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QUANTIFYING ECONOMIC REASONING IN COURT: JUDGE ECONOMICS
SOPHISTICATION AND PRO-BUSINESS ORIENTATION

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For my parents

Whereof one cannot speak, thereof one must be silent.

—Ludwig Wittgenstein, *Tractatus Logico-Philosophicus*

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ABSTRACT

By applying computational linguistics tools to the analysis of US federal district courts'
decisions from 1932 to 2016, I quantify the rise of economic reasoning in court cases that range
from securities regulation to antitrust law. I then relate judges' level of economic reasoning to
their training. I find that significant judge heterogeneity in economics sophistication can be
explained by attendance at law schools that have a large presence of the law and economics
faculty. Finally, for all regulatory cases from 1970 to 2016, I hand code whether the judge
ruled in favor of the business or the government. I find that judge economics sophistication is
positively correlated with a higher frequency of pro-business decisions even after controlling
for political ideology and a rich set of other judge covariates.

CHAPTER 1

INTRODUCTION

A duel of experts played out in the AT&T - Time Warner merger lawsuit, also called "antitrust trial of the century"[1]. Each side was represented by prominent economists, who submitted their own economic and econometric analyses of the competitive effects of the proposed merger based upon economic theories about vertical integration. These expert testimonies were extensively cited in the US district court judge's written opinions. Discussion of highly technical economic analysis would strike most as surprising, yet in fact reliance on economic expertise in court is currently quite common. It was not always this way. The law and economics movement in the 1970s catalyzed diffusion of economic thinking into law, both in academia and practice. One area where economics find an eminent place is antitrust law. Economic terms such as the elasticity of demand and marginal cost have become part of the language of antitrust law (Baker and Bresnahan, 2006; Ginsburg, 2010).

While legal scholars generally agree that the influence of economics and economists on antitrust is growing (White, 2008; Landes and Posner, 1993), no one has yet quantified when this influence began and how much it has changed over the years. This paper fills that gap and extends the analysis beyond the legal area of antitrust law. Judges write opinions that explain the way to decisions, and these texts are unique windows into what they know and how they rule. Are some judges more economically sophisticated than others? Does this dimension of heterogeneity make a difference to case outcomes? I revisit the question of judicial decision making, and propose a novel factor to explain judge differences in pro-business orientation. Along the way I discover that law school education early in life rather than post-appointment judicial training programs predicts a greater use of economic language, and this, in turn, supports the view that a greater use of economic language in case opinions reflects a better

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1. https://www.nytimes.com/2018/06/12/business/dealbook/att-time-warner-ruling-antitrust-case.html

knowledge by judges of economics.

In the first part of the paper, I quantify the use of economic reasoning in judicial opinions through textual analysis in individual legal areas. The setting I draw upon is federal district courts of the United States. I begin by defining a set of economic cases that are known to have assimilated economics knowledge and that are uncontroversial among legal scholars and practitioners. I then obtain the full text of written opinions from the Caselaw Access Project, LexisNexis, and Cheetah through a mix of API, web scraping, and hand downloading. Each opinion is also accompanied by meta data, including the court, decision date, and, most importantly, the authoring judge.

For the measure construction, I employ a simple pattern-based sequence-classification method (Manning et al., 2008) developed in computational linguistics to discriminate words and phrases archetypical of economic analysis and not legal reasoning. Hassan et al. (2019) uses this method to measure firm-level political risk from conference call transcripts. In my context, for each individual legal area of interest, I pick a training library of economics text (e.g., an undergraduate economics textbook on industrial organization) and a training library of legal text (e.g., a compilation of law school study aids in non-economic areas of law) to identify bigrams that are distinctively used in economics arguments. I then compute the "closeness" of bigram frequencies in the written opinion to the identified economic bigram frequencies in order to obtain a measure of the share of the judicial opinion that is focused on economic reasoning.

To bolster my confidence about the validity of this measure, I perform several validation exercises. First, I show that the bigrams identified to distinguish economic analysis from legal reasoning intuitively make sense. In antitrust, the most frequent economic bigrams are "marginal cost", "demand curve", "market power", and "nash equilibrium". In securities law, these are "abnormal return", "stock price", "fraud market", "price change", and "present value". In addition, these economic terms are employed by judges in case rulings.

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As another piece of evidence for validity of my measure, I show that within each legal area, the top-scoring opinions correctly identify the text that discusses issues or analysis associated with economics. For example, within antitrust cases, economic reasoning can take the form of defining market power (citing academic law and economics articles), regression analysis showing that tying increases price, or evaluating the modeling assumptions presented by opposing parties. In securities litigation, economic reasoning is used to show that statistically significant changes in price is caused by misrepresentation.

To understand how the use of economic reasoning has changed over time, I aggregate the case-level measure to years. I find that the use of economic analysis in antitrust adjudication has increased since the 1940s. Its pace picked up in the 1970s, when the Chicago school came of age, which is consistent with a prior qualitative account of the antitrust revolution (Kwoka and White, 2014). A similar trend is found in the case of securities regulation, although the adoption happens at a slower rate than in antitrust. Seeing these trends as an equilibrium outcome, I explain them in terms of legislative constraint and the institutional capability of enforcement agencies that affect idea absorption in courts.

What explains differences in the use of economic reasoning in court case opinions? To answer this question, I decompose the case-level measure of economic reasoning and find substantial variation across judges, after controlling for circuit-year and type-of-case dummies. To determine the economics sophistication of each individual judge, I project the case level measure on court-year dummies and judge dummies. Case random assignment ensures that the estimated judge fixed effect admits causal interpretation. I show statistically significant judge heterogeneity using randomization inference (Abrams et al., 2012). I then provide justifications for concluding that the language of economic reasoning reflects judges' knowledge of economics. First, I show that judges who obtained a J.D. degree from schools that have strong law and economics traditions[2] write opinions that are characterized by a greater use

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2. Coding of this variable will be explained in detail in the data chapter.

of economic reasoning. Although not causal in nature, this association is orthogonal to case characteristics once conditional on court-year fixed effect thanks to case random assignment within court. The result is also robust to an alternative definition of law and economics schools. As another piece of suggestive evidence, I present anecdotal evidence about judge James Noland, who received an MBA from the Harvard Business School in 1942 and is the highest-scoring judge in the seventh circuit. As his judicial decisions reveal, his education turned him into a firm believer in free enterprise.

Finally, I examine whether this new dimension of judge heterogeneity has important implications for how judges rule. To this end, I gather a new dataset that consists of district court regulatory economic cases that involved federal agencies from 1970 to 2016. Following the coding scheme developed in the empirical law literature to treat decisions as pro-business or pro-government, I manually read the cases and assign the direction of the ruling. I find that a one standard deviation increase in economics sophistication is correlated with a 12.9 percentage point increase in the probability that a judge will rule in favor of business. This suggests that for a given case, being assigned to a judge who is more economically sophisticated really makes a difference in the case ruling. The result is statistically significant at the 0.05 level and is robust to adding a rich set of judge controls, including political ideology.

I note that there are three main caveats to my analysis. First, I do not claim that a causal channel leads from economics learning to a pro-business leaning. It is possible that judges who study economics are systematically more pro-business than others. The main analysis and additional tests I perform, however, rule out several possible channels. The association is robust to inclusion of judge ex-ante political ideology, cohort and all career experience controls; the magnitude of the correlation remains stable across specifications. While such conditioning ensures that economics sophistication is not merely proxying for other observable judge characteristics, we might be concerned that selection on unobservables could

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drive the results. Implementing the method developed by Oster (2019), I provide suggestive evidence that is inconsistent with this conjecture. Second, because judge economics sophistication is measured, it likely contains significant measurement error. I show that the main result still holds after I apply empirical bayes shrinkage to the estimated judge fixed effects. Third, my measure of pro-business is constructed from a sample of regulatory economic cases, which does not include private lawsuits. Whether the finding of this paper generalizes to a more broadly defined pro-business tendency is an empirical question for future research.

1.1   Relationship to the Literature

This paper contributes to several strands in the literature. First, it contributes to the literature that examines the impact of economics on law. Earlier works were primarily written by legal scholars who worked in the field of law and economics. Through close readings of landmark cases, they provided qualitative accounts of how economic approaches have been adopted in legal scholarship (Posner, 1987), enforcement (White, 2008), and litigation (Kovacic and Shapiro, 2004; Kovacic, 1992, 2007; Ginsburg, 2010). A major innovation of my paper is the application of computational tools to a text corpus that is too large for humans to process without computational assistance. My approach allows me to shed light on the question of whether economics was well-received by courts long before the Chicago school became influential in the field of law and economics(Kaplow, 1987). My measurement methodology also circumvents the drawbacks[3] inherent in citation analysis, a method commonly used in the law literature to study the influence on law of a certain school of thought (Landes and Posner, 1993). Finally, measuring and comparing across different areas of law generate interesting insights into the endogenous process of

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3. Most crucially, judicial opinions rarely cite academic economics papers directly. Judges usually learn about economics ideas through legal briefs prepared by attorneys, amicus briefs submitted by experts, or other sources of exposure, including pre- and post-appointment economics training. Even when they do cite, more often than not, they cite law review articles that typically translate economics ideas into language that is more accessible to legal practitioners. See Kovacic (1992) for further discussion.

idea diffusion. I discuss the roles that legislative constraint and the institutional capacity for economic analysis at enforcement agencies play in the divergence of the rates at which economic ideas are absorbed.

The second strand of literature to which my paper speaks is judicial decision making. In this important and rapidly expanding subject of economic research, judicial decisions have substantial economic impact, shaping, for example, the regulatory environment that firms are situated in and respond to (Stephenson, 2009). Previous research on how judges reach decisions has examined factors specific to individual judges, including gender (Boyd et al., 2010), religious beliefs, race and ethnicity, and prior career experience in the context of criminal sentencing (Yang, 2014; Lim et al., 2016; Abrams et al., 2012), appellate court cases (Epstein et al., 2013), and bankruptcy proceedings (Nash and Pardo, 2012).[4] For ideologically-charged cases, such as abortion and capital punishment, Sunstein et al. (2006) find that ex ante ideology, proxied by the party of the appointing president, explains conservative voting at the appellate court level. I contribute to this literature by testing ideological voting for regulatory economic cases at the district court level and by proposing a new factor – knowledge of economics – through which case outcomes are affected by the judges assigned to them.

A third area of literature studies the effects of economics training on political and social preferences. According to a hypothesis proposed by Stigler (1959), professional economics training affects judge's political views, making them more economically conservative. Recent work in behavioral and experimental economics examines the causal effect of economics training (Fisman et al., 2009; Cantoni et al., 2017). A closely related paper by Ash et al. (2019) estimates how attendance at the controversial Manne economics training program affects ruling directions in economics-related cases, regulatory cases, and antitrust cases.

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4. These can be seen as an empirical test of legal realism, which, simply stated, argue that traditional legal sources, such as rules and precedents, are not an absolute constraint on judicial decision making. Prelegal and extralegal factors, such as political preferences, views about race or gender, and even the personality of a judge, may play a role. See Schauer (2009) for a discussion of legal formalism versus realism.

Their identification strategy relies on random assignment of judges to cases and arguably quasi-random assignment to the training program conditional on selected observables. My paper replicates the difference-in-difference strategy, but it fails to find that the economics training program has a significant effect. Data limitations are likely to have produced my statistical test's lack of power and make evaluation of the validity of my research design in the context of district court impossible. Nonetheless, my current result, which exploits cross-sectional and longitudinal variation in attendance at the Manne program, qualifies the generalizability of the treatment effect on lower court judges.

Interesting correlations between being exposed to economics and political and social preferences are also telling. Rubinstein (2006) finds that economics students favor profit maximization over worker welfare. Jelveh et al. (2014) observe that economics professors are more ideologically conservative and make fewer campaign contributions to Democratic candidates than their counterparts in other social science disciplines. My paper explores the relationship between economics knowledge and policy orientation in the context of federal court judges. Given that a defining characteristic of the US common law system is that judges make law, this is an extremely policy-relevant and consequential setting. Rather than evaluate the effect of a particular economics training program, I seek to construct an aggregate measure of economics sophistication, thereby mapping judges to a knowledge space. While I cannot claim causal identification, I believe the methodology for measuring economic reasoning, the finding of judge heterogeneity in economics sophistication, and their correlation with pro-business rulings and prior law school education produces revealing results.

Methodologically, my paper is related to the literature on judge design that exploits random assignment. The prior literature in labor and public economics has leveraged the random assignment of judges to cases to estimate the causal effect of case decisions on subsequent economic or social outcomes. Judge-level variation is used as an instrumental

variable for identification where exogeneity is guaranteed by the random assignment of judges to cases (conditional on additional observables as dictated by the institutional setting). This sort of design has been used fruitfully to investigate a variety of questions, such as the ex post impact of sentence length on earnings (Kling, 2006), the intergenerational transmission of welfare culture (Dahl et al., 2014), and the effects of consumer bankruptcy on debtor outcomes (Dobbie and Song, 2015).

A paper particularly relevant to my research is Chang and Schoar's (2013) examination of corporate bankruptcy, in which they estimate judge fixed effects in Chapter 11 rulings. Based on the measure of the pro-debtor friendliness of judges, they find worse outcome for firms that are assigned to more pro-debtor judges. My paper similarly shows that judges differ in their economics sophistication, and that regulatory economic cases assigned to judges who have a greater knowledge of economics are more likely to receive a pro-business ruling. I see this result as the first step in IV design, highlighting that a judge's economics knowledge is a relevant factor in case decisions.

Finally, my research contributes to the rapidly growing literature on text as data (Gentzkow et al., 2017)[5]. Existing works have utilized tools from NLP and computational linguistics, such as the dictionary-based method, text regression, and topic modelling, to measure the language that economic agents use (Hansen et al., 2017; Gentzkow et al., 2016; Gentzkow and Shapiro, 2010). Ash et al. (2019, 2018), who apply embedding models to judicial opinion corpora, show that numerical representation is semantically and relationally meaningful. They also make an effort to measure the use of economics language in appellate court opinions by calculating the closeness of opinion doc embeddings to a pre-specified law and economics word vector embedding. My contribution is a more comprehensive measurement of economic ideas not necessarily related to law-and-economics. My approach features a much more granular measure constructed separately for each area of law and a flexible strat-

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5. See separate surveys of the application of textual analysis/computational linguistics in political science (Grimmer and Stewart 2013), and sociology (Evans and Aceves 2016)

egy that requires less prior knowledge of the subject. I believe that quantifying economic reasoning for individual legal areas is of intrinsic intellectual value to researchers and legal practitioners.

CHAPTER 2

INSTITUTIONAL BACKGROUND

2.1   US District Court Judges

In the U.S., the decisions of the country's 94 district courts can be appealed in its 13 appellate courts. The final appeal is to the Supreme court. District courts are the largest component of the federal judiciary, handling 330,000 cases per year, or roughly 6 times as many cases as the court of appeals and 3,000 times as many cases as the Supreme Court. In district courts the number of judgeships in a given year is about 680, whereas in circuit courts it is 180. In district courts, which is where almost every case at the federal level starts, judges manage an incredible caseload, and a substantial fraction of cases filed in district courts are decided without written opinion (Epstein et al., 2013). Although district court cases with written opinions are a selective sample that have merit and substantive issues, it is in these courts that judges explicitly reveal their legal reasoning. In their opinions, district court judges discuss findings of fact and/or conclusions of law.

Unlike courts of appeals, where judges sit on a panel of three for each case hearing, or the Supreme Court, where the hearing of all cases is en banc, district courts assign one single judge to each case. Hence, the district court is a simpler judging environment because there is no collective deliberation or peer effect. The presiding district court judge has sole responsibility for a case and issues orders and opinions independently.

One notable but under-appreciated feature of district judges is the amount of discretion they have in cases, even when they are bound by legal rules and precedents issued by higher court authorities. For example, district judges exercise discretionary authority when they assess the credibility of testifying experts and the admissibility of certain kinds of evidence, and when they decide the length of a criminal sentence, all within certain limits set by legislative rules and other mandatory authorities (Lim et al., 2016; Yang, 2014). Federal district

judges can decide what evidence to allow or exclude, and how important such evidence is to their ruling decisions. Hence, in a hypothetical securities regulation case, one judge might discuss in a fair number of paragraphs an event study conducted by an economics expert, whereas another might dismiss such evidence as inconsequential.

2.2 Economic Authority and Expertise

The meaning of the diffusion of economics into the federal court is two-fold. I take antitrust law as a prime example for illustration. First, since its inception, the policy goal of antitrust law has taken a few turns, finally arriving in the 1970s and continuing to this day at "maximizing economic efficiency" as measured by consumer welfare. This shift away from the more broadly construed social and political goals, such as protecting the competitive process and limiting concentration to dominant firms as a way to combat inequality and corruption, reflects courts' adoption of the Chicago schools's[1] analytical framework[2] for formulating legal rules (Wu, 2019; White, 2008; Kwoka and White, 2014). Given this conception of what antitrust law should aim to achieve, the court further develops concrete applicable rules and standards for the kinds of evidence required from litigating parties. This is where the battle of economic experts takes place. Consider the law regarding horizontal mergers as is practiced today. The parties typically need to address two factual questions: the delineation/definition of the relevant market for merger analysis; and the potential adverse effects and efficiency gain of mergers. These questions come naturally to economists, who spend their entire career studying firm conduct, market structure, and the resulting economic out-

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1. The Chicago school is not the only intellectual impetus for the economic turn of antitrust. In fact, Harvard professors Donald Turner and Philip Areeda intensely pursued the effort to make antitrust more economically rational. They quietly made mainstream the premise that consumer welfare should be the only standard. (Wu, 2019)

2. The court has adopted the fundamental neoclassical economic assumption that firms act rationally to maximize profits and minimize loss.

comes[3]. Discussion of modeling assumptions, data, and empirical methodologies constitute an important part of antitrust case adjudication, particularly when the stakes are high. In certain instances, evidence from economic analysis is decisive.

The adoption of economic authority and the reliance on economic expertise in courts extends well beyond the legal area of antitrust. The design of securities law has long relied on the theoretical economic assumption of market efficiency, which is the understanding that securities prices reflect all available information in the market. Legal requirements like mandatory disclosure are firmly grounded in the efficient market hypothesis (Fama, 1970). The fraud-on-the-market doctrine in securities class action has the same theoretical underpinning.[4] As a matter of expertise, the court has admitted and in many instances relied on the testimony of experts who empirically determine whether a misrepresentation has caused stock price inflation in securities fraud cases. While the adoption of economic reasoning in the legal areas regulating market activities may seem natural, the fact that traditionally nonmarket areas are receptive to economic approaches could strike some as surprising. Cost and benefit analysis is now the single most important analytic device in environmental regulation cases. The court also frequently turns to economic experts to calculate damages associated with harmful conduct. In employment discrimination cases, statistical evidence of discrimination is brought to bear on claims as proof for class certification and to identify persons entitled to awards.

I end this section by placing a boundary on the set of economic ideas that have successfully diffused into legal practice. On the one hand, the source of economics authority and expertise is not limited to the academic law-and-economics approach; the scope of economic ideas in my

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3. Of course, not all economists focus on this area of inquiry. I refer here specifically to economists who specialize in industrial organization. Nevertheless, any economist would immediately sense that this type of inquiry falls exclusively within the expertise of the economics discipline.

4. This hypothesis is currently under debate, particularly because new evidence from behavioral finance indicates that people make errors and do not always rationally incorporate full market information when making investment decisions. The Supreme Court, citing Robert Shiller, challenges the EMH in Erica P. John Fund, Inc. v. Halliburton Co. 131 S.Ct. 2179, and the majority decision takes a middle ground.

paper is broader than that it is in studies that examine the impact of the law and economics movement (Posner 1987, 1993; Ash et al., 2019). On the other hand, the economic ideas adopted by courts are neither all nor a representative sample of all economics knowledge ever produced. For a variety of reasons, some rhetoric in economics affects the judiciary more successfully than others (Burgin, 2012; Teles, 2012).

2.3 Case Random Assignment

My empirical framework relies on the assumption that in a district court, cases are randomly assigned to judges at a given point of time. This allows me to separate the judge effect from case features that are important determinants of case outcomes. If this assumption holds, we shall be able to causally identify the economics sophistication of specific judges and pro-business leaning that is orthogonal to the sorts of cases he faces.[5]

Institutional setting stipulates that cases are randomly assigned to judges within a district court. While no uniform rules at the federal level dictate the exact procedure for randomization draws, each district court makes its own rule for assigning incoming cases to individual judges, and most use some variation of random drawing[6]. Apart from the rules governing random assignment in principle, we can run an empirical test that is based on judge and case observables[7]. Previous research has empirically tested district court judge random

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5. Random assignment across courts is not required for the identification strategy. In fact, forum shopping geographic localities is common in some types of cases. Plaintiffs may take a case to a court that traditionally has been more favorable to suing parties. For instance, E.D. Texas is a popular forum for patent litigation because it has a track record of ruling in favor of plaintiffs (78% compared to the national average of 59%). Source: https://www.nytimes.com/2006/09/24/business/24ward.html.

6. Administrative Office of the U.S. Courts. See https://www.uscourts.gov/faqs-filing-case. There are exceptions to complete random assignment. If a judge has been assigned more cases than a colleague in the same court or if the former's cases are particularly complicated or will take longer to resolve, the chief judge may assign new incoming cases to other judges who have easier caseloads. It is impossible to know exactly when this happens, but on average in a given year, cases should be roughly evenly distributed among active judges within the same district court.

