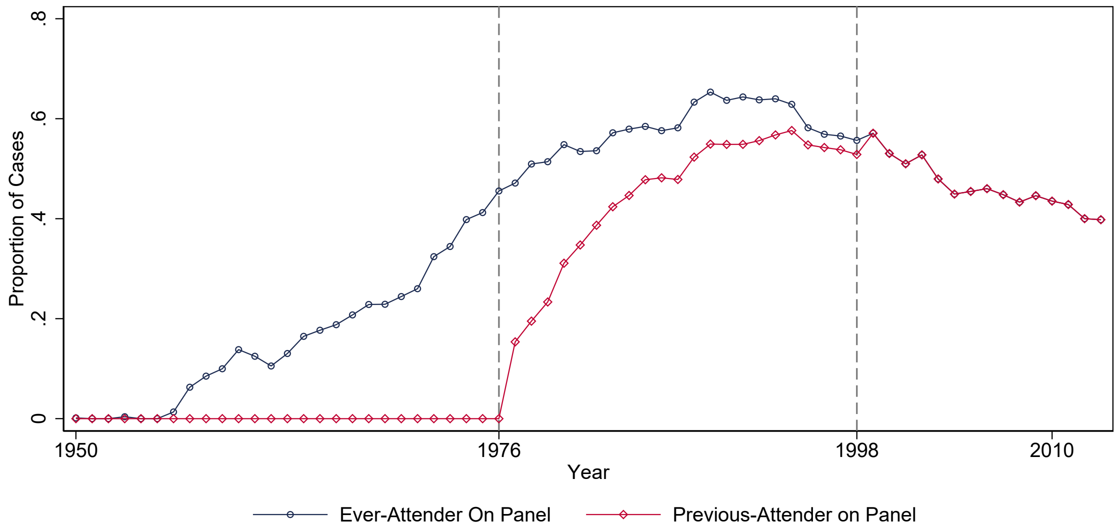
On the judges' side, the program was popular among and heavily attended by both Republican and Democratic appointees. Starting in the second class (1977) and into the late 1980s, the course was oversubscribed due to high demand, and the first-come-first-serve policy was binding (Butler, 1999). The binding attendance cap would have worked against selection into timing of attendance due to short-run shifts in judge preferences about economics. By 1990, forty percent of federal judges

[7] See Manne (1993) for a history of the LEC, including a discussion of the economics course for judges.

[8] "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.

[9] 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.

had attended this program.[10] 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 extensive 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 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 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.

[10] 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.”

The material on criminal law was based on the Becker model and deterrence theory. There was no material on behavioral economics nor on more sophisticated law-and-economics theories, such as over-deterrence, according to the syllabi listed in Butler (1999). 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.[11] 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.[12]

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*."

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, 2008b; 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

[11] 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."

[12] 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).

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, 2008b).[13] Moreover, judges are not just politicians (Choi et al., 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 leaves space for a training program to influence decision-making. Yet judicial professionalism places some standards for 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, 2008b). 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 et al., 2020).[14] Thus, we will measure the use of economic language using the opinion texts written by federal circuit judges.

Detecting the impact of economics ideas on the direction of rulings is more subtle. Even relatively simple applications of economics ideas will be sensitive to the existing legal rule and the facts of a case. To the extent that there are effects in a single direction, we might expect that to be stronger for economics-oriented cases (e.g.

[13] 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."

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

bankruptcy) than non-economics cases (e.g. reproductive rights). Thus, we will compare effects on economics and non-economics cases.

In terms of regulation, in particular, the results of an objective economic analysis would depend on context. If the status quo is over-regulation, for example, the post-Manne judges would become more conservative on regulatory issues, but an emphasis on deterrence might cause judges to be more punitive. Similarly, the effects on criminal law decisions are difficult to predict. One idea would be that judges would follow Becker (1968a) 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 from 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.

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 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 FOIA 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

[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 jursidiction 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.

[17]There are duplicates, so we present the analyses separately.

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 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 (Appendix Figure A.5).

To address this issue and measure the more implicit, subtle, contextual use of economics reasoning, we draw on word embeddings – a recently developed method in natural language often used for machine translation. Word embedding is a word

[18] See Appendix B for the enumerated list.

[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.

vectorization algorithm which learns dense numerical representations of words based on co-occurrence statistics in large corpora (Mikolov et al., 2013; Pennington et al., 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.

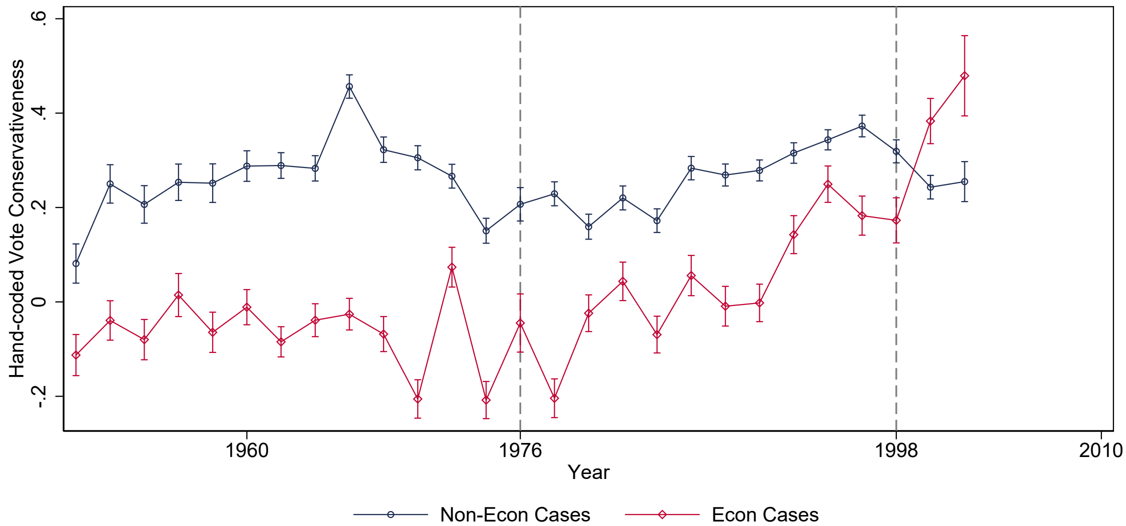
There are several word embedding algorithms to choose from, and a number of options for model training. Our implementation uses the algorithm from Mikolov et al. (2013), with the default settings from Rehurek et al. (2011). Previous work has shown that downstream measurements in social-science contexts are not that sensitive to these choices (Rodriguez and Spirling, 2021; Ash et al., 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.4 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.5 shows the distribution of the embedding-based measure and highlights that it is relatively normally distributed, contrasting with the sparsity of a count-based measure that matches the particular patterns from the lexicon.

For robustness, Appendix D.2 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

Conservative Judicial Decisions. Our first measure of conservative judicial opinion is a hand-coded measure of decision direction from the Songer-Auburn database

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.

(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.

Figure 2 shows the trend in conservatism over time. It has increased since the late 1970s, especially in economics cases (those on labor and regulation).

Labor and Environment Regulation. The Songer-Auburn measure provides an intuitive measure of conservatism. But it is hand-coded, which could lead to coding errors and subjective decisions, and is only available for 5% of cases. In addition, it could be that the use of economic reasoning in an opinion might be coded as conservative, notwithstanding the associated decision. Therefore we complement this measure with a machine-coded measure from the available metadata in each case.

In particular, we look at regulatory cases where the government is a party to the case. We look, in particular, for labor agencies and environmental agencies. The labor agencies include the National Labor Relations Board, Office of Worker's Compensation Programs, U.S. Department of Labor, Federal Labor Relations Authority, and Occupational Safety and Health Administration. The included environmental agency is the Environmental Protection Agency. We construct measures based on the voting of judges. We consider voting against the government in regulatory cases as in line with a deregulatory policy objective.

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 F.

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 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 qualitively the same with log of sentence length (plus one).

4 Econometrics

We use a differences-in-differences design to estimate the causal 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.

[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, and focus exclusively on prison sentences.

4.1 Identification

Our identification strategy relies on a parallel trends assumption. 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. 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. Based on the qualitative record, there is good reason to think that selection in timing by attending judges is minimal. As described above, attendance was first-come-first-serve, and the program was often oversubscribed. Up until the late 1980s (almost all of our circuit court judges), applicants were bumped to the next year's class. Hence, opportunities were reduced for selection of specific types of judges to specific episodes of the course. In these initial heyday years of the program, the control group (at least in the short-term event study window) is largely other applicant judges who were late and had to wait longer to attend.

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 et al., 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]

Besides endogenous timing of attendance, we are also concerned about endogenous selection of judges to cases. Fortunately, in our setting there is quasi-random assignment of cases.[23] In Circuit Courts, almost all cases are randomly assigned to a panel of three judges.[24] 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.[25]

[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). Further, we perform a more standard double-lasso approach by constructing the full matrix of year-covariate interactions and then running a set of lasso regressions with this matrix as the feature set. For these regressions, we make things computationally feasible by residualizing all of these year-demographic interactions, the treatment variable, and the outcome variables on the judge fixed effects and circuit-year fixed effects, before running lasso. First, we use the post-Manne treatment indicator as the label to be predicted. All of the lasso-selected variables are kept. Second, we run separate lasso regressions with these interaction features as inputs and the decision measures as outcomes. For each outcome, we add the additional covariates selected from the outcome lassos. We then run separate regressions with these double-lasso controls, and the results, as reported in Appendix Figure A.22, are qualitatively similar.

[23] This randomness has been used in a growing set of economics papers (Kling 2006; Maestas et al. 2013; Belloni et al. 2012; Dahl et al. 2014; Mueller-Smith 2015).

[24] The process 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.

[25] See also Chen and Sethi (2011) and Boyd et al. (2010). Previous work has examined whether

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.14).

In our context, an identification concern is whether Manne judges are systematically more or less likely to author or sit on the relevant types of cases. For the Circuits, Appendix Figure A.3 shows that Manne judges are not more likely to sit on cases published on economics topics. In addition, Manne judges 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 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

$$Y_{ijct} = \alpha_j + \alpha_{ct} + \gamma Z_{jt}^{post} + E_{jt}'\phi + \lambda_t X_j'\beta + \epsilon_{ijct} \tag{1}$$

where α_j is a judge fixed effect and α_{ct} is a court-year fixed effect. E_{jt} includes a quadratic polynomial in judge experience (years on the court), to address the issue that judges of different cohorts might have different policy views and be more/less likely to attend the Manne program.[26] $\lambda_t X_j$ includes judge covariates, selected by elastic net as predictive of the timing of Manne attendance, fully interacted with year fixed effects. Z_{jt}^{post} is an indicator variable for the years after judge j attended the Manne program. The error term is ϵ_{ijct}.

For the event studies, we report the coefficients and confidence intervals produced

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.

[26]Note that the experience trend is linear within judge and not identified in our main specification that excludes never-attenders (Borusyak and Jaravel, 2017). The quadratic term is identified, however. Further, we obtain similar results using fixed effects for years of experience, rather than a quadratic, or interacting the experience trend with year fixed effects.

from estimating

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

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).[27] The year before attendance ($k = -1$) is the excluded year from which coefficients are computed. Only judges within this event study window are included in the estimating sample.

The court-year interacted fixed effects serve to hold constant any time-varying court-level factors. For the circuits, this is at the circuit court level, while at the district, it is at the courthouse (city) level. With the inclusion of judge fixed effects, we estimate within-judge effects due to Manne attendance. Identification is the standard parallel-trends assumption for fixed effects estimates. 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.

Standard errors are clustered by judge. In addition, we re-weight the cases to account for variation in the size of the caseload, such that judge-years, the level at which treatment is assigned, are weighted equally. In the district courts, we add additional exogenous covariates to improve efficiency. These include month fixed effects and day-of-the-week fixed effects.

4.3 Choice of Control Group

Our identification strategy is designed to leverage exogenous variation in short-run timing due to the first-come-first-serve rule. The early programs were over-subscribed, and the judges applying later were bumped to subsequent sessions. Conditional on applying, then, 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.[28]

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

[28] Correspondingly, when we limit the circuit court event-study analysis to this oversubscribed period, the results are nearly identical to the baseline results (Appendix Figure A.20).

Meanwhile, we have evidence that the never-attenders do not provide a good counterfactual. As mentioned, never-attenders are different on a number of observables, including political party, which are uncorrelated with year of attendance conditional on attending (Appendix Table A.4). Further, the never-attenders are on a positively selected trend in the use of economics language in their opinions (Appendix Figure A.7). Given these differences in characteristics and behavior, the never-attenders could be on a confounded trend and thus do not provide a clean set of controls. In particular, the never-attender judges may already be learning and internalizing economics from their Manne-trained colleagues or from other sources, and consequently may not perceive a need to take an economics course. A notable example of a judge in this category 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). Indeed, law and economics was not only transmitted to judges by the Manne program; it was promoted in the legal academy through teaching and scholarship,[29] by other organizations such as the Federalist Society and its predecessors (Riehl, 2007), as well as in the popular discourse (Posner, 1987a; Hovenkamp and Scott Morton, 2019). Law and economics had begun to permeate through the legal profession and law schools in the late 1970s, well beyond the Manne program. In particular, the exposure of law clerks to economics in their law school classes could have pushed economics language into the opinions of never-attenders.

Given these issues, in our preferred specifications 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, 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). However, the standard approaches for addressing these issues do not map directly into our setting because our dataset is at the case

[29]For example, the first edition of the monograph *Economic Analysis of Law*, Posner (1972a), 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."

level with circuit-year fixed effects for block randomization, rather than a standard panel dataset at the judge-year level. Further, the dataset is imbalanced for many of our outcomes, with judges entering and leaving over time as well as not ruling on particular types of cases in every year. Appendix C.3 provides more discussion of this problem and presents diagnostics from De Chaisemartin and d'Haultfoeuille (2020) and Jakiela (2021) to show that negative weighting is only occurring for a small part of our sample, and further it does not appear that effect heterogeneity is a major concern (Appendix Table A.7). This combination of limited negative weighting and limited heterogeneity gives us confidence that our design is not vulnerable to misspecification of the control groups, despite our lack of a clean set of never-treated judges.

5 Results

This section reports the estimated effects of 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. Finally we look at results for criminal sentencing. Supporting material and results are reported in Appendices D (writing style), F (antitrust), and G (additional results and robustness checks).

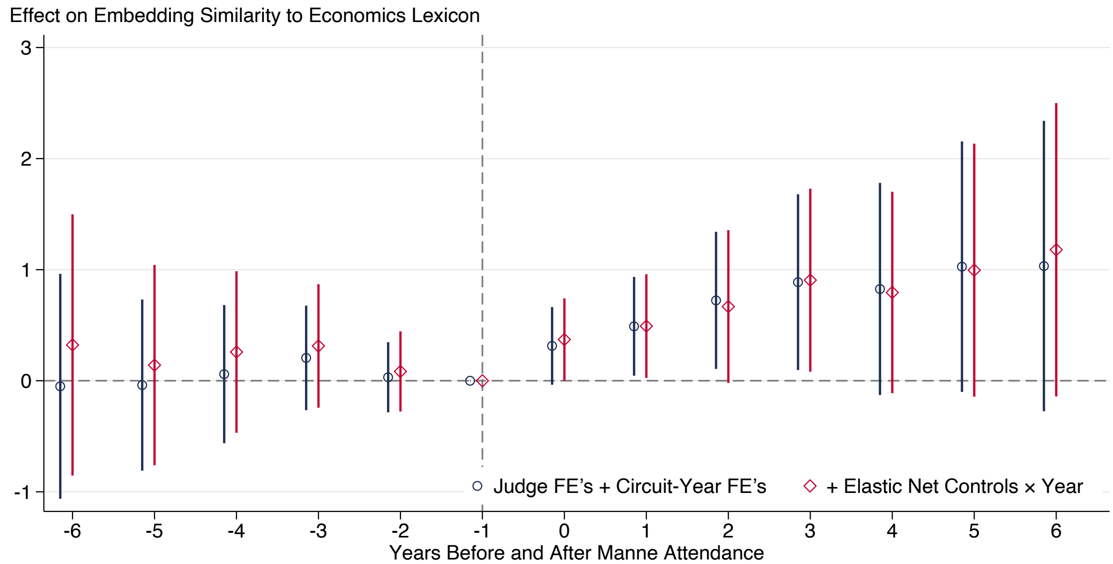
5.1 Effect of Economics Training on Judicial Opinion Language

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 for the economics embedding similarity. 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 estimates give the statistical difference from the left-out time period (the year before Manne attendance).

