Ideas Have Consequences

The Impact of Law and Economics on American Justice

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Abstract

This paper provides a quantitative analysis of the effects of the early law-and-economics movement on the U.S. judiciary. We focus on the Manne Economics Institute for Federal Judges, an intensive economics course that trained almost half of federal judges between 1976 and 1999. Using the universe of published opinions in U.S. Circuit Courts and 1 million District Court criminal sentencing decisions, we estimate the differences-in-differences effect of Manne program attendance using judge fixed effects. 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 we further adjust for machine-learning-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.

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

JEL codes: D7, K0, Z1.

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 ideas themselves can directly affect policymakers' policy decisions. This paper fills that gap by studying the effect of an influential, ideological 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 directly influence policy.

A particularly influential set of ideas in legal academia and the judiciary has been law and economics. This approach emphasizes laissez-faire economic policymaking, cost-benefit criteria, 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, 1987; Ellickson, 2000; Posner, 2008). One interpretation of these trends is that the commitment to rigorous use of social-scientific reasoning in economics has made its ideas more persuasive, even for sophisticated experts like judges (Gentzkow and Kamenica 2011).

In the early years of law and economics, a flagship organ 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).[2] 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 extremely relevant for economic policy. American law makes giants

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[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 was and continues to be the focus in the judicial training programs of the Law and Economics Center.

[2] For more background on the program, see Subsection 2.2 and Appendix A below.

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 (Gennaioli and Shleifer 2007). Federal judges (numbering roughly 180 in circuit courts and 680 in district courts) are appointed by the president and serve with life tenure. These courts handle hundreds of thousands of cases per year (roughly 67,000 in circuit courts and 330,000 in district courts), while the Supreme Court hears only 100 cases per year. Relatively few district court cases are appealed to the circuits, while fewer than one percent of circuit decisions are reviewed by the Supreme Court. Therefore almost all circuit court decisions are final, and they comprise the vast majority of what law students are reading and what judges are applying.

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 attending circuit judge, we have the portfolio of published decisions. For each attending district judge, we have detailed information on his/her criminal sentencing decisions. The datasets include the cases of non-attending colleague judges as a potential comparison group. 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, legal/undergraduate education, and previous career experience. We use circuit-year interacted 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 that, we show that initial judge partisanship (as proxied by party of nominating president) is not predictive of the timing of attendance. Moreover, we take care to check for pre-trends in the outcome variable, and our results hold even when controlling for a rich set of judicial characteristics, interacted with treatment and time, which 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 distance measure, borrowed from machine translation (Le and Mikolov (2014), 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 to economics. We find that judges significantly increase their use of economics language after attending the Manne program.

Next, we look at how the Manne program influenced decisions in policy-relevant appellate cases. Using the sample of hand-coded cases produced by Songer and Auburn (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 environment issues that early law and economics focused on. Using newly collected data on antitrust decisions, we also show 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. 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. The results are consistent with judges learning a theory of simple deterrence, a topic which we find covered in the course curriculum. When added to their pre-existing views on punishment, deterrence would advise harsher sentences in order to reduce crime by increasing its expected costs. We see from course archive documents that criminal deterrence was a topic explicitly covered by the Manne program. With many instructors like Milton Friedman against the drug war, the fact that we find no increase in sentencing harshness for drug crimes is consistent with our hypothesis.

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, ideas have consequences. In Section 6 below, 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).

The contribution of this paper can be situated into four literatures. First, previous work on the spread of policy ideas has studied the effects of political advertising and partisan media on voting and other outcomes (DellaVigna and Kaplan, 2007; DellaVigna and Gentzkow, 2010; Enikolopov et al., 2011; Spenkuch and Toniatti, 2018; Galletta and Ash, 2020). While law and economics is a specialized jargon, and the Manne program was aimed at sophisticated and expert judges, it was nevertheless associated with the economic conservatism of the 1970s and 1980s Republican party. In this spirit, a more recent paper, Cantoni et al. (2017), analyzes a staggered Chinese curricular reform which caused students (as intended) to be more skeptical of free markets. Since judges vote on outcomes, we can compare our estimates to other ideological information interventions that have altered partisan voting behavior. Unlike voting, however, we can document a direct policy impact as what these judges decide is law. Unlike election or legislative outcomes, moreover, judges write extensive judicial opinions documenting their reasoning. We can thus observe directly whether patterns of reasoning are altered by the program.[3] This dimension of the contribution is also related to the broader literature on the spread of knowledge, which includes not just policy ideas but also technology. For example, Brownson et al. (2017) explore the diffusion (or lack thereof) of scientific ideas into medical practice.

Second, we build on a literature in behavioral and experimental economics that studies the effects of economics training on normative beliefs and social preferences. The closest paper, by Fisman et al. (2009), uses random assignment of law students to law professors to look at whether exposure to a professor with an economics PhD (as opposed to no PhD, or a PhD in Philosophy or History) has an impact on social preferences. The paper found that students exposed to an economics-trained professor behaved less pro-socially in lab experiments 1 and 3 years later. Meanwhile, 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).[4] and. Economics professors

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[3]A recent paper by Hjort et al. (2019) randomizes informing mayors in Brazil about the results from economic policy experiments and finds that mayors update beliefs and alter policies in response to information about experimental results. Azgad-Tromer and Talley (2017) analyze a finance training program for utility regulators and find that after the program, the regulators set pricing more in line with standard asset pricing theory.

[4]Bleemer and Mehta (2020) find using a regression discontinuity that economics majors tend to

are less ideologically liberal and less likely to be registered Democrats (or contribute to Democratic candidates) than the other social sciences (Jelveh et al. 2018). Our paper builds on these papers, as well as others that are more qualitative (Hirschman 1978, 1991), by looking at the effect on established professionals (judges), and by looking at high-stakes decisions in real-world courtrooms.[5]

The third literature we relate to is a burgeoning one on text-as-data (Gentzkow et al. 2017). Jelveh et al. (2018) classify academic economics text as conservative or liberal using the political donations of authors, finding (for example) that *Journal of Law and Economics* consistently ranks as right-wing. Related work on congressional speech includes Gentzkow and Shapiro (2010), Jensen et al. (2012), Ash et al. (2017), and Gentzkow et al. (2019). Our text measures are distinct from these papers, which measure partisanship, as we measure the influence of economics reasoning. In addition, judges have limited control over their caseload, which holds the topic of discussion constant (unlike Congress, where speakers can choose what they talk about).[6] 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. (2020).

The fourth related literature concerns constitutional constraints on policymaking (Seabright 1996; Besley and Coate 1997) and the importance of ideas versus institutions in determining policy (Romer 2002; Rodrik 2014). For example, the expansion of economic regulation is one hallmark of the modern administrative state. Yet the effect of this sort of state power on American government and society is not well understood (Hamburger 2014). Judges vary widely in their approach to law, and decisions are correlated with characteristics (Stephenson 2009; Ash and MacLeod 2015). But an open question is whether the judges are *per se* biased for particular outcomes, as opposed to following different legal ideologies (Posner 1973; Cameron 1993; Kornhauser 1999). For example, a judge might in principle employ a strict interpretation of the Constitution, while not necessarily hewing to the preferences of political parties for specific outcomes. Quantifying the role for legal schools of thought – such as law and economics – is a key contribution of this paper.

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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.

[5]We also contribute to a broader literature on the impacts of training on professional decision-making. For example, Giorcelli (2019) finds that management training increased performance in Italian firms.

[6]We also contribute to a large legal scholarship that conducts textual analysis including but not limited to the question, "Do judge writing styles matter?" raised by Judge Richard Posner (1995).

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

2.1 Background

Three canonical examples from contracts, torts, and criminal law illustrate the potential impact of economic thinking. In contracts law, what used to be a general duty to keep promises became "efficient breach theory". The latter is the idea that walking away from a contract should not be penalized, beyond compensating the aggrieved party, so long as it is economically efficient to do so (Goetz and Scott, 1977). 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. This principle can also carry into criminal law, where 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.

In a common-law system, judges have significant discretion in their decisions. Therefore, a judge taking economic ideas to heart may end up coming to very different outcomes in their judging, despite working within shared legal rules.[7] This discretionary aspect of judging leaves scope for ideas, such as economics, to have an effect on judging. For example, in criminal cases, if a judge figures that the cost of detecting more crimes is higher than the cost of increasing penalties, the judge may intensify penalties in an effort to reduce crime rates.

The application of such ideas – an economic approach – to law went from the fringe to the mainstream in the latter decades of the twentieth century. Teles (2012)

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[7]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.

provides a detailed history of the conservative legal movement, and the role of law and economics in particular. This intellectual community and movement has advanced the application of economic principles to jurisprudence and prioritizing economic efficiency (Posner, 1987). It has been used not only as a set of normative principles for judges to follow, but also to provide positive explanations for past jurisprudence. As documented 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.

Economics has diffused into almost all areas of law (Posner, 1987). Looking at U.S. judicial opinions, Clarke and Kozinski (2019) find that the use of economics terms increased in the 1970s and was most prominent in the 1980s. Ellickson (2000) documents that law and economics has also grown in importance in legal scholarship. In 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). Almost all environmental regulations can be construed as a form of government expropriation that limits how property owners can develop their property (Blumm 1995).

Reliance on economic analysis in antitrust has attained nearly complete consensus (Ginsburg 2010). 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. In the recent time period, judges who attended law-and-economics training were less likely to have their antitrust decisions appealed (Baye and Wright 2011).

A more controversial branch of law and economics is the use of incentives reasoning in criminal law. In Becker's (1968) analysis of crime and punishment, the notion of "rational criminals" works against the then-prevailing wisdom of crime as a product of mental illness or other non-economic circumstances. This change in perspective motivates deterrence (more police, more punishment) rather than rehabilitation (more social services and mental health treatment) as the preferred focus

of crime policy. The theory of optimal deterrence laid out by Becker suggests that severity of punishment can make up for low probabilities of detection.[8] Raising the probability of detection (more policing) is socially costly, while severity of punishment is relatively cheap. While cash fines are preferred over punishments, many criminal agents are liquidity-constrained and therefore additional punishments are required. Arguably, deterrence theory has provided some justification for the massive build-up of prisons since the 1980s.[9] 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).