7. Strictly speaking, we do not observe everything about a case; thus, we can never have an exhaustive test of judge random assignment. Statistically, failing to reject the null hypothesis of random assignment only means we do not have evidence inconsistent with random assignment.

assignment in criminal sentencing samples(Cohen and Yang, 2018; Ash et al., 2019). For my sample of economic cases and regulatory economic cases, I separately run the following regression specification to test the null of random assignment:

$$z_{i(jt)} = \alpha_{ct} + \alpha_c + \alpha_t + \beta X_{\boldsymbol{i}} + \epsilon_i \tag{2.1}$$

where α's are the fully interacted court-year fixed effect and X_i is one observable characteristic of case i, such as subject matter, party type, and firm industry (if applicable). The left-hand side is one characteristic of judge j, who is assigned case i in year t. In addition to the circuit FE and year FE, it is important to condition on the interaction because this is the level of where randomization occurs. Within a given court-year cell[8], the set of judges open to case assignment remains stable, and the case mix for each judge should be fairly similar.

This equation tests for the selection of judges to cases. Under the null, once conditional on court-year FE, all judges should have similar case profiles, i.e., $\beta = 0$. In practice, I use the subject area as the case characteristic and I use the (estimated) judge's economics sophistication score as the dependent variable because my main result crucially relies on cases decided by judges who have varying degrees of economics sophistication. Standard errors are clustered at the judge level to allow for possible correlations within judges.

For the sample of regulatory economic cases, the number of observations (3364) is not large enough to allow for fully interacted court-year FEs (92*46). As a compromise, I substitute circuit-year interaction (12*46) and circuit FE for court-level dummies. This imposes a stronger assumption because, for case mix to be comparable, we have to assume that the district courts within a circuit share the same trend. In analysis that uses the economic case sample, the preferred specification has circuit-year, court, and year dummies.

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8. Technically, if the number of cases is large, we can afford to use a shorter time period unit, such as the day or month.

Table B.1 in the Appendix shows the result of this balance test. Economically sophisticated judges are not assigned to different types of regulatory cases. All the coefficients are non-significant and close to zero in magnitude. We cannot reject the null hypothesis that judges are assigned similar cases.

CHAPTER 3

DATA

3.1   Economic Cases and Judicial Opinion

I construct the measure of economic reasoning for a sample of district court cases that together comprise my economic case sample. In this section, I provide a justification for the choice of legal areas that fall into the following category: antitrust, intellectual properties, securities regulation, environmental regulation, labor and employment, bankruptcy, and federal taxation. I limit myself to areas of law concerning business and economic activities primarily because they are of economic nature and, hence, are amenable to the study of economic science. Posner (1987) states that law and economics as a legal school of thought has influenced how cases are decided in these fields. Moreover, in the data that I compile on all economists' amicus briefs ever submitted to the Supreme Court, the selected legal areas capture the majority of relevant case issues [1]. The fact that the "economic cases" category is used in the canonical Songer Court of Appeals Database[2] and the Supreme Court Database[3] assures that my sample construction of economic cases is reasonable. The defining characteristic of this set of cases is that legal issues explicitly adopt economic criterion. The seemingly non-economic field of environmental law draws on nonlegal disciplines, often incorporating scientific, technical, and economic concepts into legal standards and policy documents.

Data on antitrust cases are obtained from Cheetah's Trade Regulation Reporter. I wrote a python script to automate the downloading process. Other areas of cases are searched and manually downloaded from LexisNexis [4]. The search queries are carefully constructed to

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1. The full list of cases with detailed information on economist amicus brief is available upon request

2. http://www.songerproject.org/us-courts-of-appeals-databases.html

3. http://scdb.wustl.edu/

4. While it may appear straightforward to obtain cases by legal topics, in actuality no existing legal database automatically provides cases sorted by areas. This is so in part because one case can span several

minimize both false positive and false negatives [5].

For the rest of the paper, I focus on antitrust and securities regulation for purpose of illustration, and all the results are obtained under this narrower set of economic cases. Future work could extend the sample and check the robustness of the conclusion. Because the trade reporter dates back to 1932 and most other economic areas emerged after that date, the time frame of my analysis runs from 1932 to 2016. The sample contains 17,869 antitrust cases and 21,632 securities cases. I parse the raw case data to identify opinion text, the last name of the authoring judge[6], decision date, and court.[7] Judge identity is parsed by matching to the federal judiciary center's judge biographical information, which I describe below in a separate section. Merging on the judge's last name and her court, I am able to uniquely establish a match in 95 percent of the cases. Cases that have with more than two matches are dropped[8].

3.2   Regulatory Economic Cases and Pro-business Rulings

To acquire cases that involve federal regulatory authorities, I turn to the Caselaw Access Project (CAP), which is a collection of US case law reporters recently digitalized by the Harvard law school library. It is specifically designed for text mining[9]. I collect federal

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areas. Standard professional legal database such as Westlaw and LexisNexis have their own system of assigning legal issues to cases which are typically finer than the much broader "securities regulation" category I am aiming to obtain.

5. False positives are those cases that should not be included in the sample but that I received while running the search query; false negatives are the opposite. In an example of constructing search queries on LexisNexis, securities cases in my economic case sample are returned from searching "(act w/3 (1933 or 1934)) or (securities w/3 exchange w/3 act) or 'the exchange act' or 'the securities act'". All searches are first confined to federal district court jurisdiction and the time period of 1932-2016.

6. This is inevitable regardless of the choice of legal database. This is how it appears in the original court file, and the databases have not invested in structurally parsing the full name of the judges.

7. Caselaw Access Project has API access to all published case opinions in the official federal reporters and associated meta data. I combine this source with LexisNexis for faster processing of the data as well as cross-validation.

8. This happens when two district judges share the same last name and court.

9. `https://case.law/`

district court cases on labor relations administered by the National Labor Relations Board (NLRB), environmental and energy regulation from the Environmental Protection Agency (EPA) and the Federal Energy Regulatory Commission (FERC), securities regulation issued by the Securities and Exchange Commission (SEC), antitrust and unfair competition regulation issued by the Federal Trade Commission (FTC), communications regulations issued by the Federal Communications Commission, and employment discrimination regulation issued by the Equal Employment Opportunity Commission (EEOC). The regulations enforced by these agencies are designed to protect consumers, investors, employees and citizens in their relationships with the more economically powerful business interests.

While this is not an exhaustive list of government agencies, it covers much regulatory ground in a variety of industries. The ideological stakes are often high when these agencies are involved in lawsuits heard in higher courts (Sunstein et al., 2006). The majority of these regulatory agencies were created under legislation enacted between 1887 and 1940 during two waves of legislative activity: the first from 1909-1916, the second from 1933-1940 (Viscusi et al., 2005). The third burst began in the 1970s and entailed the partial or full deregulation of many previously-regulated industries.

Since the role of economics ideas in the formulation of public policy has been particularly noticeable since the 1970s (Layzer, 2012; Teles, 2012), I focus on cases decided during the period 1970-2016. I search for cases in the CAP database whose case name contains one of the aforementioned agencies. My sample consists of a total of 3,364 cases. Tabulation by regulatory agency is provided in Table 3.1.[10] The associated meta data, including court,

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10. Some may be surprised by the small number of agency-related cases litigated in district courts given that trial courts see many more cases than appellate courts. As a comparison, during the same sampling period, the number of reported cases filed at courts of appeals was 5,708, 999, 1124, and 990 for NLRB, EPA, FCC, and FERC, respectively. This is so because agencies generally enforce the law by holding administrative proceedings whose decision can be directly appealed in a court of appeals rather than a district court. Some agencies, such as the EPA and SEC, can bring civil actions in district courts; they also have the option of administrative proceedings heard by administrative law judges. For agencies whose court actions mainly take place in a court of appeals, selected challenges may still be brought before a district court for injunctive or other relief. For example, the Communications Act that the FCC enforces allows direct actions in district court against common carriers, rather than broadcasters; section 206 allows the awarding of damages and

date, and the last name of the authoring judge, are retrieved from the API access point, which is a uniquely convenient feature of the CAP database. Judge identities are matched in the same way that they are matched in the sample of economic cases. Most cases are successfully matched to one judge; those that are not uniquely determined are identified manually.

Table 3.1: Case Tabulation by Federal Agency

Agency	Number of Cases
EEOC	1123
SEC	928
EPA	586
NLRB	365
FTC	281
FCC	65
FERC	34

Notes: This table shows the distribution of cases across federal regulatory agencies in my regulatory economic case sample. Total number of cases $N =$3,364.

To decide whether a case decision is pro-business, I manually read on LexisNexis and Westlaw the case summaries that document the disposition of the case. For a sample of appellate court cases I follow the coding scheme used by Sunstein et al. (2006)[11]. The coding requires identifying the winning party and the claim alleged. In more than 90% of the regulatory case sample, the case concerns an agency enforcement action against corporate/employer misconduct or a defendant that challenges agency administrative decisions that concern violations of the act. Under this circumstance, a court decision is coded pro-

attorneys' fees at district courts for violations of the Communications Act common carrier provisions (Botein, 1995). Other agencies, such as the EEOC and FTC, hear comparable numbers of cases at the district and appellate court levels. The legal provision varies from agency to agency, as specified in legislative statutes and past court interpretations. The upshot is that we do see agency actions in both courts of appeals and district courts, although their proportions differ by agency.

11. Epstein et al. (2017) extends the Sunstein database to include district court decision direction of the case being appealed

business if the agency loses.[12] In cases where public interest group challenges an agency for failing to promulgate certain regulations[13] that target a business interest, a decision is coded pro-business if the agency prevails. Following the general conservative-liberal dichotomy that previous empirical legal studies have employed, in a small proportion of cases that do not fall into this category – for example, cases that challenge an agency's failure to fulfill a Freedom of Information Act (FOIA) request – the coding is case-specific[14].

As noted in Epstein et al. (2013)'s study of pro-business decisions, some cases cannot be classified due to the neutrality of the court decision[15]. These cases are dropped from my sample. In cases in which the judge find faults in both parties, I treat the decision as against the government agency and, therefore, as pro-business.

3.3   Federal Judge Characteristics

Judges' biographical information is primarily sourced from the Federal Judicial Center (FJC). Demographic variables, such as race, gender, birth cohort, and the proxy for judge ex-ante ideology (the party of the appointing president), are available. Additional variables, including self reported political affiliation and religious, beliefs are obtained from the District Courts Attributes Data[16]. The FJC data also provides detailed information about judges' educational backgrounds, including the degree granted, the granting institution, and year awarded. This allows me to track which university each judge attends for law school education – an experience that arguably plays an important role in shaping a judge's political

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12. Admittedly, this is a simplification that follows Epstein et al. (2013), given that I do not distinguish between a decision that favors a business party and a decision that favors the overall interests of business communities.

13. 5% of all cases in this category, the vast majority of which involves the EPA.

14. See Epstein et al. (2013) footnote 21. The decision direction is also consistent with the coding scheme used in the well-known Supreme Court Database.

15. Summary judgment postponed until further date, or third-party action.

16. http://artsandsciences.sc.edu/poli/juri/attributes.htm

and social preferences (Fisman et al., 2009).

To demonstrate that my constructed measure of economic reasoning provides meaningful content, I test its correlation with pre-appointment exposure to economics knowledge. As a proxy for this early educational influence, I focus on the schools that judges attend for their J.D. degree [17]. On the basis of the history of law and economics narrated by Teles (2012), I code a binary indicator variable for judge attendance at law schools that have a strong law and economics culture. Specifically, the variable equals one if the judge obtained her J.D. from the University of Chicago, UVA, USC, the University of Miami, Emory, or George Mason after the 1970s; otherwise it is zero. The first three are well-known for having long been intellectual harbors for law and economics studies, whereas the rest are campuses that have successively hosted the Law and Economics Center, offering its judicial economics training program. Additional detail about the historical background for constructing law-and-economics-heavy law schools can be found in the Appendix. As an alternative definition, I use only Chicago, Virginia, and Southern California to define law-and-economics-heavy law schools. My main regression results are robust to the choice of definition.

The full career trajectory of the judge is also given in the FJC biographical data. I parse binary indicator variables for whether the judge has held a particular position (e.g., assistant attorney general, state governor) prior to being appointed to a judgeship in a federal district court. The full list of career variables is presented in Table 3.2. These are possible correlates with judge pro-business leanings, and I use them both as an explanatory variable of interest and as controls in my empirical analysis.

To replicate the analysis of the effect of the Manne training program on case decisions, I also gather records of judge attendance at the annual seminar that started in 1976 and

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17. This degree is a prerequisite for anyone who practices law in the United States in the 20th century. It typically involves a three-year program. Denominated the LL.B. in the 19th century, it is a degree that in most common-law countries is the primary professional preparation for lawyers. The LL.B was replaced by the J.D. in the US in the late 20th century. Hence in the FJC data, I look for both LL.B. and J.D. when defining the variable for law school attendance.

Table 3.2: Summary Statistics of District Court Judges

	Mean	Median	St. Dev.	Count
Demographics				
Male	0.87	1	0.337	400
Black	0.068	0	0.251	400
Catholic	0.252	0	0.435	329
Protestant	0.538	1	0.499	329
Cohort 1960s	0.022	0	0.148	400
Cohort 1950s	0.105	0	0.307	400
Cohort 1940s	0.23	0	0.421	400
Cohort 1930s	0.208	0	0.406	400
Cohort 1920s	0.242	0	0.429	400
Cohort 1910s	0.148	0	0.355	400
Ex-ante ideology				
Republican	0.533	1	0.5	375
Democrat	0.427	0	0.495	375
Appointed by Republican President	0.552	1	0.498	400
Economics exposure				
Eligible for Manne Program	0.882	1	0.322	400
Ever Attended Manne Program	0.248	0	0.432	400
Attended Core L&E Law School After 1970	0.032	0	0.178	400
Attended Core L&E Law School+ After 1970	0.035	0	0.184	400
Other covariates				
Has Graduate Law Degree	0.058	0	0.233	400
Prior career experience				
Private Practice	0.942	1	0.233	400
Law Professor	0.208	0	0.406	400
Bankruptcy Judge	0.022	0	0.148	400
Magistrate Judge	0.055	0	0.228	400
U.S. Attorney	0.07	0	0.255	400
Assistant U.S. Attorney	0.232	0	0.423	400
U.S. or State Attorney General	0.125	0	0.331	400
U.S. or State Solicitor General	0.015	0	0.122	400
Public Defender	0.032	0	0.178	400
Special Prosecutor	0.025	0	0.156	400
State Court Judge	0.29	0	0.454	400
Local Court Judge	0.065	0	0.247	400
District/City/County Attorney	0.112	0	0.316	400
Assistant District/City/County Attorney	0.14	0	0.347	400
Federal Government	0.018	0	0.131	400
State Government	0.072	0	0.26	400
Local Government	0.052	0	0.223	400
Other Federal Experience	0.268	0	0.443	400
Other State Experience	0.07	0	0.255	400
Other Local Experience	0.015	0	0.122	400
Any Government Experience	0.822	1	0.383	400

Notes: This table shows the summary statistics of judge characteristics in my regulatory economic case sample that comprises 400 district court judges, each of whom authored at least 10 opinions during the sampling period of 1970-2016.

ended in 1998. Ash et al. (2019) filed a FOIA request, and the returned raw documents can be accessed via the public repository[18]. From these records I obtain the exact dates of the program each year and the list of district court judges who participated in those sessions. An important limitation of my data that did not occur to Ash et al. (2019) is that the exact timing of the attendance of each judge for 1976-1986 is missing. This reflects the fact that the law and economics center (LEC) has undergone several relocations, from the University of Miami to Emory in 1980, and then to George Mason University in 1986. Unfortunately, as of the date of this draft, I do not have access to the earlier records. As a consequence, my sample for the difference-in-difference analysis is restricted to cases before 1976 and after 1986, when I know precisely whether the authoring judge has been treated or not. To ensure that the data is accurate, I cross-validate by reference to Butler (1998), who documents the full list of judges who attended the Manne program from 1976 to 1998.

In the final sample of my analysis, I exclude 8 judges who have sat in more than one circuit, either because they were visiting or through re-appointment. Cases authored by these judges are dropped. 24.7% of the regulatory economic cases in my sample are authored by judges who participated in the Manne economics training program.

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18. `https://www.muckrock.com/foi/virginia-128/judge-attendance-at-events-conferences-and-seminars-36073/`

CHAPTER 4

QUANTIFYING ECONOMIC REASONING IN JUDICIAL OPINIONS

This chapter explains the methodology for measuring the use of economic reasoning in written opinions. To show that the constructed measure can be interpreted as a proxy for economic reasoning, I perform several validation exercises, including manual audits and regression analysis. With confidence in the constructed measure, I plot the evolution of economic reasoning over time and across different legal areas and find that the pattern is consistent with previous qualitative accounts. I end this chapter with a discussion of the limitations of the measurement methodology and caveats for interpreting the results.

4.1   Methodology

The economic quantity of interest that I aim to capture is the extent to which legality explicitly adopts economic criterion. Put differently, I ask how much of the opinion that deals with a specific legal issue centers on economic reasoning that is framed in economic terms, relations, and arguments. The general approach is to first obtain a list of words and phrases that are distinctively characteristic of economic reasoning in court cases, and then calculate the "similarity" between the opinion at hand and this dictionary of words. Although there are potentially many different ways to realize this goal, I take one particular route and discuss its advantages over alternative methods previously employed in the literature on text as data.

The measure is separately constructed for each area of law. Consequently, the methodology described here should be thought of as individually applied to one particular legal area – for instance, antitrust law. To obtain a list of terms that distinguish economic reasoning

from plain legal reasoning, I define training library[1] $\mathbb{E}$ and $\mathbb{L}$ corresponding to corpus exemplifying economic and legal reasoning accordingly.[2] Theoretically, any collection of text that is representative of economic reasoning can be used for $\mathbb{E}$, and the same goes for legal corpus $\mathbb{L}$. For example, options for the economics training library include economics textbooks and journal articles. In the case of the law corpus, things are a little different. The lack of a general introductory law text notwithstanding, the casebooks that law schools in the United States assign to courses contain expert analyses of important cases written by renowned law school professors who specialize in the corresponding area of law. To obtain a collection of texts representative of legal reasoning, we would need to combine a handful of casebooks, each of which deals with a specific subject. Alternatively, we could use general law review articles or court opinions of non-economic cases to construct training library $\mathbb{L}$.

In my application, I use bigrams, which are two-word combinations, as the unit of words and phrases. This turns out to be important and performs much better than unigrams (i.e., single words). This is because bigrams encode much richer semantic meanings than unigrams and, hence, they better detect economic phrases. With the training library defined, I obtain a list of bigrams that appear in $\mathbb{E}$ but not in $\mathbb{L}$. Denote the generic element of this set as b_k, such that with a slight abuse of notation $\mathbb{E} \setminus \mathbb{L} = (\boldsymbol{b_k})$. Similar to the notion of cosine similarity widely used as a measure of distance between two document vectors, I define the use of economics language in case opinion i in the following way:

$$EconLang_i = F_i \cdot E = \sum_k c_{k,i} f_{b_k,di} \frac{f_{b_k,\mathbb{E}}}{|\mathbb{E}|} \tag{4.1}$$

where F_i and E are both vectors of dimension K, reflecting the frequency distribution of the economic bigrams. Writing things out, $\frac{f_{b_k,\mathbb{E}}}{|\mathbb{E}|}$ is the frequency of bigram b_k in the economics

training library $\mathbb{E}$ divided by the total number of bigrams in that library, and $f_{b_k,di}$ is the frequency count of bigram b_k in opinion i. Summation is over all bigrams in the set $\mathbb{E} \setminus \mathbb{L}$.[3] The dot product captures the closeness of case document i to a pure economic reasoning text in vector space. $c_{k,i}$ is a normalizing factor that adjusts for the length of the opinion document and inverse-document-frequency (idf).[4] This measure looks immediately intuitive once I assign a particular form to the normalizing factor.

Let $c_{k,i} = 1/|d_i|$ where the denominator is the total number of bigrams in the opinion. This measure then boils down to counting the number of occurrences of bigrams that indicate economic reasoning rather than legal reasoning; they are normalized by the length of the opinion and weighted by the relative frequency of the bigram in the original training library $\mathbb{E}$. The weight accounts for the importance of a certain bigram in the economics discourse. Notice that this formula corresponds to the firm-level political risk defined by Hassan et al. (2020). Another choice of normalization assigns $c_{k,i} = 1/\sum_k f_{b_k,d_i}$. Here the denominator is the total number of economic bigrams in the opinion. Ash et al. (2017) adopt this formula when they measure similarity of case opinions to the law and economics corpus.

Building upon these two forms of normalizing factor, I further incorporate an idf adjustment to obtain $c_{k,i} = idf_k / \sum_k f_{b_k,d_i} idf_k$. This common technique is used in natural language processing to increase the signal-to-noise ratio; bigrams that appear in too many opinions will be downweighted. In practice, I use a variant of this formula: rather than dividing by the L_1 norm, I normalize by the L_2 norm, i.e. $c_{k,i} = idf_k / \|f_{b,d_i} idf\|_2$[5]. As robustness checks, I verify that the results are not influenced by different choices of $c_{k,i}$.

For the sake of accessibility, in my quantification exercise, I use economics textbook for $\mathbb{E}$ and a compilation of law study aids in non-economic areas of law for $\mathbb{L}$. The latter, a concise version that complements the full-fledged casebook, is chosen to achieve a length comparable

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3. Again, this is an abuse of notation.

4. Note this is not exactly cosine similarity since the denominator is not Euclidean norm.

5. Both of these idf normalization have straightforward implementation in the python sklearn module.

to that of a typical economic textbook. For antitrust law, I choose *Industrial Organization: A Strategic Approach* (Church and Ware, 2000), which is a widely-used undergraduate textbook on industrial organization. I experiment with several other major IO/antitrust economics textbooks, including Carlton and Perloff (2004), Motta (2003), and Whinston (2008). Not surprisingly, the bigrams they identify are slightly different, as are the ranks. Nevertheless, manual inspection indicates that the bigrams overlap substantially, and the choice of $\mathbb{E}$ has no material impact on the main results of this paper.