The event study graphs report statistics from two specifications. First, the left specification (blue circles) reports the baseline with judge fixed effects and circuit-

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.

year fixed effects. Second, the right specification (red diamonds) reports the baseline with the addition of elastic-net-selected controls (predicting time of attendance), interacted with year fixed effects.

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 three specifications. The effect is persistently positive, and significant for three years after the program. Meanwhile, there are not significant effects in the pre-trend period.

Table 1 report the effects of Manne attendance using differences-in-differences regressions. Precisely, we estimate $\hat{\gamma}$ from Equation (1) with the text measure as the outcome. As before, standard errors are clustered by judge and we weight the observations to account for different caseload sizes.

The first way that we vary the specification is by changing the sample of judges. In Columns 1-3, we limit to the event study sample (only Manne attendees, and only six years before and after attendance). Column 4 includes Manne attendees but for all years of their career (between 1970 and 2005), so it measures more long-term treatment effects. Column 5 includes all judges, including never-attenders, so

Table 1: Effect of Manne Program on Economics Language

	Embedding Similarity to Economics Lexicon				
	(1)	(2)	(3)	(4)	(5)
Post Manne	0.0104**	0.0107**	0.0115*	0.00370	-0.000758
	(0.00382)	(0.00391)	(0.00562)	(0.00339)	(0.00186)
N (Opinions)	5290	5290	5290	10305	42975
adj. R-sq	0.329	0.329	0.361	0.271	0.202
Event Study	X	X	X		
Ever Attenders				X	
All Judges					X
Circuit-Year FE	X	X	X	X	X
Judge FE	X	X	X	X	X
Experience Vars		X	X	X	X
Party × Year FE			X	X	X
E-net × Year FE			X	X	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. Experience Vars includes quadratic in judge years on court. Party refers to party of judge nominating president. E-net refers to elastic-net selected controls for predicting timing of Manne attendance. Event Study includes cases with Manne judges, within six years before/after attendance. Ever Attenders includes cases of Manne judges for all years of their career. All Judges includes all cases. Sample is limited to case opinion authors. Standard errors clustered at the judge level in parentheses. $+p < .1, *p < 0.05, **p < .01$. Observations are weighted to treat judge-years equally.

that the comparison group includes an additional cross-sectional dimension where never-attenders enter the circuit-year fixed effects.

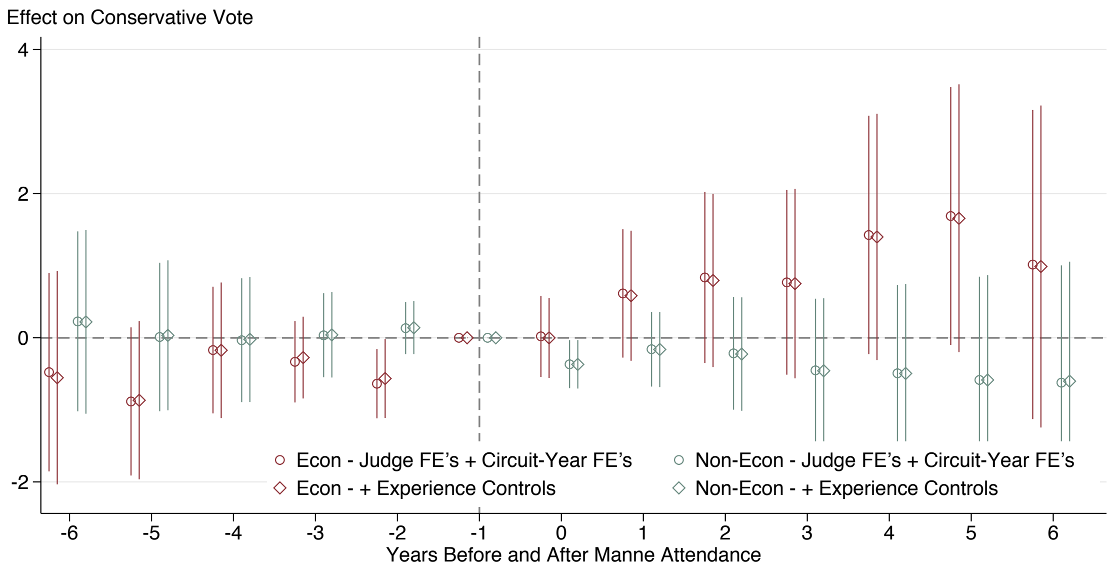
The second way we vary the specification is through fixed effects and controls. Column 1 has the baseline specification with circuit-year fixed effects and judge fixed effects. Column 2 adds experience controls. Columns 3-5 include all of these variables, plus judge party and the elastic-net-selected controls, interacted with year.

Consistent with the event study, there is a positive effect of Manne attendance on the use of economics language, and the effect is statistically significant. The difference is about one-third a standard deviation in the outcome. The effects are robust to including the experience controls (Column 2), as well as the party and elastic-net-selected controls (Column 3). When looking at the whole career for Manne judges (rather than just the six-year event-study window), however, the language effect shrinks significantly and becomes non-significant (Column 4). This estimate suggests that the effect on language is mostly in the short run and does not persist over the long run, consistent with a broad diffusion of economics language over time across all judges.

Meanwhile, the effect on language becomes zero when looking at the full sample including never-attenders (Column 5). Our interpretation of this last estimate is that, as discussed above in Section 4.3, the never-attenders are not a good counterfactual for the Manne treatment. With language especially, it is likely that economics ideas can diffuse at low cost to never-attenders. Besides other sources of economics knowledge in the academy, recently graduated clerks, and organizations like the Federalist Society, economics language could diffuse directly to non-Manne judges. Hence, we see a zero coefficient when including never-attenders in the control group.

Appendix Section D.2 reports analogous results for an alternative measure of economics language using a supervised learning approach, which predicts, based on the text of an opinion, how similar it is to an opinion written on an economics topic. The results are quite similar, with a statistically significant positive event-study effect from the Manne program. The DD effect for the alternative measure is significant in the ever-attender and all-judges samples (analogous to Columns 4 and 5 from Table 1). See Appendix Figure A.17 for a summary of estimates across all of these sample and specification choices, for both language outcomes.

Figure 4: 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.

5.2 Effect of Economics Training on Conservative Rulings

We now move to an analysis of the effects of economics training on decisions. In this section we consider the effect on conservative voting by circuit court judges. The measure of conservative voting is hand-coded by the Songer-Auburn Database, available for 5% of the cases. We look separately at effects on conservatism in economics-related cases and in non-economics-related cases.

Figure 4 shows the event study estimates for the effect of Manne attendance on conservative voting. The statistics in red come from estimates of Equation (2) for the subset of economics cases (labor and regulation) with hand-coded conservatism labels. In teal, statistics are from estimates subsetting on non-economics cases (everything else). We report two specifications, with the left item of the pair giving the baseline and the right item including judge experience controls.[30]

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

[30]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. DD estimates with elastic net controls are included in Table 2.

program attendance. The difference between the trends persists over five subsequent years. While conservatism is increasing for economics cases, it is slightly decreasing for non-economics cases. For economics cases, there is a sign of a pre-trend, however.[31]

Table 2 presents differences-in-differences regressions for the effect of economics training on conservative votes. Starting with the event study sample estimated for Equation (1), we see that there is no effect in non-econ cases (Column 1) but a positive and significant short-term effect for economics cases (Column 2). This effect does not persist, however, as reflected in Column 3 where we look at the whole career of Manne attenders.

Given the divergence in conservatism between economics and non-economics cases seen in the event study, we next focus on an interacted regression specification

$$Y_{ijct} = \alpha_j + \alpha_{ct} + \gamma_E E_{ijct} + \gamma_Z Z_{jt} + \gamma_{ZE} Z_{jt} E_{ijct} + \lambda_t X'_{jt}\beta + \epsilon_{ijct} \tag{3}$$

where $E_{ijct} = 1$ for economics-related cases and zero otherwise, and as above Z_{jt} is the post-Manne treatment indicator. The treatment effect of interest is $\hat{\gamma}_{ZE}$, which gives the change in the difference in conservatism between economics and non-economics cases after Manne attendance. If, as we saw in the event study, the Manne program especially increases conservatism in economics cases, we would see $\hat{\gamma}_{ZE} > 0$.

The estimates from Equation (3) are reported in Columns 4 through 7 of Table 2. Economics cases tend to have lower conservatism on average ($\hat{\gamma}_E < 0$). In the interaction specification, meanwhile, the effect on non-economics cases ($\hat{\gamma}_Z$) is a zero. The relative effect for economics cases ($\hat{\gamma}_{ZE}$), however, is positive and significant. This result holds for the baseline (circuit-year fixed effects and judge fixed effects, Column 4), adding experience and party-year controls (Column 5), and also adjusting for elastic net controls interacted with year (Column 6). An effect size of 0.2 is about one quarter of a standard deviation of the outcome and corresponds to judges deciding in the conservative direction about 20 percent more often relative to the mean liberal-conservative decision rate. The effect in the strict specification also holds in the full sample of all judges, including never-attenders (Column 7).[32] Overall, the

[31] To assess the importance of this pre-trend, we applied the statistical test from Rambachan and Roth (2019). According to that test, we cannot rule out that the effect on conservative economics voting is due to a confounding pre-trend.

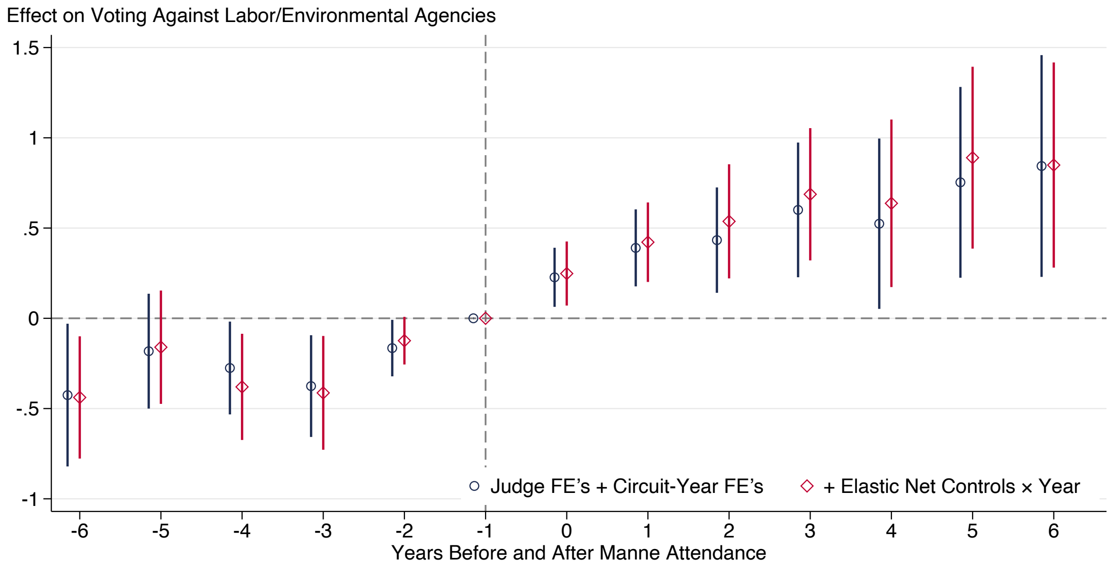
[32] Appendix Figure A.18 provides a summary of estimates across the full set of sample and specification choices.

Table 2: Effect of Manne Program on Conservative Voting

	Conservative Vote						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Econ Case				-0.324**	-0.325**	-0.319**	-0.205**
				(0.0864)	(0.0883)	(0.0924)	(0.0269)
Post-Manne	0.0522	0.304*	0.0517	0.0288	-0.0275	-0.0219	-0.0372
	(0.0728)	(0.130)	(0.0703)	(0.0937)	(0.0965)	(0.102)	(0.0660)
Econ Case ×				0.215*	0.219*	0.197+	0.122+
Post-Manne				(0.100)	(0.101)	(0.105)	(0.0667)
N (Votes)	2416	808	1589	6568	6568	6568	27799
adj. R-sq	0.367	0.323	0.392	0.351	0.359	0.375	0.232
Case Type	Non-Econ	Econ	Econ	All	All	All	All
Event Study	X	X					
Ever Attenders			X	X	X	X	
All Judges							X
Circuit-Year FE	X	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X	X
Experience Vars					X	X	X
Party × Year FE					X	X	X
E-net × Year FE						X	X

Notes. Effect of Manne economics training on conservative voting, handed-coded by Songer-Auburn (+1 is conservative, -1 is liberal, 0 is neither). Experience Vars includes quadratic in judge years on court. Party refers to party of judge nominating president. E-net refers to elastic-net selected controls for predicting timing of Manne attendance. Event Study includes cases with Manne judges, within six years before/after attendance. Ever Attenders includes cases of Manne judges for all years of their career. All Judges includes all cases. Standard errors clustered by judge. Observations are weighted to treat judge-years equally. $+p < .1, *p < 0.05, **p < .01$. Includes years 1970 through 2002 (when hand-coded conservatism ends).

Figure 5: Effect of Manne Program on Rulings 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.

Manne program pushes economics-related cases in a conservative decision direction, especially relative to non-economics-related cases.

5.3 Effect on Ruling against Regulatory Agencies

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

Figure 5 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) and with elastic net controls interacted with year (red diamonds). We see that Manne-trained judges exhibit a sharp and significant increase in propensity to vote against federal labor and environmental regulatory

Table 3: Effect of Manne Program on Rulings Against Labor/Environmental Agencies

	Voting Against Environmental or Labor Agency							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Manne	0.155*	0.144	0.163**	0.158**	0.162**	0.149**	0.164**	0.0959**
	(0.0667)	(0.0979)	(0.0467)	(0.0515)	(0.0481)	(0.0518)	(0.0555)	(0.0297)
N (Votes)	2653	2653	4244	4244	4244	4244	4244	19521
adj. R-sq.	0.447	0.467	0.403	0.403	0.414	0.438	0.444	0.323
Event Study	X	X						
Ever Attenders			X	X	X	X	X	
All Judges								X
Circuit-Year FE	X	X	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X	X	X
Experience Vars		X		X			X	X
Party × Year FE		X			X		X	X
E-net × Year FE		X				X	X	X

Notes. Effect of Manne economics training on voting against labor and environmental agencies. Experience Vars includes quadratic in judge years on court. Party refers to party of judge nominating president. E-net refers to elastic-net selected controls for predicting timing of Manne attendance. Event Study includes cases with Manne judges, within six years before/after attendance. Ever Attenders includes cases of Manne judges for all years of their career. All Judges includes all cases. Standard errors clustered by judge. Observations are weighted to treat judge-years equally. $+p < .1, *p < 0.05, **p < .01$.

agencies. The effect is quite robust to the inclusion of controls.

An important caveat is a significant negative estimate three years before attendance. But this is a decrease in the outcome from the previous two years, so it does not seem to be part of a longer-term pre-trend. Part of the pre-trend may be due to the imbalance in this sample, as few judges see regulatory cases every year. Consistent with this explanation, when we add indicators for missing observations in the pre-Manne years and 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 trends. The event studies with the missing indicator interactions, and with judge trends, are shown in Appendix Figure A.11.

The regression results for Equation (1) are reported in Table 3. In the event study sample, there is a positive effect on deregulatory voting (Column 1), although the estimate is not quite significant when including all controls (Column 2). In the ever-attender sample, the coefficient is similar but more precisely estimate (Column

3). It is robust to experience controls (Column 4), party-year controls (Column 5), elastic net controls (Column 6), and all of them together (Column 7). This most demanding specification holds even when including all never-attending judges in the sample, although the coefficient magnitude is smaller (Column 8). 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. Appendix Figure A.17 provides a summary of estimates across the full set of sample and specification choices.