Given its implications for labor, environmental, antitrust, and criminal law, it is not surprising that early law and economics is perceived as ideologically conservative. Yet none of the ideas or modeling approaches were outside the bounds of mainstream economics. This intellectual movement comprised a broad academic, political, and legal effort, which ended up influencing not only law schools but also judging and policymaking.

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. The program was founded in 1976 as a 2-3 week economics course for district and circuit judges. The institute began at the University of Miami in 1974, then moved to Emory University, prior to its current location at George Mason University. The course was founded and organized by Henry Manne, an influential conservative in the early law-and-economics movement. The institute was the the flagship program

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[8]To quote Becker (pg 17): "an increased probability of conviction obviously absorbs public and private resources in the form of more policemen, judges, juries, and so forth. Consequently, a compensated reduction in this probability obviously reduces expenditures on combating crime, and, since the expected punishment is unchanged, there is no 'obvious' offsetting increase in either the amount of damages or the cost of punishments. The result can easily be continuous political pressure to keep police and other expenditures relatively low and to compensate by meting out strong punishments to those convicted."

[9]Generally, 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.

Figure 1: Share of Cases with Manne Judge on Panel, 1950-2013
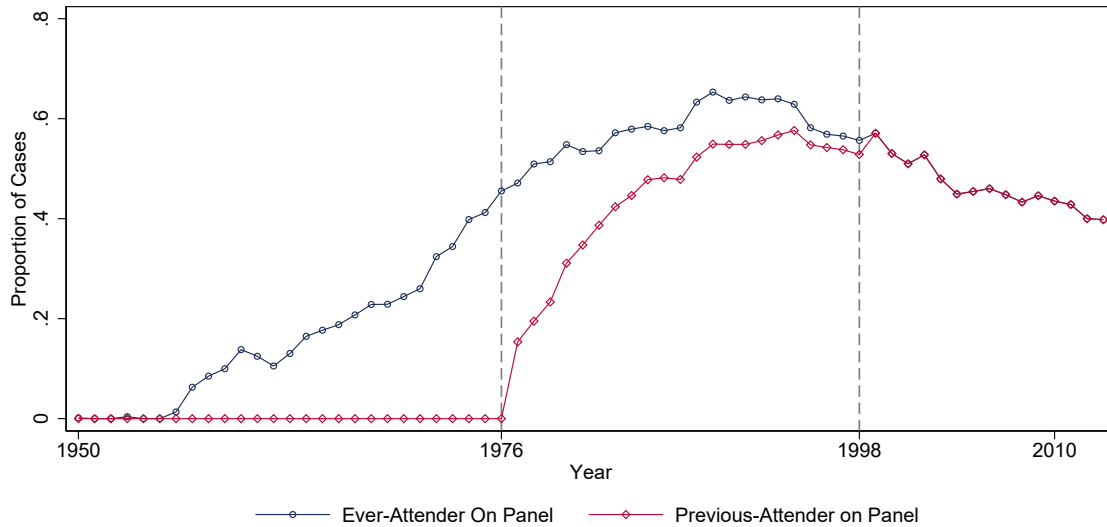
Notes. Share of cases with a Manne judge on the panel, plotted by year. Blue line gives judges who ever attended; red line gives judges who have already attended.

of the Law and Economics Center, the first academic research center devoted to law and economics. It was funded mainly by donations from conservative foundations and business interests.[10]

An excellent summary of the program is provided by Butler (1999), written by a former director. The course ran continuously, with courses running once or twice a year, until 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.[11] This means, importantly, that there was no selection of particular judges for attendance on the side of the program organizers.

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[10]"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.

[11]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).

On the judges' side, the program was almost universally popular among 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), further evidence against selection into timing of attendance due to short-run shifts in judge preferences about economics. By 1990, forty percent of federal judges had attended this program.[12] 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.

In the Appendix, we provide extensive qualitative evidence on how the program was perceived by the public and the judicial participants, along with extensive quotations from judges 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. Covered 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 statistics, and basic regression. The main reading material 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. The material on criminal law was based on the Becker model and deterrence theory. There was no material on behavioral economics nor more sophisticated law and economic theories, such as over-deterrence, according to the syllabi listed in (Butler 1999).

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. The former director Henry Butler (personal communication) writes: "Samuelson [lectured] on whatever the heck he wanted to, usually personal investment strategies; Friedman

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[12]Hundreds of judges attended, despite "being swamped with criminal cases ... and not seeing the relevance of economics" (Butler 1999). We will see below that economics did in fact impact sentencing.

always started on legalization of recreational drugs; Ashenfelter used climate to predict quality and prices of wine, followed by wine tasting." It is clear there was an effort to teach standard econometrics in a relatively enjoyable environment. Notwithstanding this balanced list of instructors, the instruction itself was more emphatically delivered by the conservative instructors. As George Priest, a regular participant, 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).

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...*" Judge Williams included the first diagram of marginal- and average-cost curves in an opinion right after attending the program.

More background detail on the Manne Program is reported in Appendix A. To summarize, our treatment for exposure to law and economics ideas is a two-week, eight-hours-a-day intensive economics course attended by almost half of federal judges over the span of twenty years. The program had a recognized conservative bias, yet the attending judges were effusive in their praise regardless of ideological standpoint. What was the impact on how judges made decisions in their cases?

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). Judges are appointed by the U.S. President and confirmed by the U.S. Senate. 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.[13] Judges have life tenure.

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.[14] 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.[15] For the years 1992 through 2003 (used for the within-judge event study), there are approximately 1.03 million cases. For the years 1999 through 2011 (used for analyzing the effect of discretion provided in *Booker*), there are approximately 856,000 cases.

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

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[13]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.

[14]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.

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

of birth, and education.[16]

Manne Program Attendance. To the FJC data we have added the record of attendance by all federal judges to the Manne program. Butler (1999) contains a list of all the judges that had attended through 1998, when the program as such ended (other economics trainings continued but were on more specific topics, e.g. antitrust, or were smaller in scale, e.g. 2-3 day workshops). We supplemented this list with 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. Specifically, we use 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 phrases that are characteristic of the use of economic analysis in legal contexts. We take the words from these phrases for constructing the measure.[17]

With the index in hand, we next draw on recently developed methods in neural nets applied to natural language. Following the method in Arora et al. (2016), we construct semantic vectors for each case. This method starts by training Word2Vec embeddings on the corpus, a word vectorization algorithm in which words that are semantically related tend to be located near to each other in the vector space (Mikolov et al., 2013). We then represent a judicial opinion as the average of the vectors for each word in the opinion.[18] Meanwhile, we take the average of the word vectors from the Ellickson Lexicon to identify the law-and-economics dimension in language. The

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[16]See Appendix B for the enumerated list.

[17]Ellickson used the following Lexis 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, effidency, deterrence, benefit, benefits, capital, market, markets, marketplace, economic, economics.

[18]As in Arora et al. (2016), we use a weighted average: the word vectors are weighted by each word's inverse corpus-wide frequency. This means that rare, more informative, words are upweighted in constructing the vectors.

set of words that are closest to the Ellickson Vector are shown in Appendix Figure A.3, where the size of the word corresponds to the closeness to the vector.

We then compute the cosine similarity of each case vector to that economics embedding dimension in economics-related cases. In Appendix Section D.1 we show example sentences that rank highly on the measure; they are all related to economics and most are applying economic reasoning. For the regression analysis, we include only the author of the case, and not the other two co-panelists.

In Appendix D.2 we describe 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

Our primary outcomes will be proxies for conservative judicial opinion.

Conservative Judicial Decisions. Our first measure of conservative judicial opinion is a hand-coded measure of decision direction from the Songer-Auburn database (e.g. Songer and Tabrizi 1999). This is a 5% random sample of Circuit cases, available until the year 2002. The sample is hand-labeled for vote valence: liberal, conservative or neutral/hard-to-code. For example, a conservative vote includes rejecting the defendant in a criminal procedure case, rejecting a plaintiff asserting violation of First Amendment rights, and rejecting the Secretary of Labor who sues a corporation for violation of child labor regulations.

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. 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 the case.

In particular, we look at regulatory cases where the government is a party to the

Figure 2: Increasingly Conservative Rulings in U.S. Federal Courts
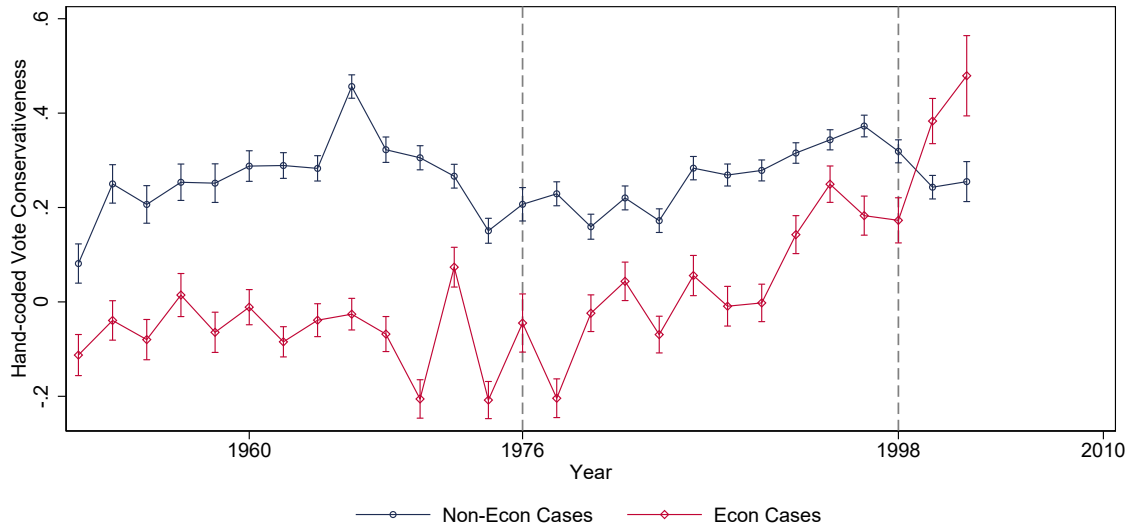
Notes. Average conservative vote rate circuit courts using 5% hand-coded Songer Auburn data, plotted by year and separately by economics and non-economics cases. Error spikes give standard error of the mean. Data weighted to treat judge-years equally.