For the general law training library, I combine the Westlaw *Nutshell* study aids series on criminal law, family law, civil procedure, civil jurisdiction, and federal courts. These are definitively non-economic areas of law, and, hence, they are ideal for pure legal discourse. I check the results against alternative choices of the law training library, including *Overview of U.S. Law* (Podgor, 2012)[6] and an open casebook on the same set of non-economic legal areas.[7] They remain valid.

I apply standard pre-processing steps to all of the text data, including lemmatization, punctuation and number normalization, and stopwords removal. I drop sentences that contain fewer than two words after the procedure. The resulting training library $\mathbb{E}$ contains 158,292 tokens, whereas $\mathbb{L}$ has 163,224 tokens. Taking the difference of the two sets, the final vocabulary contains $K = 90,854$ distinctively economic bigrams. Figure 4.1 visualizes the bigrams associated with economic reasoning; the different sizes of the characters reflects their relative frequency in the training library. Panel (a) shows the bigrams that appear in $\mathbb{E}$, but not $\mathbb{L}$. The most commonly used bigrams are "market power", "marginal cost", "market share", "nash equilibrium", and "demand curve". These unambiguously economic concepts indicate an exercise of economic analysis. The terms, however, will not contribute

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6. Chapters that examine economic-related legal areas are removed.

7. These are a digital compilation of landmark cases that law professors put together as substitute for the expensive hardcopy casebooks traditionally used in law school education. This effort is made possible by the initiative of Harvard Law School library, which leverages the Caselaw Access Project mentioned above. See `https://opencasebook.org/` for sample casebooks.

Figure 4.1: Bigrams from the Training Library for Antitrust
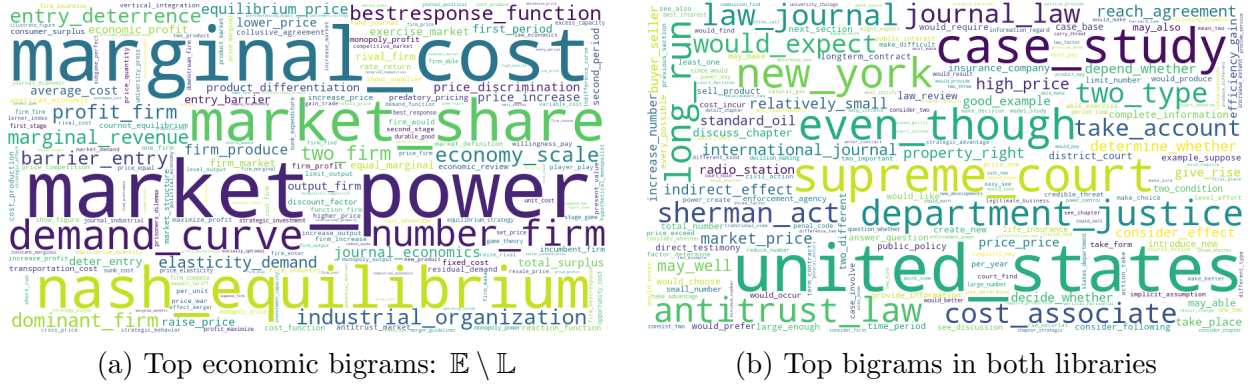
(a) Top economic bigrams: $\mathbb{E} \setminus \mathbb{L}$

(b) Top bigrams in both libraries

Notes: This figure shows the top 100 bigrams that are distinctively characteristic of economic reasoning in antitrust (panel a) or that appear in both the economics and law training libraries (panel b). The training libraries used to obtain the bigrams are $\mathbb{E}$ = Church and Ware (2000), $\mathbb{L}$ = Nutshell study aids.

to the economics language measure unless they also appear in judicial opinions. I will show that they do and that the context in which these bigrams appear points to judges engaging with economic analysis and not to legal reasoning.

The right panel of Figure 4.1 displays bigrams that appear in both the economics and law training libraries. It is clear from this visualization that the legal reasoning corpus is useful and necessary to screen out terms that appear in economic textbook but that are not actually associated with economic analysis. For example, generic phrases in antitrust law, such as "antitrust law", "sherman act", and "department of justice" should not be contributing bigrams to the measure of economics language because they describe the institutional features of the law rather than economic content. Other terms are common language that do not have exclusively economic connotations. Examples of this include "United States", "supreme court", "public policy", "even though", and "long run".

While this training library approach might look quite unsophisticated – no statistical learning is involved – its performance has been shown to be surprisingly robust (Mishra and Vishwakarma, 2015). One particularly nice feature of this method is (semi-)automatic

selection of a dictionary of words and phrases indicative of economic reasoning. Previous research that exploited text analysis used pre-defined phrases (Baye and Wright, 2011; Ash et al., 2019; Loughran and McDonald, 2011). I do not claim this is inappropriate – it works well when the researcher has strong and reliable prior knowledge. When the prior is weak or incomplete, however, it is useful to let the training libraries figure out the phrases. An offshoot of this procedure is importance weights associated with each term in the dictionary. Of course, the choice of training library requires prior information, which in my case comes from course syllabi, journal review articles and handbooks written on the economics of specific areas of law. After I decide what to include in the training libraries, the rest can be automated.

Compared to the simple frequency count of economic bigrams or the binary indicator as a measure of economics language, my method yields a continuous metric, and, hence it contains richer variation. Previous studies have used Latent Dirichlet Allocation (LDA), aka topic modeling, to capture an economic quantity of interest (Hansen et al., 2018). In legal contexts, the main challenge is weak information in the data. It is likely that the factor structure of the topic model fails to return a separate topic on "economic reasoning". The problem is particularly acute for legal areas such as environmental law and labor and employment, which do not see the adoption of as much economic argument as antitrust. In the Appendix, I present descriptive results that use LDA to extract a topic of "economic analysis" in antitrust case opinions. It is highly correlated with the preferred measure, and it identifies similar top-scoring opinions upon manual audit.

Finally, I compare and contrast Ash et al.'s (2019) law-and-economics language measurement with my approach, spelling out the latter's advantages. A starting difference is the object we want to measure. Using an index of pre-specified law-and-economics phrases

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(Ellickson, 2000)[8], the authors employ word embedding[9] method to measure the similarity between written opinions and law and-economic reasoning, which is only partly representative of the use of economics in court. As discussed in the institutional background, law-and-economics jurisprudence plays the role of economic authority in providing a conceptual positive and normative basis for what the legal rules would/should imply for economic outcomes. On the other hand, economic expertise is called upon in court cases to provide evidence that assists judges with the "finding of facts", and they use this expertise when they interpret the law that applies to this factual evidence. By mapping court opinions to academic economics discourse[10], I provide a more precise and concrete depiction of the actual use of economic reasoning in judicial opinions.

Without imposing a common dictionary of economic phrases over all legal areas under consideration, I am able to detect nuances specific to individual areas, thereby generating rich variation across cases even within a legal topic. The general nature of the Ellickson phrase index used in Ash et al. (2019) makes this rather difficult. An appealing feature of the training library methodology is its applicability to different areas of law. In this paper, for example, to measure the use of economic reasoning in securities regulation cases, I supply the economics research handbook *Economics of Securities Law* (Miller, 2016) as the economics training library to the algorithm and keep using the same *Nutshell* compilation as the law training library. The left panel of Figure 4.2 shows the list of bigrams that in the sample of securities regulation cases distinguish economic reasoning from legal reasoning.

There is an additional twist with the measurement for securities cases. Apart from the generic legal terms ("securities law", "fiduciary duty") and everyday language ("united

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8. Root phrases are externality, transaction cost, efficiency, deterrence, cost and benefit, capital market, game theory, chicago school, law and economics.

9. The main advantage of the word embedding method is its ability to uncover semantic relations between word vectors. In contrast, the training library approach preserves semantic meaning by focusing on bigrams rather than unigrams. The ultimate performance of measuring economics language cannot be compared using any objective single metric because no ground truth exists.

10. This includes law and economics as a field, but is much broader.

Figure 4.2: Bigrams from the Training Library for Securities Regulation
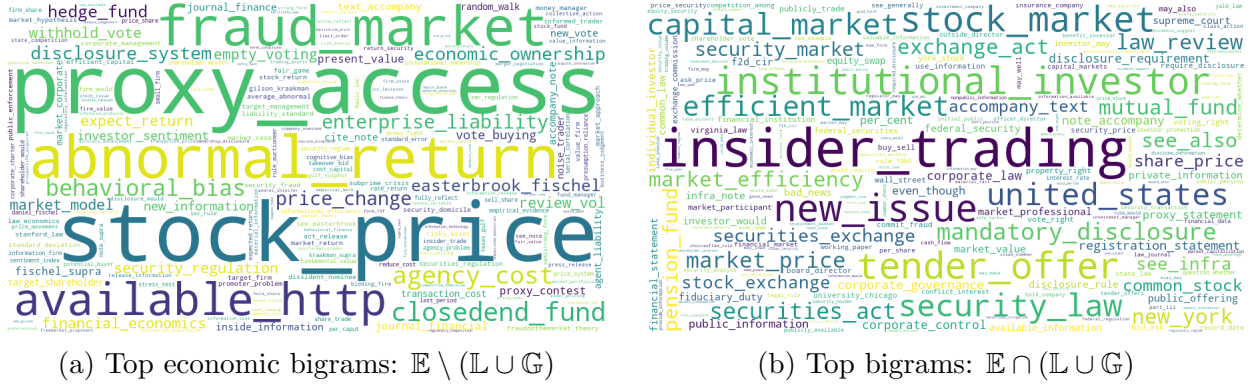
(a) Top economic bigrams: $\mathbb{E} \setminus (\mathbb{L} \cup \mathbb{G})$
(b) Top bigrams: $\mathbb{E} \cap (\mathbb{L} \cup \mathbb{G})$

Notes: This figure shows the top 100 bigrams that are distinctively characteristic of economic reasoning in securities (panel a) or appear in both economics and law training libraries (panel b). The training libraries used to obtain the bigrams are $\mathbb{E}$ = Miller (2016), $\mathbb{L}$ = Nutshell study aids, and $\mathbb{G}$ = Securities Act of 1933, 1934.

states", "see also") that are taken care of with the introduction of $\mathbb{L}$, the economics text-book also contains technical reference terms specific to the law, such as "insider trading", "stock market", "institutional investor". These terms, which appear quite often in academic economic papers on financial economics and securities market, are not concepts indicating practice of economic analysis. To deal with this complication, I introduce a generic subject training library $\mathbb{G}$ that is useful for filtering out common reference terms. Possible choices of $\mathbb{G}$ include newspaper articles and the original federal statute that created the law. They are not too technical to mention core economic analysis, but the coverages are broad enough to include the reference subject terms.

For securities regulation, I choose the main federal statutes, *The Securities Act of 1933* and *The Securities Exchange Act of 1934*, as the generic subject training library. The training libraries contain $319,900$ tokens in $\mathbb{E}$, $163,224$ in $\mathbb{L}$, and $243,331$ in $\mathbb{G}$. The resulting vocabulary has $K = 183,840$ economic bigrams. In panel (a) of Figure 4.2, we see that the most frequent economic terms are "stock price", "fraud on the market", "abnormal return", and "price change". They all pertain to the efficient market theory and empirical test using

event studies, which are essential topics in financial economics. Panel (b) displays terms in the economics training library that are not associated with economic analysis. An interesting observation is that "efficient market" belongs to this category. Although the efficient market hypothesis (Fama, 1970) is a landmark theory in finance, the phrase "efficient market" first appeared in non-academic economics texts in the first half of 20th century. Hence, judges who use this phrase could be merely alluding to the statute and using it in a general sense without engaging in any economic theory. This example illuminates the importance of having a generic subject library $\mathbb{G}$.

Comparing the economic bigrams for antitrust versus securities regulation cases, I find that the top scoring terms rarely overlap. This could suggest that judges, when deciding cases from different legal areas, are drawing upon disparate vocabularies.

4.2   Validation

Given the unsupervised nature of the measurement task, it is crucial to validate the accuracy with which the proposed methodology is capturing the economic quantity of interest. In this section, I perform several validation exercises to show that my measure reflects the use of economic reasoning in judicial opinions.

For ease of demonstration, I focus on antitrust law. Additional figures and tables for securities regulation are available in the Appendix. As a first step, to extend the bigram word cloud, I plot the top 100 economic bigrams with their relative frequency $\frac{f_{b_k,\mathbb{E}}}{|\mathbb{E}|}$ on the horizontal axis and the total count of occurrences in all opinions of the case sample on the vertical axis[11]. Figure 4.3 shows the result. The four most frequently used bigrams in the economics training library, "market power", "marginal cost", "nash equilibrium", and "market share" are kept out of the plot to maximize visibility of all of the other bigram texts.

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11. I apply $log(1 + x)$ transformation to compress the magnitude so that the bigrams can be visually fit into one scatterplot.

Note that among the top 100 bigrams, most make actual appearances in written opinions. Referring to the texts that surround these bigrams, I find that terms associated with price change, such as "price increase", "higher price", and "lower price", are frequently used by judges to discuss evidence of potentially illegal firm conduct. During the sampling period of 1932-2016, the word "industrial organization" was mentioned 62 times in all antitrust cases at the trial court level. This was used either to introduce an economic expert working in this research area or to cite academic consensus in this literature to be used as scientific evidence in court.

Not surprisingly, not all terms heavily used in the academic economics corpus become part of judges' language. Game theoretical concepts such as "best response function" and "equilibrium strategy" never appear in district court judicial opinions. As will be shown below, this could be due to the fact that game theory is still a relatively new approach compared to the earlier structure-conduct-performance paradigm used to analyze market competition or the lack thereof. Adoption takes time (Kovacic, 1992). The other possibility is that the increasingly mathematical formulation of economic problems renders the terms too technical to be accessible to judges. Similar visualization of bigrams for securities regulation is shown in Figure B.1 in the Appendix.

Table 4.1 lists the 10 opinions that score the highest $EconLang_i$ in the antitrust sub-sample, ordered from high to low. I report the case name, citation as it appears in official reporter, date of the decision, court name, the authoring judge, and the alleged violation and associated industry the suit pertains to. One may quickly recognize the case involving the credit card business American Express[12]. The suit against Amex, which concerned an anti-steering provision, received a Supreme Court ruling only recently[13], but the trial for

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12. United States of America, et al. v. American Express Co., et al., 21 F. Supp. 3d 187 (E.D.N.Y. 1988)

13. `https://www.nytimes.com/2018/06/25/us/politics/supreme-court-american-express-fees.html`. The case detail and prior history can be accessed here: `https://www.scotusblog.com/case-files/cases/ohio-v-american-express-co/`. For a discussion on the economic analysis the court uses, see this Kansas Fed post: `https://www.kansascityfed.org/en/publications/research/rwp/psrb/`

Figure 4.3: Top 100 Economic Bigrams Used to Measure Economic Reasoning ($EconLang_i$) in Antitrust Cases
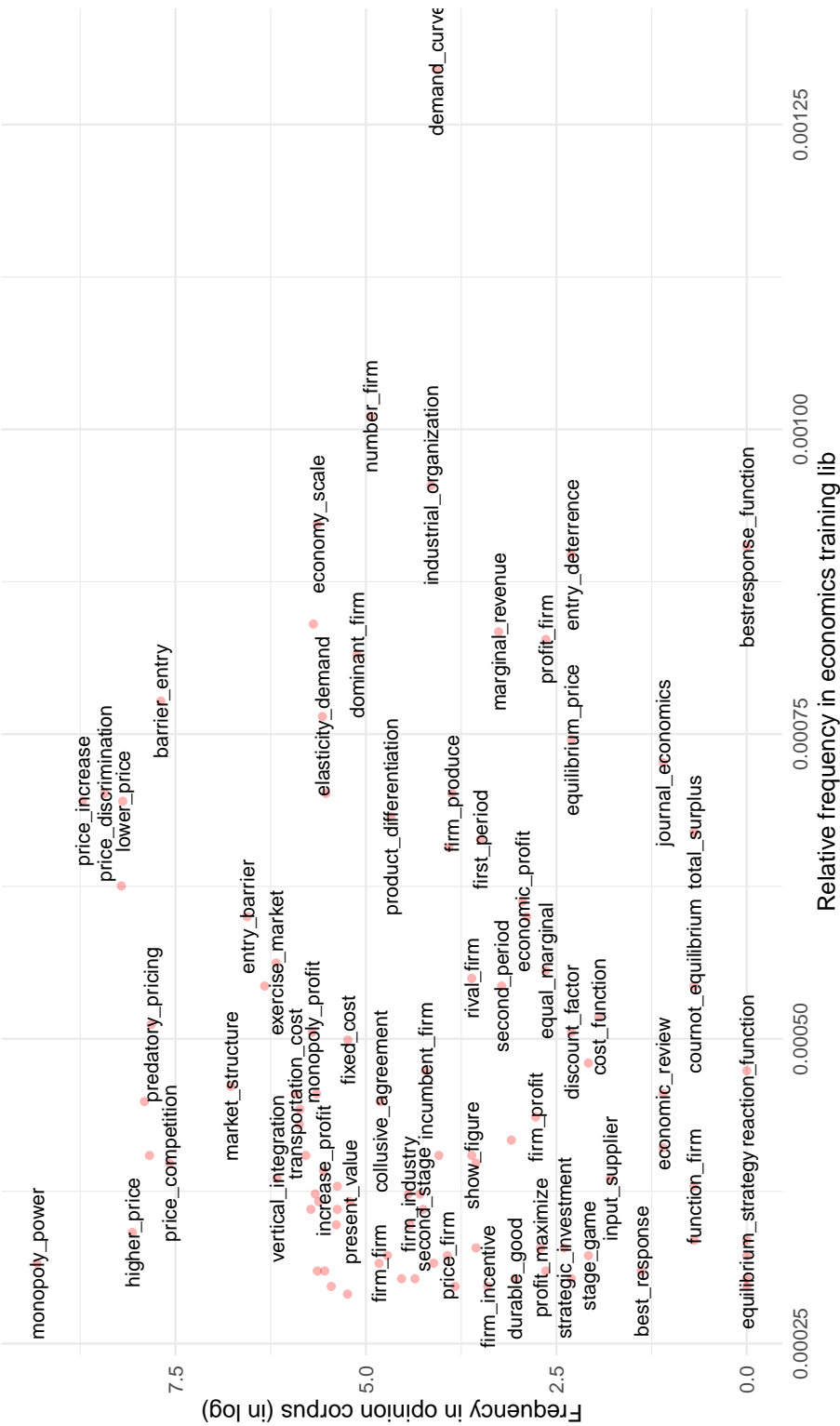
Notes: This scatterplot shows the top 100 economic bigrams in antitrust, identified from the training libraries. The x-axis reflects the relative frequency of the bigram in the economics training library, $\frac{f_{b_{k,\mathbb{E}}}}{|\mathbb{E}|}$. The y-axis corresponds to the total number of occurrences of the bigram in the opinion corpus, in log scale for visualization. The red dots are exact positions, labeled by the respective bigram. To avoid overlapping texts, not all bigram labels are displayed. The top 4 bigrams, "market power", "marginal cost", "nash equilibrium", and "market share" are not shown in this graph because they are used too frequently to fit in the plot.

a series of related cases started in district courts at much earlier dates. My measurement strategy successfully identifies such high-profile cases.

The top scoring cases on this list span several decades, although all ten were decided after 1980. This is consistent with the general consensus of the antitrust revolution, in which economics plays an ever important role in shaping legal rules. In addition, the top 10 cases range multiple district courts and circuits, although the Northern District of Illinois and the Seventh Circuit (Northern District of Indiana, Northern District of Illinois) are over-represented. This makes sense intuitively given that the seventh circuit is home to Judge Richard Posner and Frank Easterbrook, both of whom are Chicago school law-and-economics luminaries who have written extensively about the economic structure of the law. Such influence carries over to district courts in the circuit because circuit court decisions have a binding precedential authority for the lower court. The last column of the table makes clear that economic reasoning is used in various types of alleged antitrust violation and industries.

To show that my constructed measure of economics language truly captures the use of economic reasoning in judicial opinions, I zoom in on the top 10 opinions recorded in the previous table and look for excerpts that explicitly draw on economics learning. Tables 4.2 and 4.3 exhibit exemplary texts of this kind. The words are marked in bold to highlight economic concepts, citation to economic authority, or reference to testifying economic experts. Many of the terms marked in bold in Tables 4.2 and 4.3 are not top economic bigrams, as previously shown; that these excerpts are not a direct input to the measurement algorithm, and instead are uncovered because of it, is reassuring. A close reading of the case opinion indicates that the top-ranking case [14] is an action against the five largest U.S. carriers of wireless telephone services. Here consumers sue because handset and wireless telephone

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articles/2020/still-on-trial-courts-use-economic-analysis-american-express-case

14. In Re: Wireless Telephone Services Antitrust Litigation, 385 F. Supp. 2d 403 (S.D.N.Y. 2005)

services are tied together. The excerpt discusses an economics expert's regression analysis, which the judge finds to be methodologically unsound, hence not admitted as evidence.[15]

As another illustration, the case *A. O. Smith Corp. v. Lewis, Overbeck & Furman* concerns tying in motor distribution. The key legal issue is whether tie-in sales should be treated under the *per se* rule. In reaching his conclusion, the judge resorts to a Chicago school economic analysis that provides an efficiency rationale for this sort of practice. The excerpt shown explicitly refers to scholar-judge Robert Bork and his influential treatise that has shifted the Supreme Court's approach to antitrust laws since the 1970s. The case at the bottom of the list, *Digital Equip. Corp. v. Uniq Digital Technologies*, has market power at the center of the legal dispute. Interestingly, it cites the *A. O. Smith* decision at appellate court as a legal precedent to follow.