5.4 Effect of Economics Training on Antitrust Decisions

We look at the effect on decisions in antitrust cases, where the outcome is defined as decisions tending to favor lax enforcement. In principle, economics training could have either a positive or negative effect on the strength of antitrust. On the one hand, exposure to economics ideas could make a judge perceive the benefits of competition and thus oppose mergers and price fixing. On the other hand, the Manne Program's approach to antitrust was rooted in the 1970s price theory revolution against structure-conduct-performance (Berman, 2017). This approach could make judges appreciate the efficiency gains and consumer welfare benefits realized by economies of scale, and believe that even concentrated markets would be disciplined by potential entrants. The Manne curriculum's preference for lax enforcement reflects both the intellectual currents in economics at the time and the interests of its funders.[33]

The construction of the antitrust outcome, which combines information from multiple sources, is described fully in Appendix F. Due to the relatively few antitrust cases in our appellate court sample (only 100 in the event study sample, for example), we cannot precisely estimate the same event study specifications as with the previous outcomes. In the baseline specification with judge fixed effects and circuit-year fixed effects (or adding experience controls), we estimate positive, but quite imprecise, coefficients in the years after Manne attendance (see Appendix Figure A.12). When adding the full set of elastic net controls interacted with year, however, we do not have enough observations to identify all of the leads and lags.

[33]Henry Manne himself 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).

Table 4: Effect of Manne Program on Antitrust Rulings

	Voting in Favor of Lax Enforcement				
	(1)	(2)	(3)	(4)	(5)
Post-Manne	0.129	0.314*	0.271+	0.0528	0.0762
	(0.0850)	(0.128)	(0.147)	(0.0543)	(0.0567)
N (Votes)	656	656	656	2486	2486
adj. R-sq.	0.437	0.321	0.255	0.476	0.474
Ever Attenders	X	X	X		
All Judges				X	X
Circuit-Year FE	X	X	X	X	X
Judge FE	X	X	X	X	X
Experience Vars			X		X
Party × Year FE			X		X
E-net × Year FE		X	X		X

Notes. Effect of Manne economics training on voting against claimant relief in antitrust cases. Experience Vars includes quadratic in judge years on court. Party refers to party of judge nominating president. E-net refers to elastic-net selected controls for predicting timing of Manne attendance. Event Study includes cases with Manne judges, within six years before/after attendance. Ever Attenders includes cases of Manne judges for all years of their career. All Judges includes all cases. Standard errors clustered by judge. Observations are weighted to treat judge-years equally. $+p < .1, *p < 0.05, **p < .01$.

We therefore focus on the differences-in-differences regressions (Table 4) because there are fewer treatment effects to estimate, and we can include observations outside the event study window. In the ever-attender sample, we do see positive and statistically significant effect when adjusting for elastic net controls (Column 2 and 3). The coefficient is positive, stable, yet imprecise in the all-judges sample (Columns 4 and 5).

With these results, we can rule out Manne judges becoming more pro-antitrust-enforcement, consistent with the curriculum's focus on the welfare benefits of permissive competition policy. Overall, however, the precision of these estimates are mixed and sensitive to sample and specification. Future work looking at the effect of the Manne program should examine the much larger set of antitrust cases ruled on by district judges, which are not yet systematically available.

5.5 Effect of Economics Training on Criminal Sentencing

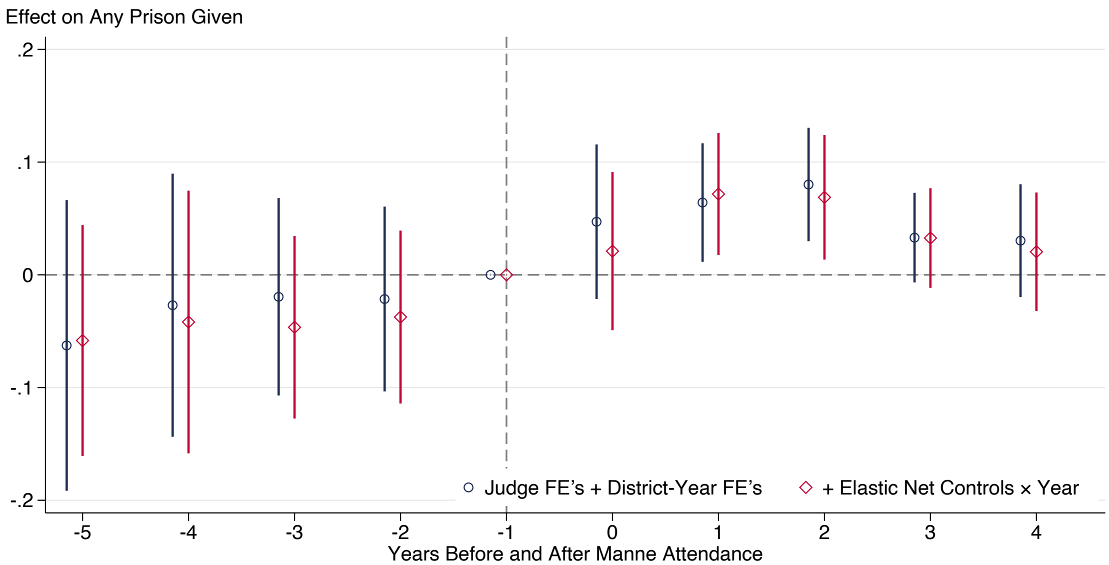
Now we move from appellate decisions in the circuit courts to criminal sentencing decisions in the district courts. This section reports results on how the Manne program influenced district court judges who attended, with the idea that the program's emphasis on deterrence reasoning might increase harshness in sentencing. In practice, we don't understand judges as accepting deterrence theory as a substitute for retribution theory or other pre-existing views on punishment. Instead, post-Manne judges would now have an additional factor in their decision – reducing future crime via a behavioral response to the increasing costs of crime – which would be additive with previous factors. First we look at the within-judge effect of program attendance. Second, we look at the effect of Manne program attendance, interacted with a reform (the *Booker* case) increasing sentencing discretion.

The event study estimates from Equation (2) for our criminal sentencing outcomes are reported in Figure 6. The data is at the case level and there are two outcomes: an indicator for any prison given (panel a) and the (inverse hyperbolic sine) sentence length (panel b). We report two specifications: the baseline (blue circles) includes judge and courthouse-year fixed effects, while the additional specification adds elastic net selected judge characteristics (predicting time of attendance) interacted with year fixed effects (red diamonds).

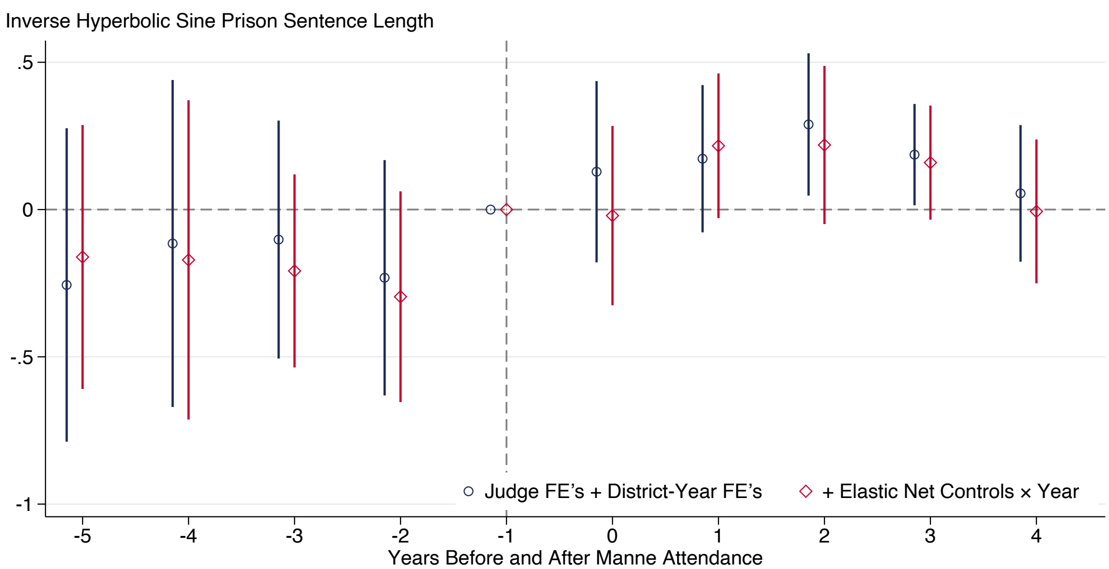
For the any-prison outcomes (panel a), we see a positive jump in the outcome in the year and after attendance in the Manne program. In the two years after

Figure 6: Effect of Manne Program on Criminal Sentencing Harshness

(a) Any Prison Given



(b) IHS Prison Sentence Length



Notes. Event study effect of Manne attendance on criminal sentencing outcomes in district courts, 1992-2003. Panel (a): Outcome is any prison given. Panel (b): 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.

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, there is no sign of a pre-trend. For IHS prison length (panel b), there is again a positive effect but it is not quite significant at the 5% level. The pre-trend coefficients are also quite noisy.

In Table 5 we look at the differences-in-differences estimates for how Manne attendance affected district judge sentencing. We find again evidence of harsher penalties on both measures, in the event study sample (Columns 1 and 8), in the ever-attender sample (Columns 2-5 and Columns 9-12), and in the full sample of judges (Columns 6-7 and 13-14). The effect for ever-attenders is robust across specifications including controls for experience and party interacted with year (Columns 3 and 10) and elastic net controls interacted with year (Columns 4 and 11). For the full-sample of judges, the results are significant with the inclusion of elastic net controls (Columns 6 and 13). In the fully saturated models with all controls together (Columns 5, 7, 12 and 14), the coefficients are similar in magnitude but not quite statistically significant. According to these estimates, after Manne attendance the chance of giving prison time increases by at least 4 percent. The average length of prison time increases by at least 13 percent. Appendix Figure A.19 shows the robustness of these results across the full set of specification and sampling choices.

If economics training leads judges to give longer criminal sentences, that effect may be larger when judges have more discretion over sentencing. A 2005 Supreme Court Case, *United States v. Booker,* loosened the U.S. Sentencing Guidelines, which beforehand were mandatory for district judges. After *Booker*, judges had more discretion and could deviate from the guidelines. Note that the event study dataset goes only up to 2003, so the less robust effects for sentence length (relative to any-prison, see Figure 6) could be explained in part by strict sentencing mandates.

The specification for analyzing discretion is slightly different than that used so far. We model the crime sentencing outcomes (any-prison, and IHS prison length) as

$$Y_{ijct} = \alpha_c + \gamma_\alpha \alpha_t + \gamma_Z Z_j \alpha_t + X'_{ijct}\beta + \epsilon_{ijct} \tag{4}$$

where α_c is a courthouse fixed effect and X_{ijct} includes case-level and judge-level covariates. At the case level, we add 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

Table 5: Effect of Manne Program on Criminal Sentencing Harshness

	Effect on Any Prison Given						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post Manne	0.0612*	0.0492*	0.0499*	0.0400*	0.0332	0.0399*	0.0244
	(0.0280)	(0.0198)	(0.0202)	(0.0199)	(0.0213)	(0.0187)	(0.0185)
N (Cases)	70784	260516	260516	260516	260516	1006820	1006820
adj. R-sq	0.135	0.122	0.123	0.124	0.125	0.095	0.096
	Effect on IHS Sentence Length						
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Post Manne	0.240+	0.198*	0.194*	0.168+	0.142	0.158+	0.0920
	(0.137)	(0.0893)	(0.0914)	(0.0905)	(0.0968)	(0.0837)	(0.0833)
N (Sentences)	70528	259600	259600	259600	259600	1003989	1003989
adj. R-sq	0.129	0.115	0.115	0.116	0.117	0.091	0.092
Event Study	X						
Ever Attenders		X	X	X	X		
All Judges						X	X
District-Year FE	X	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X	X
Experience Vars			X		X		X
Party × Year FE			X		X		X
E-net × Year FE				X	X	X	X

Notes. Diffs-in-diffs estimates for effect of Manne economics training on criminal sentencing outcomes (an indicator for any prison, and inverse hyperbolic sine of the sentence length in months). Experience Vars includes quadratic in judge years on court. Party refers to party of judge nominating president. E-net refers to elastic-net selected controls for predicting timing of Manne attendance. Event Study includes cases with Manne judges, within six years before/after attendance. Ever Attenders includes cases of Manne judges for all years of their career. All Judges includes all cases.Standard errors clustered by judge. $+p < .1, *p < 0.05, **p < .01$. Includes years 1992 through 2003.

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. Standard errors are clustered by courthouse.

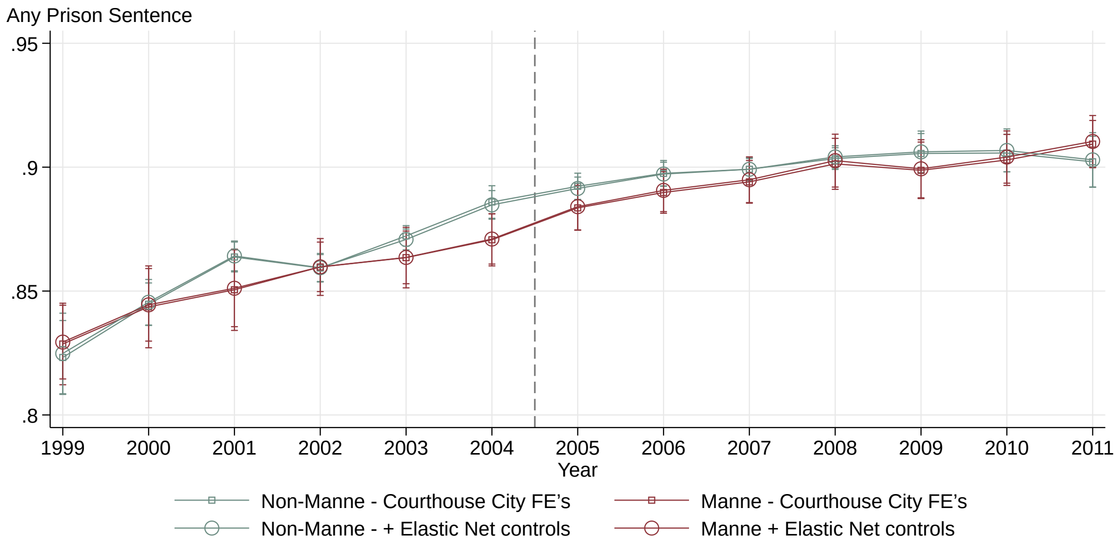
To visualize the estimates from Equation (4), we use marginal effect estimates to produce linear predictions for the outcomes by year and separately for Manne and non-Manne judges. Figure 7 reports these linear predictions for any-prison (panel a) and IHS sentence length (panel b). For each outcome, we have the predictions from specifications with and without elastic-net-selected controls. We can see that there is no difference between Manne and non-Manne judges, before or after the *Booker* decision, in terms of the probability that a defendant receives prison time (panel A). 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 Table 6, 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 7. The estimated effect in Column 2 translates to roughly 10 months in prison. Column 3 presents the intensive margin, conditioning on any sentence. The most restrictive specification (Column 4) includes judge fixed effects and shows a similar Manne effect on sentence lengths.

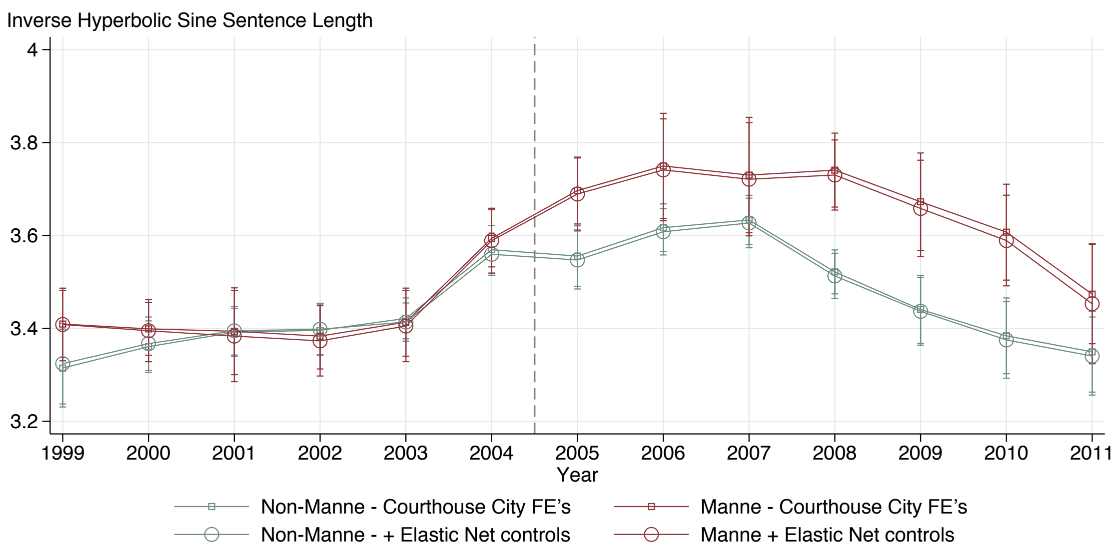
In Columns 5 and 6, we focus on one crime type that has particular relevance for economics training: drug crimes. Some of the Manne instructors, including most notably Milton Friedman, were known for advocating the legalization of drug use as it is a victimless crime.[34] In the first row of estimates, we see that the baseline post-*Booker* change for non-Manne judges is similar for drug (Column 5) and non-drug (Column 6) crimes. In the bottom row of estimates, we see that Manne judges were not significantly harsher on drug crimes (the coefficient is actually negative). The

[34] According to Butler (1999), "Friedman always started [his Manne lectures] on legalization of recreational drugs."

