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GENERALIST JUDGES?
THE IMPACT OF ECONOMIC COMPLEXITY &
JUDICIAL TRAINING ON APPEALS**

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Is Antitrust Too Complicated for Generalist Judges? The Impact of Economic Complexity & Judicial Training on Appeals

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Abstract

Modern antitrust litigation sometimes involves complex expert economic and econometric analysis. While this boom in the demand for economic analysis and expert testimony has clearly improved the welfare of economists—and schools offering basic economic training to judges—little is known about the empirical effects of economic complexity or judges' economic training on decision-making in antitrust litigation. We use a unique data set on antitrust litigation in district courts during 1996–2006 to examine whether economic complexity impacts decisions in antitrust cases, and thereby provide a novel test of the frequently asserted hypothesis that antitrust analysis has become too complex for generalist judges. We also examine the impact of one institutional response to economic complexity: basic economic training by judges. We find that decisions involving the evaluation of complex economic evidence are significantly more likely to be appealed, and decisions of judges trained in basic economics are significantly less likely to be appealed than are decisions by their untrained counterparts. Our results are robust to a variety of controls, including the type of case, circuit, level of judicial experience with antitrust claims, and the political party of the judge. Our tentative conclusion, based on a revealed preference argument that views a party's appeal decision as an indication that the district court got the economics wrong, is that there is support for the hypothesis that some antitrust cases are too complicated for generalist judges.

Keywords: antitrust, *Daubert*, complexity, economic training, expert witness

JEL Classifications: A2, All, K21, K41, L4

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1 Introduction

Antitrust analysis is becoming increasingly complicated. Modern antitrust litigation and agency practice typically involves judicial evaluation of complex economic and econometric analysis. The “battle of the experts” has become a standard, and critical, battle in the antitrust litigation wars. Mandel (1999) describes the expert witness “boom” in antitrust and a handful of other areas over the past several decades and the growing reliance by judges and regulators on economic consultants to inform decisions. While this boom in demand for expert economic analysis and testimony has clearly improved the welfare of economists, there has been little empirical examination of the effects of economic complexity in antitrust litigation.

There are a number of plausible explanations for the increased reliance on expert economic analysis in antitrust litigation. One explanation is that advances in industrial organization (and economics more generally) have rendered antitrust a more technically demanding field. A second, not mutually exclusive, explanation is changes in substantive antitrust doctrine. Fifty years ago, antitrust law consisted primarily of *per se* rules and bright line prohibitions, and thus economic analysis was not required to determine whether business conduct violated the antitrust laws. The success of the law and economics movement over the past 50 years, however, has resulted in a shift towards a modern antitrust landscape favoring a case-by-case, rule of reason approach to evaluating business conduct. Under this modern, “effects-based” approach, judges and juries are

frequently called upon to determine which business arrangements are anticompetitive, and which are not.

The largely effects-based structure of modern antitrust law invites economic expert testimony in large part because the Sherman Act's broad language delegates to the judicial branch the task of developing a body of "federal common law," including the design of legal standards to identify unreasonable restraints of trade. This task can be daunting for a generalist judge grappling with questions involving merger simulations, demand elasticity, critical loss analysis, the competitive effects of vertical restraints, and evaluating conflicting econometric analyses. Discussing statistical evidence, Judge Richard Posner (1999) argues that "econometrics is such a difficult subject that it is unrealistic to expect the average judge or juror to be able to understand all the criticisms of an econometric study, no matter how skillful the econometrician is in explaining the study to a lay audience." This statement paints a bleak picture for those with hopes of the antitrust enterprise continuing to incorporate modern economic techniques and methods.

The economic complexity of modern antitrust is partly attributable to the success of the law and economics movement. From an historical perspective, economically incoherent decisions are now relatively rare compared to the state of affairs that lead to Bork's (1978) seminal and devastating critique of the paradoxical nature of the antitrust enterprise. The last half century has seen a dramatic increase in the economic sophistication of antitrust analysis in litigation as well as agency practice. Merger

enforcement decisions are no longer based on the elimination of “small dealers and worthy men,” populist considerations, or slavish reliance on industry concentration as a predictor of market performance. Instead, modern merger analysis involves sophisticated predictions of the merger’s probable impact on consumer welfare grounded firmly in economic theory and econometrics. Leading antitrust commentators have praised these developments. Describing the FTC’s successful challenge of the proposed Staples/ Office Depot merger, which relied on complex econometric testimony showing the merger would result in higher prices to consumers, Judge Posner (2001) announced that “economic analysis of mergers had come of age.”