4.3   Descriptive Results

Having validated the constructed measure, I now use it to produce new quantitative description of the evolution of economic reasoning in the federal judiciary, with a special focus on federal district courts. In Figure 4.4, I separately plot the average $EconLang_i$ by year for antitrust cases and securities regulation cases. The years before 1943 are not shown because the sampling errors are sizable when the number of cases per year is rather small. Looking at antitrust law first, the use of economic reasoning in district court opinions has increased since the 1940s. Furthermore, the pace of its use increased around the 1970s when the Chicago school came of age. This is consistent with the general consensus in the economics and law literature that is derived from qualitative case studies (White, 2008; Landes and Posner,

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15. Careful readers might question whether these indicate statistical thinking rather than core economic thinking. It is true that regression analysis is not economic theory, but it has become the pillar of modern empirical economics. In fact, the court regards data and empirical results as important factual evidence. Panel (b) of Figure B.2 in the Appendix illustrates this point. This trend echoes the empirical turn in academia economics over the past several decades. Moreover, statistical methods applied to economic problems are typically presented by economic experts rather than statisticians and are a crucial part of the economic reasoning in court cases that this paper considers

Table 4.1: Antitrust Judicial Opinions with Highest Measure of Economic Reasoning (*EconLang$_i$*)

Case name	Citation	Decision date	Court	Judge	*EconLang_i* (1e-3)	Issue, industry
In Re: Wireless Telephone Services Antitrust Litigation.	385 F. Supp. 2d 403	8/29/2005	S.D. New York	Denise Cote	3.75	tying; telecommunications
Gumwood HP Shopping Partners, L.P. v. Simon Property Group, Inc..	2016-2 Trade Cases P79,801	10/19/2016	N.D. Indiana	Jon Deguilio	3.51	tying; real estate
United States of America, et al. v. American Express Co., et al..	21 F. Supp. 3d 187	5/7/2014	E.D. New York	Nicholas Garaufis	3.41	vertical restraints; credit card
Independent Ink, Inc. v. Trident, Inc., Illinois Tool Works, Inc., and Does 1 through 50.	210 F. Supp. 2d 1155	6/3/2002	C.D. California	Nora Manella	3.12	patent tying and monopolization; manufacturing
Metrix Warehouse, Inc. v. Mercedes-Benz of North America.	1989-1 Trade Cases P68,457	2/7/1989	D. Maryland	Edward Northrop	3.07	tying; automobiles
Town Sound & Custom Tops, Inc. v. Chrysler Motor Corp.	743 F. Supp. 353	7/3/1990	E.D. Pennsylvania	Jan DuBois	2.92	tying; automobiles
In Re: Solodyn (Minocycline Hydrochloride) Antitrust Litigation.	2018-1 Trade Cases P80,260	1/25/2018	D. Massachusetts	Denise Casper	2.88	patent misuse; pharmaceuticals
Hannah's Boutique, Inc. v. Surdej	112 F. Supp. 3d 758	7/2/2015	N.D. Illinois	Amy St. Eve	2.82	boycotts, exclusive dealing, and monopolization; retail
A.O. Smith Corp. v. Lewis, Overbeck & Furman	777 F. Supp. 1405	10/29/1991	N.D. Illinois	Milton Shadur	2.75	tying; manufacturer
Digital Equip. Corp. v. Uniq Digital Technologies	1993-2 Trade Cases P70,378	9/1/1993	N.D. Illinois	James Holderman	2.73	tying and monopolization; manufacturer

Notes: This Table lists the top 10 written opinions in antitrust law from the economics case sample ranked by *EconLang$_i$*. Associated meta data, including citation in the official reporters, decision date, trial court, name of the authoring judge, nature of antitrust violation, and the industry, are provided for each case.

Table 4.2: Use of Economic Reasoning in Top 1-5 Antitrust Opinions

Case name	Excerpt exemplifying economic reasoning
In Re: Wireless Telephone Services Antitrust Litigation.	In performing his **regression analysis**, Economides used United States International Trade Commission ("USITC") data on the declared value of imported handsets as a measure of the average wholesale price of handsets in the U.S. and then performed **linear regressions** of this data on each of three Bureau of Labor Statistics (BLS) indices concerning other electronic devices, such as computers and electronic equipment, or component parts of handsets, such as semiconductors. At no point, however, did Economides introduce any **independent variables** into his analysis of the inflation of handset prices vis a vis the prices he predicted using the abovereferenced BLS indices.
Gumwood HP Shopping Partners, L.P., Plaintiff v. Simon Property Group, Inc., Defendant.	The ability to exclude competition is a corollary to the ability to profitably raise prices, as "any firm that has and exercises the power to raise price above the competitive level must also be able to exclude entrants; otherwise it would not be able to maintain the higher-than-competitive price." **Landes & Posner, Market Power, 94 Harv. L. Rev. at 977**
United States of America, et al., Plaintiffs v. American Express Co., et al., Defendants.	One can make a strong argument that in this and other industries with high up-front costs and low marginal costs, such as the airline industry or the software industry, price discrimination can coexist with a high degree of competition. See Ill. Tool Works, 547 U.S. at 45 (price discrimination occurs in fully competitive markets); **William J. Baumol, Daniel G. Swanson, The New Economy and Ubiquitous Competitive Price Discrimination: Identifying Defensible Criteria of Market Power, 70 Antitrust L.J. 661, 674-76, 682-83 (2003).**
Independent Ink, Inc., Plaintiff v. Trident, Inc., Illinois Tool Works, Inc., and Does 1 through 50, Defendants.	The record in the instant case suggests that Plaintiff's proposed market definition was derived not from **economic analysis of cross-elasticity of supply and cross-elasticity of demand**, but rather from a report prepared by Plaintiff's vice president in a few hours.
Metrix Warehouse, Inc. v. Mercedes-Benz of North America.	The jury came to its determination after hearing testimony on a wide range of matters relating to **market power**, including Mercedes' unique position in the market place, **consumer preference** for the Mercedes-Benz, the automobile history, status and reputation, the power to **raise price over cost**, dealership profitability and the failed attempts of other companies to compete with Mercedes

Notes: This Table shows excerpts of the top 5 opinions sorted on $EconLang_i$ (corresponding to the first five entries in Table 4.1). Words in boldface are a direct use of economic concepts, citing economic authority, or reference to economic experts who offered testimony. These excerpts are manually selected from the entire opinion because they best exemplify a judge's embrace of economic ideas. Not all boldface words are (top) economic bigrams, although some are.

Table 4.3: Use of Economic Reasoning in Top 6-10 Antitrust Opinions

Case name	Excerpt exemplifying economic reasoning
Town Sound & Custom Tops, Inc. v. Chrysler Motor Corp.	Plaintiffs offer the affidavit of F. Gerard Adams, **Professor of Economics at the University of Pennsylvania**, as support for their tying claim. Dr. Adams states that Chrysler has **market power** in a market limited to the [31] installation of automotive sound equipment in new Chryslers. Although Dr. Adams' statement is correct, it is of no assistance to plaintiffs. It is a given that Chrysler has complete control in a market in which Chrysler is the only seller.
In Re: Solodyn (Minocycline Hydrochloride) Antitrust Litigation.	Rosenthal and Baum conduct a **quantitative analysis of cross-price elasticity.** Rosenthal Rpt. ¶¶57- 60; D. 741-2 ¶¶13- 20 ("Baum Rpt."). Baum is a **professor of economics** and social work at Boston College with a focus on **econometrics**. Baum Rpt. ¶¶1- 2. Rosenthal is a **Professor of Health Economics and Policy at the Harvard [12] T.H. Chan School of Public Health**. Rosenthal Rpt. ¶1. Rosenthal and Baum use IMS data on dispensed prescriptions to determine Solodyn's top competitors in acne treatment and conduct an **econometric test** of observed **price competition** between them. Rosenthal Rpt. ¶¶57- 59. They use an **econometric model** known as the **"AIDS"—Almost Ideal Demand System—model to examine cross-price elasticity**
Hannah's Boutique, Inc. v. Surdej	Here, Plaintiff concedes that it cannot show that Defendants possessed a substantial **market share** because it cannot calculate it. Dr. Schafer testified at the hearing that she did not conduct a **market share analysis** because "there were not **reliable data or facts** available to calculate **market share**, even if [a market share analysis] was warranted." (5/20/15 A.M. Tr.) Thus, Hannah's does not make an initial showing that Peaches possessed a substantial share of the **relevant market**.
A.O. Smith Corp. v. Lewis, Overbeck & Furman	First, per se classification is said to be unjustified because **economic analysis** demonstrates that many tie-in arrangements have no adverse effects on competition. Just the opposite is suggested: tie-ins can **enhance efficiency and benefit consumers** in various ways—through the creation of **economies of scale**, through evasion of **price regulation**, and through certain forms of price discrimination (**Robert Bork, The Antitrust Paradox 365-81 (1978)**).
Digital Equip. Corp. v. Uniq Digital Technologies	In A.O. Smith, the Seventh Circuit, in ascertaining whether tied **market power** was required in 1985 for purposes of evaluating a legal malpractice case, stated that a showing of **market power** was required for a tying contract action under both the per se and rule of reason standards.

Notes: This Table shows excerpts of the top 6-10 opinions sorted on $EconLang_i$ (corresponding to the first five entries in Table 4.1). Words in boldface are a direct use of economic concepts, citing economic authority, or reference to economic experts who offered testimony. These excerpts are manually selected from the entire opinion because they best exemplify a judge's embrace of economic ideas. Not all boldface words are (top) economic bigrams, although some are.

1993). On the other hand, it casts doubt on the claim in Kaplow (1987) which argues that economics was well-received by courts long before the Chicago school. Kaplow reached this conclusion after looking into landmark Supreme Court cases in early and the more recent eras. It is true, as Kaplow states, that the courts have long viewed economic analysis as a useful source of wisdom for antitrust law, and concepts such as "cross-elasticity of demand" appeared in decisions as early as the 1940s. Most importantly, over the past several decades, the use of economic reasoning has *on average* become ever more prevalent in antitrust case adjudication. This trend echoes the larger picture of rising law-and-economics language in all court of appeal cases during the same period, as charted in Ash et al. (2019). Figure B.3 in the Appendix uses alternative normalizing factors and economics training libraries to calculate $EconLang_i$ for antitrust law. The trend resembles that shown in the main graph.

Figure 4.4: Evolution of Economics Language in District Court Opinions
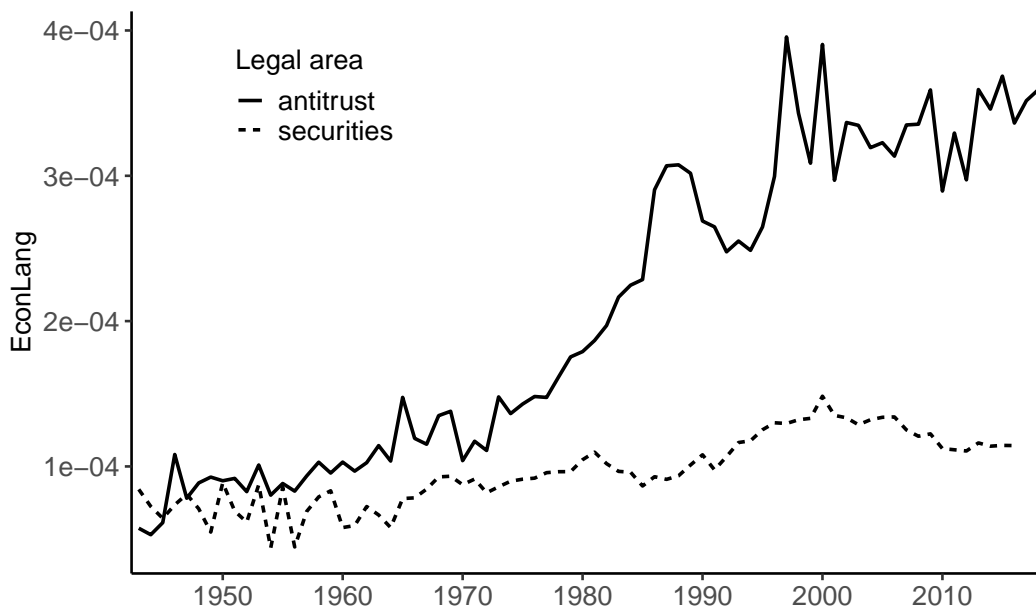
Notes: The economic case sample, which consists of antitrust and securities regulation cases from 1932 to 2016. This figure plots the average case-level measure of economic reasoning $EconLang_i$ by year. The measure is constructed using L_2 norm, such that the normalizing factor $c_{k,i} = idf_k/||f_{b,d_i} idf||_2$. The trend before 1943 is not shown because the small number of cases would create vast sampling error in the mean.

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Moving on to securities regulation, we see an increased adoption of economic reasoning in those cases, although at a much slower pace than in antitrust law. The uptick in the 1990s corresponds to an increase in the use of expert witnesses following the passage of the Private Securities Litigation Reform Act (PSLRA). In typical PSLRA class action suits, expert testimony may be drawn upon to prove (disprove) materiality or loss causation and to determine damages in cases of securities fraud. More specifically, economic experts may be retained to conduct event studies to show that price movements are indeed due to alleged fraud. Key to this methodology is a regression analysis of stock price changes that must control for market-wide and industry-wide trends unrelated to fraud.

Viewing the plot as showing the equilibrium outcome of the idea diffusion/absorption process, a natural question is what explains the divergence in both the level and growth rate of economic reasoning across different legal fields. One possibility is the varying openness of an area of law to economic ideas because of legislative constraint. The Sherman Act of 1890 is well-known for its extremely open-ended and indeterminate language. The main provision of the act prohibits "every contract, combination, or conspiracy in restraint of trade" and any "monopolization, attempted monopolization, or conspiracy or combination to monopolize." The broad language was intentionally crafted by the Congress, allowing the courts to slowly define and narrow it through judicial opinions, in the fashion of common law. The courts, in turn, are free to consider a wide range of analytical criteria to resolve antitrust disputes, the single most notable of which is the one devised by economists.

In contrast to the vague language of the antitrust statutes, the statutory scheme in the majority of federal regulations is comprehensive and precise. For example, the Securities Act of 1933 provides lengthy and detailed instruction about securities registration and the disclosure of information to the public. The Clean Air Act of 1970, 464 pages long in the United States Code, is augmented by even more comprehensive federal and state regulations that control air pollution (Schauer, 2009). Regulation in workplace safety, equal employ-

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ment, and taxation are similarly detailed and precise. Therefore, the antitrust statutes are unique in that they make room for economists to affect court rulings, administrative rule formulation, and enforcement to a degree unreachable under most other federal regulatory schemes (Kovacic, 1992).[16]

Nevertheless, it would be incorrect to attribute the slower growth and lower level of economic reasoning in securities regulation litigation entirely to the ambiguity of the statute language. After all, Section 10b in the Securities Exchange Act of 1934, the main antifraud provision, simply prohibits "any manipulative or deceptive device". The rules and regulations promulgated by the enforcement agency authorized under the Act are equally brief. Hence, it leaves vast space for the courts to fashion the entire law concerning securities fraud, not unlike the area of antitrust. An additional difference in the adoption of economic ideas may be the level of institutional capability in economic analysis at the regulatory agencies (Verret, 2013). Since the 1970s, antitrust enforcement by the Federal Trade Commission (FTC) and the Department of Justice (DOJ) has brought claims grounded in economic analysis that have allowed the courts to afford a high level of deference to prosecuting agencies. The Securities and Exchange Commission (SEC) lacked a comparable institutional commitment to economic analysis, and only recently has it begun to staff economists and increase interaction with the external economics profession[17].

I conclude this chapter on quantification by mentioning caveats that concern the interpretation of the measure. Although the constructed measure of economics language is shown to capture the use of economic reasoning in judicial opinions, it says nothing about

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16. This by no means denies the crucial role of statutory interpretation and interpretation of administrative regulations even when texts of the rules are seemingly determinate. The rules are rarely self-sufficient and require judges to interpret and make their decisions on the basis of their own judgment of fairness and reasonableness.

17. The Division of Economic and Risk Analysis (DERA) was created in 2009, with the goal to "integrate financial economics and rigorous data analytics into the core mission of the SEC". It contributes to a wide array of agency activities, including policy-making, rulemakings, and enforcement of securities law. As a comparison, the economics division of the FTC, known as the Bureau of Economics, was created in 1915. The core functions such as merger review and antitrust analysis can be traced back to 1950s.

the particular ideas in the legal area of interest. Antitrust law is an example. The antitrust revolution, as it is coined and discussed by Kwoka and White (2014), maps the transition from the structure-conduct performance paradigm of the first half of the 20th century to the Chicago school of economics since the 1970s, and then to an ongoing practice that features more refined theory and empirics. This latest development incorporates information, learning, and behavioral economics into the antitrust analytical framework and deploys more complicated econometric methodologies. My measure construction does not treat an "idea" as a unit, and, therefore, it cannot grasp this evolution in paradigms. In Appendix, Figure B.2, I perform a simple counting of characteristic terms that represent ideas over time. The trend is consistent with the qualitative account of the antitrust revolution.

CHAPTER 5

HETEROGENEITY IN JUDGE ECONOMICS SOPHISTICATION

The previous chapter focuses on measuring economic reasoning in individual case opinions. We also observe substantial variation of this measure over time and across different legal areas. Could the remaining variation be a consequence of the judges to whom cases are assigned? After all, judges have significant discretion about deference to optional authorities and how opinions should be written. To formally look into this question, I perform an analysis of variance. Table 5.1 shows the amount of variation in $EconLang_i$ that can be explained by each set of fixed effects. The use of economics language in judicial opinions is highly volatile and heterogeneous. The time trend can explain only 3% of the variation in economics language. There is large amount of variation within circuit-year cells. The third row shows that the case category explains a fairly large proportion of the variation (8.5%), which is in keeping with the consistently lower level of economic reasoning in securities regulation cases compared to antitrust, as demonstrated in Figure 4.4. After controlling for type-of-case, year, and circuit-year fixed effect, 85.7% of the total variation remains unaccounted for. It turns out that permanent differences across judges (i.e., the judge fixed effect), can explain another 6.8%.

The results from this variance decomposition exercise suggest that even within a case category at a given circuit-year, substantial variation in economic reasoning in case opinions exists among judges. I now show through a formal econometric framework that judge effects can be identified under the assumption of case random assignment, and I discuss approaches to estimation.

Table 5.1: Variance Decomposition of Economics Language ($EconLang_i$)

Variable	Proportion Explained
Year FE	3%
Circuit-Year FE	2.8%
Type of Case FE	8.5%
"Case-judge level"	85.7%
Permanent differences across judges (Judge FE)	**6.8%**
Residual	79%

Notes: Analysis of variance by projecting the economics language measure $EconLang_i$ on various sets of fixed effects. Judges who authored fewer than 10 opinions during 1932-2016 are excluded from the sample. $J = 946$ judges author a total of $N = 30,005$ opinions.

CHAPTER 6

EMPIRICAL FRAMEWORK

Building on case random assignment, I can identify judge-specific tendencies from the following general specification:

$$Y_i = X_i'\beta + \mu_j + \epsilon_i \tag{6.1}$$

where y_i is an outcome variable of case i, X_i are case controls, which in the baseline regression include fully interacted circuit-year dummies. The identifying assumption for treating linear regression (6.1) as a causal model is $\epsilon_i \perp \mu_{j(i)}|X_i$, which is satisfied by the case random assignment discussed in Chapter 2. As a result, the fixed effect parameter μ_j can be interpreted as the causal effect of being assigned a certain judge for a case outcome. The error term is allowed to be correlated within judges and heteroskedastic. The data structure resembles unbalanced panel data in which each judge has a different number of cases, denoted by N_j. I use OLS to estimate equation (6.1). Robust standard errors are clustered at the judge level, and the results go through if we cluster at the circuit level.

To obtain a measure of judge economics sophistication, I project the case-level share of economic reasoning ($EconLang_i$) on circuit-year controls as well as judge dummies, as in regression equation (6.1). A formal justification for treating economics language as a proxy for actual knowledge of economics is provided later in this chapter.

Specifically, I run the following regression:

$$EconLang_i = \delta_t + \delta_{ct} + \delta_k + \theta_j + \epsilon_i \tag{6.2}$$

where the measured use of economic reasoning of case i is regressed on circuit-year dummies δ_{ct} and year dummies δ_t. To absorb additional variation in $EconLang_i$, I also include

dummies for the legal area of the case δ_k.[1] Notice that in this regression circuit (or court) effects must be dropped; otherwise they would be perfectly colinear with the judge effects[2]. In order for all the parameters, including the judge fixed effects, to be consistently estimated, I exclude judges who author fewer than 10 opinions in the sample. Notice that this equation is exactly decomposing $EconLang_i$ into various components, yielding the breakdown of total variability in Table 5.1.

6.1   Statistical Test of Heterogeneity

To see whether judges are heterogeneous in terms of economics sophistication, we can use the F test of the null $\mu_1 = \cdots = \mu_J$. This gives a F statistic equal to 1.394 as well as a p-value of 3.568e-6, which rejects the null hypothesis. Nevertheless, one known issue of F test is the tendency to overreject the null when the number of cases per judge is small (Abrams et al. 2012). The problem arises from finite sample bias, wherein the large N assumption upon which the distribution of the F-statistic relies will fail due to a small caseload within a given circuit-year cell. The other source of problem is non-normally distributed errors. A useful alternative to the F test is randomization inference, which is a straightforward and intuitive solution to the challenges discussed.