Figure 7: 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.

Table 6: 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.

differential effect of Manne under *Booker* discretion is focused on non-drug crimes – that effect is correspondingly larger than the average effect for all cases.

These effect sizes are slightly larger than previously estimated differentials for black defendants relative to comparable white defendants arrested for the same crimes.[35] Manne judges have contributed to disparities in sentencing when judges are given discretion. 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*.

5.6 Robustness and Additional Results

In this subsection we discuss some of the appendix results and unreported analysis. The statistics and some additional material are reported in Appendix G. Some of these results have already been mentioned above.

In the appendix we report regression estimates for some additional measures of ideology and conservatism. 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.10), 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.13).

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.[36] We find no effect on this outcome (Appendix Table A.10).

Next, we produce some additional measures of conservative legal reasoning. In Appendix Table A.11, we look at the citations choices of judges. In particular, we ask whether after Manne attendance judges tend to cite opinions written by circuit

[35] For example, Rehavi and Starr (2014) find that black defendants receive ten percent longer sentences than comparable white defendants for the same crimes.

[36] 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.

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 Appendix Table A.11 that there is a positive effect on this measure.

As discussed above in Section 4.3, our identification strategy in the Circuit Courts is motivated by short-run exogenous timing due to the Manne program being first-come-first-serve and applicants being bumped to later courses. Motivated by this point, Appendix Figure A.20 shows the main Circuit Court results limiting to the heyday period (before 1987) when the program was oversubscribed. The estimates are the same, showing that our main results are mostly driven by this heyday period (when most treated circuit judges attended).

From an econometric perspective, a potential threat to identification is selection of different types of cases to judges. As mentioned above, Levy and Chilton (2015) find that in a recent time period (2008-2013), the cases for four circuits (2nd, 8th, 9th, and D.C.) are not assigned randomly. Appendix Figure A.14 reports our main event study results for economics language, conservative vote, and ruling against regulatory agencies after dropping those courts, with little change in the results. The results are also robust to instead controlling for case topics (Appendix Figure A.15).

While the main event study plots have used a consistent specification throughout the paper, the differences-in-differences regression tables have used varying specifications and samples. We found that the differences-in-differences regression estimates are somewhat sensitive to specification and sampling choices. To summarize this sensitivity, the full set of sampling and specification choices are reported as coefficient plots in Appendix Figures A.17, A.18, and A.19.

As an alternative to the previous approach to selecting covariates, we perform a double-lasso approach by constructing the full matrix of year-covariate interactions and then running a set of lasso regressions with this matrix as the feature set. For these regressions, we make things computationally feasible by residualizing all of the year-demographic interactions, the treatment variable, and the outcome variables on the judge fixed effects and circuit-year fixed effects before running lasso. First, we use the post-Manne treatment indicator as the label to be predicted. All of the lasso-selected variables are kept. Second, we run separate lasso regressions with these interaction features as inputs and the conservatism measures as outcomes. For each outcome, we add the additional covariates selected from the outcome lassos. Appendix Table A.15 shows statistics on the number of variables selected (almost 900 covariates for the language measures). We then run separate regressions with these double-lasso controls. The estimates, while jumping around some, are the same as the main results (Appendix Figure A.22).

Next, we check that our event-study results are not driven by selective attrition. We produced our main results for a balanced sample of judges, for a shorter time window (three years before and after).[37] Those are reported in Appendix Figure A.23. The estimates are consistent with our main results.

A recent paper by Rambachan and Roth (2019) suggests testing for non-linear pre-trends in panel event study designs. We applied their approach to our data. As shown in Appendix Figure A.24, we can rule out substantial non-linear pre-trends for our main outcomes, especially after conditioning on the elastic-net-selected controls. We cannot rule out non-linear pre-trends for the hand-coded conservative vote outcomes.

We experiment with a range of additional fixed effects and covariates. In the circuit regressions, adding fixed effects for more detailed legal topics (94 categories) does not change any of the results (Appendix Figure A.15). In the criminal results, adding fixed effects for the associated crime type (345 categories) tends to strengthen statistical significance (Appendix Figure A.16). Political party indicators, interacted with year fixed effects or with treatment indicators, do not make a difference. Adding judge-specific trends strengthens some results (labor-EPA, Appendix Figure A.11), weakens others (conservative vote in economics cases), and induces a pre-trend in others (embedding measure of economics language, any prison, and IHS prison length). For the differences-in-differences regressions, results are not sensitive to coding the treatment variable as starting in the year of attendance, or the year after. All results are robust to including as a control the share of judges from the same law school cohort who have attended, suggesting that diffusion within law school cohort is at least not immediate.

In addition, we produce all event studies separately by the party of the nominating president. The language results are similar for judges from both parties. The results on conservative voting and regulation are driven mostly by Democrat appointees, while the results on criminal cases are driven mostly by Republican appointees. Similarly, we produced results based on pre-trend levels for the main outcomes (Appendix

[37]It was not possible to make these regressions with hand-coded conservative vote, given the small number of cases.

Figure A.21). The regulatory effect is largest for judges who were relatively liberal before attending. Economics language increases regardless of the pre-trend usage.

6 Magnitudes and Mechanisms

This section interprets the evidence reported in Section 5. First, we contextualize the magnitudes of the estimates in terms of persuasion rates. Second, we discuss possible mechanisms by which the Manne program could influence judge decision tendencies.

6.1 Magnitudes

One must be careful in interpreting the magnitudes of our estimates, as judicial decisions are difficult to compare to other political outcomes. Further, they are valid only for the attenders and not generalizable to the broader population of judges. But consider the effect on conservative voting in economics-related cases (Table 2). Rescaling the conservative vote variable to lie between 0 and 1, an effect size of 0.2 for ever-attenders is about one quarter of a standard deviation of the outcome and corresponds to $\Delta y = 0.11$ on the binary scale. With this number, we can calculate a persuasion rate and compare it to other interventions that alter partisan voting outcomes (DellaVigna and Gentzkow, 2010). The persuasion rate for conservative voting is

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

where we assume that attendance is coextensive with exposure ($\Delta e = 1$) and y_0 is the mean (binary) outcome for the ever-attenders in economics cases in the six years before attendance ($y_0 = 0.45$). The resulting persuasion rate is $p = 19.9$ percent.[38]

To put this effect size in historical context: From 1976 to 2002, the Songer database documents an increase of 0.3 in the likelihood to vote conservative rather than liberal. Taking the Manne coefficient of 0.2 and multiplying by 0.4 (the share of circuit judges who attended) renders a substantial fraction (0.08) of the overall 0.3 shift. Taken together, these numbers imply the Manne program could account for between a quarter and a third of the rise in (economic) judicial conservatism. If peers and precedent also impact the non-Manne judges, then the total Manne impact may

[38] If we use never-attenders as the baseline (rather than ever-attenders), we have $y_0 = 0.51$ and a computed persuasion rate of $p = 22.6$ percent.

be even larger.

Our estimated persuasion rate is comparable to that estimated for other interventions that shift partisan vote share. It is somewhat larger than the effect of Fox News estimated by DellaVigna and Kaplan (2007) (p =11.6 percent). It is close to the effect of an experimentally induced 10-week subscription to the Washington Post estimated by Gerber et al. (2009) (p =19.5 percent).

While judges are potentially much more sophisticated than average voters, the Manne program was a much more intensive educational program than these comparison interventions. Full-time immersion for 2-3 weeks in an enjoyable environment with credentialed experts is a strong treatment. The judges who selected into the program likely felt they needed help in navigating complex cases, such as corporate bankruptcies or securities regulation disputes, where economics can clarify the relevant issues.[39] By providing confidence to judges with tools and ideas, we see a significant shift in their decisions. On top of these curriculum effects, the Manne program established long-term intellectual attachments, with subsequent informational mailings and events maintaining social networks and relationships. All of these factors make the substantial impacts plausible.

These magnitudes 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).

6.2 Mechanisms

In this section we discuss *how* the Manne Program influenced the attending judges. Was the Manne program just a business lobbying vehicle? Was it an ideologically persuasive curriculum? Or was it purely pedagogical, providing objective tools for analysis?

A first possibility is that the Manne program consists of lobbying judges by inter-

[39]This is beyond a more general desire to be informed on economic issues – that is, for everyone, not just judges – documented for example by Blinder and Krueger (2004).

ested business parties (Grossman and Helpman, 2001; Teles, 2012; Bertrand et al., 2021). In that case, the program is a type of bribe that could nudge decisions for businesses, especially in the sectors of 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 cut into company bottom lines through environmental cleanup and supporting stronger labor unions. On antitrust, the post-Manne judges tend to make pro-merger decisions, which would clearly benefit the business interests funding the program.

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. 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 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.11). 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 other evidence suggests that the Manne program is not only partisan messaging. Many Democratically affiliated judges attended the Manne program and celebrated it.[40] 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 see no increase in conservatism on social issues in the circuit courts.

The qualitative record on the structure and content of the Manne program also 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,

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

and a handful of more normative seminars. Overall, the curriculum was only indirectly related to business or politics. 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.

A third possibility 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 content of the course composed a more-or-less fair representation of contemporaneous mainstream economics. Hence, the program provided a bundle of economics ideas plus some tools in economic analysis. An additional piece of evidence in favor of this third explanation, rather than a simple lobbying or partisanship story, is the observed effect on judicial opinion language. After attending, judges adopt the language of economic reasoning, suggesting a change in the decision process. After all, judges could find other reasons besides economic analysis to change their decisions to favor business litigants or push partisan priorities (Posner, 2008b).

Yet the effect of the Manne program is likely subtler than simply making economics arguments in court opinions. The ruling in *Conair Corp. vs NLRB*, 721 F.2d 1355 (D.C. Cir. 1983) provides a case in point. In *Conair*, the D.C. Circuit reviewed a mandatory bargaining order issued by the NLRB after a failed union certification election. In a split panel, the court overturned the NLRB and held for the company against the union.[41] Remarkably, the majority panel consisted of two future Supreme Court justices: Ruth Bader Ginsburg (who had recently attended the Manne program in the 1982 cohort) and noted conservative regulatory skeptic Antonin Scalia. While Ginsburg's opinion for the majority offered mostly statutory justifications, rather than economic analysis,[42] it is clear that the consequence of the decision was to prevent unionization and reduce penalties for the employer's unfair labor practices.[43] Meanwhile, a dissent in favor of the NLRB and the union was

[41] While the NLRB identified "egregious" NLRA violations by the company, which the D.C. Circuit did not deny, the court ruled that, when there was no clear signal of a majority desire by workers for a union, the NLRB could not infer such a desire.

[42] However, Ginsburg's opinion does discuss the deterrence goals of the mandatory bargaining policy and cites an MIT economics PhD dissertation chapter (by William Dickens) on how employer opposition affects union election win rates. The chapter, notes the majority opinion, shows that unions usually lose elections even without employer opposition; hence, majority worker support could not be inferred from employer opposition. In the dissent, Wald notes that the chapter also shows that employer opposition is effective in reducing union support.

[43] The NLRB had required the company owner to personally "read aloud to the assembled employees the Board's notice of employee rights and employer obligation." In a separate dissent, Ginsburg

filed by judge Patricia Wald – who, like Ginsburg, had been appointed by President Carter, but unlike Ginsburg, had not attended the Manne program.

Generalizing to the full case portfolio, standard economic analysis could explain any of the main observed post-Manne shifts reported in the paper. For example, economics would support lower regulation, if the pre-existing regulation levels were suboptimally high. A Becker (1968b) 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 et al., 1997).

Beyond the policy ideas, the Manne program was designed to provide some human capital support, including economic analysis and statistics.[44] 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 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.13). 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 use-

even objected to that shame penalty.

[44] 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."

fulness of an opinion to future judges (e.g. Ash and MacLeod, 2021), do not increase after Manne attendance (Appendix Table A.12). Finally, the use of quantitative or statistical language actually decreases relative to not-yet-attenders post-attendance (Appendix Table A.13), suggesting that the attendees are not becoming more numerate afterward. Overall, this evidence suggests that the ideas promulgated by the Manne program, rather than the analytical tools, were most impactful on the attending judges.

The next important question is why a course on economics should be so successful in delivering such impactful ideas. The course could be changing judges' attitudes or preferences about policy objectives or classes of litigants. Or it could be changing judges' beliefs about the impacts and incidences of judicial policy choices (Mukand and Rodrik, 2018; Stantcheva, 2020). We don't have sufficient data to disentangle these mechanisms, but based on the judges' appreciation letters, it is plausible that both changes in attitudes and in beliefs were occurring.

Whether it is through preferences or through beliefs, our evidence suggests that the persuasion was effective. 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 can reveal the truth, becomes more powerful than other idioms as a tool for guiding the decisions of sophisticated agents.

A number of other factors, for example the group aspect, could have 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, 2007b). The program may have had a lasting effect on the policy preferences of judges by altering their 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).[45] 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

Economics-trained judges significantly shift legal outcomes in U.S. courts. They use economic analysis in their written opinions, render conservative votes and verdicts, rule against regulation, are somewhat more permissive on antitrust, and mete out 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-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.

[45] 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).

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].[46]

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.[47]

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

[46]"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.

[47]"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."

As a testimony to the program's impact, Judge Williams took the lessons to heart. Then fresh out of the center's program, he included a diagram of marginal- and average-cost curves in an opinion. This was "the first significant opinion in history to do 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 David 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 principle 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 L. 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:

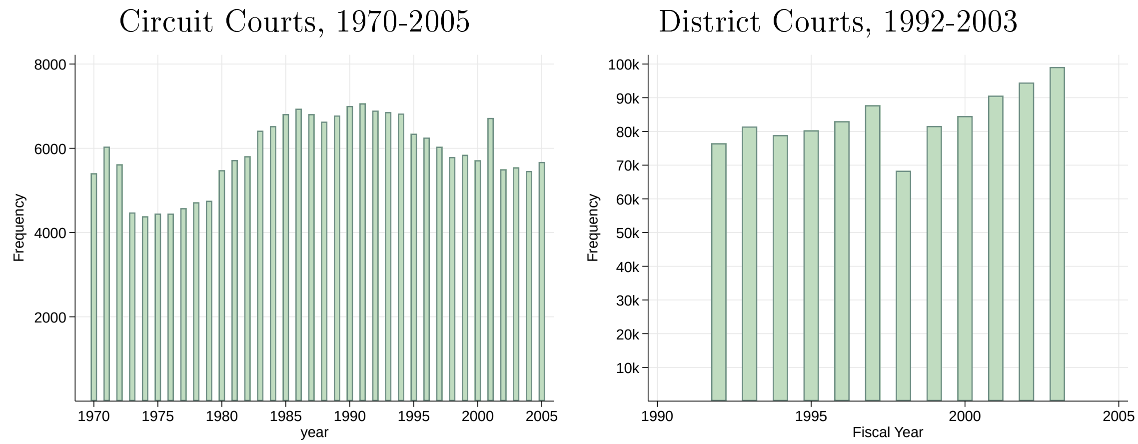
> While we are circumscribed by the parameters of existing statutes, regulations and case law, there is a wide area of decision entrusted to us where the result can go either way, depending on how we view the evidence. That area is called "judicial discretion." This is the area that is most affected 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.

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.

B Data

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).

We have judge biographical characteristics from the Appeals Court Attribute Data,[48] Federal Judicial Center, and previous data collection.[49] 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,

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

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

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.

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

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.

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.