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 a sign of conservative decision-making.

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). A conservatism measure is the rate at which these antitrust cases are decided against the claimant. More detail on this process is included in Appendix F.

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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.[19] 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

Our identification strategy relies on a parallel trends assumption. As formalized below, we estimate the within-judge effect of Manne attendance on decision outcomes, relative to colleagues on the same court. This section provides information on the internal validity of the research design. Additional information is reported in Appendix C.

4.1 Identification

A major concern in an empirical analysis of the Manne program is endogenous selection into the program, both in terms of the type of judge and, within-judge, the timing of attendance. As discussed in Butler (1999), there is no 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 such selection is minimal. Because attendance was first-come-first-serve, and the program was often oversubscribed, the later applicants were bumped to the next class. This context reduced opportunities 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

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[19]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.

wait longer to attend.

More formally, 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 (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 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.

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. In the regressions, we will adjust for the elastic-net-selected characteristics, fully interacted with year fixed effects. For example, we allow judges born in the 1940s to have a different intercept in each year.[20]

Besides endogenous selection into the program, we are also concerned about endogenous selection of judges to cases. Fortunately, in our setting there is quasi-random assignment of cases.[21] In Circuit Courts, almost all cases are randomly

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[20]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 conservatism 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 are the same.

[21]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).

assigned to a panel of three judges.[22] In District Courts, cases are randomly assigned to judges within the same courthouse. In the circuit panels, one judge among the three is chosen to author the opinion. Authorship is determined by the most senior judge on the case (in terms of years on the court), or the chief judge. When there is a dissent on the panel, the senior judge in the majority assigns the opinion.

Previous work has assessed judge randomization through interviews of courts and orthogonality checks on observables. For example, Sunstein et al. (2006) code 19 characteristics determined by the lower court for a sample of gender-discrimination cases and find that case characteristics are uncorrelated with judicial panel composition.[23] However, Levy and Chilton (2015) take a more rigorous approach and find nonrandom assignment for four circuits (2nd, 8th, 9th, and D.C.). The approach in Levy and Chilton requires data on the case calendars, which they obtained for the years 2008-2013. Unfortunately that data are not available for most of our time period (1970-2005), so we cannot check directly for nonrandomness using the Levy-Chilton method. Still, we show that our main results hold when limiting to the circuits for which they found randomness (Appendix Figure A.11).

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.2 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.

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[22]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.

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

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 be both more/less conservative and more/less likely to attend the Manne program.[24] $\lambda_t X_j$ 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 from estimating

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

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). 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 controls for experience, and the elastic-net-selected controls interacted with year, that adds reassurance that there are not confounding judge-level factors driving the results.

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[24] We obtain similar results using fixed effects for years of experience.

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.

Our setting features a staggered treatment, where different treated judges attend the program at different times. In this case, the standard two-way fixed-effects estimator might not capture the average treatment effect on the treated due to the potential for negative weights when previously treated judges are present in the comparison group (e.g. Goodman-Bacon, 2018). We perform the diagnostic method from De Chaisemartin and d'Haultfoeuille (2020) and show that there are no negative weights in our two-way fixed effects regressions (Appendix Table A.7). Therefore our estimates can be validly interpreted as average treatment effects.

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. The steps taken to construct the measure is described in Subsection 3.2 above.

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).

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 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.

The event study graphs report statistics from three specifications. First, the leftmost specification (blue circles) reports the baseline with judge fixed effects and circuit-year fixed effects. Second, the middle specification (red diamonds) reports the baseline with additional controls for judge years-of-experience. Third, the right-most specification (green triangles) additionally adds the 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

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.

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 that the comparisons include a cross-sectional dimension where never-attenders enter the circuit-year fixed effects as a control group.

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).

The embedding effect is a zero when looking at the whole career for Manne judges (Column 4), and when looking at the full sample including never-attenders (Column 5). One interpretation of these zeroes is that all judges in the courts are being exposed to economics ideas and language, separate from the Manne program. As shown in Appendix Figure A.5, it turns out that never-attenders actually score higher on economics language and were positively trending in the early years of the Manne program, possibly due to younger judges being more influenced by economics. Law and economics was not only transmitted to judges by the Manne program; it was promoted in the legal academy and legal scholarship, as well as in the popular discourse. With language, it is possible that economics ideas can diffuse costlessly. In particular, an effect of the law-and-economics movement, of which the Manne training program was just a part – one that is difficult to isolate – could be that economics language diffuses to never-attenders.

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).

Figure 4: Effect of Manne Program on Conservative Voting
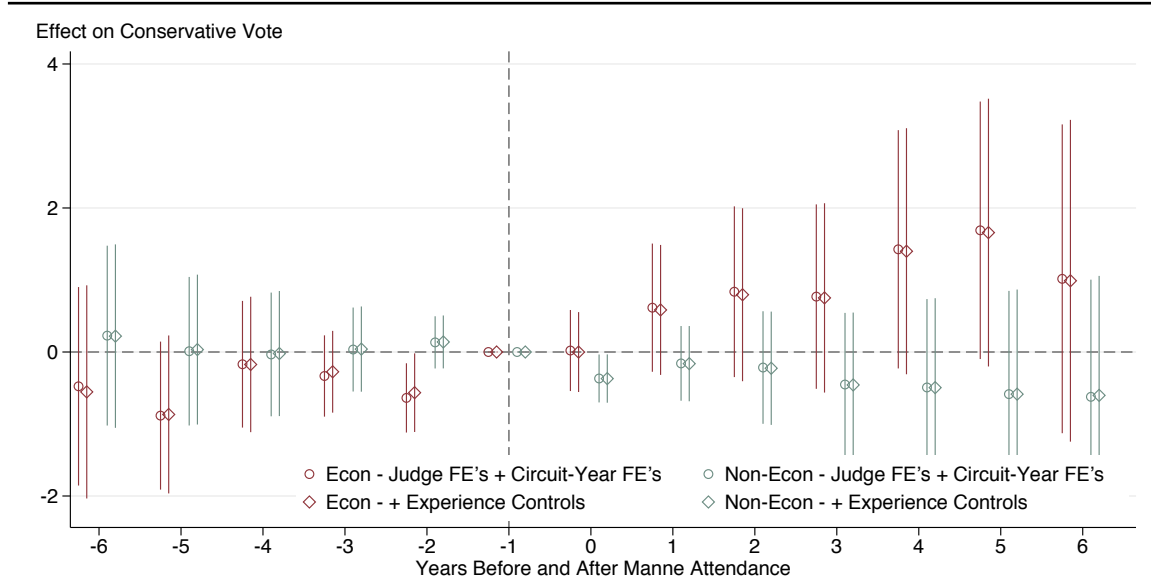
Notes. Event study effect on conservative vote in economics cases (regulation and labor; in red) and non-economics cases (in teal). Baseline specification (left dot in pair) includes judge and circuit-year fixed effects. Second specification (right dot in pair) includes controls for judge experience. 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.[25]

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

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[25]We do not include a third 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. In neither group of cases do we see significant pre-trends.

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

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
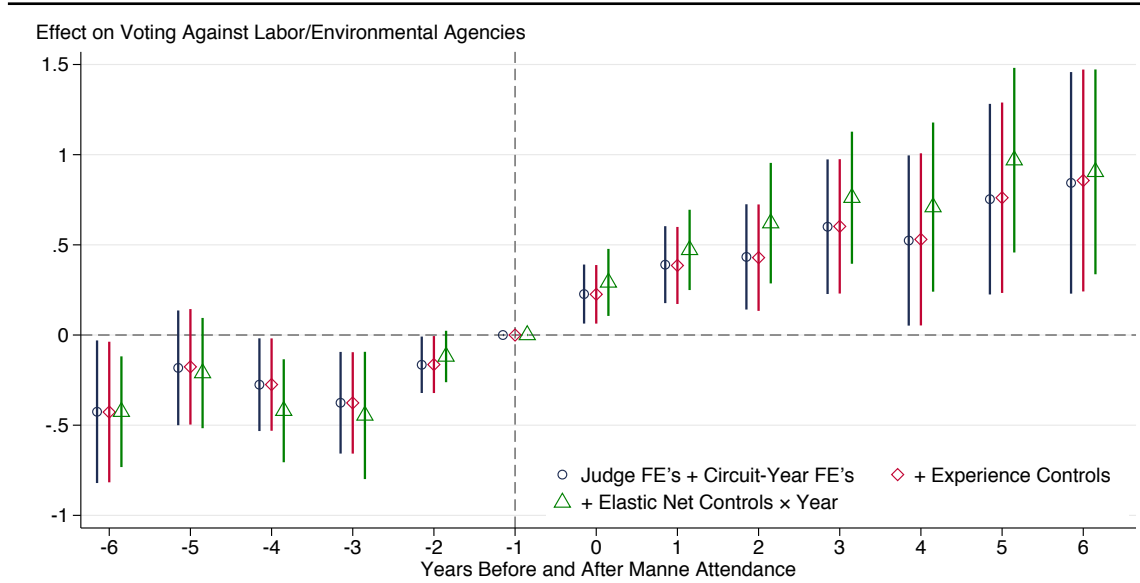
Notes. Event study effects on voting against government agency on labor and environmental issues, relative to year before attendance at Manne economics training. The baseline specification (blue circles) includes judge and circuit-year fixed effects. Additional specifications add experience controls (red diamonds) and 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.

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 conservative regulatory votes as the outcome. As with the language outcomes above, we report three specifications: the baseline (blue circles), experience controls (red diamonds), and elastic net controls interacted with year (green triangles). We see that Manne-trained judges exhibit a sharp and significant increase in propensity to vote against federal labor and environmental regulatory agencies. The effect is quite robust to the alternative specifications.

An important caveat is a significant negative estimate three years before atten-

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

dance. But this is a decrease in conservatism 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.9.

The regression results for Equation (1) are reported in Table 3. In the event study sample, there is a positive effect on conservative regulatory 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.

5.4   Effect of Economics Training on Antitrust Decisions

We look at the effect on conservative decisions in antitrust cases, which we interpret 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.[26]

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.10). 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.

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

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[26]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 Enfrocement				
	(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 conservative voting 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. $+p < .1, *p < 0.05, **p < .01$.