   The idea is to simulate S datasets from the empirical distribution, assuming the null. Procedurally, in simulation s, for each case of a given judge j, draw from the $EconLang$ of cases that are within the same circuit-year cell. Under homogeneity, this will also be judge j's use of economic reasoning for the case. Repeat this step until all cases are filled with simulated $EconLang$. I then calculate the mean for each judge and compute interjudge disparity, such as interquartile range (IQR) D_s^{25-75}. Plot the empirical disparity against the

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1. Another reason to include legal area controls is that $EconLang_i$ is separately constructed for each area of law using different training libraries. Although they are in comparable units, and random assignment should make the case characteristic irrelevant to judge fixed effect once conditional on circuit-year FEs, I nonetheless control for legal areas to have more precisely estimated judge fixed effects.

2. Recall moving judges together with the cases they author are dropped from the sample.

distribution of simulated disparity; under the null, it should be indistinguishable from the simulated scenarios. We can compute the p-value, which is the probability of observing the empirical disparity, if cases are randomly assigned and all judges have the same economics sophistication. If the value is small, we can reject the null and conclude that there are statistically significant judge-specific differences.

Figure 6.1: Randomization Inference: Judge Specific Economics Sophistication
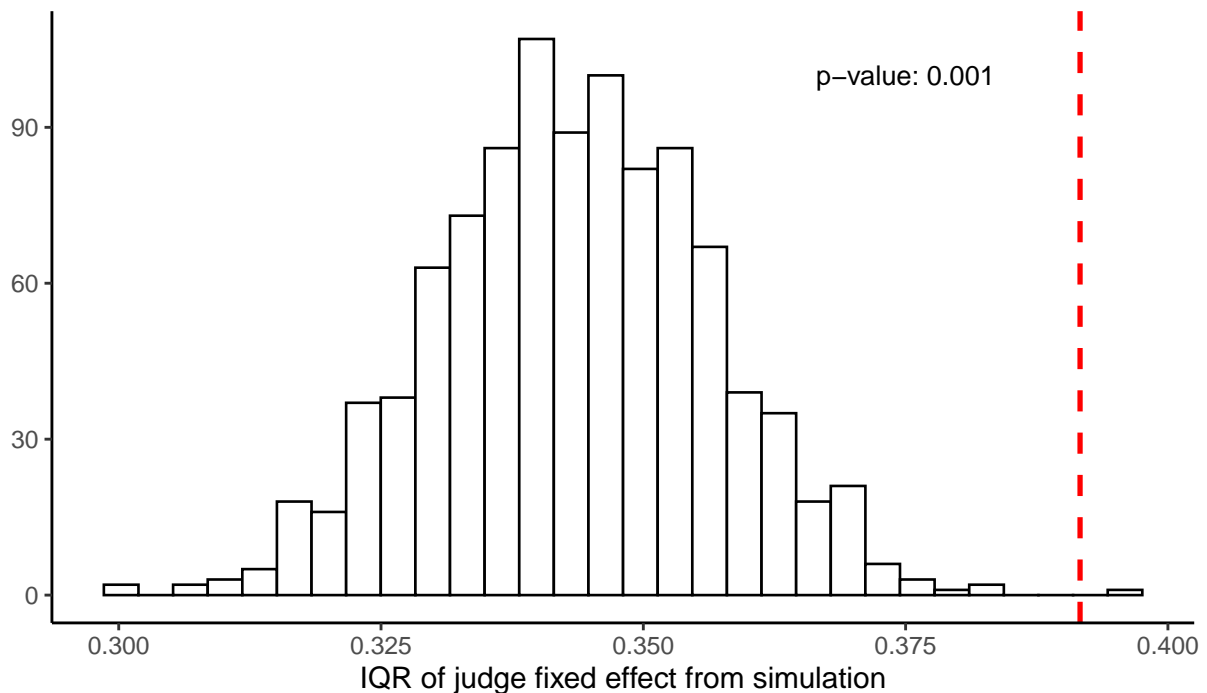
Notes: Economic case sample, consisting of antitrust and securities regulation cases during 1932-2016. A total of $J = 946$ judges authoring $N = 30,005$ opinions and each judge writes at least 10 opinions. Histogram shows the distribution of interquartile range (IQR) of the judge fixed effect from 1,000 simulations, under case random assignment and homogeneity of judge economics sophistication. Dashed red line is the empirical IQR from the data. The p-value indicates the proportion of the simulated distribution that have a larger IQR spread than the empirical distribution.

Figure 6.1 shows the result of this randomization inference exercise. The histogram corresponds to the distribution of IQR of the judge fixed effect from $S = 1,000$ simulations under the null of case random assignment and homogeneity of judge economics sophistication. The dashed line in red shows the empirical IQR in the original data plotted against the

histogram. The p-value is equal to 0.001, indicating that in a given case, there is a large and significant heterogeneity in the inclination to use economic reasoning in judicial opinions among judges. Put differently, although federal judges have a general expertise[3], some judges exhibit a higher degree of economics sophistication than others, and they have substantial discretion when it comes to referencing economics treatise as secondary authorities and evaluating economics evidence in written opinions.

6.2   Documenting Judge-Specific Economics Sophistication

Figure 6.2 plots the kernel density of the estimated judge fixed effect $\hat{\theta}_j$ (standardized, in the solid blue line) against its best fitting normal (the dashed green line). Since the equation to identify judge effects cannot additionally include circuit dummies, I demean estimated θ_j by its circuit average. In fact, my measure of economic reasoning at the case (judge) level shows clear separation by circuit. Figure B.4 in the appendix shows the mean of θ_j across circuits from a projection of $\hat{\theta}_j$ (standardized) on a complete set of circuit dummies without a constant. Even after controlling for case category and circuit-year FEs, judges in different circuits employ significantly disparate levels of economic reasoning in their judicial opinions. This is consistent with systematic differences in legal language across circuit, conditional on legal area and year found (see Ash and Chen [2019])[4]. The point estimate implies that the Eighth, First, and Seventh Circuits are most comfortable of all circuits using economic reasoning in judicial opinions. Interestingly and perhaps not coincidentally, these same three circuit are the most conservative, judging by a simple measure of conservatism (Sunstein et al., 2006). A systematic examination of this link is presented in Chapter 7.

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3. In the United States, specialized courts have jurisdiction over particular fields of law (e.g., U.S. Tax Court, Bankruptcy Court). Whether specialist judges make better decisions than generalist judges is controversial.

4. They represent each case opinion as a multi-dimensional document embedding. Conditional on topic-year and averaged by judge, they show clustering of case embedding by circuit in two-dimensional visualization.

Figure 6.2: Density of Judge Specific Economics Sophistication (Standardized)
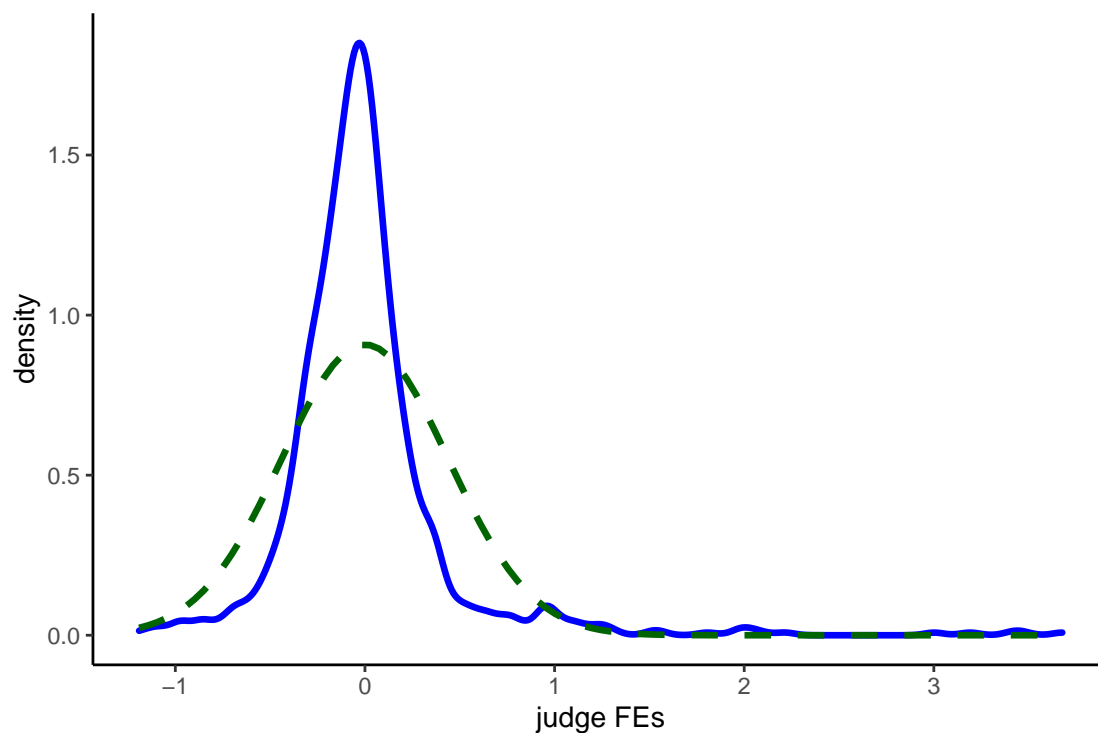
Notes: The solid line shows the density of judge fixed-effects estimates $\hat{\theta}_j$, demeaned by circuit and standardized to have a unit standard deviation. The dashed line shows its best fitting normal density. $J = 946$ judge FEs are estimated from $30,005$ opinions where each judge writes at least 10 during 1932-2016.

In what follows, I discuss why it is conceptually appropriate to conclude that the use of economic reasoning reflects the economics sophistication of judges, and I provide supportive anecdotal evidence. If measured economics language captures the actual use of economic reasoning, we would expect judges who acquired their J.D. education after 1970 in law schools known to be strong in the law-and-economics approach to, on average, show a greater familiarity with economics ideas, and, therefore, to use more economic reasoning in case adjudications and to issue opinions that have higher $EconLang_i$ scores. I test the association between attending a law-and-economics law school and the use of economics language in judicial opinions. For ease of interpretation, I standardize the measure by its sample standard deviation. Table 6.1 shows the result.

Columns 1-4 record case-level regressions in which the measure of economic reasoning in case i $EconLang_i$ (standardized) is regressed on covariates of the judge who sits on the case, controlling for circuit-year, court, and year fixed effects. To absorb variation in $EconLang_i$, all regressions also control for legal area of the case. Column 1 presents the baseline regression in which I regress $EconLang_i$ on the binary variable of judge attendance at law-and-economics law school after 1970, controlling for circuit-year, court, and year FE. Conditioning on these fixed effect dummies takes out the confounding case characteristics; consequently, the variation in $EconLang_i$ is not driven by differences in cases. The main coefficient is positive and statistically significant at the 1% level, suggesting that acquiring a JD education in a law school that has a strong law and economics intellectual environment is associated with a 0.178-standard-deviation increase in the use of economic reasoning in future district court case opinions. Column 2 builds upon the baseline regression by adding other judge covariates, including demographics (gender and race), ex-ante ideology (party of appointing president), and other education background (has a graduate law degree, i.e., S.J.D.). None of these covariates are statistically different from zero, but the main coefficient remains significant. In fact, the magnitude of the association increases.

Ash et al. (2019) document a causal effect of the Manne training program on conservative ruling and the use of law-and-economics language in appellate court opinions. Column 3 controls for an indicator of ever having attended the training program; the correlation is close to zero, but it is not statistically significant. The main coefficient of interest does not change with the inclusion of cross-sectional judge attendance at Manne. Column 4 considers an alternative definition of law-and-economics-heavy law schools that only includes Chicago, UVA, and USC. While the size of the coefficient decreases, it remains statistically significant at the 0.05 level.

Column 5 estimates the same object as the first column, but, adopting a two-step approach, it first estimates judge economics sophistication from equation (6.2) and then projects it onto judge covariates while controlling for court dummies. See Chapter 7 for more details about identification and the two strategies for estimation. Note that the results are materially the same.

This set of results shows that J.D. education at law schools that were strong in the law-and-economics approach after 1970 is significantly correlated with a higher usage of economics language in judicial opinions later written by those who became district court judges. This is true when I focus on within court-and-year variation, which amounts to random case assignment and within-court variation across judges. This association gives us confidence that $EconLang_i$ indeed captures the use of economic reasoning in written opinions. Meanwhile, the exercise lends validity to the act of regarding the use of economics language in court opinions as a proxy for a judge's ability to deploy economics knowledge, otherwise known as judge economics sophistication. If not through acquiring economic ways of thinking, what other channels can explain the correlation between being exposed to economics knowledge in law school and using more economic reasoning?

Another piece of evidence that suggests that economics language can be mapped onto knowledge is the finding by Ash and Chen (2018) that circuit and supreme court judges who

Table 6.1: Association with Law School Attendance

| | Dependent variable: EconLang (standardized) | | | | |
	(1)	(2)	(3)	(4)	(5)
Attended L&E Law School+ After 1970	.178***	.182***	.182***		.166***
	(.060)	(.063)	(.063)		(.060)
Attended L&E Law School After 1970				.157**	
				(.064)	
Appointed by Republican President		-.022	-.022		
		(.016)	(.016)		
Male		-.032	-.032		
		(.024)	(.024)		
Black		-.041	-.041		
		(.029)	(.029)		
Has Graduate Law Degree		-.011	-.011		
		(.040)	(.040)		
Ever Attended Manne Program			-.001		
			(.017)		
Court FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	No
Circuit-Year FE	Yes	Yes	Yes	Yes	No
Observations	25,106	25,106	25,106	25,106	742
R^2	.165	.165	.165	.165	.954
Adjusted R^2	.137	.138	.138	.137	.949

Notes: The economic case sample. L&E law school refers to those that have a strong law and economics intellectual environment, such as the University of Chicago, the University of Virginia, and the University of Southern California. L&E law school+ additionally includes the University of Miami, Emory, and George Mason. See the data chapter for detailed description of the variable coding and source. Columns 1-4 follow the reduced form approach of estimation in equation (7.2). Standard errors are clustered at the judge level. Column 5 uses the two-step approach in which estimated judge economics sophistication is regressed on judge covariates. White robust standard errors are reported. All regressions control for legal area of the case. $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

write similarly to Judge Richard Posner are also known to employ economic analysis in their opinions. Moreover, these judges show an unusual familiarity with and interest in economic ideas. [5]

My own estimation of the judge fixed effect from equation (6.1) provides additional corroboration of the measurement methodology and the interpretation of language as a proxy for knowledge. I rank judges by their estimated fixed effects, which I obtain by regressing $EconLang_i$ on circuit-year dummies, year dummies, legal area dummies, and judge dummies. Judge James Noland turns out to be the highest-scoring judge among all district court judges in the seventh circuit. Remarkably, according to a biography[6] published by the district court of Southern Indiana, James Noland earned an MBA degree while attending Harvard Business School in 1942, and afterwards the remained a firm believer in free enterprise until the end of his life:

> Years later, Judge Noland credited his experiences at the Harvard Business School as having had "a very strong impact upon my thinking and upon my conviction that the free enterprise system should be given as much leeway as possible to permit business and industry to continue to operate without being trammeled with excessive regulation and either excessive congressional or judicial regulation." While acknowledging that he might not always support every action undertaken by a business, he viewed the free enterprise system as "the basis for the strength and the greatness of this country" and believed that "corporate and business responsibility transcends the evils that are oftentimes ascribed to them."
>
> ...
>
> Judge Noland once explained what he thought accounted for the conservative

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5. For example, seventh circuit judge Frank Easterbrook coauthored a well-known book titled *The Economic Structure of Corporate Law*; while Supreme court justice Stephen Breyer published an article on "economic reasoning and judicial review" in *The Economic Journal*.

6. `https://www.insd.uscourts.gov/sites/insd/files/James%20Ellsworth%20Noland.pdf`

label some placed on him, saying that he had a deep faith in the importance of the free enterprise system in the development of the United States and no personal desire to tamper with it unless such was unavoidable."

This anecdotal evidence not only suggests that my constructed measure $EconLang_i$ captures the use of economic reasoning, as reflected in judges' knowledge of economic ideas; it also illuminates the potential of ideas to shape pro-business orientations. I now turn to a formal examination of this relationship.

CHAPTER 7

ASSOCIATION BETWEEN ECONOMICS KNOWLEDGE AND PRO-BUSINESS RULINGS

This section revisits the question of judicial decision-making for a sample of district court regulatory economic cases from 1970 to 2016. I begin by showing that judges differ significantly in their pro-business voting tendencies. I then correlate this dimension of judge heterogeneity with judge economics sophistication, uncovered in the last chapter, and show that they are positively associated. This motivates a novel channel not previously considered in the literature: degree of economics sophistication, which affects a judge's inclination to be or not be pro-business. I further investigate this relationship in a regression framework that allows me to control for additional judge characteristics. In particular, I consider political ideology, which the previous literature finds to be predictive of conservative ruling and a pro-business attitude[1]. I discuss the possible mechanisms that underlie this correlation and provide suggestive evidence against a few of these channels.

7.1   Heterogeneity in Pro-business Leaning

Consider regression equation (6.1), in which the dependent variable is a binary indicator for whether a decision in case i is pro-business or pro-government. Leveraging case random assignment in the same way as I identify judge economics sophistication, the judge fixed effect μ_j captures the pro-business propensity of a given judge. A higher coefficient corresponds to a judge who is more sympathetic to business and less favorable towards government regulation. This is an improvement over Epstein et al. (2013), who rank the pro-business leanings of individual Supreme Court justices during the 1946-2011 terms on the basis of

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1. Business-conservative does not imply conservativeness in all matters. However, as Epstein et al. (2013) show, they largely agree for economic activity related cases, which includes the regulatory economic cases that I consider in this paper.

the fraction of those justices' votes for business. Although it is valid to compare judges who sit on exactly the same cases, ranking them without controlling for changing case mixes over the course of court terms is problematic. The differences in judge's pro-business scores might be an artifact of their having faced very different types of cases. In other words, case characteristics could confound judge fixed effects.

By conditioning on fully interacted circuit-year dummies (circuit dummies omitted to avoid multi-colinearity), I can tease out the effect of judges from case level features. To make sure all judge effects are consistently estimated, I exclude from the estimation sample judges who author fewer than 4 opinions. Figure 7.1 plots the kernel density of judge-specific pro-business scores estimated from OLS regression, demeaned by circuit average. The distribution is close to normal. Using the same randomization inference strategy used in the previous chapter, I show a statistically significant heterogeneity in judge pro-business orientation.

7.2 Bivariate Heterogeneity

Why are some judges more pro-business than others? The previous literature identifies judge political ideology (proxied by the party of appointing president or author-constructed measures) as one explanation (Epstein et al., 2013), but there are likely to be others. It is plausible that judges who are particularly familiar with economic thinking are more wary about excessive government intervention, and, hence, they display more favorable leanings toward businesses, such as those of Judge Noland. Also consistent with this view is the temporal correlation between prevalence of economic ideas in law and an increasingly pro-business Supreme Court since the 1960s. In what follows, I formalize this prediction exercise and show simple scatterplots that relate a judge's pro-business orientation to his economics sophistication.

To account for judges' pro-business orientation using a rich set of characteristics, I propose

Figure 7.1: Density of Judge Specific Pro-business Leaning Estimates (Demeaned)
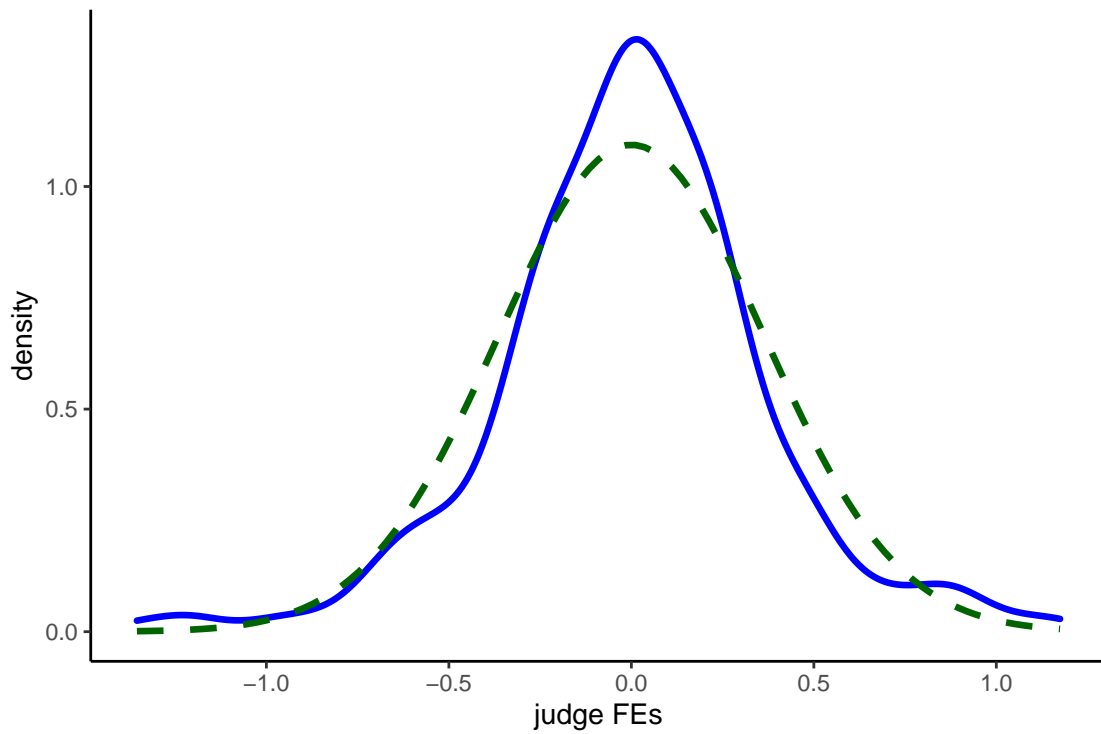
Notes: The solid line shows the density of judge fixed-effects estimates $\hat{\mu}_j$, demeaned by grand average. The dashed line shows its best fitting normal density. $J = 277$ judge FEs are estimated from $2,037$ opinions. Only judges who wrote at least 4 opinions from 1970 to 2016 are included in the sample for this analysis.

the following linear regression:

$$\mu_j = W_j'\delta + \nu_j \tag{7.1}$$

where μ_j is the population judge effect defined at the beginning of this chapter that captures pro-business leanings. W_j is one or a vector of judge characteristics collected or constructed in this paper, and it also includes circuit fixed effects. Hence, the main coefficient of δ, δ^p, measures within-court variation. The goal is to consistently estimate δ. An obvious approach is the two step estimator, in which I first estimate judge fixed effect μ_j and in the second step project that on covariate(s) of interest W_j (Dobbie and Freyer, 2013). Because the LHS of equation (7.1) is estimated with varying degrees of precision, I use White robust standard errors to make inference on δ[2].