C Additional Material on Econometrics

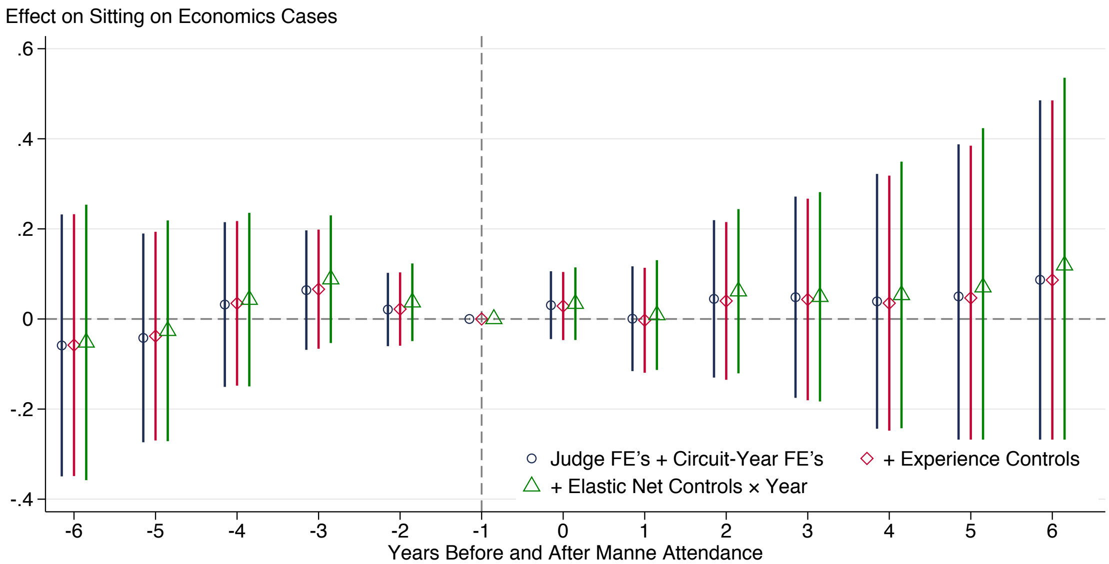
C.1 Checks on Selection into Different Case Types

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.

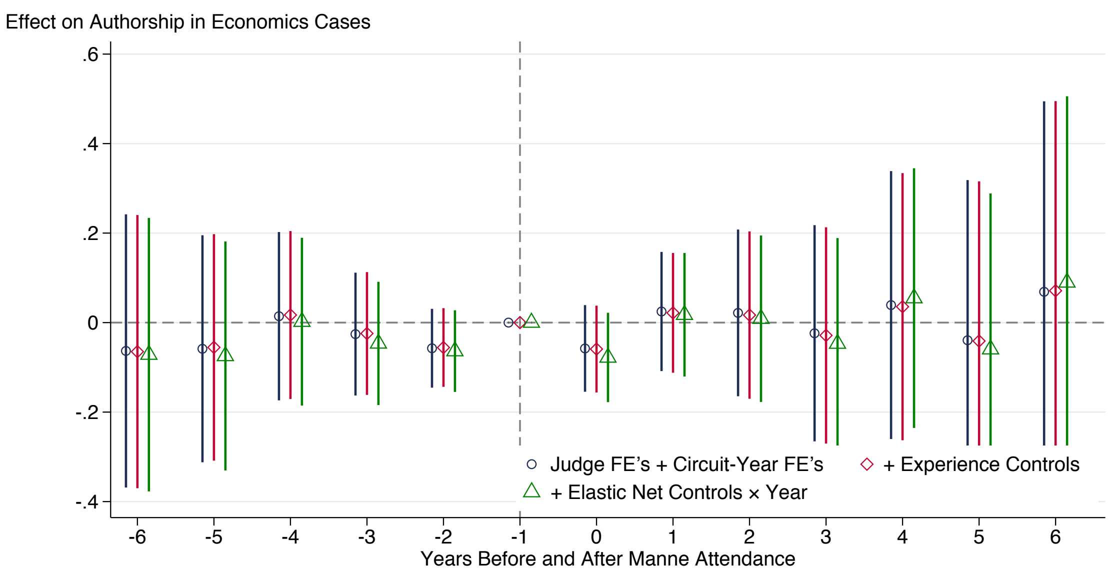
For the district courts, Appendix Table A.3 presents an omnibus check for endogenous settlement or selection of cases by judges. It shows that economics judges are not systematically appearing on certain types of crimes before or after *Booker*.

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.

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		Year of Attendance			Ever Attend		Year of Attendance	
	(1)	(2)	(3)	(4)		(1 cont.)	(2 cont.)	(3 cont.)	(4 cont.)
Republican Appointee	0.0640**		-0.0427		District Attorney	-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 Appoint	-0.0548		-0.282		County 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 Exper	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		Cohort: 1910s	0.0977**	0.0673*	-2.881	-2.878**
	(0.124)		(2.338)			(0.0276)	(0.0289)	(2.869)	(1.076)
State Atty General	-0.0305	-0.0261	-0.518	-1.219	Cohort: 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		Cohort: 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*		Cohort: 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	Cohort: 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**		Magistr Judge	-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 Attorney	-0.0179		-0.347	
	(0.0105)	(0.0105)	(2.805)	(2.790)		(0.0176)		(0.818)	
Cross-Party Appoint	-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 Appointee	0.0539**	0.0537**	-3.862	-3.894	County 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	Assit 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 Supr 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 Exper	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 Office	-0.144*		0		Black	0.0512	0.0522	0.255	0.263
	(0.0676)		(.)			(0.0298)	(0.0298)	(1.060)	(1.053)
Solicitor General	0.0632		3.548		Cohort: 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	Cohort: 1920s	0.344***	0.349***	-2.121	-2.140
	(0.0278)	(0.0270)	(.)	(.)		(0.0248)	(0.0247)	(4.044)	(4.041)
State Atty General	-0.00128		-0.962		Cohort: 1930s	0.289***	0.297***	1.791	1.791
	(0.0239)		(0.928)			(0.0253)	(0.0252)	(4.047)	(4.046)
Private Practice	0.00217	0.000786	-0.867	-0.774	Cohort: 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	Cohort: 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*		Magistr Judge	-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 Negative-Weighting Issues from Staggered Treatment Timing

A recent line of papers, starting Goodman-Bacon (2018), had 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 to this problem. However, the approach taken by these papers does not map directly into our findings. 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. Thus, the off-the shelf estimators would not work well in our context.

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

Next, we apply the complementary diagnostic by Jakiela (2021), focusing on the event-study sample. First, we check for negative weights by looking at the distribution of residualized treatment indicators – that is, after partialling out circuit-year and judge fixed effects. Since $\hat{\gamma} = \sum_i \frac{Y_i Z_i}{Z_i^2}$, if Z_i is negative then some observations are weighted negatively. We regress the residualized outcomes on a residualized

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

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

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

B. Diagnostic from Jakiela (2021)

	(1) Labor/EPA Conservative	(2) Conservative Econ Vote	(3) Conservative Non-Econ Vote	(4) Embedding Similarity
Heterogeneity by	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).

Table A.8: Regression Results Using Never-Attenders, Adjusting for Staggered Treatment Timing by Averaging Across Cohorts

	Econ Language		*Rule Against Labor/EPA*	
	(1)	(2)	(3)	(4)
Post Manne	0.144	0.215+	0.143*	0.178**
	(0.127)	(.127)	(0.065)	(0.061)
Circuit-Year FE	X	X	X	X
Judge FE	X	X	X	X
Cohort $\geq$ 1987		X		X

Notes. Regression estimates for the effect of Manne attendance on embedding economics language similarity (Columns 1 and 2), and ruling against labor/environmental agencies (Columns 3 and 4), after adjusting for staggered treatment timing by averaging across cohort-specific regressions

treatment indicator (i.e. partialling out circuit-year and judge FE). Table A.7 Panel B shows that the correlation between the residuals within pre-Manne observations and within the post-Manne observations is very similar, 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.

Still, for the main results, 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 where all never-attenders are included. In this comparison group, Manne judges are included if they attended more than six years in the future from this cohort. We then averaged these cohort-level estimates to produce adjusted estimates for the overall effect, weighted by the number of attending judges in each cohort.

The results for the main outcomes (economics language and labor/EPA) are reported in Table A.8 We can see that after adjusting for staggered treatment, the coefficients are all positive. For Labor/EPA, the results are statistically significant (Column 3). For economics language, the result is not statistically significant (Column 1), yet much larger in magnitude than the comparable zero estimate using never-attenders in Table 1 Column 5. The comparable estimates for conservative voting (economics or non-economics) or for antitrust are noisy and not statistically different from zero, reflecting that the small sample size issues are more problematic when estimating separate regressions by cohort.

Further, motivated by the different trends in economics language for the never-attenders in the early years (Appendix Figure A.7), we run adjusted regressions limited to the second half of Manne cohorts (1987 and after). In that subsample, we find a statistically significant effect of Manne attendance on economics language with the adjustment (Column 2). The effect of labor/EPA is also slightly larger and statistically significant in this sample.

Figure A.4: Words Correlated with Law-and-Economics Lexicon Dimension

(a) Positively Associated Words (a) Negatively Associated Words

Notes. The left word cloud lists the set of words that have the highest cosine similarity to the average word vector for Ellickson phrases in the word embedding space. The right word cloud gives the words that have the lowest (most negative) cosine similarity to this vector.

D Additional Material on Judge Writing Style

D.1 Embedding Similarity to Ellickson Lexicon

Figure A.4 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.

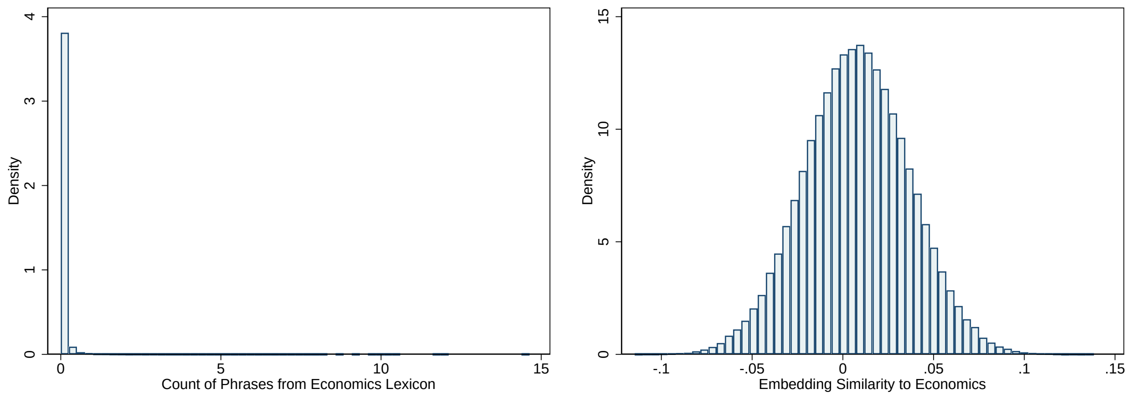
5. In an oligopolistic market characterized by few producers, price leadership occurs when firms engage in interdependent pricing, setting their prices at a profit-maximizing, supracompetitive level by recognizing their shared economic interests with respect to price and output decisions.

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.5: 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).

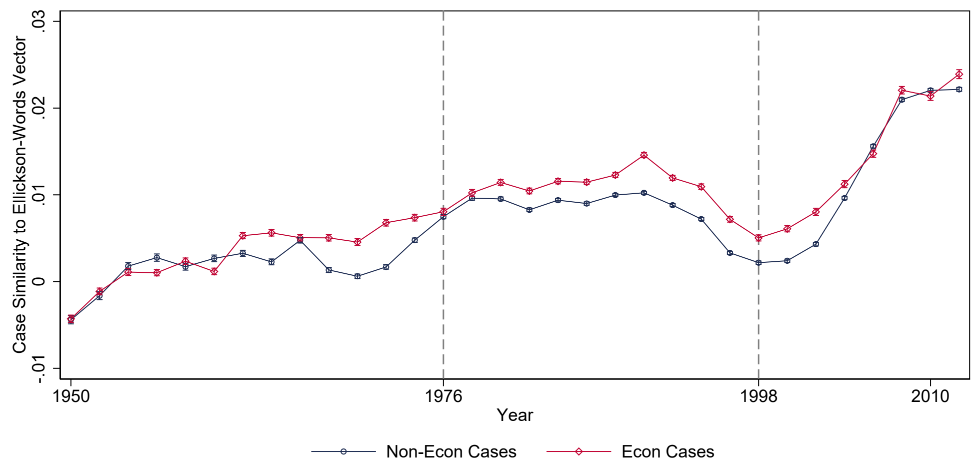
9. Reducing the number of interchanges and reducing the average length of haul have no economic significance in themselves, though both might reduce average transit time, which would be a benefit to shippers and hence a genuine efficiency gain

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.

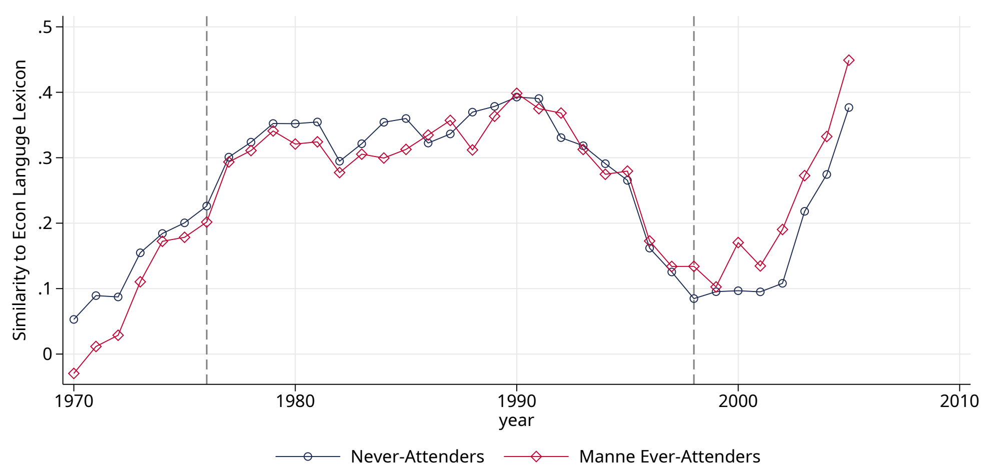
Figure A.6 shows the trend in the average case similarity to the law-econ dimension since 1950. We see that economics cases tend to score more highly, as expected. In addition, the use of economics language has been increasing over time. Figure A.7 shows the trends separately by circuit judges who attended 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.

Figure A.6: 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.

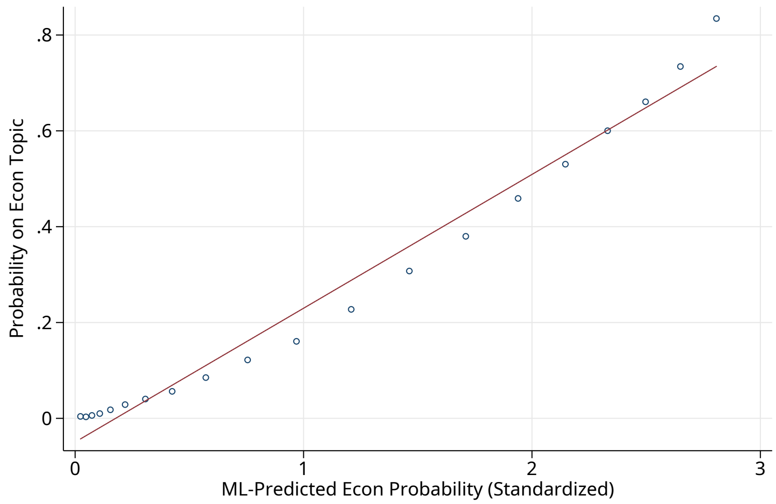
Figure A.7: Trends in Economics Language, by Manne Attendance



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 the Manne program is not affecting the cases that judges review or author. so the shift in the language measure is due to the use of economics reasoning.

Figure A.8: 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.