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 three specifications: the baseline (blue circles) includes judge and courthouse-year fixed effects, while the additional specifications add judge-experience controls (red diamonds) and elastic net selected judge characteristics (predicting time of attendance) interacted with year fixed effects.

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 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.

Figure 6: Effect of Manne Program on Criminal Sentencing Harshness

(a) Any Prison Given

(b) IHS Prison Sentence Length
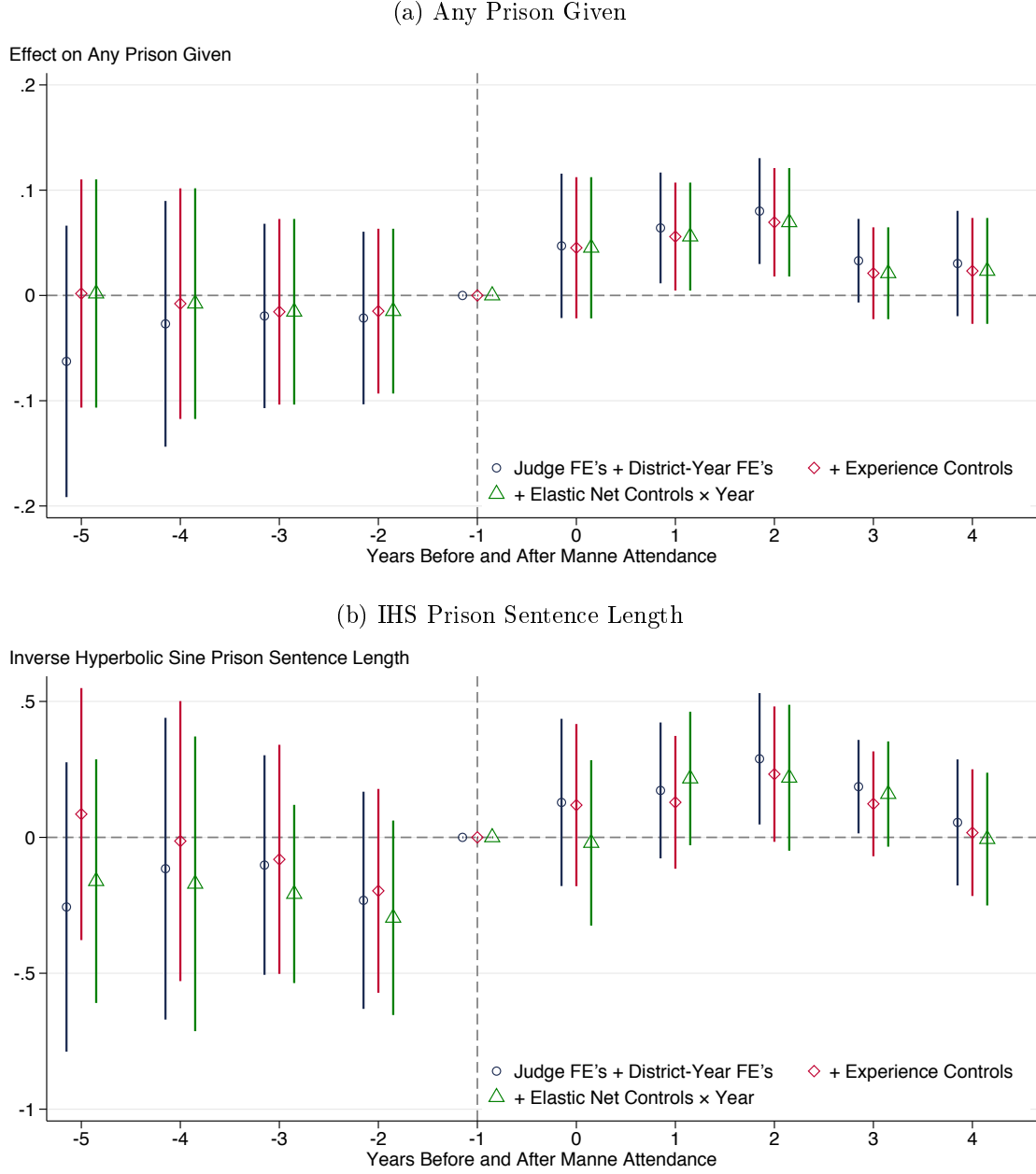
Notes. Event study effect of Manne attendance on criminal sentencing outcomes in district courts, 1992-2003. Panel (a): Outcome is any prison given. Panel (b): Outcome is inverse hyperbolic sine of prison sentence in days (plus one, to allow for zeros). Regressions include judge and district-year fixed effects (blue circles), plus 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.

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.

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 magntitude 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.

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 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.

Figure 7: Effect of Manne Program on Sentencing under Higher Discretion

Any Prison Sentence

(a) Any Prison Given

Inverse Hyperbolic Sine Sentence Length
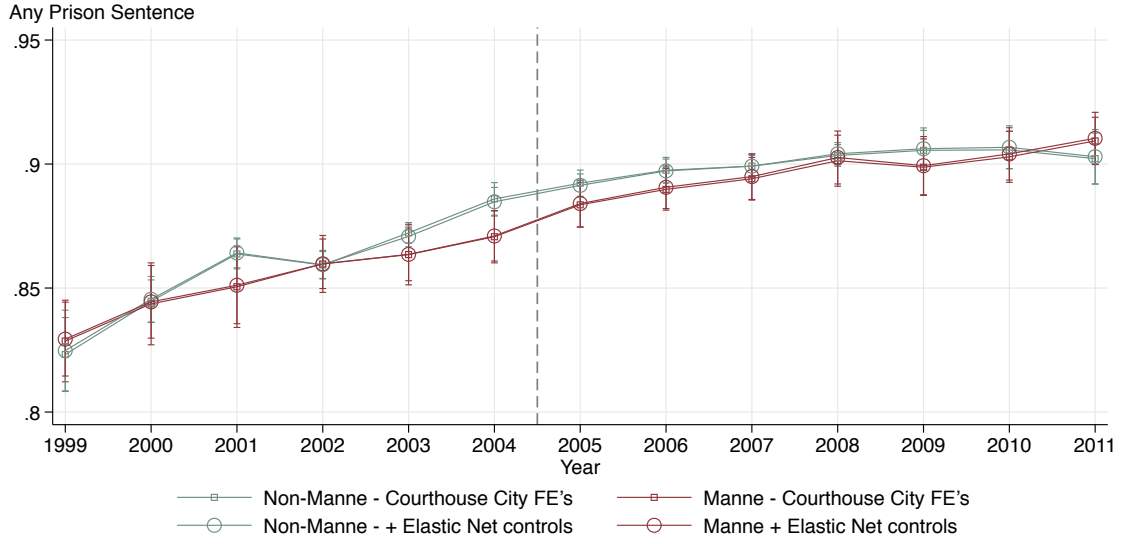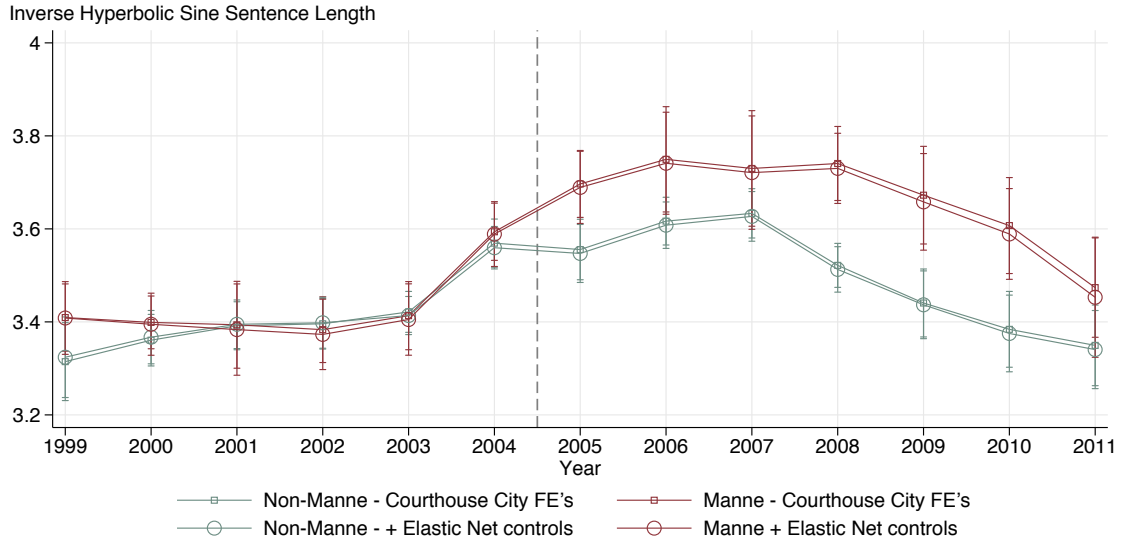
(b) IHS Prison Sentence Length

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 ($\geq$2005)	0.0350**	0.0681	-0.0815	0.105*	0.126**	0.187*
	(0.00504)	(0.0601)	(0.0528)	(0.0485)	(0.0366)	(0.0760)
Econ Training	-0.00141	-0.0319	-0.0287		0.0306	-0.0609
	(0.00725)	(0.0417)	(0.0388)		(0.0339)	(0.0556)
Econ Training *	0.00887	0.154*	0.129*	0.117*	-0.0470	0.196**
Booker ($\geq$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.

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. 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 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.[27] 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

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

published in recent decades. We find no effect of Manne attendance on a scholarly style (Appendix Table A.9), consistent with an economics approach mattering more than an academic approach.

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.[28] We find no effect on this outcome (Appendix Table A.9).

Next, we produce some additional measures of conservative legal reasoning. In Appendix Table A.10, we look at the citations choices of judges. In particular, we ask whether after Manne attendance judges tend to cite opinions written by circuit court judges nominated by Ronald Reagan or George H.W. Bush. There is no effect on this measure.

Besides citations, another important 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.10 that there is a positive effect on this measure.

From an econometric perspective, an important 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.11 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.12).

We undertake an array of robustness checks to ensure that our results are not sensitive to specification choices. All results are robust to alternative clustering of standard errors, including two-way clustering by judge and panel, and two-way clustering by judge and court-year. The post-*Booker* sentencing effects lose precision in unweighted regressions (rather than weighting to treat judge-years equally). Results are also robust to dropping judges with the smallest and largest caseloads.