Figure 7.2 plots judge pro-business leanings against economics sophistication (standardized to have unit variance for ease of interpretation), both residualized by circuit means. The dots correspond to 277 judges for whom we have estimates for these two dimensions of heterogeneity. This excludes judges who author fewer than 4 opinions in the regulatory case sample or 10 opinions in the economic case sample that I use to measure judge economics sophistication. Panel (a) in this figure shows the scatterplot and regression fit using the raw judge fixed effects. It displays a positive correlation between a greater knowledge of economics and a higher propensity to side with businesses, with a regression coefficient of 0.239 statistically significant at the 0.1 level. The judge fixed effect estimators are unbiased, but noisy due to substantial sampling error. To alleviate this concern, I use empirical bayes shrinkage to compress judge fixed effect estimates[3]. The idea is to use information on

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2. The results are robust to inverse precision weighting where each observation in the OLS regression is weighted by the inverse of the standard error associated with $\hat{\mu}_j$

3. Researchers are embracing the empirical bayes method to reduce prediction error in fixed effect estimates when the number of observations per unit is small for selected cells. See its recent application to the estimation of teacher quality (Angrist et al., 2017; Chetty et al., 2014), neighborhood effects (Chetty and Hendren, 2018), and hospital quality (Hull, 2018)

judges for whom there are plenty of observations and, thus, are precisely-estimated to infer quantities for those for whom we have only a handful of data points, assuming they come from a common prior distribution. The resulting posterior has a lower mean squared error for predicting the true individual judge effect (Morris, 1983). Panel (b) shows the plot when both sorts of judge effects are estimated with shrinkage. The positive correlation between pro-business leaning and economics sophistication is sustained and the regression coefficient is statistically significant at the 0.05 level.

Notice that substituting equation (7.1) into (6.1) yields

$$Y_{jt} = X'_{jt}\beta + W'_j\delta + \xi_{jt} \tag{7.2}$$

where $\xi_{jt} = \nu_j + \epsilon_{jt}$. Case random assignment and linear projection imply that the unobserved component ξ_{jt} is uncorrelated with the regressors. This suggests an alternative method to consistently estimate δ. The coefficients can be directly estimated in one step via OLS, and standard errors are clustered at the judge level due to the common judge component in the error term within a judge and independence across judges.[4]. The main coefficient δ^p can be interpreted as the correlation between judge feature W^p and his pro-business tendency. In the next section, I use this reduced form regression to further analyze determinants of judge pro-business orientation. Its major advantage is that to estimate judge fixed effects in pro-business rulings consistently it does not require a large N_j for each judge.

7.3   Main Results

To better understand the positive association between judge knowledge of economics and the pro-business orientation that the scatterplots show, and to determine whether this correlation is robust to inclusion of additional judge covariates, I now turn to regression analysis. I

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4. In the absence of homoskedasticity, OLS is consistent but not efficient. I also run feasible GLS where the variance structure is estimated from data, the efficiency gain is moderate and results are similar to OLS.

Figure 7.2: Within-Circuit Variation in Judge Economics Sophistication and Pro-business Leaning

(a) Fixed-effect

(b) Empirical bayes shrinkage
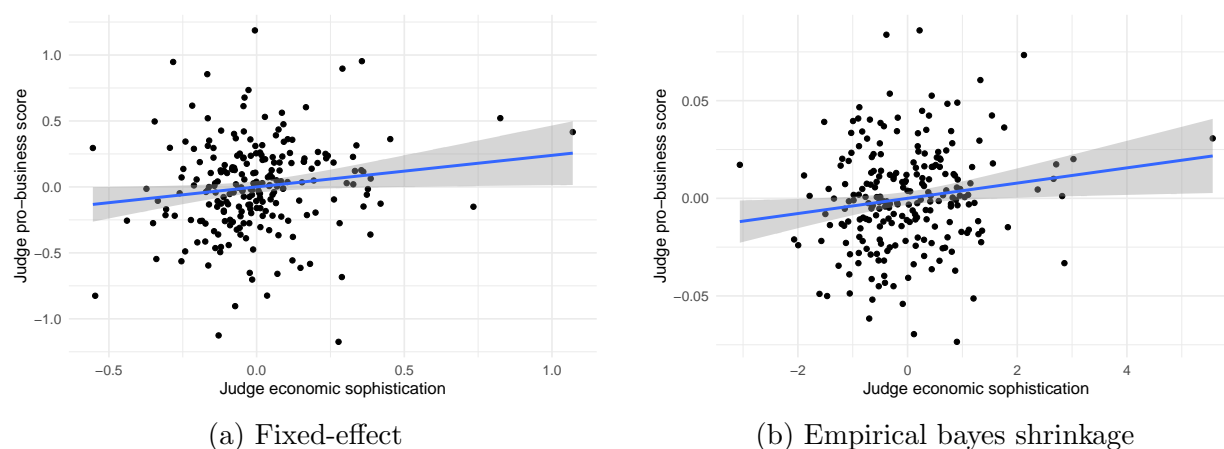
Notes: This figure plots judge pro-business leanings against economics sophistication (standardized to have unit variance), both net of their circuit means. The sample includes 227 judges who authored at least 4 opinions in the regulatory economic case sample during the 1970-2016 time period and at least 10 opinions in the economic case sample during the 1932-2016 time period. The left panel shows the scatterplot and regression fit using estimated judge fixed effects from equation (6.1). The right panel shows the same thing but uses empirical bayes shrinkage estimators for both judge effects to adjust for sampling error. This is necessary because there are a small number of observations for selected judges in the corresponding estimation sample.

empirically test if judge economics sophistication predicts pro-business rulings in regulatory economic cases at federal district courts. Table 7.1 reports the results. In column 1, I regress the binary indicator of the case decision in favor of business on judge-level economics sophistication, standardized to have unit variance. The regression controls for the fully interacted circuit-year fixed effect and case category dummies. I find a statistically significant and economically meaningful association between judge capability for economic reasoning and deciding against government regulation. A one standard deviation increase in measured judge economics sophistication is correlated with a 11.9 percentage point increase in the likelihood that the judge will rule in a pro-business direction. As a benchmark, my regulatory case sample includes 45 percent of the cases that receive a pro-business decision.

In column 2-5, I include additional judge-level covariates to check the robustness of this association. Column 2 controls for the party of the appointing president, and the result suggests that even after conditioning on judge ex-ante ideology, familiarity with economics knowledge positively correlates with pro-business rulings. This is remarkable because it conveys the message that among judges appointed by Republican (Democratic) presidents, those who are more versed in economic reasoning tend to rule more often in favor of business in cases against regulatory agencies. This dimension of judge heterogeneity has not been studied in the previous economics and empirical law literature, which typically has focused on judge party affiliation or other measures of ex-ante political leaning (Fischman and Law, 2009).

Column 3 additionally controls for cohort and career experience variables; the main coefficient remains statistically significant at the 0.05 level. Taking column 3 as my preferred specification, I add in column 4 ever having attended a Manne training program. Ash et al. (2019) find that attending this economics training program has a significant effect on appellate judge decisions. However, as the column shows, in my sample, cross-sectional variation in economics training participation is not correlated with pro-business rulings. Additionally,

the main coefficient of judge economics sophistication stays stable in magnitude. Column 5 examines the association while controlling for prior law school education in a school that embraces a strong law and economics intellectual environment. Although in Chapter 5 I show that this variable is statistically correlated with judge economics sophistication, it does not appear to have a direct effect on pro-business ruling. Our main coefficient remains statistically significant, and the size of the coefficient does not change with inclusion of L&E school attendance. Some might be concerned that the measure construction is performed on a sample of economic cases that includes some of the cases from the regulatory economic case sample[5]. To address this concern, I report in column 6 the regression result using the split sample idea in which the main variable of interest, judge economics sophistication, is constructed excluding regulatory cases. The positive correlation remains significant at the 0.05 level and the magnitude shows a slight increase.

As this regression analysis illustrates, judge specific differences in economics sophistication strongly predict varying inclinations to render pro-business rulings. In light of the fact that judge economics sophistication contains measurement error, and thus it is likely to suffer from attenuation bias toward zero, the precisely estimated positive association is reassuring. I caution that this correlation cannot be interpreted as causal. Under the premise that economics as a branch of knowledge has a philosophical thrust in favor of market and small government, we should expect judges more versed in economic ideas to display less sympathy for government regulation in cases that involve regulatory bodies, given that ideas do have consequences. The correlation can also arise if judges who have a natural inclination towards businesses choose to read more economic relative to their peers, even if they do not subscribe to any economic teachings. Identifying and testing causal mechanisms that underlie this correlation is beyond the scope of this paper. In what follows, I evaluate a few

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5. 757 out of 35004 cases involve federal regulatory agencies considered in my regulatory case sample. Breaking them down by legal area, 125 antitrust cases in my economic case sample have the FTC as parties to the cases, whereas 632 securities cases have the SEC as parties.

Table 7.1: Testing Judge Economics Sophistication

	Dependent variable: Pro-business ruling					
	(1)	(2)	(3)	(4)	(5)	(6)
Economics Sophistication (standardized)	.119**	.122**	.129**	.129**	.129**	.131**
	(.058)	(.059)	(.058)	(.058)	(.059)	(.063)
Appointed by Republican President		.035	.044*	.044*	.044*	.043*
		(.024)	(.025)	(.026)	(.025)	(.026)
Ever Attended Manne Program				.004		
				(.027)		
Attended L&E Law School+ After 1970					.003	.006
					(.080)	(.081)
Type of case control	X	X	X	X	X	X
Cohort control			X	X	X	X
Career exp control			X	X	X	X
θ_j excludes regulatory cases						X
Observations	2,525	2,504	2,504	2,504	2,504	2,455
R^2	.252	.253	.263	.263	.263	.269
Adjusted R^2	.066	.065	.065	.065	.065	.069

Notes: The regulatory case sample. Judges' economics sophistication score is identified and estimated from regression equation (6.1) in which judges who author fewer than 10 opinions from 1932 to 2016 are excluded. To ease interpretation, the core is divided by the sample standard deviation. Pro-business ruling is a binary variable and equals one if the decision is anti-regulation. In the last column, the judge economics sophistication measure is constructed using cases exclusively in the economic case sample, i.e., it excludes 757 cases that also appear in the regulatory case sample. All regressions control for fully interacted circuit-year fixed effects so that results are not driven by the selection of judges into particular cases. Standard errors are clustered at the judge level. *p<0.1; **p<0.05; ***p<0.01

competing explanations and suggest feasible empirical tests in future work that could shed light on this issue.

First, the result is consistent with the story that a knowledge of economics has a practical influence on judicial decision making, and the direction is pro-business[6]. Holding judge ex-ante ideology, cohort, and prior career experience fixed, increasing judge economics sophistication is associated with boosting the probability by 12.9 percentage points that a pro-business vote will be cast. Notwithstanding the fact that in several specifications the coefficient of the party of the appointing president is statistically significant at the 0.1 level, it does not cancel the other plausible channel through which economics knowledge effectively persuades judges about the perils of government regulation.

While causality cannot be established without a natural experiment that generates exogenous variation in judge economics sophistication, detailed information on demographics (gender, race, religious belief, cohort), ex-ante ideology, and prior career experience allows me to directly control for other features of judges that might correlate with both economics sophistication and pro-business rulings. For example, one might be concerned that older judges born some years ago might know less about economics because when they were students economics was a less popular major or because economics formerly played a less of a role in policy making. An older cohort might also be more liberal, and not because their knowledge of economics was limited. In this case an economics sophistication measure might mostly be a proxy for birth cohort. Controlling for cohort and many other judge specific covariates alleviates concern about omitted variables. However, we may still be concerned

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6. This presumes that economics has a laissez-faire thrust, which is not unreasonable for the period under study. Furthermore, we have some evidence that people with economics training have social and political preferences to the right of the ideological spectrum (Fisman et al., 2009). Of course, such findings do not indicate that economics knowledge per se is to the right. Yet no evidence so far suggests the opposite. Future judges who are trained in economics today probably have a more nuanced understanding of government regulation than their predecessors. In cases where big businesses are sued by the regulatory agency, economically sophisticated judges might favor a competitive market rather than big business. This would imply a pro-market, but anti-business coding of the ruling direction. Nevertheless, this is not likely a serious issue here since the majority of cases in my regulatory economic case sample do not concern market competition (only a handful of the FTC-litigated cases concerns this issue).

about the selection on unobservables.

Consider the following hypothetical scenario. judge A is a "number person" who has a natural inclination towards science and precision, whereas judge B is predisposed towards the arts and humanities, and, particularly, concerns regarding the complexity and uncertainty of human existence. Both judges went to law school and later became federal district court judges. Judge A might have acquired a greater knowledge of economics than B because he took economics courses in college, law and economics during J.D. education, or read some general economics books out of personal interest after graduating. At the same time, judge B majored in philosophy in college, took law and literature course when working towards a J.D., and read history books after graduation in his leisure time. Later when rending rulings in regulatory economic cases as district court judges, judge B, due to a lack of interest in and exposure to economics, uses less economic reasoning and more frequently rules in favor of the underdog, which in regulatory cases against business interest means the government. Under such circumstances, economics sophistication and pro-business ruling are positively correlated, but through a channel that does not require economics knowledge to persuade judicial decision makers of the undesirability of government regulation.

To assess the problem of selection (on unobservables), I use the test proposed by Oster (2019). This method is reliant on two assumptions: that the relationship between treatment and unobservables can be recovered from the relationship between the treatment and observables; and that such a relationship is captured by the proportionality parameter δ. Given a value for R_{max}, which is the theoretical population R-squared from a hypothetical regression of pro-business ruling on all explanatory variables, observed and unobserved, we can calculate the value of δ. This can be interpreted as the degree of selection on unobservables relative to observables needed to explain away the result, i.e., $\beta = 0$, in the hypothetical full model.

Table 7.2 shows the result of this test. Column (1) reports the coefficient and R-squared

from the baseline regression, which is conditional on circuit-year fixed effects and case categories, but it does not control for any other judge covariates. Column (2) shows the same under fully controlled regression where judge demographics, Manne program attendance, law-and-economic heavy law school attendance, and prior career experience are controlled for. The two regression are exactly the specifications in the first and last column of Table 7.1. Column (3) lists three values of R_{max}, per suggestions in Oster (2019): 1.5, 2 times the R-squared of the fully controlled regression, and the upper bound 1. I finally calculate δ for each value of R_{max}, and report the results in column (4). With the recommended benchmark of $\delta = 1$, the results suggest that selection on unobservables is not a major concern. The degree of selection on unobservable characteristics would need to be 10 times as large as selection on observables to drive the main effect of economics sophistication to zero.[7]

7.4   Other Possible Correlates with Pro-business Ruling

The results in Table 7.1 suggest that the explanatory variables for judge pro-business voting considered in the prior literature – political ideology and Manne program attendance – have the right sign in my sample of analysis, but are only marginally or not significant at all. In this last section, I closely examine the correlation in the absence of the constructed judge economics sophistication in order to compare and contrast my results with the findings in those previous studies.

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7. Another means of directly determining whether an innate scientific versus a humanities tendency is a potential confounding factor is to draw on a sample of both economic and non-economic cases that have ideological stakes. If this innate inclination, rather than economics knowledge, is driving the correlation between economics sophistication and pro-business ruling, we would expect the relationship to be present in both samples. A finding of the impact of economics sophistication in an economic case sample but not in a non-economic case sample would disfavor the innate-inclination explanation and lend support to a channel in which knowledge of economics shifts judges' political ideology by changing how they think about the market vs the government.

Table 7.2: Oster Test of Selection on Unobservables

	(1) Baseline	(2) Controlled	(3) R_{max}	(4) δ for $\beta = 0$ given R_{max}
β	0.119	0.129	0.3945 (1.5×)	10.873
			0.536 (2×)	5.475
R-squared	0.252	**0.263**	1	1.963

Notes: This table shows the test results produced by the Oster (2019) method of analyzing the impact of judge economics sophistication on pro-business ruling in regulatory economic cases. As illustrated in the first column of Table 7.1, the baseline regression includes only type-of-case dummies and the fully interacted circuit-year dummies. The associated main coefficient β and R^2 are reported. Column (2) shows the regression with full controls, including party of appointing president, ever-attendance in Manne program, an indicator for attending law-and-economics-heavy law schools, birth cohort, and all prior career experience indicators. This is the same regression as in the last column of Table 7.1. Column (4) shows the value of δ, which is the degree of selection on unobservables as a proportion of the selection on observables, would produce $\beta = 0$ given the values of R_{max} specified in column (3). Following suggestions made by Oster (2019), R_{max} is set to be 1.5, 2 times the R^2 from the controlled regression, and the upper bound 1.

7.4.1 Ideological Voting

District court judges, like circuit judges and Supreme Court justices, are appointed by the U.S. president for a lifetime. Sunstein et al. (2006) find that circuit judges appointed by Republican presidents are more likely to vote in a conservative direction in selected area of cases. To see whether this carries over to district court cases, and specifically the sample of regulatory economic cases that involve federal government agencies that I hand collected and coded, I run the same regression in equation (7.2) and report results in Table B.2.

Overall, I do not find that in district courts judges divide along party lines when they make pro-business decisions in regulatory economic cases that involve federal government agencies. This stands in contrast to the statistically significant correlation between being appointed by a Republican president and ruling conservatively – i.e., ideologically voting in certain of the case categories found in Sunstein et al. (2006). On the other hand, the result is in line with the findings of Epstein et al. (2013) regarding supreme court judges in a sample

of business-related litigations. One tentative explanation for the lack of ideological voting in my sample is that district court cases tend to be less ideologically charged, and, thus, legal rules and precedents play a larger part in determining case outcomes. However, this cannot be the case because the ideological stakes increase as we move up the court ladder.

A more plausible explanation is that the subject matter of cases affects whether and to what extent extralegal factors, such as judge ex-ante ideology, play into decision-making. While Sunstein et al. (2006) find evidence of ideological voting in EPA, NLRB and FCC cases, respectively, during the 1984-2005 time period[8], he does not gather cases that involve other regulatory agencies, such as EEOC, SEC, FTC, and FERC, which my sample includes. Due to sample size limitations, I cannot individually test ideological voting by the regulatory agency involved. This exercise nonetheless illustrates the empirical nature of the question about ideological voting and cautions against generalizing to various subjects and levels of judicial decision making.

7.4.2   The Manne Economics Training Program

Ash et al. (2019) hypothesize and find that attending the Manne training program has a causal effect on conservative rulings in appellate court cases. They exploit the Manne economics training program for judges from 1976 to 1998 in order to test whether judges rendered more conservative rulings after attending the program, relative to those never treated. One of the samples they use is environmental and labor regulatory cases that involve the EPA or NLRB. To investigate whether the training program had a similar effect on district court judges, I first tap into the cross-sectional variation across judges in terms of Manne program attendance. The variable *Ever attended manne program* is equal to one if a judge has at least participated in one session of the basic economics institute offered

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8. Data collection follows a different procedure in their paper and is not limited to cases in which an agency is a party to the case. Hence, the sample is not comparable even when the focus is on EPA-related cases.

between 1976 and 1998[9]. The results are reported in Table B.3. I do not find that judges who have ever attended Manne program are more pro-business.

This lack of association between Manne attendance and pro-business rulings may be due to a selection in which judges who choose to participate have less of an understanding of economics to begin with. Those already familiar with economics thinking may not find it worthwhile to halt their work and fly to a resort far from home for an intensive two-week economics course. A simple regression of judge economics sophistication (as a proxy for familiarity with economics knowledge) on ever attending a Manne program, controlling for circuit FEs, suggests a negative correlation between the two, which is consistent with this conjecture. This poses challenges to treating Manne attendance as being quasi-randomly assigned among judges, even after controlling for observables. Unobservable characteristics could well be driving selection into participation–characteristics that themselves are correlated with a pro-business orientation. The other possibility is that the Manne training program simply has no causal effect on how district court judges rule.

To probe this question further and control for unobservable judge characteristics, I consider a difference-in-difference design that takes advantage of the before and after variation within a judge, treated or not. In so doing, I replicate the research specification that Ash et al. (2019) employ in theirs study of appellate court conservative voting in environmental and labor regulation cases. Specifically, I run the following regression:

$$Y_{icjt} = \alpha_{ct} + \alpha_j + \alpha_t + D_{jt}\delta + \zeta_{icjt} \tag{7.3}$$

where the pro-business decision in a case Y_{icjt} is regressed on post-treatment dummy D_{jt}, controlling for circuit-year, judge, and year fixed effect. The identifying assumption is that absent the Manne training program, the treatment and control group will have the same

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9. In my sample some judges have attended the economics training program multiple times over many years.

time trend. One limitation of the data, as mentioned in Section 3.3, is that I do not have the exact timing of program attendance for the period 1976-1986. Hence, in the difference-in-difference analysis, I restrict the sample to cases decided before 1976 or after 1986 for which I know exactly the treatment status of a judge. An inevitable downside is that the number of cases used for estimation shrinks even further when the judge fixed effect parameters are plentiful. Table B.4 in the Appendix reports the estimation results. As in the cross-section, I do not find a statistically significant change in judge pro-business ruling after a judge attends the Manne economics training program.