D.2 Text-Predicted Similarity to Economics Topics

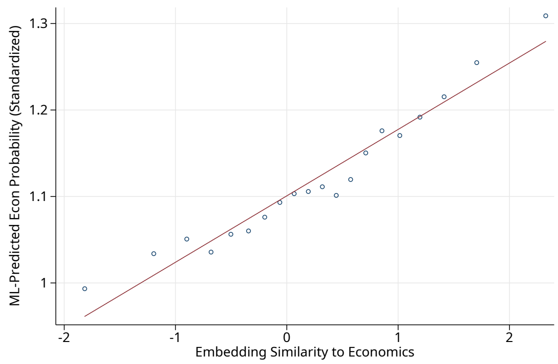
We produced a second measure of economics language using supervised learning on corpus metadata. For each case in our corpus, we have labels for whether it is an economics-related case (regulation or labor). We take this label (economics case) as an outcome and predict it based on the text features of the case. For the text features, we used the Arora et al. (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. As shown in the calibration plot in Appendix Figure A.8, the model also effectively replicates the ranking and distribution of the outcome.

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.8 visualizes how well our prediction model replicates the probability

Figure A.9: Econ Embedding Similarity Correlated with Text-Predicted Econ



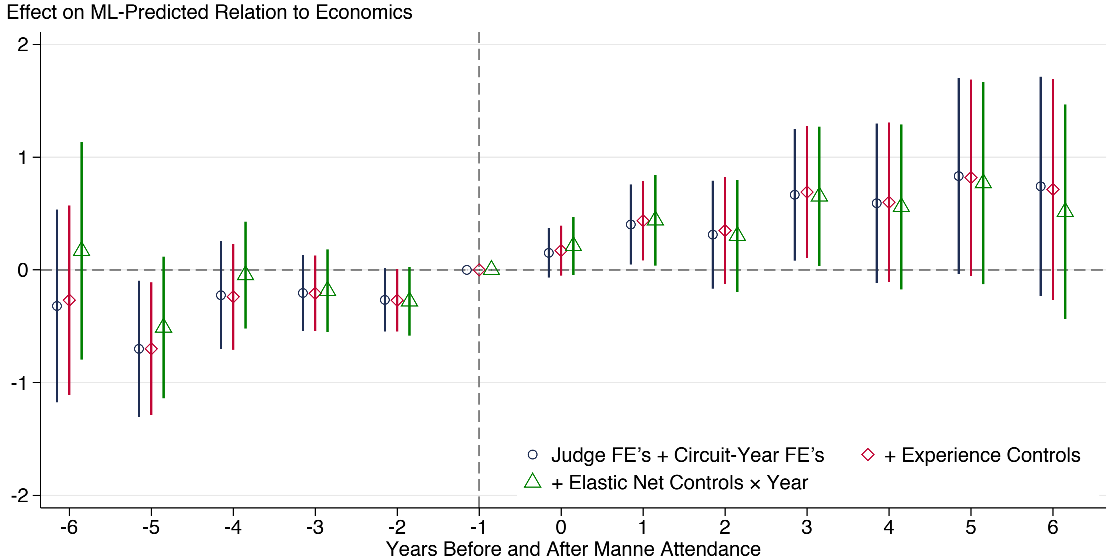
Notes. Binscatter of a case's embedding similarity to the Ellickson Law-and-Economics lexicon, against the predicted probability that a case is republican-appointee-authored and concerning economics topics.

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.9 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.10 reports the event study for the machine learning measure. The effect is significant even five years later. There is no significant pre-trend. In the differences-in-differences estimates (Table A.9), again there is a positive effect of Manne attendance on the use of economics language. The effect is about one-sixteenth of a standard deviation. The effects are robust to including the experience controls (Column 2), as well as the elastic-net-selected controls (Column 3). The effect is robust when looking at the whole career for Manne judges (Column 4), and when looking at the full sample including never-attenders (Column 5).

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



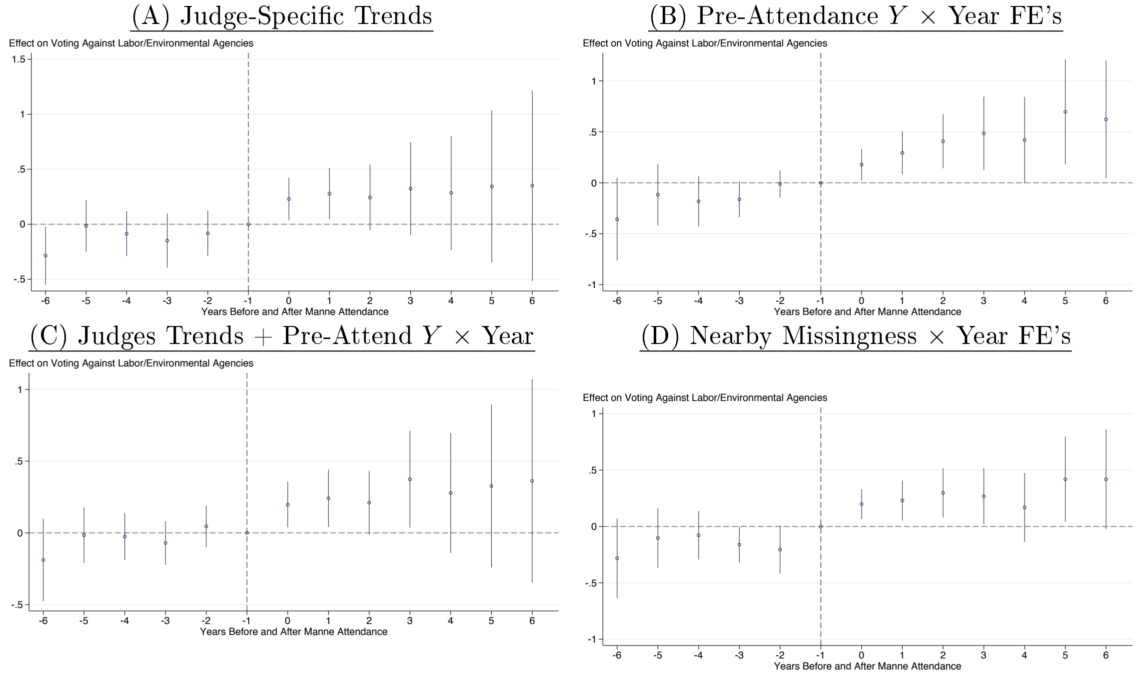
Notes. Event study effect of Manne attendance on text-based predicted probability that case is on an economics topic (regulation or labor). Sample is limited to case authors. Regressions include judge and circuit-year fixed effects (blue circles), with additional specifications adding quadratic in judge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

Table A.9: Effect of Manne Program on Alternative Economics Language Measure

	Text-Predicted Relation to Economics				
	(1)	(2)	(3)	(4)	(5)
Post Manne	0.0503	0.0600+	0.0620	0.0497*	0.0221+
	(0.0354)	(0.0353)	(0.0412)	(0.0238)	(0.0133)
N (Opinions)	9963	9963	9963	20241	93387
adj. R-sq	0.279	0.280	0.302	0.241	0.175
Event Study	X	X	X		
Ever Attenders				X	
All Judges					X
Circuit-Year FE	X	X	X	X	X
Judge FE	X	X	X	X	X
Experience Vars		X	X	X	X
Party × Year FE			X	X	X
E-net × Year FE			X	X	X

Notes. Estimated effects of Manne training on text-predicted probability that a non-economics case is on an economics topic, described in Subsection 3.2. Sample is limited to case opinion authors. Standard errors clustered at the judge level in parentheses. $+p < .1, *p < 0.05, **p < .01$. Observations are weighted to treat judge-years equally.

Figure A.11: 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.

E Alternative Specifications for Regulatory Agency

Figure A.11 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 5.

F Antitrust Analysis

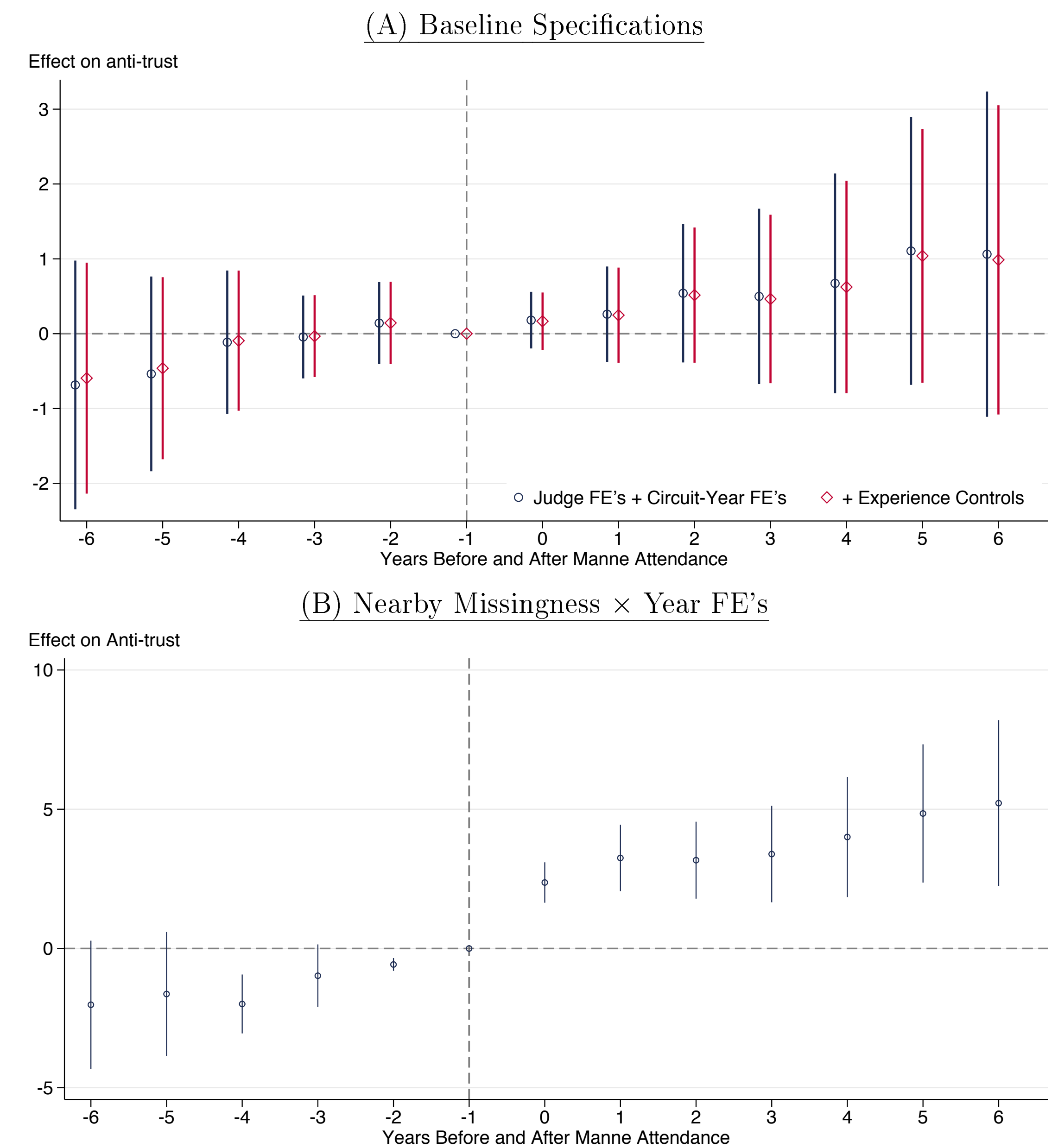
Antitrust cases were collected and annotated in three ways. We had two sources for previous annotations. First, the Songer-Auburn dataset has a handful of antitrust cases (5% sample) annotated as liberal or conservative, following a rubric similar to ours (we verified this by re-annotating some of these 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.

The event study estimates for antitrust are reported in Appendix Figure A.12. 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.

Figure A.12: Effect of Manne Program on Antitrust Decisions



Notes. Event study effects on voting against antitrust claimants, relative to year before attendance at Manne economics training. 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.10: 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
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 × 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. Standard errors clustered at the judge level in parentheses. $+p < .1, *p < 0.05, **p < .01$. Observations are weighted to treat judge-years equally.

G Additional Results

This section collections additional results and robustness checks for the circuit courts.

Table A.10 provides a placebo test for the event-study impact of Manne program on language. We show in Columns 1 through 3 that similarity to (non-economics) academic legal writing does not change discretely at the time of attendance. In Columns 4 through 6, another measure of movement conservatism (constitutional concerns, measured by citation to bill of rights amendments), also does not change discretely at the time of attendance. We tried other measures of constitutionalist reasoning, such as citations directly to the Constitution's articles, with similar zero effects.

Next we look at two more measures of conservative decision-making. In Table A.11, we show that Manne attendance does not affect the probability that a judge cites Reagan or Bush nominees (Columns 1-3). However, we do see that there is a positive and significant effect on a conservative dissent measure: the rate that a judge dissents against a Democrat-nominated opinion author.

Table A.12 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.11: 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 (Opinions)	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.

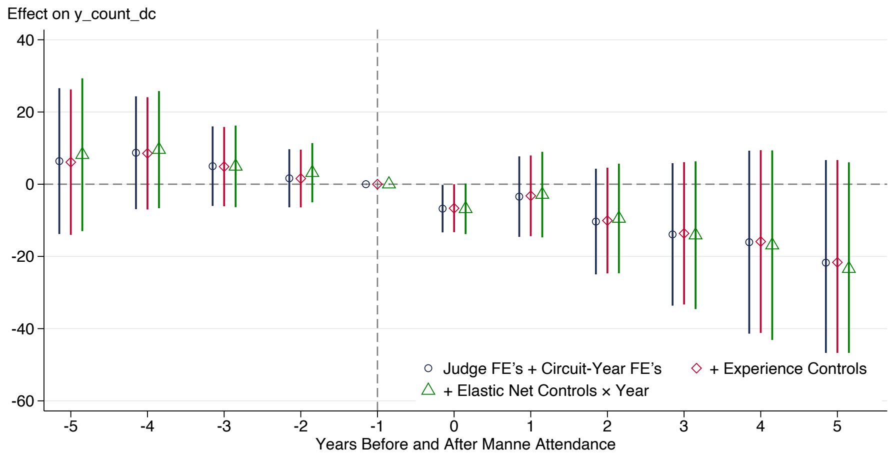
Table A.12: Effect of Manne Program on Forward Citations to Opinions

	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 × Year FE			X			X
E-net × Year FE			X			X

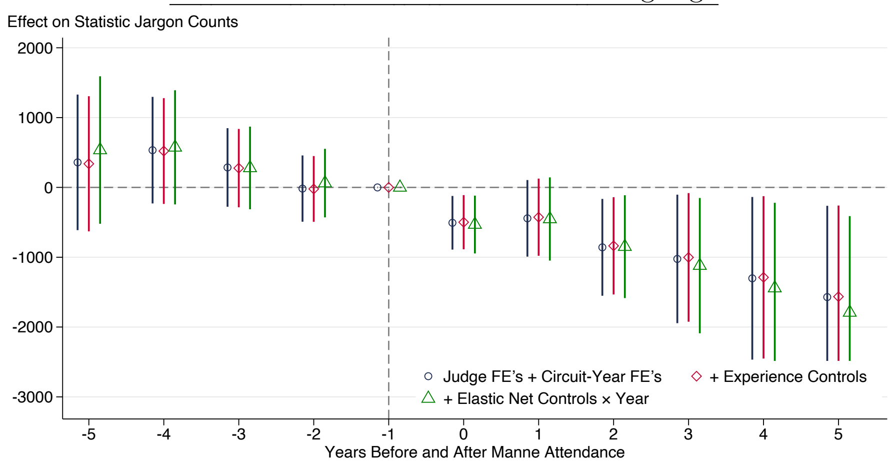
Notes. Estimated effects of Manne training on citations to a judges opinions from circuit court cases. Total means all circuits; Outside means other circuits. Standard errors clustered at the judge level in parentheses. $+p < .1, *p < 0.05, **p < .01$. Observations are weighted to treat judge-years equally.

Figure A.13: 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.