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.12). In the criminal results, adding

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

fixed effects for the associated crime type (345 categories) tends to strengthen statistical significance (Appendix Figure A.13). 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.9), 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.

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. 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 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 non-attenders in economics cases ($y_0 = 0.55$). The resulting persuasion rate is $p = 22$ percent. This rate is comparable to that estimated for other interventions that shift partisan vote share, such as the effect of Fox News estimated by DellaVigna and Kaplan (2007) ($p = 11.6$ percent) and 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 attractive environment with credentialed experts, plus long-term intellectual attachments and subsequent informational mailings from the Law and Economics Center, make the somewhat larger magnitude plausible.[29]

To put this another way: 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.

These regression estimates only account for own-attendance. If peers and precedent also impact the non-Manne judges, then the true Manne impact may be even larger. However, the Manne program was just one part of the broader law and economics movement; many of the effects we detect may have occured, perhaps with a lag, even without this particular program. In line with this point, note that the specification including never-attenders (using across-judge variation) is usually weaker than the event-study effect (using only the within-judge variation). One interpretation of this difference is that law-and-economics ideas were already influencing the Manne attenders' peer judges. These ideas could be coming not just from the Manne program but from legal academia, the media, and other sources. Economics might already be influencing many judges, so comparing Manne attendees to never-attenders partly crowds out the effect.

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[29]These magnitudes, interpreted as an educational program, are comparable to those reported in Cantoni et al's (2017) study of a curriculum change in China.

6.2 Mechanisms

We can only make limited inferences about the specific informational or behavioral mechanisms driving the changes. One interpretation is that the Manne program simply provided information about the economic costs and benefits of various decisions, improving the rationale and direction of economic judgments. 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. Economics training is a compressed, highly informative signal about the incidence of many different types of policies. The attending judges could have been drawing on this training for many years, with the overall quality of judicial decision-making going up. Some evidence against this interpretation is that the forward citation rate by future judges does not increase after Manne attendance (Appendix Table A.11).

By other measures, however, the program arguably increased judicial professional performance. Appendix Table A.12 shows that Manne attendance of district judges appears to have increased the probability of promotion to higher appellate courts. On the other hand, we also see (Columns 4 and 5) that these promotions are only significantly different for Republican appellate nominees. We do not know whether this promotion effect is due to improved judicial decision making, or rather a partisan affinity between Republican administrations and the conservative economic jurisprudence promoted by the Manne program.

Another way to interpret the results is through a Bayesian persuasion model (Gentzkow and Kamenica, 2011). We have a motivated agent (a Manne-approved economics professor) who can conduct an economic analysis of a given legal issue and send the result to a rational Bayesian welfare-maximizing decision maker (the judge). The economist's expert analysis corresponds to choosing a signal structure with commitment: Regardless of the true state, the economist is bound (perhaps by academic or scientific norms) to reveal the results of the analysis. In the relevant example from Gentzkow and Kamenica (2011), the agent will choose either a completely informative signal or none at all.

In that case, 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. This shift can happen precisely because the economist is committed to revealing the signal generated by the economic analysis. If the economist did not tie their hands with the signal structure, they would not be able to influence the

rational judge as effectively.[30] 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.

In simple versions of Bayesian persuasion, the average beliefs of the receiver are unchanged. Our results on significant shifts in opinion language go against this implication and suggest changes in beliefs (in addition to decisions). On the other hand, it could be that judicial language is changing despite beliefs remaining constant. Economics instruction could alter the costs of thinking (and therefore writing) about different outcomes, e.g. business and economic costs, by making them more salient (without changing the underlying mental model). We lack a direct measure of beliefs (e.g. an incentivized survey) to rule this mechanism in or out.

A notable feature of this empirical context, peculiar to common law justice, is that judges' decisions form precedent and are followed by other judges. Therefore judges rule in the shadow of precedents established by previous rulings. In Gennaioli and Shleifer's (2007) model of the common law, for example, judges can alter previous decisions at a cost but want to avoid having their decisions altered. In this respect, the Manne program could have protected judicial opinions from appeal by giving judges literacy in economic analysis (Baye and Wright, 2011), while favoring particular outcomes. The exuberant reflections on the program in the judges' thank-you letters could support this view. The precedent mechanism also helps explain why inviting judges to participate in groups would make the program more effective: 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. If the Manne program increased probability of promotion, that would further amplify this effect. Notably, this mechanism would even apply in the Supreme Court, as economic analysis could fortify a precedent against being overturned by future cohorts of Justices.

Beyond rational explanations of our results, a variety of cognitive biases could be at play. For example, if judges are vulnerable to confirmation bias (where judges selectively pay attention to arguments that confirm what they heard in the class), motivated beliefs (Benabou 2007), or information processing costs, they may overweight the information provided during the Manne program. Any of these factors (or others) could make the simple economic arguments taught in the Manne program

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[30]More likely, an uninformative babbling equilibrium could emerge, as in the analysis of cheap talk by Crawford and Sobel (1982).

readily deployed in judicial decision making, and could strengthen other mechanisms.

Our findings might also be explained by social preferences and social learning. Indeed, the Manne 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. After jointly absorbing and discussing the same material over an intense 2-3 week period, judges might be readily susceptible to social learning subject to persuasion bias (DeMarzo et al., 2003). That is, they may forget that fellow judges applying economics are not independent observations. New colleagues and friends may have been formed at the Manne seminars, cementing shared beliefs gained there. 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 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. These social factors could have strengthened the mechanisms discussed.

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 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 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.

This research opens the door to many new questions. The Manne program is part

of a broader movement and set of organizations that uses conservative economics to pursue pro-business policies and outcomes. More evidence could be produced on the motivations, actions, and impacts of this movement. One could start with information on the Manne program's financial donors, for example.

Outside of the Manne program. the effect of economics training likely depends on the content of the coursework. Economics training of judges may have changed since the Manne program's heyday, reflecting the rise of information asymmetries, behavioral economics, and empirical methods. Understanding the heterogeneity in ideological impacts of economics remains an open question. In the judiciary, some other important schools of thought are Originalist jurisprudence, which tries to give constitutional language its original intent, and textualism, which gives special weight to the plain-text meaning of legal language. Beyond these conservative approaches, it could also be fruitful to explore left-wing frameworks, for example from the Critical Legal Studies community. Future work could use the citation network to explore the genealogy of legal ideas and what makes them spread.

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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].[31]

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.[32]

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

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[31]"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.

[32]"19 U.S. Judges Study Economics to Help Them in Work on Bench"

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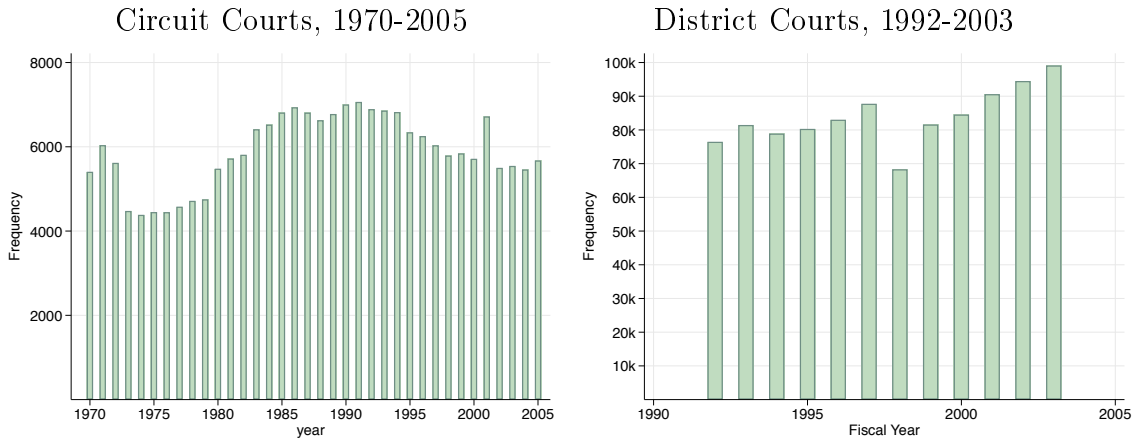
Notes. Number of case observations in the circuit courts (left panel) and district courts (right panel) in main analysis samples.

B Data

Figure A.1 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,[33] Federal Judicial Center, and previous data collection.[34] 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,

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

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

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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.2 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.2: Manne Program has no Effect on Assignment to Economics Cases