How do I reconcile the finding of a significant correlation between economics sophistication and pro-business rulings with the lack of effect of the Manne economics training program, given that both capture the notion of exposure to economics knowledge? One explanation, as mentioned above, is that the Manne program has a causal effect, that my lack of knowledge about the exact timing of Manne attendance, together with the limited sample of a fairly large set of judges, might have biased the estimation, reducing the statistical tests' power. In future work this can be addressed by expanding the regulatory case sample to include cases that involve regulatory bodies beyond what I currently consider, such as the EEOC, SEC, EPA, NLRB, FTC, FCC, and FERC. Another possibility is that the Manne program simply does not affect district court judge jurisprudence in the way that it does for appellate court judges. If district judges participate in the program for reasons not related to learning economics – for example, to build connections to higher court judges or enjoy an all-expense-covered break from the regular heavy courthouse workload–this channel will not work. It is also hard to believe that a two-week-long bootcamp would affect a judges' ability to engage in economic reasoning and, thus, affect her decision making over the long term.[10]

One appealing feature of the judge economics sophistication measure, as compared to

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10. Admittedly, Ash et al. (2019) find a causal link between Manne attendance and conservative voting in court of appeal cases using difference-in-difference strategy. Quasi-random assignment of program participation is difficult to justify nonetheless, even conditional on all the observable characteristics.

other proxies for economics exposure, is that it is constructed ex-post and, thus, summarizes all available information encoded in written opinions a judge has produced thus far. It has been proven to be highly correlated with pre-appointment law school education, which is coded ex-ante, before the judge sits in court or issues any opinion. More generally, the measurement methodology shows promise as an approach to quantifying knowledge based on texts, which is otherwise difficult to measure and, hence, to study.

CHAPTER 8

NORMATIVE DISCUSSION

Before concluding this paper, I situate the empirical findings of my research in the broader legal system and dwell on what they imply for the law. While my research cannot make causal claims about the effect of economics learning on judicial preferences, I contemplate the normative aspects of the diffusion of economics into the U.S. judiciary, which is an ongoing process and shows no sign of abating. Starting with the goal of judging, the number-one objective is dispute resolution, where judges rely on legal sources and apply legal reasoning to reach a clear decision. When existing rules and standards do not offer sufficient guidance to resolve the current dispute, judges will, in interpreting statutes or other kinds of rules and regulations, create new rules and precedents that will become binding, or at least instructive, to future case adjudication. Economics knowledge plays a part on both occasions. For example, economics expertise is frequently called upon to resolve factual issues, including, but not limited to, damage calculation in patent infringement, air pollution, securities fraud litigation, and determination of market definition in antitrust cases. In such instances, economics expert testimony is used as an optional authority, in opposition to mandatory authority, to give credibility to a judge's legal argument.

On a more fundamental level, economics is one among many forms of knowledge that motivate a policy value of law, placing enormous emphasis on allocative efficiency. In fact, renowned scholars in law and economics have argued that efficiency should be the only goal that law should pursue (Kaplow and Shavell, 2001) [1]. As a result, values such as morality, equities, and consistency with legislative intents are assigned far less, if not zero, value from a purely economics point of view. From a policy perspective, there are pros and cons to the consequences of legal rules and standards formulated according to these preferences.

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1. Evaluation of this claim is beyond the scope of this paper. The claim drew heated opposition immediately after its publication in the law journal, and the debate about it is far from settled.

The primary merit of devising law to maximize economic efficiency is the predictability and stability attained for legal rules. This is due to the fact that efficiency can be evaluated with an analytical framework that is all too familiar to economists. Antitrust and regulatory economics have over time developed a rich set of theories and empirical approaches for examining firm conduct and the impact of government regulation (Viscusi et al., 2005). The law and economics movement has produced analytical devices for many other areas of law, such as contract, tort, and property. The mathematical formulation of these frameworks makes for a powerful abstraction that can serve as the prototype applicable to different cases across courts and time. Because of this, it limits excess discretion, thus constraining judicial activism.

The downside of a policy view that overemphasizes economic efficiency is it can propagate bias in a certain direction. While my finding of a robust correlation between judge economics sophistication and pro-business orientation does not imply causality, and it may not hold outside my sample of regulatory cases at federal district courts, it is compatible with a channel through which economics induce judges to value free enterprise unfettered by government interventions. If this is the case, what makes economics useful and appealing to judges also makes it problematic. Economics is proud of its scientific methodology and mathematical precision, which is indeed unparalleled among all the social sciences (Lazear, 2000). This lends a natural advantage to economics knowledge being the most well-received expertise used in courts. It may crowd out other sources of evidence that are less structured, technical, or "scientific". Despite the fact that judges have been relying on or even explicitly citing articles from the political science, sociology, and psychology literature, they are cited much less frequently than economics. Figure 8.1 shows a simple plot of the number of cases per year that mention "economist", "sociologist", "anthropologist", or "statistician" in written opinions. In an era that endows an enormous trust on numbers (Porter, 1996) unseen in history, be it in the realm of policy making or everyday life, this problem becomes

ever more pressing.

The dim side of an increased adoption of economics in the federal judiciary does not negate the fact that in many instances, economics knowledge is indispensable to making reasonable judgment in courts. Damage calculation or statistical evaluation of discrimination cannot be correctly done without the use of economics. One question we can ask and have an empirical grip on is whether economics helps judges reach better decisions. A common measure of decision quality in lower courts used in the law literature is the likelihood that a decision will be appealed to a higher court or will be reversed conditional on appeal (Baye and Wright, 2006). Be that as it may, legally good decisions are not necessarily desirable from a social or economic point of view. An alternative way to think about decision quality is through the actual impact a legal decision has on what happens after it has been issued. If a piece of legislation is intended to achieve a certain goal, we can evaluate a court decision in terms of whether and how well the policy goal is obtained in the real world because of the ruling.

For example, the initial policy goal of antitrust legislation is to preserve the competitive process, thereby maximizing total welfare. For a set of merger cases that get approved, if in a comparable case the decision that comes out of heavy economic analysis actually results in higher prices and markups compared to decisions without these, we may conclude that economics in antitrust litigation does not improve or even dampen welfare.[2] In another example, intellectual property law was instituted to protect innovation. However, there is a tradeoff between incentivizing the inventor or patent holder and granting monopoly status to them. If we find that case decisions reached with extensive economics analysis induce future innovation[3] in the related field to a greater extent than those without, this would

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2. One empirical strategy is matching where a case using much economic analysis is matched to a comparable case only with less economic reasoning. An actual empirical test in this context is difficult to execute because the number of merger cases that are litigated in court is extremely small. Most of the proposed mergers are settled in DOJ or FTC administrative proceedings or they are voluntarily dropped after enforcement actions start.

3. Empirical studies can use firm or industry-level R&D expenditure or investment as a measure of innovation.

suggest that economics knowledge does help judges make better decisions. As a first step in empirical studies of this sort, we can regress future firm (market) outcome on the economics sophistication of the judge assigned to the case. This would be similar in research design to the study of firm outcome following court bankruptcy decisions rendered by judges who differ in their pro-debtor tendency (Chang and Shoar, 2013).

Figure 8.1: Frequency of Feferences to Social Scientific Expertise in Court
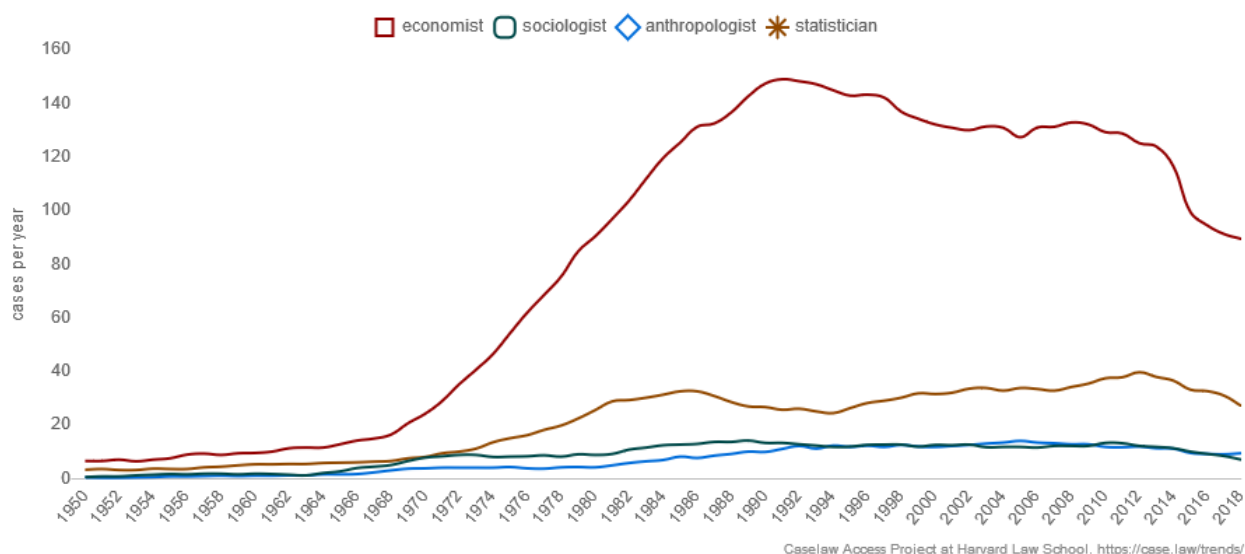
Notes: All cases available in the Caselaw Access Project database. The plot shows the number of cases containing selected terms each year, from 1950 to 2018. Similar to Google Ngram Viewer. Smoothing applied by averaging the nearest 2% of other points of a given data point.

CHAPTER 9

CONCLUSION

Modern states rely on expert knowledge to make policies. Among all areas of expertise, economics is perhaps the most sought after, and it is distinctly visible in the public policy sphere. This is mirrored in the judiciary, where, more than in other social sciences, judges are reliant on economic evidence and influenced by economic authorities. Yet the literature that describes and reflects on this phenomenon is still sparse and mostly resorts to case studies. In this paper, I embark on a quantification endeavour to systematically characterize the use of economic reasoning in district court opinions. Using tools from computational linguistics and drawing upon the full text of written opinions in a large sample of economic cases that span antitrust and securities regulation, I propose a novel measure of economics language for each particular area of law. This measure allows me to capture the use of economic reasoning for individual case opinions, and it goes beyond the law-and-economics concepts to include concrete acts of performing economic analysis.

I report a set of validation results that show that my measure successfully captures the notion of economic reasoning. I find that the distinctively economic bigrams are indeed archetypical of economic analysis and that most make appearances in judicial opinions. Manual audit of the ten highest scoring case opinions demonstrates that my measurement strategy identifies high-profile cases that are known to involve extensive economics, and these opinions contain substantial references to economic concepts, authority and expertise.

With the measure, I produce new descriptive results of the evolution of economic reasoning in federal district courts and across different areas of law. Consistent with prior qualitative accounts, I show that the share of economic reasoning in judicial opinions has been growing since 1943 in both antitrust and securities, although growth has been more rapid in the former. My explanations emphasize that the divergent absorption of economic ideas in different legal fields is affected by legislative constraints and the institutional capac-

ities of enforcement agencies to perform economic analyses.

Variance decomposition of the case-level measure reveals that meaningful variation exists across judges. Leveraging case random assignment to judges within courts, I project a case-level measure of economic reasoning in the economic case sample onto judge indicators and circuit-year, case category controls to form a measure of the economics sophistication of judges. Randomization inference shows that some judges are more economically sophisticated than others, and the differences are statistically significant. I find that going through J.D. education at universities that have a strong law and economics intellectual environment is significantly correlated with a higher measure of the economics language that judges use in case opinions. This corroborates my interpretation that the constructed measure reflects variation in economic reasoning and judges' ability to deploy economics knowledge. Anecdotal evidence from a judge scoring top on the basis of this measure of economics sophistication further supports this interpretation.

To see whether this newly uncovered dimension of judge heterogeneity matters to case outcomes, I collect a novel sample of district court cases involving major federal regulatory agencies that oversee a broad range of markets, including labor market, environment, securities exchange, and telecommunication. I find that assigning a judge who is more capable of economic reasoning shift the ruling direction in favor of business. One implication of this result is that a proposed merger challenged by the Federal Trade Commission is more likely to receive a favorable court decision if it is assigned to a judge who is more adept at economic reasoning. While causality cannot be obtained without a natural experiment, I show that the association is robust to inclusion of additional predictors of conservative ideology, notably ex-ante ideology and judge economics training program attendance. The test of selection on unobservables suggests that it is not a weighty concern.

My finding contributes to the literature on judicial decision making. Previous studies have examined demographics, political ideology, and career experiences to explain differential case

outcomes. This paper proposes a new factor – knowledge of economics – to account for the fact that some judges are more pro-business than others. When the ex-ante political party affiliation of judges does not seem to matter, we can turn to their economics sophistication score to predict how they would rule in upcoming cases. This research opens up a new line of questions regarding knowledge and social/political preferences. Texts contain rich information about what agents know or believe, which may be hard to elicit from surveys or other sources. They can be harnessed to map the agents to a knowledge space, which, in turn, affects how the agents act. For example, using a similar measure construction approach, we can estimate how scientifically versed a patent judge is and whether this trait affects how well he rules in intellectual property cases that involve complicated technological inventions.

This research can be extended in several ways. First, we can look at whether economically sophisticated judges tend to rule in a certain direction along an issue[1] in tort, property, and commercial contracts. These are subject areas that have been significantly influenced by the law and economics approach. Second, the hypothesis proposed and analysis conducted in this paper can also be applied to circuit court judges. There we expect to find an even greater impact of economics knowledge because the ideological stakes are typically higher than in lower courts. Furthermore, my results suggest that economics sophistication as a shifter of decision outcome during the first stage can be used as an instrumental variable to investigate the causal effect of such decision on subsequent individual/household/firm outcomes. Finally, I hope my measure of judge economics sophistication will spur future efforts to causally identify the effects of judge knowledge through exogenously generated variation in it.

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1. For example, determination of liability or damages. Outcome can also be a policy value metric, e.g., tendency to resort to moral value judgment.

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APPENDIX A

DATA

A.1 Data Collection

A.1.1 Law and Economics Heavy Law Schools

Teles (2012) narrates the law and economics movement in which he charts the diffusion of this approach to legal education. The University of Chicago is undoubtedly the cradle of ideas thanks to Aaron Director, Ronald Coase, and Richard Posner, among others. The movement gained momentum in the 1970s largely as a result of the critial entrepreneurial role played by Chicago trained law professor Henry Manne. He launched the Economics Institutes for Law Professors at the University of Rochester before moving to the relatively undistinguished law school of the University of Miami, then under the deanship of former Chicago law professor Soia Mentschikoff. There he created the Law and Economics Center (LEC) and started the economics program for federal judges. Law and economics did not make a splash at elite schools beyond Chicago in the 1970s, yet it became quite successful at the University of Virginia and the University of Sourthern California. They were able to attract exceptional faculties who later became influential figures in the law academia[1].

A.2 Topic Modeling to Extract Economic Reasoning

This section describes an alternative measurement strategy for economic reasoning applied to antitrust using the same cases from my economic case sample. Topic modelling, also known as Latent Dirichelete Allocation, is an unsupervised genarative language model. Word occurances are modelled to follow a multinomial distribution where the probability is a function

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1. See Teles (2012) Chapter 4 for detailed discussion on why these two schools were early adopters of law and economics.

of topic weights and word distributions associated with each topic, both of which are estimated from text data. The goal is to uncover topic(s) indicative of economic reasoning and to compute the topic share in every case opinion, which is our measure of economic reasoning. I implement this algorithm using python package *gensim* on 17,869 case documents and a vocabulary capped to the top 10,000 words by tf-idf. I experimented with the number of topics $K =$10, 20, 30, and 40, and use 20 as the preferred specification.

Table A.1 shows the number of words and size of the vocabulary after each step of text pre-processing to increase signal-to-noise ratio in the data. The table below shows the estimated topics and the associated word distribution ranked from highest to lowest under each topic. Each row corresponds to a topic, represented by the words filling that row. Only the top 15 words are shown. Examining the tf-idf weighed words with domain knowledge about antitrust economics and law, I label topic #1, 6, and 18 as pertaining to economic reasoning. Figure A.2 zooms in the word distribution corresponding to these three topics, and they intuitively show economic reasoning regarding market definition, anticompetitive effect in various types of alleged violations, price effect of proposed mergers, and judge evaluation of economic evidence.

In contrast, other topics do not seem to suggest the practice of economic reasoning. Rather, they are legal or generic subject terms about a particular industry under antitrust investigation. For example, panel (a) and (b) of Figure A.3 show the words associated with topic #9 and topic #15, which speaks about antitrust cases involving health care industry and patent infringement, respectively. The rest of the topics uncovered capture industry dimension of the antitrust litigation, such as insurance, banking, telecommunication, and utilities.

My alternative measure of economic reasoning at case level is therefore defined as the sum of estimated share on topics 1, 6, and 18. The time trend computed with this measure depicted in Figure A.4 is very similar to the series for antitrust in Figure 4.4 from the main

Figure A.1: Topic-Term Distribution Estimated from Antitrust Cases

	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
0	class	settlement	court	member	counsel	plaintiffs	case	action	claim	fee	certification	plaintiff	rule	litigation	see	common
1	evidence	price	defendant	conspiracy	plaintiff	would	expert	testimony	summary	judgment	fact	court	damage	jury	see	show
2	court	motion	state	action	case	claim	plaintiff	law	act	issue	party	federal	file	defendant	f2d	judgment
3	defendant	jurisdiction	court	united	states	plaintiff	district	foreign	case	personal	forum	see	corporation	inc	transfer	business
4	arbitration	agreement	court	see	visa	card	claim	clause	party	bank	merchant	mastercard	issue	contract	network	also
5	plaintiffs	defendants	allege	complaint	conspiracy	claim	allegation	court	dismiss	motion	price	agreement	f3d	antitrust	defendant	plaintiff
6	market	would	price	product	customer	see	merger	competition	service	increase	cost	share	also	use	court	relevant
7	price	store	retailer	would	plaintiffs	egg	publisher	apple	sale	agreement	retail	amazon	franchise	also	sell	business
8	claim	inc	counterclaim	allege	motion	court	dismiss	plaintiff	complaint	allegation	cir	defendant	f3d	act	action	fact
9	hospital	health	medical	service	care	plaintiff	patient	physician	contract	plan	blue	defendant	provider	privilege	insurance	provide
10	judgment	final	states	united	propose	section	defendant	court	decree	consent	may	antitrust	shall	public	entry	compliance
11	agreement	cable	license	nfl	market	would	right	see	restraint	music	television	antitrust	league	game	comcast	player
12	divestiture	shall	judgment	final	united	states	trustee	court	assets	defendants	propose	defendant	section	asset	include	provide
13	agreement	shall	service	defendant	plaintiff	provide	judgment	party	order	term	contract	wireless	business	state	may	provision
14	ncaa	member	injunction	rule	school	association	court	preliminary	board	harm	plaintiff	would	program	irreparable	college	broker
15	patent	generic	drug	claim	antitrust	court	infringement	litigation	inc	market	issue	fda	see	would	agreement	license
16	ftc	call	milk	see	dairy	use	dish	make	dfa	act	order	avaya	consumer	court	also	mark
17	plaintiff	claim	defendant	allege	antitrust	act	state	injury	law	complaint	dismiss	court	see	inc	action	cir
18	market	product	plaintiff	power	claim	inc	relevant	defendant	competition	court	act	monopoly	service	f2d	price	antitrust
19	document	court	information	microsoft	discovery	order	request	party	counsel	software	would	motion	plaintiff	privilege	case	provide

Notes: This table shows the topic-term distribution estimated from topic modelling with the number of topics fixed at $K = 20$. Each row represents one topic, which is represented by the distribution of words listed. Only the top 16 ranked by estimated weights are shown. LDA is performed on a corpus of 17,869 antitrust case opinions during 1932-2016.

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Figure A.2: Words Associated with "Economic Reasoning" Topics

(a) Topic #18


(b) Topic #6

(c) Topic #1

Notes: This figure shows the top 100 words under each of the three topics concerning economic reasoning estimated from topic modelling. Top relevant terms of topic #18 after applying tf-idf weighting are: market, power, monopoly, monopolization, relevant, tying, predatory, anticompetitive. For topic #6, such words are: merger, acquisition, airline, price, guidelines, increase, share, percent. For topic #1, these are: evidence, expert testimony, report, opinion, testify, show, regression, daubert, analysis, variable, fact.

Table A.1: Dimension Reduction of Each Processing Step

	Raw.text	Lemmatization	Alpha	Remove.stopwords	Tf.idf.adjustment
Total words	46,700,106	46,752,695	36,980,672	20,922,847	19,653,224
Unique words	275,654	264,288	156,307	155,297	10,000

Notes: This table shows the number of words and unique tokens at each step of text pro-processing for the corpus of 17,869 antitrust case opinions during 1932-2016.