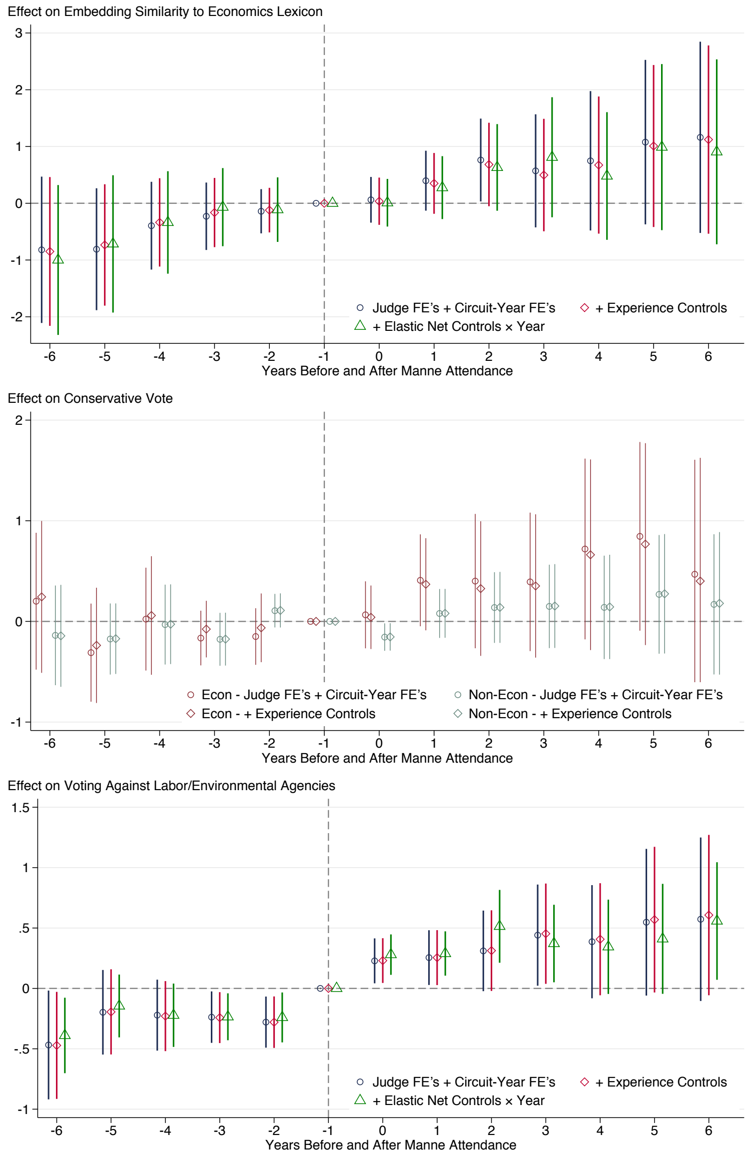
Table A.13: Effect of Manne Program on Promotion of District Judges to Circuit

	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. $+p < .1, *p < 0.05, **p < .01$.

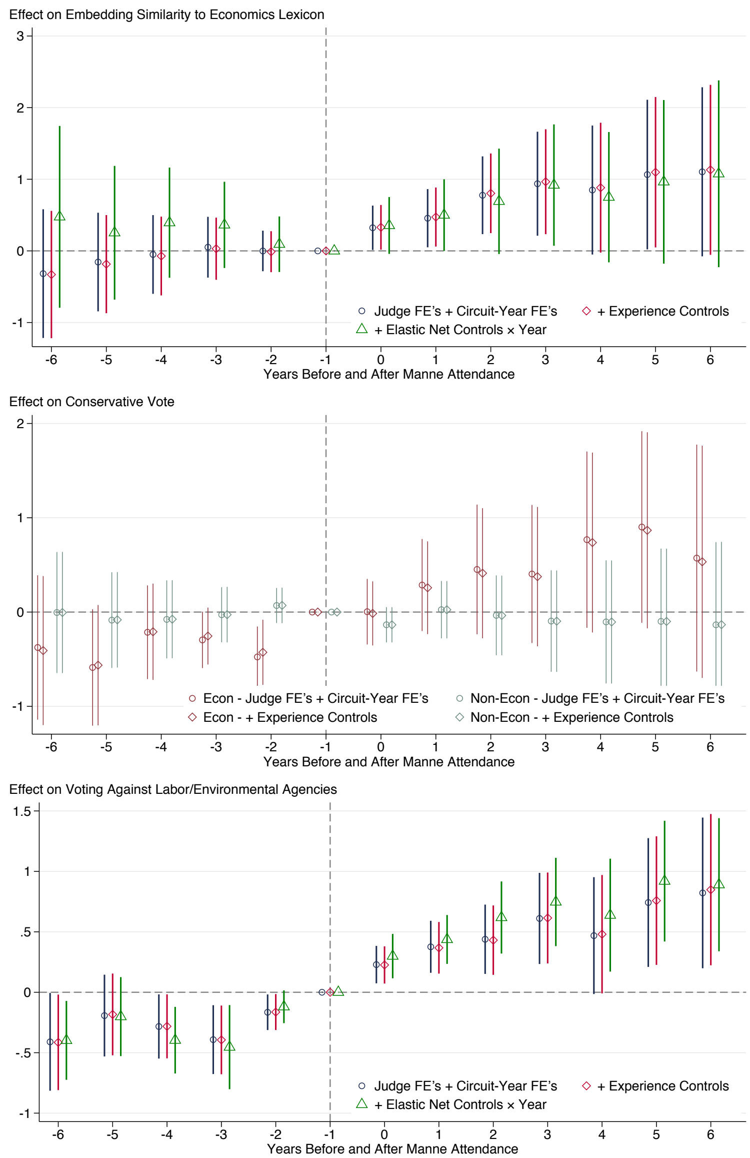
Table A.13 shows the effect of Manne training on being elevated from a district judgeship to a circuit judgeship. District judges who attended Manne are more likely than their court colleagues to be promoted. The effect is robust to starting-year fixed effects and judge biographical controls. Interestingly, we can see that the effect is concentrated totally among Republican presidents (Column 4). Democrat presidents do not selectively promote Manne judges.

Figure A.14: Event Study Robustness: Dropping 2nd, 8th, 9th, and D.C. Circuits



Notes. Main event study results for the circuit courts (from Figures 3, 4, and 5) 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). Outcomes are Economics Language, Conservative Vote in Econ and Non-Econ Cases, and Voting against Labor/Environmental Agencies. For other details see notes in the associated main-text exhibits.

Figure A.15: Circuit Event Studies with Legal Topic Fixed Effects



Notes. Main event study results for the circuit courts (from Figures 3, 4, and 5) but including fixed effects for 94 detailed legal topics. Outcomes are Economics Language, Conservative Vote in Econ and Non-Econ Cases, and Voting against Labor/Environmental Agencies. For other details see notes in the associated main-text exhibits.

Figure A.14 has our main results after dropping the four circuits which Levy and Chilton (2015) find to have nonrandom assignment of cases. The results are qualitatively the same to those reported in the main text. In addition, the results hold with case topic fixed effects (Figure A.15).

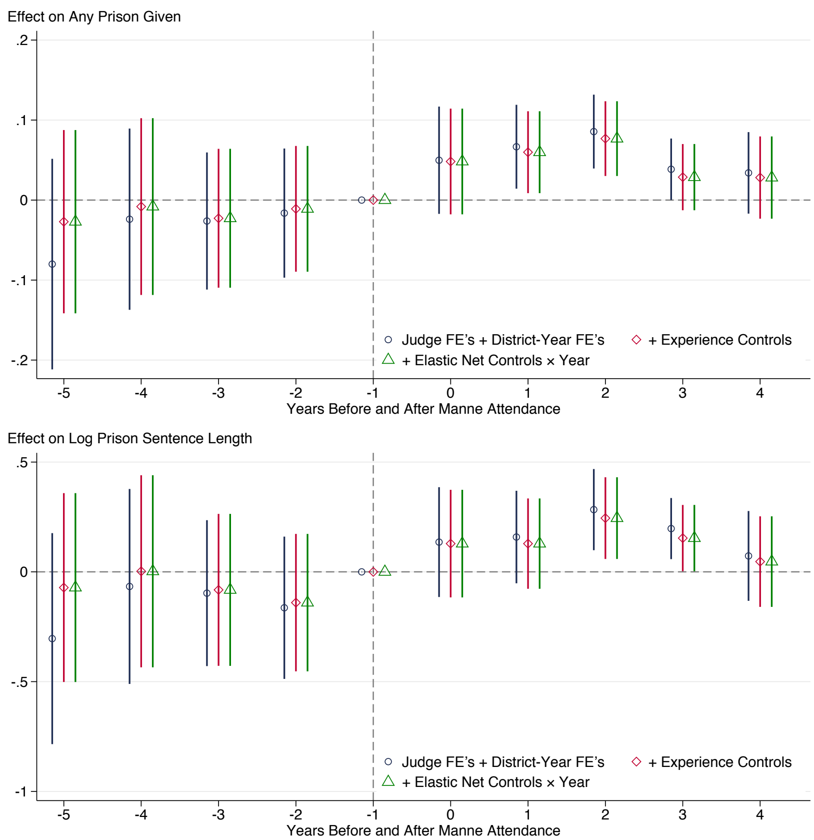
Table A.14: Effect of Manne Judges on Criminal Sentencing, by Crime Type

	Log of Total Sentence				
	(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. $+p < .1, *p < 0.05, **p < .01$.

Table A.14 shows the *Booker* results when dropping some crime types. We can see in Column 1 that the effects of the Manne-*Booker* interaction are largest when dropping drug crimes. In addition, harshness is elevated for weapon crimes. The effects are smallest when dropping immigration crimes, suggesting harshness is concentrated for immigration crimes. The vast majority of charges in the immigration category are for (1) reentry of deported alien and (2) entry of alien at improper time or place.

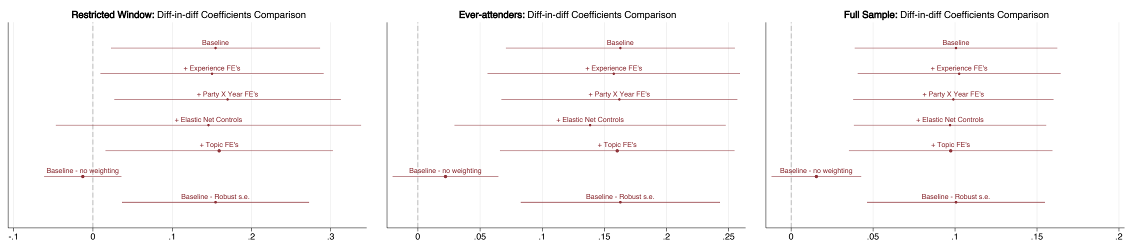
Figure A.16: District Event Studies with Crime Charge Fixed Effects



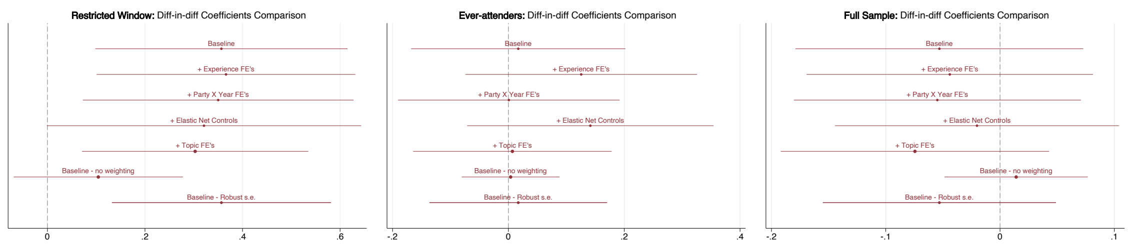
Notes. Main event study results for the district courts (from Figure 6) but including fixed effects for crime type (345 categories). Outcomes are Any Prison Given and Log Sentence Length. For other details see notes in the associated main-text exhibit.

Figure A.17: Summary of Sampling/Specification Checks (Circuit Courts 1)

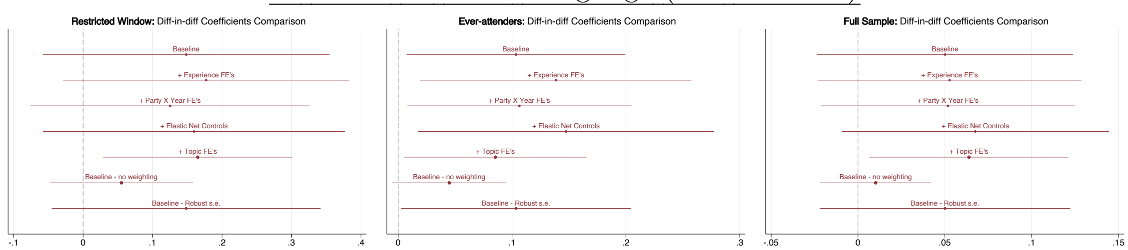
A. Effect on Labor/EPA



B. Effect on Econ Language (Embedding Measure)



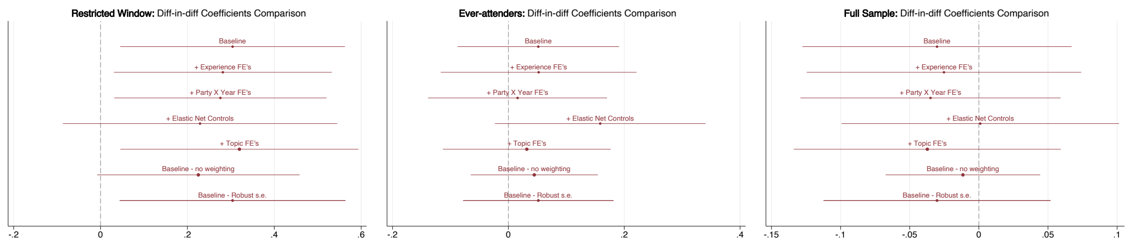
C. Effect on Econ Language (ML Measure)



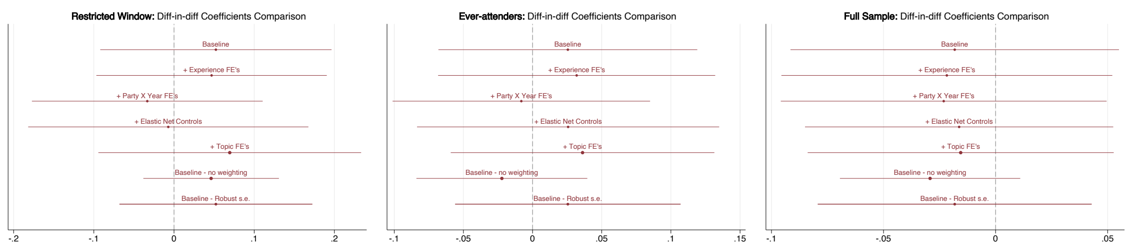
Notes. These coefficient plots summarizs robustness of the differences-in-differences results to sampling and specification choices. For each outcome, the left, middle, and right plot shows the estimates with the different samples: event window, ever attenders, and full sample of judges, respectively. Each plotted coefficient corresponds to another regression specification: baseline, experience controls, party X year fixed effects, elastic net controls X year fixed effects, legal topic fixed effects, unweighted regressions, and robust (rather than clustered) standard errors.

Figure A.18: Summary of Sampling/Specification Checks (Circuit Courts 2)

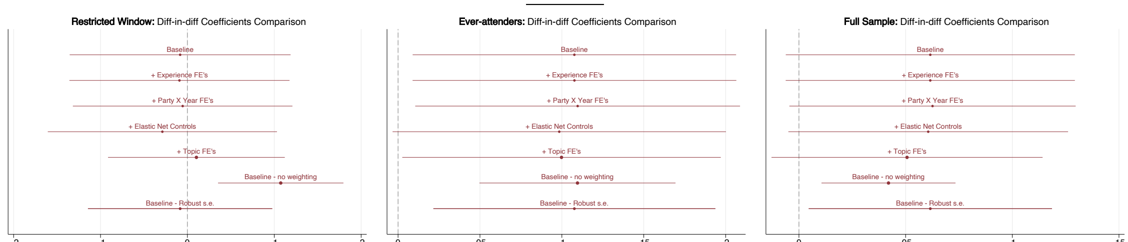
A. Effect on Conservative Voting in Economics Cases



B. Effect on Conservative Voting in Non-Economics Cases



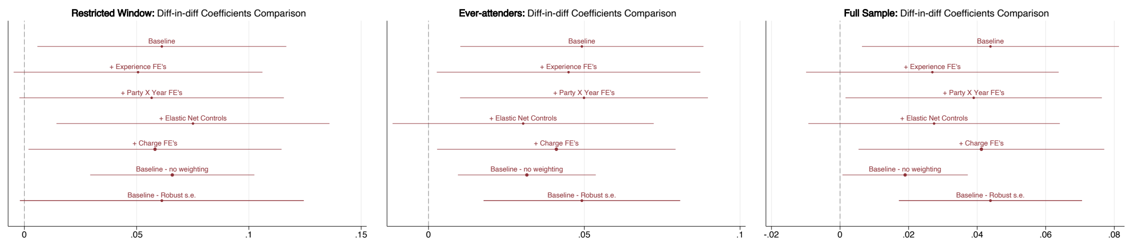
C. Effect on Difference in Conservative Voting in Economics vs. Non-Economics Cases



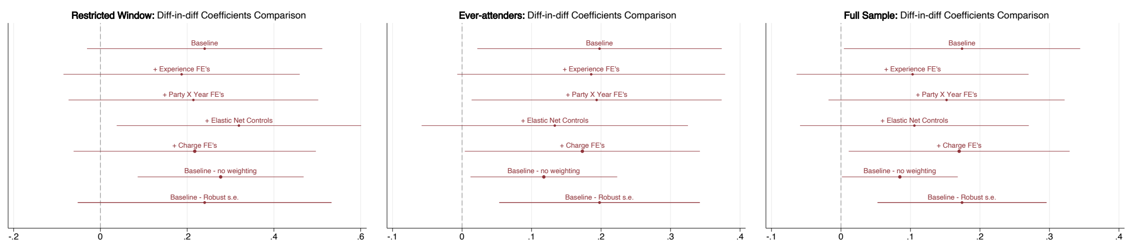
Notes. These coefficient plots summarizs robustness of the differences-in-differences results to sampling and specification choices. For each outcome, the left, middle, and right plot shows the estimates with the different samples: event window, ever attenders, and full sample of judges, respectively. Each plotted coefficient corresponds to another regression specification: baseline, experience controls, party X year fixed effects, elastic net controls X year fixed effects, legal topic fixed effects, unweighted regressions, and robust (rather than clustered) standard errors.