(a) Probability of Sitting on Economics Cases

(b) Probability of Authoring Economics Cases
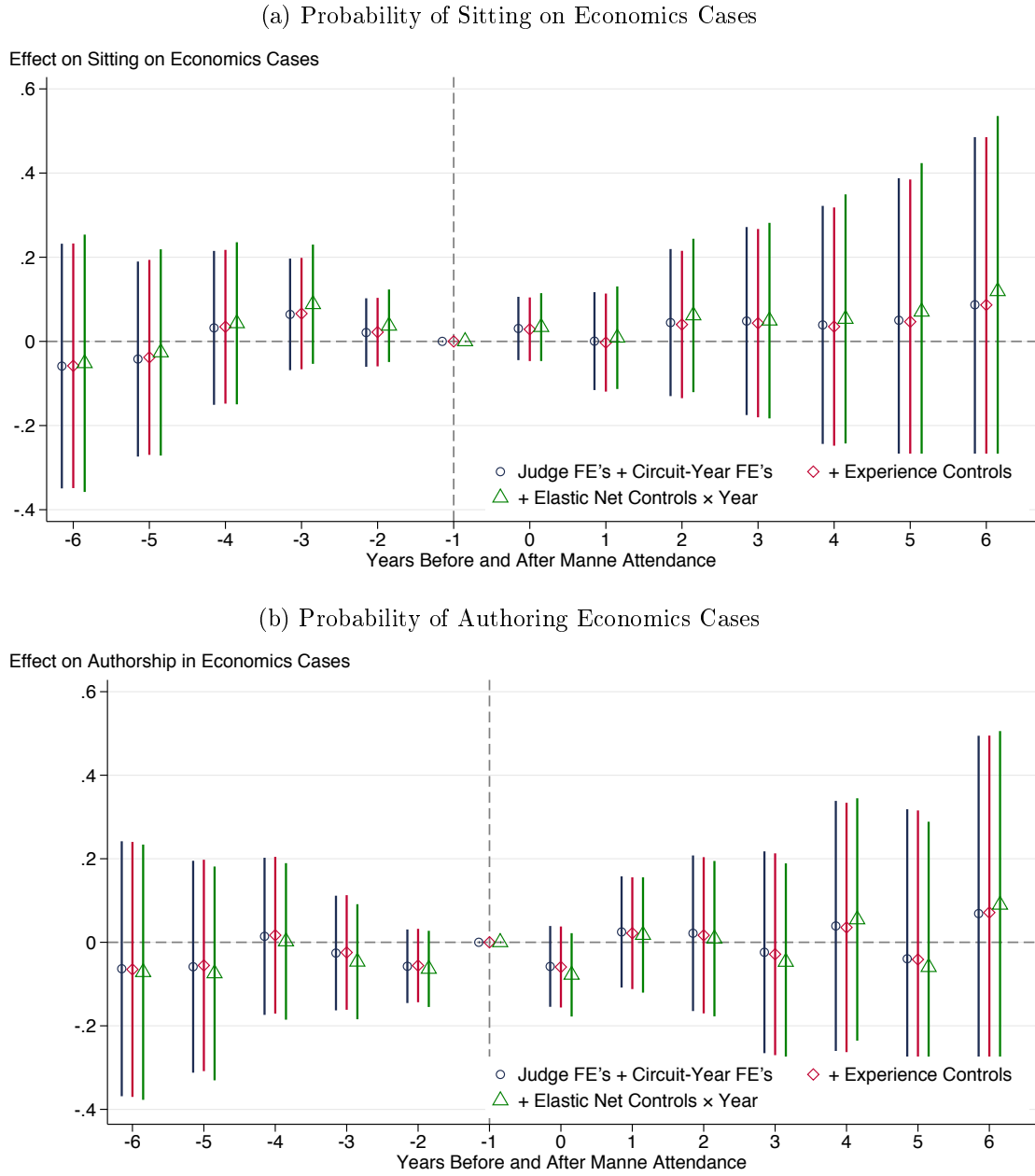
Notes. Event study effect of Manne attendance on working on economics cases. Panel (a): Probability of sitting on economics-related cases. Panel (b):Probability of authoring economics cases. Regressions include judge and circuit-year fixed effects (blue circles), with additional specifications adding 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 Aginst 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.

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

	LATEs with Positive weights	LATEs with Negative weights	LATEs with Positive weights	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

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).

C.3 Checks for Negative Differences-in-Differences Weights

De Chaisemartin and d'Haultfoeuille (2020) provide a method for diagnosing the presence of negative weights in staggered two-way fixed-effects (TWFE) regressions. In the paper, they show that the TWFE estimator can be decomposed as a weighted average of "the local average treatment effect (LATE) of the 'switchers' in each group and at each period, where 'switchers' are units that experience a change in their treatment between two consecutive periods." 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. We can see that for almost all treated units ("LATEs"), the weights are positive. According to De Chaisemartin and d'Haultfoeuille (2020), therefore, we can safely interpret our linear regression estimates as capturing the average treatment effect.

Figure A.3: Words Correlated with Law-and-Economics Lexicon Dimension

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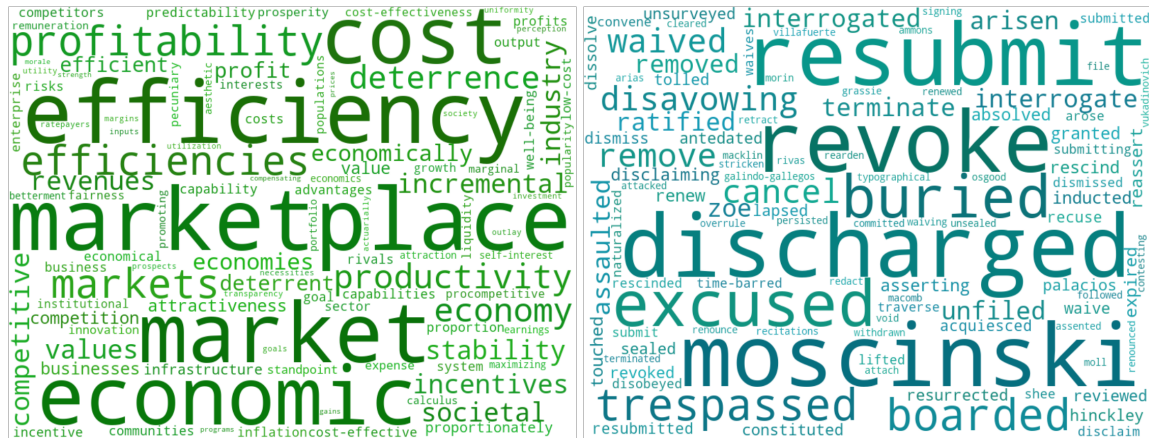
Notes. The left word cloud lists the set of words that have the highest cosine similarity to the average word vector for Ellickson phrases in the word embedding space. The right word cloud gives the words that have the lowest (most negative) cosine similarity to this vector.

D   Additional Material on Judge Writing Style

D.1   Embedding Similarity to Ellickson Lexicon

Figure A.3 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.4: Trends in Economics Language, by Econ and Non-Econ Cases
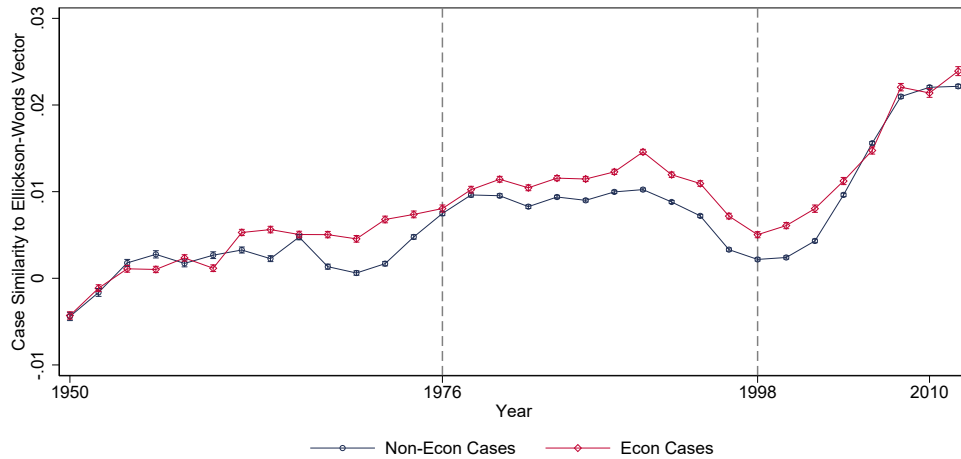
9. Reducing the number of interchanges and reducing the average length of haul have no economic significance in themselves, though both might reduce average transit time, which would be a benefit to shippers and hence a genuine efficiency gain

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.4 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.5 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

Figure A.5: Trends in Economics Language, by Manne Attendance
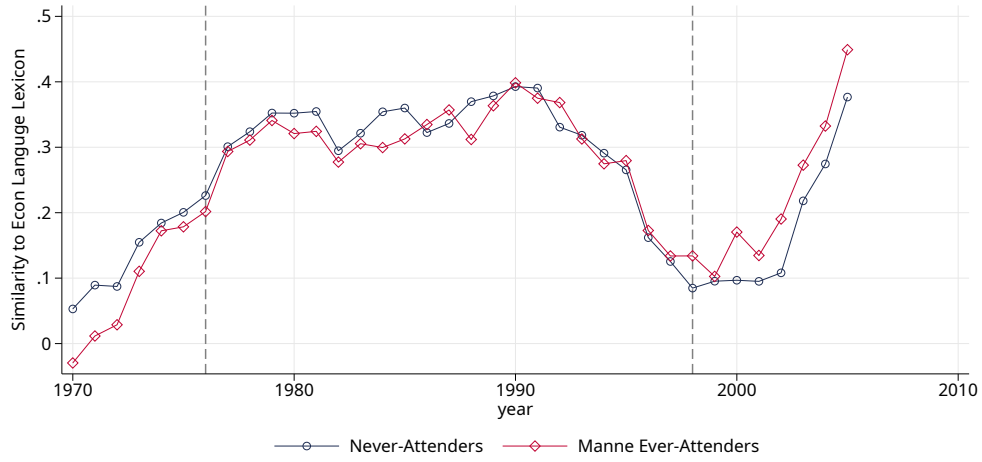
Notes. Average embedding similarity to Ellickson law-and-economics lexicon, plotted by biennium and separately by economics cases (regulation and labor) and other cases. Error spikes give standard error of the mean. Data weighted to treat judge-years equally.

on average.

Figure A.6: Calibration Plot for Predicted Econ-Related Case
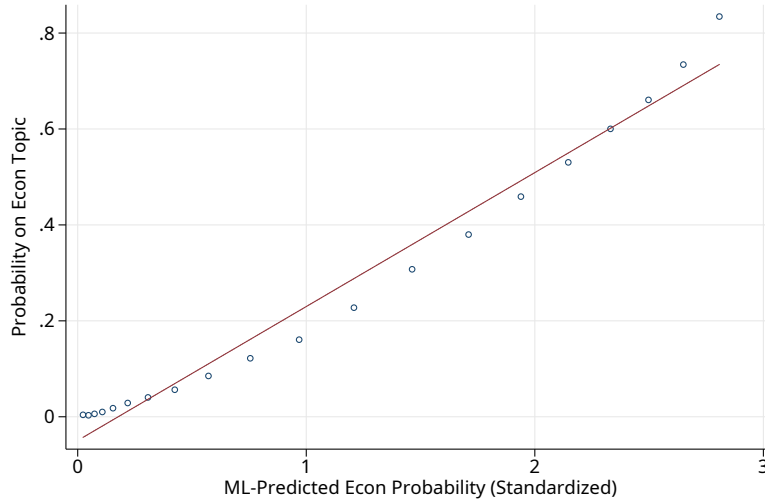
Notes. Binscatter of L2 logistic prediction for y = text-predicted economics case, in held out test sample. Horizontal axis is the predicted probability that a case is on an economics topic. The vertical axis is the true rate by bins of the prediction.