Figure A.3: Words Associated with Other Topics
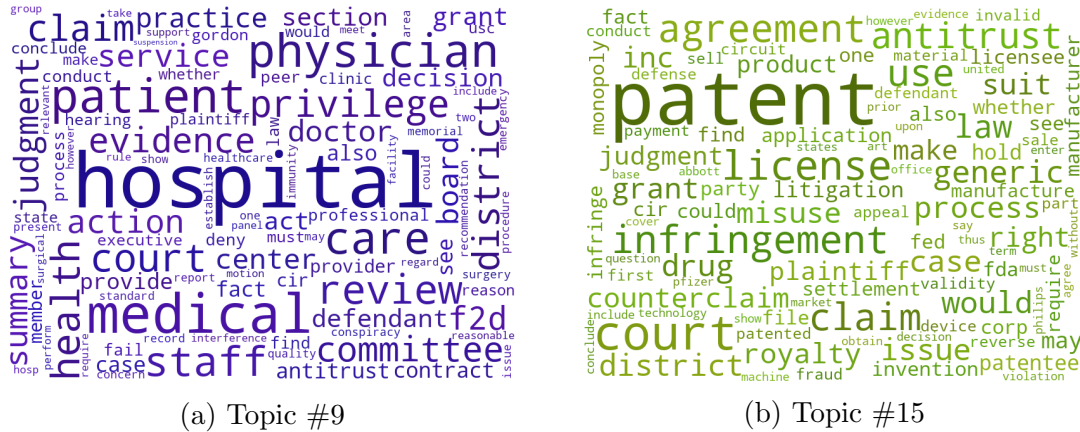
(a) Topic #9

(b) Topic #15

Notes: This figure shows the top 100 words under two selected non-economic topics estimated from topic modelling. Topic #9 by words associated with it seems to capture health care industry, whereas topic #15 reflects alleged vialation relating to intellectual properties.

text, although the scales are not directly comparable.

Figure A.4: $EconLang_i$ Constructed from Topic Modelling
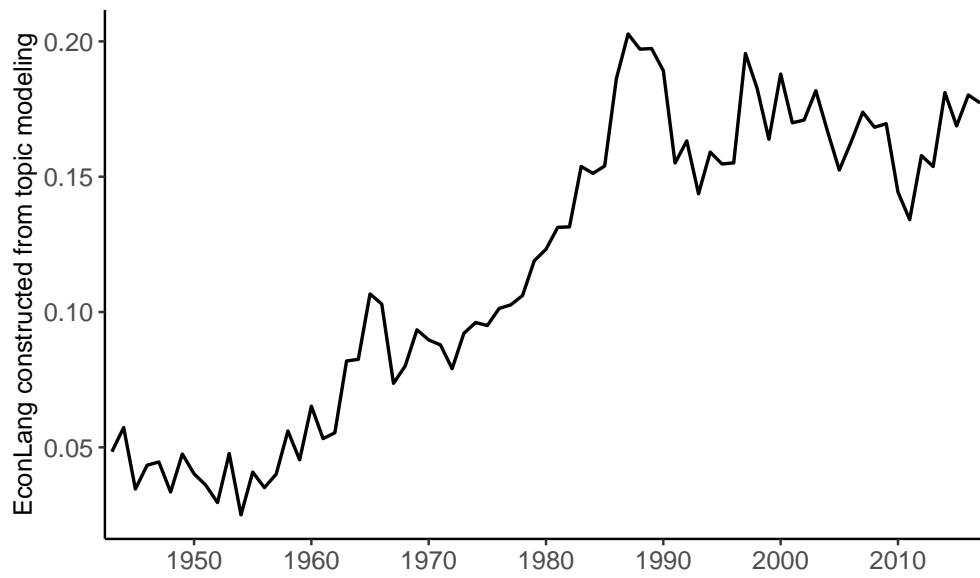
Notes: This figure plots the average case-level measure of economic reasoning $EconLang_i$ by year, constructed via topic modelling. LDA estimated with the number of topics $K = 20$. Trend before 1943 is not shown due to the small number of cases, thus vast sampling error with the mean.

APPENDIX B

ADDITIONAL FIGURES AND TABLES

Table B.1: Random Assignment Test Using Regulatory Economic Cases

|	Dependent variable: judge economics sophistication						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
agency_EEOC	.008						.023
	(.012)						(.037)
agency_EPA		.003					.019
		(.012)					(.036)
agency_SEC			.0001				.017
			(.012)				(.038)
agency_NLRB				-.0004			.018
				(.016)			(.039)
agency_FCC					-.007		.010
					(.037)		(.053)
agency_FTC						-.017	.002
						(.016)	(.039)
Observations	2,525	2,525	2,525	2,525	2,525	2,525	2,525
R^2	.974	.974	.974	.974	.974	.974	.974
Adjusted R^2	.968	.968	.968	.968	.968	.968	.968

Notes: Regulatory economic case sample, consisting of cases involving government agencies during the period of 1970-2016 (See Chapter 3 for further details). The judge economics sophistication variable is constructed by regressing case level measure of economic reasoning on fully interacted circuit and year dummies and judge dummies on a different sample of economic cases. OLS estimation of equation (2.1). The first to sixth columns display OLS estimates from separate regressions of judge economics sophistication on case subject area. The last column shows a joint regression. All regressions include fully interacted circuit and year dummies. *p<0.1; **p<0.05; ***p<0.01. Standard errors (in parentheses) are clustered at the judge level.

Figure B.1: Top 100 Economic Bigrams Used to Measure $EconLang_i$ in Securities Regulation Cases
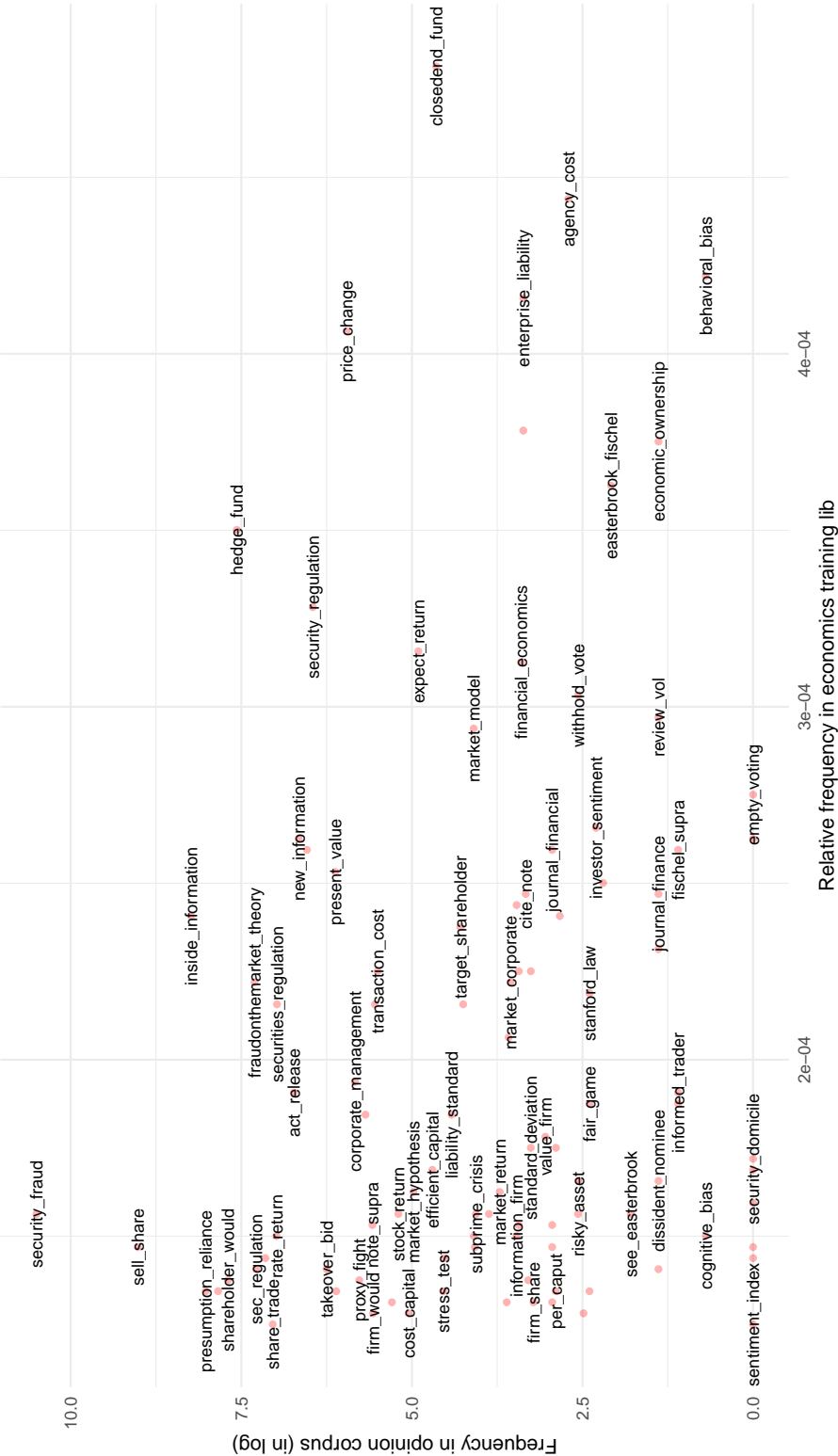
Notes: This scatterplot shows the top 100 economic bigrams in securities regulation, identified from the training libraries. The x-axis reflects the relative frequency of the bigram in the economics training library, $\frac{f_{b_k,\mathbb{E}}}{\|\mathbb{E}\|}$. The y-axis corresponds to the total number of occurences of the bigram in the opinion corpus, in log scale for visualization. The red dots are exact positions, labeled by the respective bigram. To avoid overlapping texts, not all bigrams are displayed. The top 4 bigrams, "proxy access", "stock price", "abnormal return", and "fraud market" are not shown in this graph because they are used too frequently to fit in the plot.
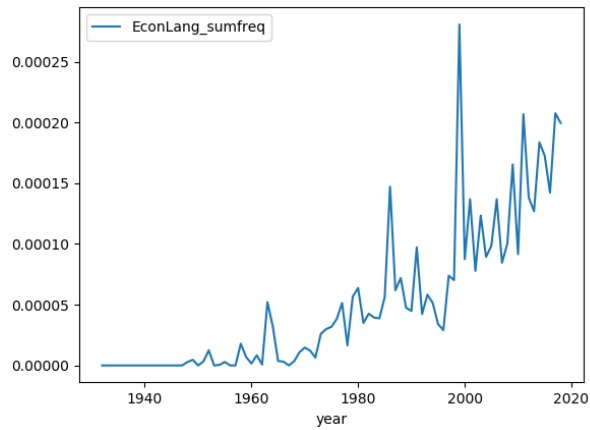
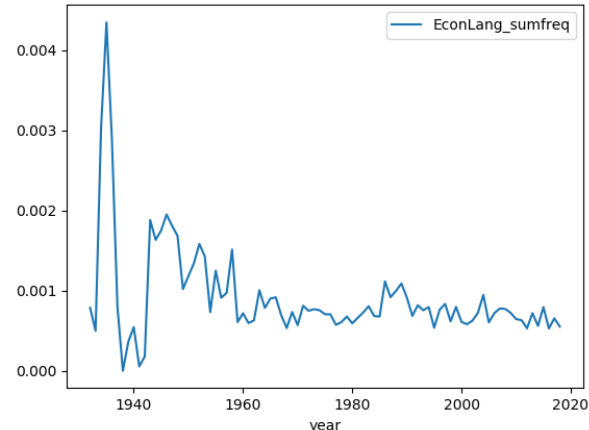Figure B.2: Frequency Count of Economics Ideas over Time
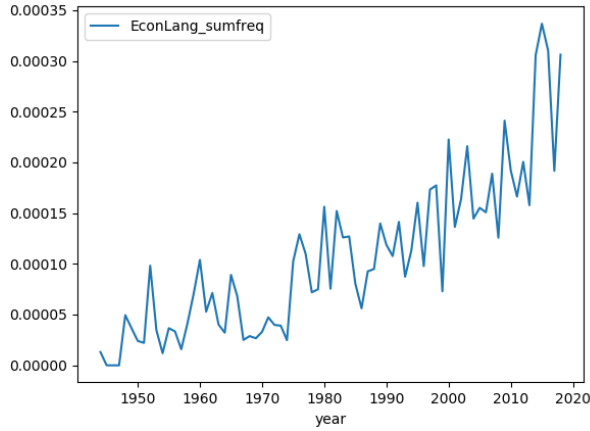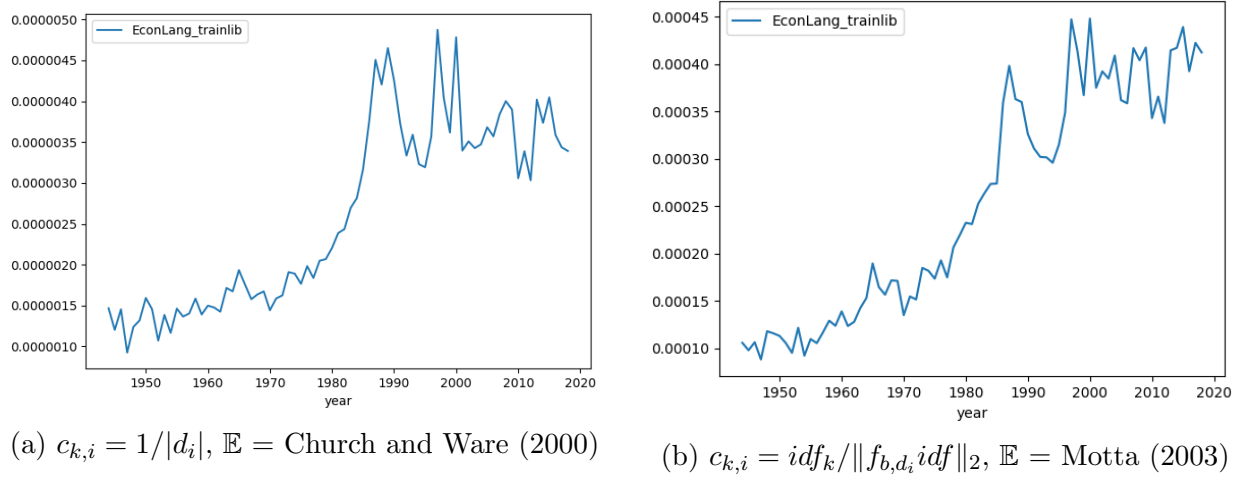

(a) Economic Expertise
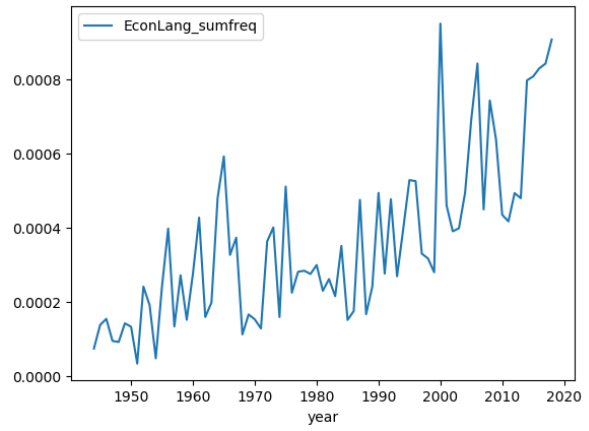
(b) Empirical Tools

(c) New Branch of Economics

(d) Placebo

Notes: Antitrust cases from the economic case sample, all district court opinions from 1943 to 2016. This figure shows the frequency count of specific terms indicative of general expertise and analytical framework in antitrust economics. Panel (a) considers the list of phrases used in Baye and Wright (2011) which signifies presence of economic expertise in courts. The terms are "professor of econmics", "economist", "economic analysis", "econometrics", "industrial organization", "statistics", "regression", and "economics expert". Panel (b) looks at terms indicating empirical analysis, including "statistics", "regression", "data", "econometric", and "empirical". Panel (c) focuses on more recent development in microeconomics that start to make into antitrust analytical framework. Terms are "game theory", "informational asymmetry", and "strategic". Panel (d) plots the frequency of the word "monopolization" as a placebo.

Figure B.3: $EconLang_i$ over Time, Robustness Checks

(a) $c_{k,i} = 1/|d_i|$, $\mathbb{E}$ = Church and Ware (2000)

(b) $c_{k,i} = idf_k/\|f_{b,d_i}idf\|_2$, $\mathbb{E}$ = Motta (2003)

Notes: This figure shows the result of a sensitivity analysis by adopting alternative normalizing factors and economics training libraries in construction of the measure $EconLang_i$ for antitrust law.

Table B.2: Correlation between Judge Political Ideology and Pro-business Voting

	Dependent variable: pro-business ruling		
	(1)	(2)	(3)
Appointed by Republican President	.029	.035	
	(.020)	(.022)	
Republican			.028
			(.047)
Democrat			.007
			(.048)
Type of case control	Yes	Yes	Yes
Cohort and career exp control	No	Yes	No
Observations	3,341	3,341	3,049
R^2	.217	.227	.229
Adjusted R^2	.066	.069	.063

Notes: Regulatory economic case sample, consisting of cases involving government agencies during the period of 1970-2016. OLS estimation of equation (7.2). Pro-business ruling is a binary variable and equals to one if the decision is anti-regulation. Hand coding scheme is detailed in Section 3.2. All regressions include fully interacted circuit and year dummies. Standard errors are clustered at judge level. *p<0.1; **p<0.05; ***p<0.01

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Figure B.4: Mean of θ_j (Standardized) across Circuits
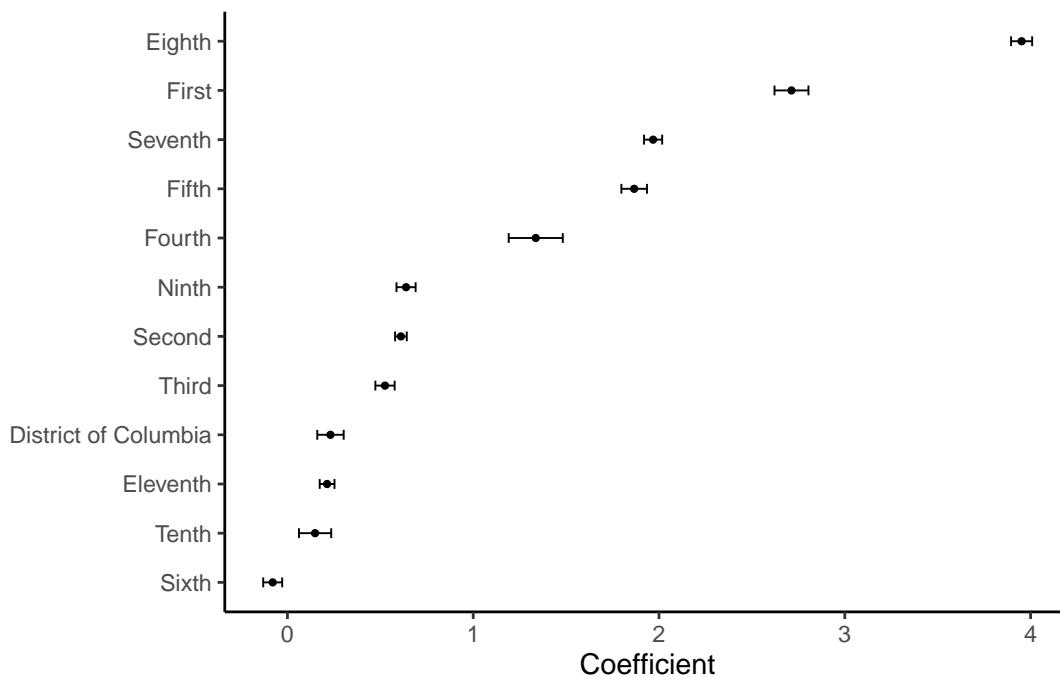
Notes: This figure plots the coefficients and 95% confidence intervals from a projection of judge fixed-effects estimates $\hat{\theta}_j$ (standardized) on a complete set of circuit dummies without a constant. White robust standard errors are adopted to compute the confidence intervals. θ_j is estimated for 946 judges who wrote at least 10 opinions during 1932-2016.

Table B.3: Correlation between Manne Economics Training Program Attendance and Pro-business Voting

	All sample				Eligible subsample
	Dependent variable: pro-business ruling
	(1)	(2)	(3)	(4)	(5)
Ever Attended Manne Program	.011	.008	.009	.012	.009
	(.023)	(.024)	(.024)	(.024)	(.024)
Appointed by Republican President		.028	.029	.034	
		(.021)	(.021)	(.022)	
Type of case control	Yes	Yes	Yes	Yes	Yes
Cohort control	No	No	Yes	Yes	No
Career exp control	No	No	No	Yes	No
Observations	3,364	3,341	3,341	3,341	2,754
R^2	.216	.217	.221	.227	.233
Adjusted R^2	.066	.066	.068	.069	.054

Notes: Regulatory economic case sample, consisting of cases involving government agencies during the period of 1970-2016. OLS estimation of equation (7.2). Pro-business ruling is a binary variable and equals to one if the decision is anti-regulation. Hand coding scheme is detailed in Section 3.2. Eligible subsample considers only judges who are active during the period of Manne program offering, i.e. 1976-1998. All regressions include fully interacted circuit and year dummies. Standard errors are clustered at judge level. *p<0.1; **p<0.05; ***p<0.01

Table B.4: The Effect of Manne Economics Training Program on Pro-Business Voting (Difference-in-Difference)

| | Dependent variable: pro-business ruling | | | |
| | All sample | | Ever attenders | eligible sub-sample |
	(1)	(2)	(3)	(4)
D	.024	.036	.030	.017
	(.035)	(.155)	(.106)	(.163)
Circuit FE	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
Circuit-Year FE	Yes	Yes	Yes	Yes
Judge FE	No	Yes	No	Yes
Observations	2,605	2,605	569	2,000
R^2	.190	.547	.466	.564
Adjusted R^2	.037	.068	.072	.051

Notes: Regulatory economic case sample, consisting of cases involving government agencies during the period of 1970-2016. OLS estimation of equation (7.3). pro-business ruling is a binary variable and equals to one if the decision is anti-regulation. Hand coding scheme is detailed in Section 3.2. Eligible sub-sample considers only judges who are active during the period of Manne program offering, i.e. 1976-1998. Standard errors are clustered at judge level. *p<0.1; **p<0.05; ***p<0.01