Figure A.19: Summary of Sampling/Specification Checks (District Courts)

A. Effect on Any Sentence Given (District Courts)



B. Effect on i.h.s. Sentence Length (District Courts)



Notes. These coefficient plots summarize robustness of the differences-in-differences results to sampling and specification choices. For each outcome, the left, middle, and right plot shows the estimates with the different samples: event window, ever attenders, and full sample of judges, respectively. Each plotted coefficient corresponds to another regression specification: baseline, experience controls, party X year fixed effects, elastic net controls X year fixed effects, legal topic fixed effects, unweighted regressions, and robust (rather than clustered) standard errors.

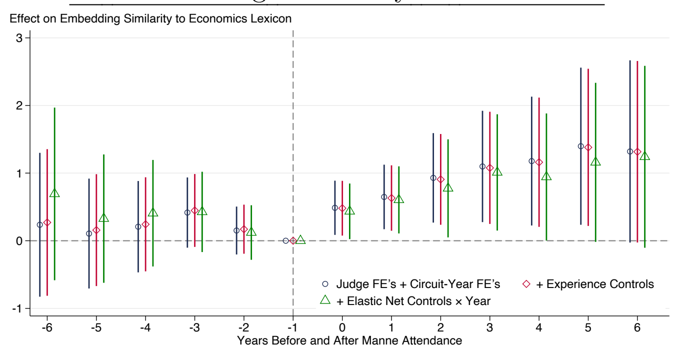
Table A.15: Covariates Selected in Double Lassso Approach

Outcome Variable	Covariates Included		
	Post-Manne Treatment	Outcome Variable	Both (Union)
Labor/EPA	43	571	587
Embedding Similarity	43	856	859
ML Similarity	115	867	878

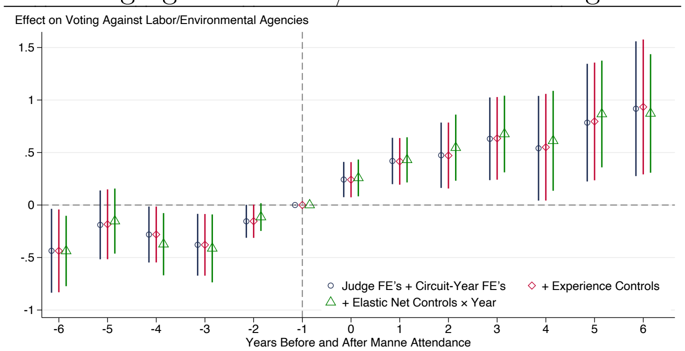
Notes. We perform a double-lasso approach by constructing the full matrix of year-covariate interactions (30 covariates, times 36 years, is 1080 interactions) and then running a set of lasso regressions with this matrix as the feature set. For these regressions, we make things computationally feasible by residualizing all of these year-demographic interactions, the treatment variable, and the outcome variables on the judge fixed effects and circuit-year fixed effects, with judge weighting, before running lasso. First, we use the post-Manne treatment indicator as the label to be predicted. All of the lasso-selected variables are kept. Second, we run separate lasso regressions with these interaction features as inputs and the conservatism measures as outcomes. For each outcome, we add the additional covariates selected from the outcome lassos. This table shows the number of year-covariate terms selected by the double lasso process, by outcome. For conservative vote, there were insufficient observations to run regressions, given the large number of selected variables.

Figure A.20: Main Circuit Court Results for Oversubscribed Period

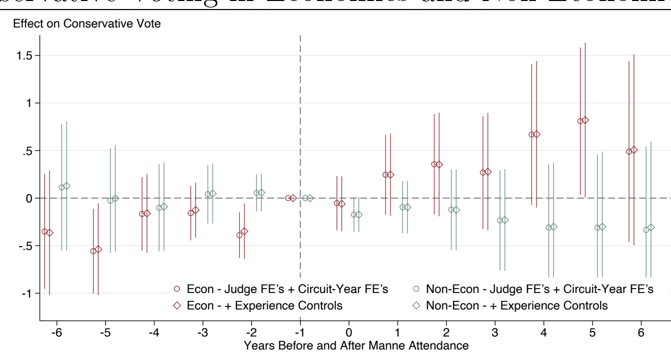
A. Embedding Similarity to Economics



B. Voting against Labor/Environmental Agencies



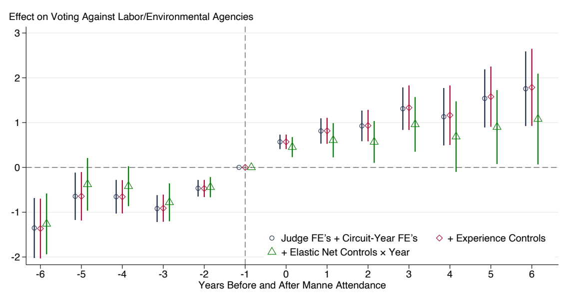
C. Conservative Voting in Economics and Non-Economics Cases



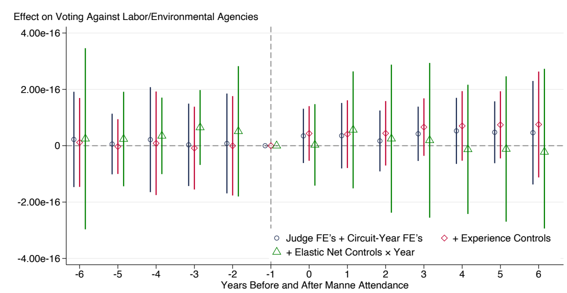
Notes. Main event-study results for embedding similarity to economics (Panel A), voting against labor/environmental agencies (Panel B), and conservative voting (Panel C), limited to the heyday period when the Manne program was oversubscribed on a first-come-first-serve basis (pre-1987).

Figure A.21: Heterogeneous Effects of Manne Program, by Pre-Attendance Outcome Levels

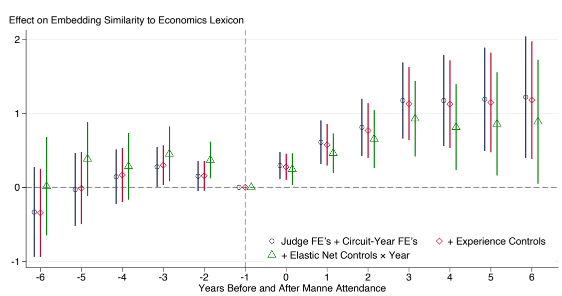
A. Effect on Labor/EPA, Below Median Before Attendance



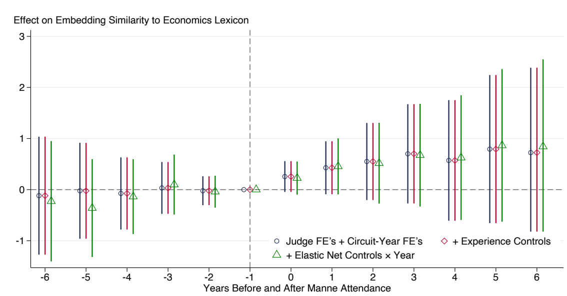
B. Effect on Labor/EPA, Above Median Before Attendance



C. Effect on Econ Language, Below Median Before Attendance

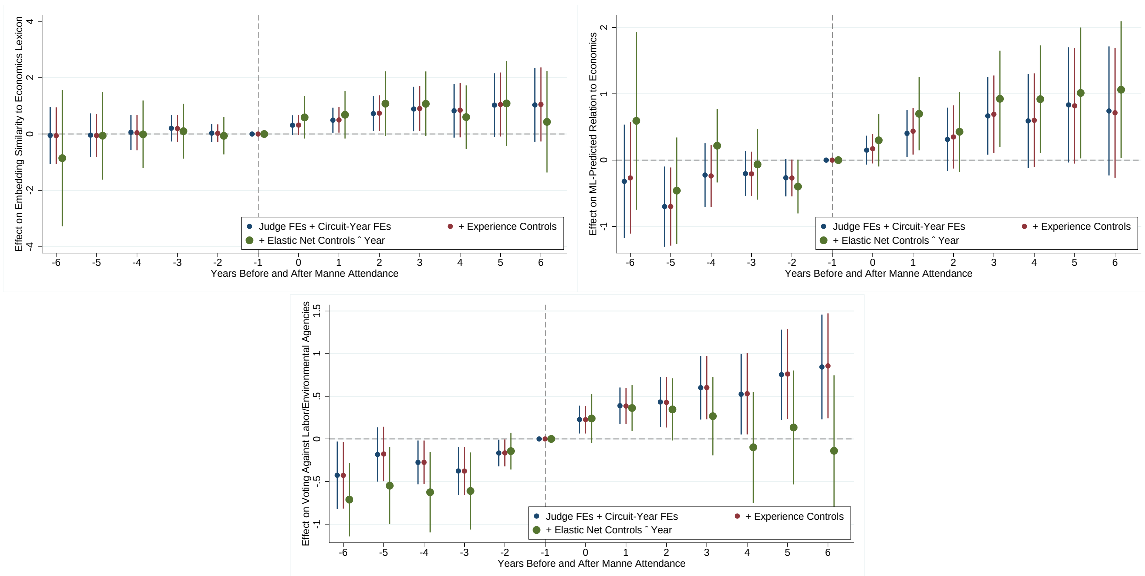


D. Effect on Econ Language, Above Median Before Attendance



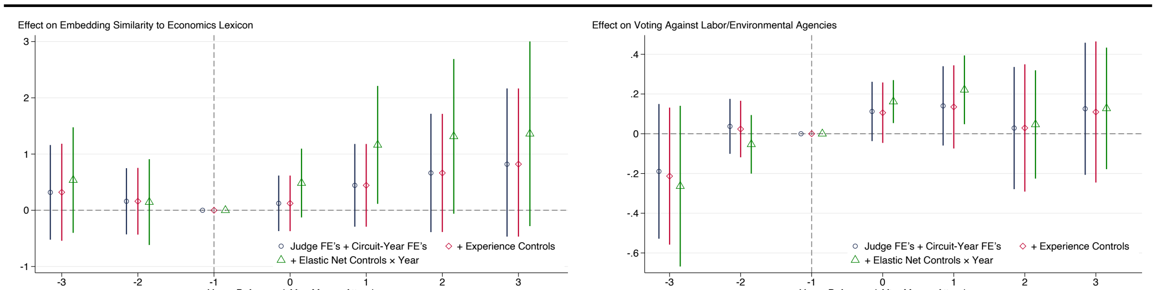
Notes. Estimated effect of Manne training on the Labor/EPA (Panels A and B) and economics language (Panels C and D) outcomes. For heterogeneity analysis, samples are split according to the median average judge value of each outcome, computed from the six years before attendance. Three series give the three baseline specifications. 95% confidence intervals constructed using standard errors clustered at the judge level.

Figure A.22: Results with Double Lasso Selected Covariates



Notes. Event study regressions using the double lasso selected covariates (as outlined in Table A.15). Main event study results for the circuit courts: Outcomes are Economics Embedding Similarity, ML Econ Similarity, and Voting against Labor/Environmental Agencies. For conservative vote, there were insufficient observations given the large number of selected variables. For other details see notes in the associated main-text exhibits.

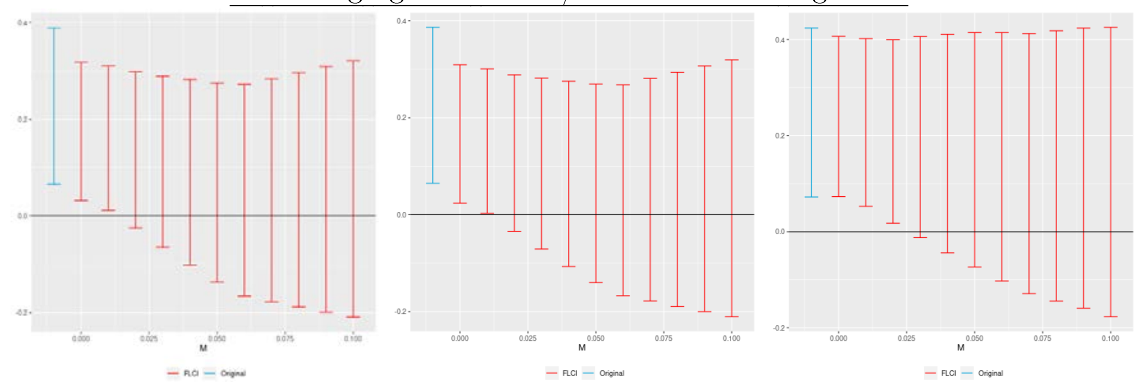
Figure A.23: Results with Balanced Panels and Shorter Windows



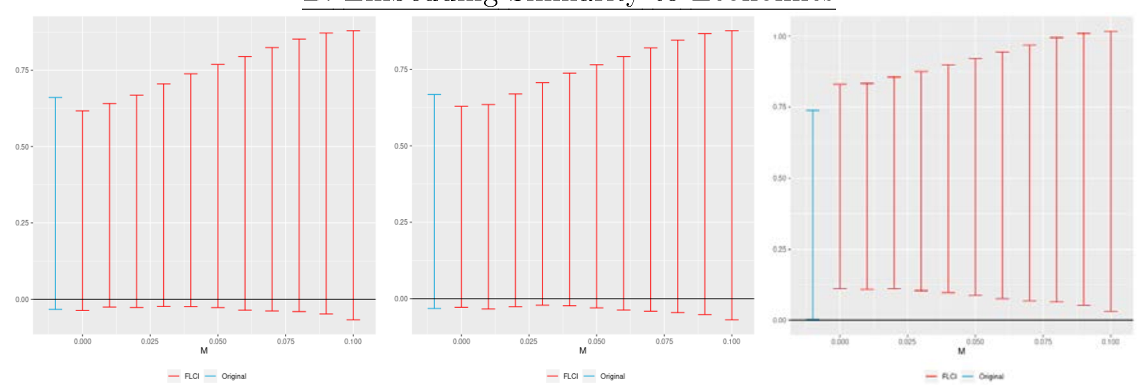
Notes. Event study regressions with balanced panels of judges, for three years of lags and leads, for the main outcomes (Embedding Similarity to Economics and Labor/EPA Regulatory Vote). For other details see notes in the associated main-text exhibits. These regressions were not possible to do with hand-coded conservative vote or antitrust given the small number of cases precluding a balanced sample.

Figure A.24: Pre-Trend Sensitivity Analysis

A. Voting against Labor/Environmental Agencies



B. Embedding Similarity to Economics



Notes. Sensitivity graphs for violation of the parallel trends assumption, applying the method from Rambachan and Roth (2019); see also Ang (2021). Panel A: Results for Labor/EPA decisions, for the three baseline specifications (judge FE, plus experience controls, then with elastic-net-selected controls). Panel B: Results for the Embedding Economics Language score, for the three baseline specifications (judge FE, plus experience controls, then with elastic-net-selected controls). The axis-crossing value of of $\bar{M}$ indicates that the significant treatment effect of Manne attendance 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.