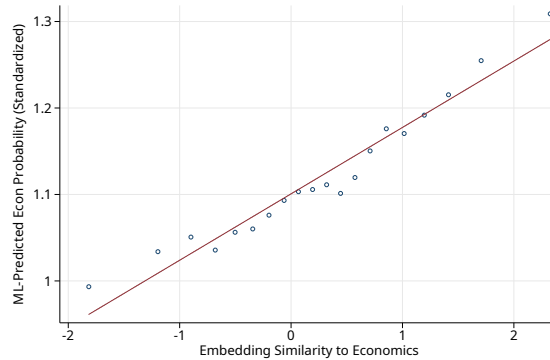## D.2 Text-Predicted Similarity to Economics Topics

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.6, 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.6 visualizes how well our prediction model replicates the probability

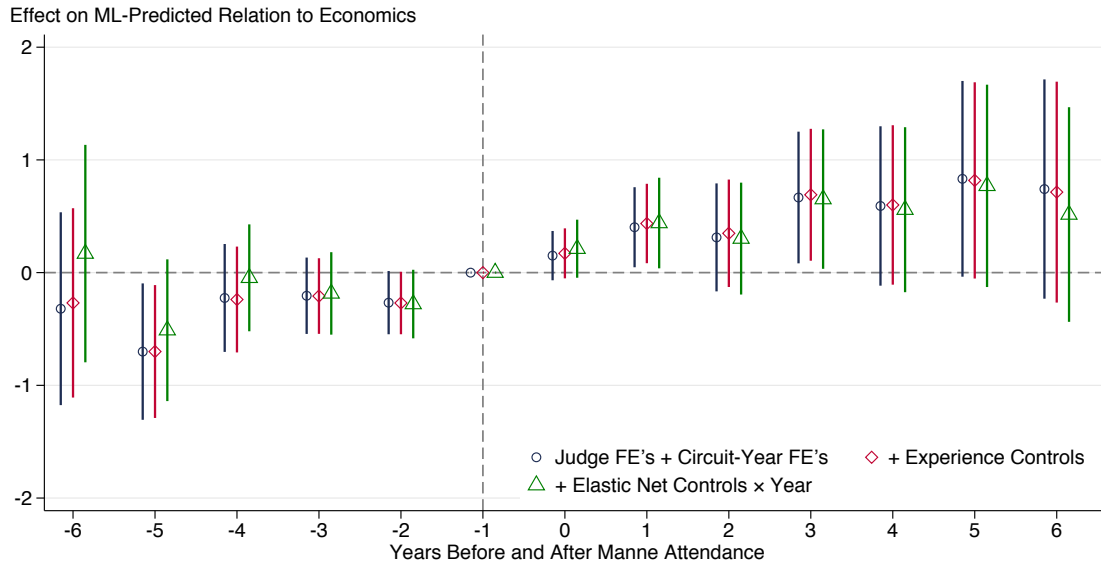Figure A.7: Econ Embedding Similarity Correlated with Text-Predicted Econ

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.7 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.8 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.8), 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.8: Effect of Manne Program on Alternative Economics Language Measure

Effect on ML-Predicted Relation to Economics

Legend:
- ○ Judge FE's + Circuit-Year FE's
- ◇ + Experience Controls
- △ + Elastic Net Controls × Year
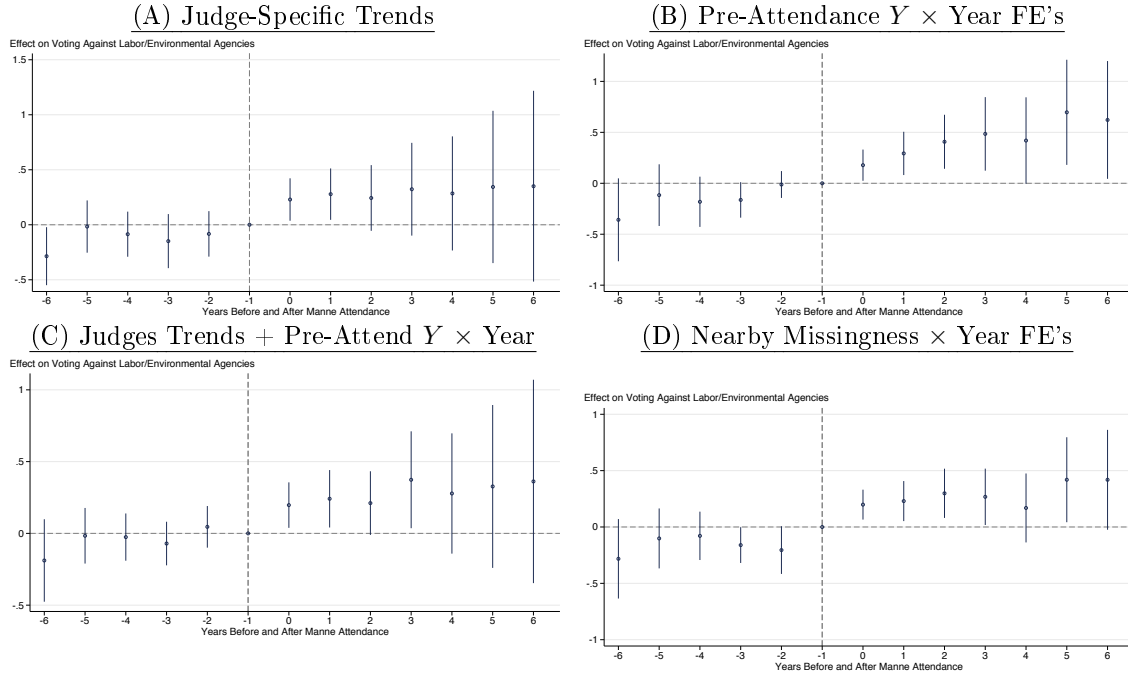
X-axis: Years Before and After Manne Attendance

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.8: 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.9: Event Study for Labor/Environmental Agencies, Alternative Specifications

(A) Judge-Specific Trends

(B) Pre-Attendance Y × Year FE's

(C) Judges Trends + Pre-Attend Y × Year

(D) Nearby Missingness × Year FE's

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.9 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.10. 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.10: Effect of Manne Program on Antitrust Decisions

(A) Baseline Specifications

(B) Nearby Missingness × Year FE's
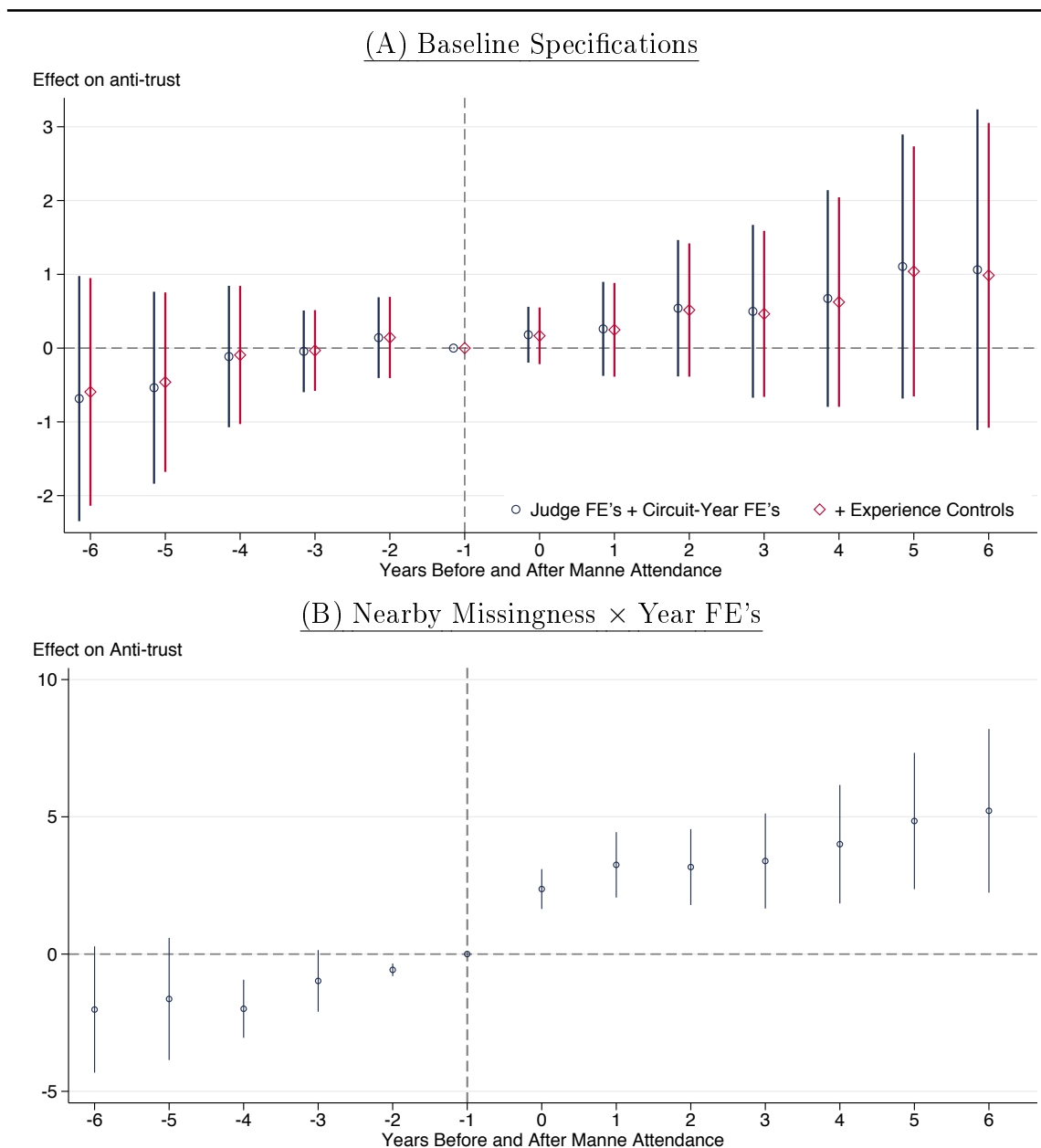
Notes. Event study effects on voting against antitrust claimants, relative to year before attendance at Manne economics training. In Panel A, the baseline specification (blue circles) includes judge and circuit-year fixed effects. Additional specifications add experience controls (red diamonds). Panel B includes indicators for whether a labor-EPA case is present in two years before/after the attend year, interacted with year. Observations are weighted to treat judge-years equally. Error spikes give 95% confidence intervals, with standard errors clustered by judge.

Table A.9: 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.9 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.10, 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.11 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.10: 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.11: 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.

Table A.12: 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.12 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.11 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.12).

Table A.13 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.

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Figure A.11: Event Study Robustness: Dropping 2nd, 8th, 9th, and D.C. Circuits
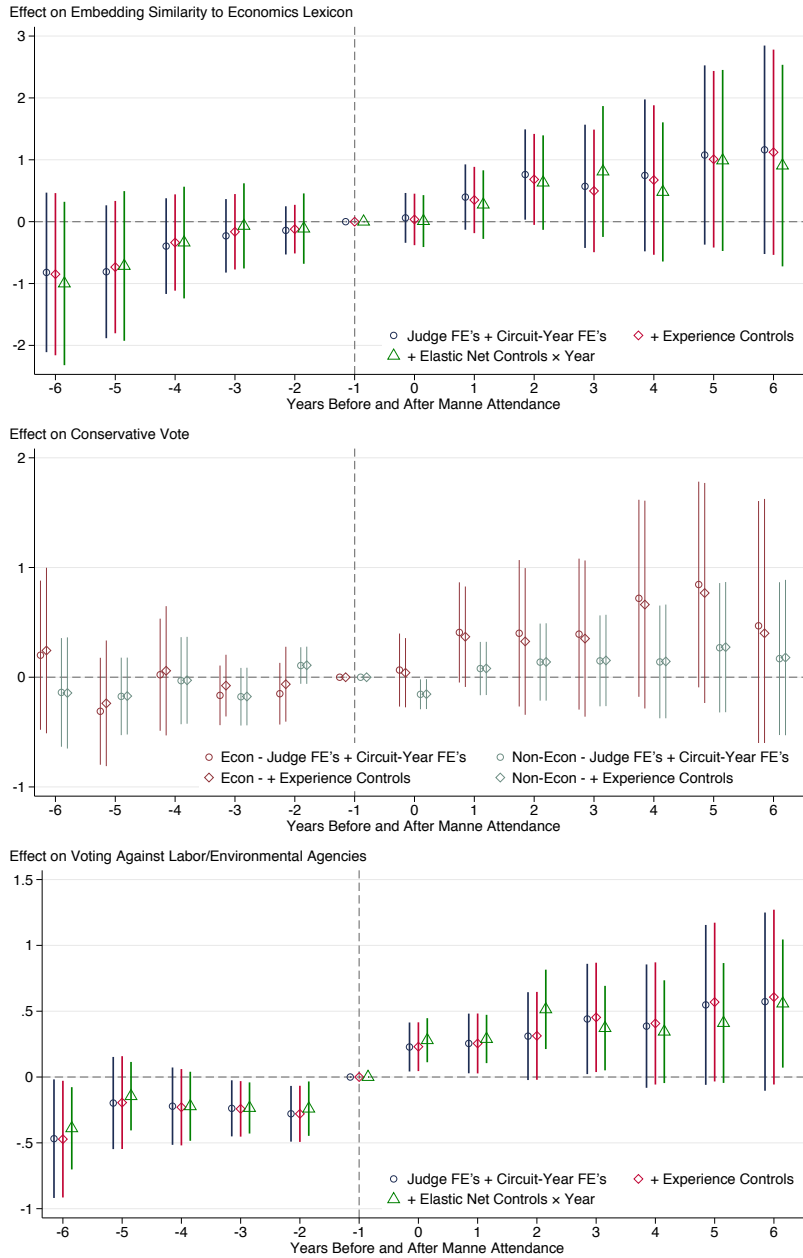
Notes. Main event study results for the circuit courts (from Figures 3, 4, and 5) but dropping those circuits for which Levy and Chilton (2015) find nonrandom assignment in their calendar dataset from the years 2008-2013 (2nd, 8th, 9th, and D.C. Circuits). Outcomes are Economics Language, Conservative 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.12: Circuit Event Studies with Legal Topic Fixed Effects
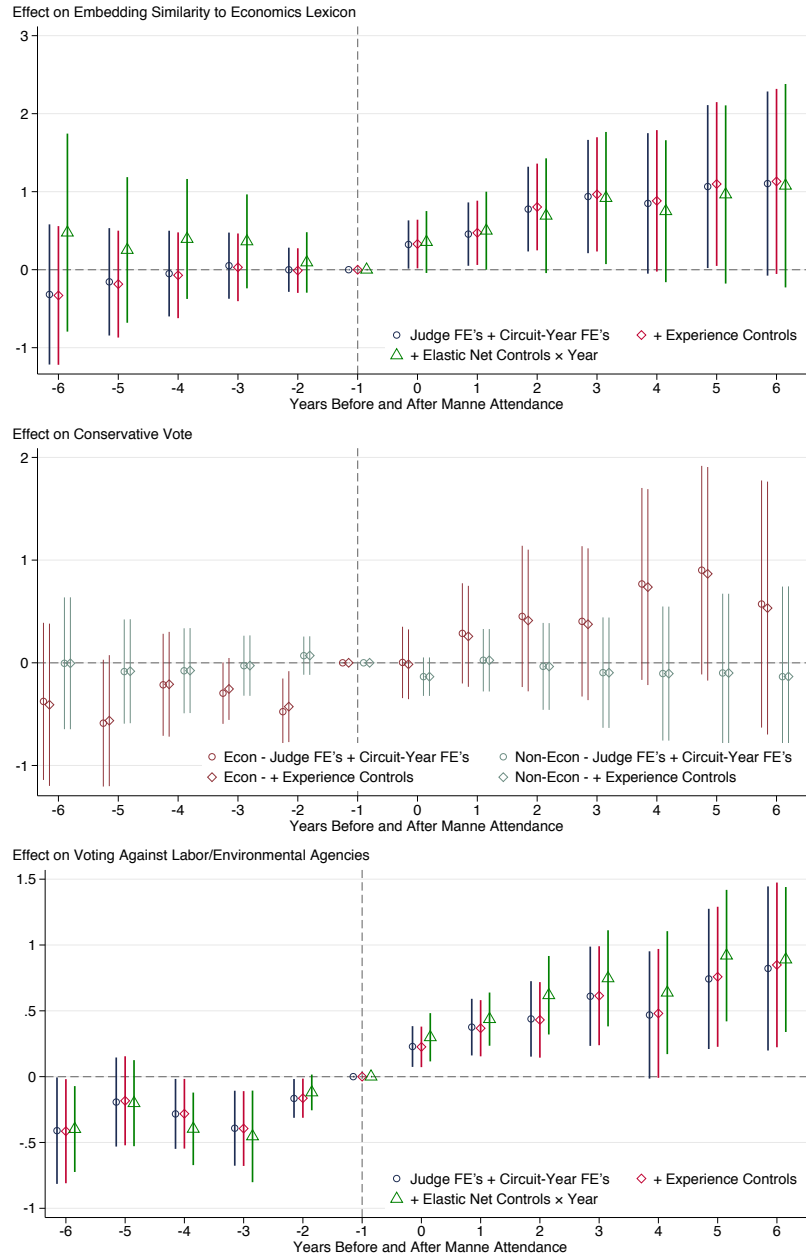
Notes. Main event study results for the circuit courts (from Figures 3, 4, and 5) but including fixed effects for 94 detailed legal topics. Outcomes are Economics Language, Conservative Vote in Econ and Non-Econ Cases, and Voting against Labor/Environmental Agencies. For other details see notes in the associated main-text exhibits.

Table A.13: 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. $+p < .1, *p < 0.05, **p < .01$.

Figure A.13: District Event Studies with Crime Charge Fixed Effects

Effect on Any Prison Given

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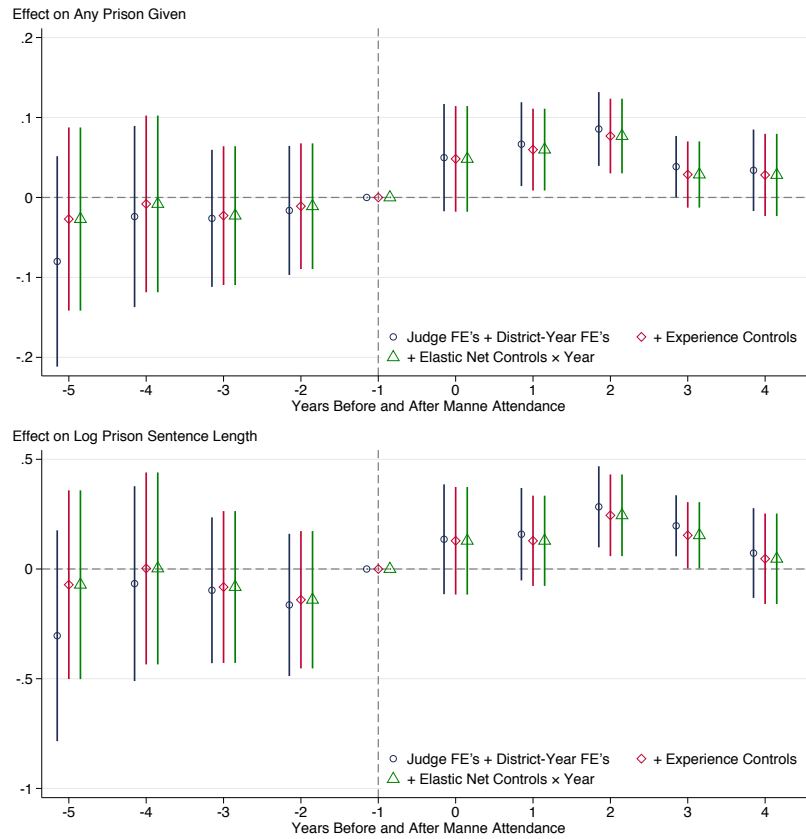
Notes. Main event study results for the district courts (from Figure 6) but including fixed effects for crime type (345 categories). Outcomes are Any Prison Given and Log Sentence Length. For other details see notes in the associated main-text exhibit.