There is now little doubt that complex economic and econometric analyses are standard fare in modern antitrust litigation, but there is a dearth of empirical evidence addressing what impact, if any, this complexity has had on judicial decision-making. An ABA Antitrust Section Economic Evidence Task Force consisting of leading economists, lawyers, academics, and a federal judge undertook a study of the role of economic evidence in federal court. The Task Force Report (Baker and Morse, 2006) reached general consensus “regarding the importance of economics in modern antitrust law and the recognition, therefore, that it is critical that judges and juries understand economic issues and economic testimony in order to reach sound decisions” and that “these problems can seriously affect the adversarial process by skewing judicial outcomes, by leading decision makers to ignore conflicting economic testimony or come to ‘wrong’ conclusions, and can

increase litigation costs.” An ABA Task Force survey of 42 antitrust economists revealed that only 24 percent believe that judges “usually” understand the economic issues in a case. The ABA Task Force Report and other commentators have suggested a number of possible solutions to the “problem” of economic complexity and expert evidence ranging from increasing use of court appointed experts pursuant to Federal Rule of Civil Procedure 706 (a), expanded use of *Daubert* to deter unsupported economic testimony, introduction of concurrent evidence procedures, creating specialized courts, and supplying basic economic training to judges (Posner, 1999).

The training of judges, particularly in economics, has become a controversial topic. The appeal of providing judges with basic microeconomic training in antitrust and other areas where the law has adopted an economic approach is obvious. It is difficult to imagine how a judge untrained in economics might evaluate the competitive effects of a defendant’s complex pricing scheme solely by relying on precedent, statutory interpretation, casual empiricism, and untrained intuition. Posner (2008) notes the promise of improved judicial performance in antitrust, an area where legalist techniques are particularly unlikely to resolve open questions, in a hypothetical legal system where judges would be “armed with basic economic skills and insights.” The ABA Task Force recommends “greater education for judges about antitrust economics, given the limited antitrust and economics expertise that most judges bring with them to the bench when appointed.”

Nonetheless, some have criticized educational programs designed to teach judges economics and other subjects. The George Mason University Law and Economics Center (LEC), the most successful of these programs, has been the focus of much of the criticism at least in some part because it has been the most successful of the judicial training organizations. The LEC began training judges in 1976 and has trained hundreds of federal judges currently on the bench. Teles (2008) notes that by its height in 1990, the LEC Economic Institute for federal judges had trained 40 percent of the federal judiciary, including two Supreme Court Justices and 67 members of the federal courts of appeals.¹ The LEC programs have not been without controversy. Critics have claimed that the programs amount to nothing more than junkets designed to influence judicial decision-making, and are a thinly disguised attempt at indoctrinating judges with a particularly conservative, free-market oriented style of economics. Opposition to these programs recently led to proposed legislation that would effectively prohibit privately funded programs for federal judges (Teles, 2008).

This paper represents a first attempt to examine the effects of economic complexity and basic economic training on judicial decisions in antitrust. We find that that economic complexity significantly increases the likelihood that a court's decision is appealed. This effect is statistically and practically significant, with the appeal rate of economically

¹ The George Mason Law and Economics Center claims that more than 50 percent of the current federal Article III bench has attended LEC programs, though this figure does not distinguish economics programs from the numerous other programs offered by the LEC. George Mason Law and Economics Center Homepage (<http://www.lawecon.org/>) (last visited January 14, 2009).

complex decisions 11-17 percent greater than for “simple” cases.² Additionally, we find that the decisions of judges with basic LEC training are appealed in simple cases at significantly lower rates than their untrained counterparts. We find no evidence that basic LEC training has an impact on appeals in economically complex cases, which is consistent with basic economic training being useful for simple antitrust cases but not ones involving complex economic issues. Our results are robust to specifications controlling for the district court judge’s political ideology, a time trend, type of plaintiff, the type of case, the circuit in which the case is litigated and other controls.

We believe our results shed light on the relationship between economic complexity and the quality of judicial fact-finding, and in particular on the claim that is often made that antitrust analysis has become too complex for generalist judges to evaluate accurately. We argue that the parties—who have typically invested in expert economists and thus are in a strong position to understand the relative strengths and weaknesses of complex economic arguments—are in relatively good position to determine whether the district court got the economics right or wrong in a case. Thus, by revealed preference, the fact that a party is willing to bear the cost of appealing a judge’s opinion is strong evidence that (at least one party thinks) there was an error in the judge’s decision. Thus, we interpret our findings that economic complexity increases the likelihood of an appeal, and that the decisions of judges with basic economic training are appealed at a significantly

² In this context, “simple” describes only the absence of *economic* complexity. Like most other forms of civil commercial litigation, antitrust litigation can be quite complex as the result of considerations unrelated to technical economic sophistication.

lower rate than their untrained counterparts, as evidence that supports the view that some antitrust cases are too complex for generalist judges.

Indeed, modern critiques of important antitrust decisions usually amount to a claim that the judge misunderstood the relevant economics or relied on the wrong expert. But while the assumption that the federal judiciary is not equipped to competently evaluate complex economic and econometric evidence in antitrust cases is often made, and also drives many of the proposed reforms designed to improve judicial accuracy, there has been little empirical evaluation of whether such an assumption is appropriate.

One consequence of the increasing economic complexity of modern antitrust is the emergence of institutional responses designed to mitigate its potentially deleterious effects. As discussed, judicial training in basic economics, using specialized courts, or appointing neutral experts have all been proposed as potential strategies to minimize the impact of excessive technical demands on generalist judges. This paper focuses on the first of these solutions: basic economic training for judges. Specifically, we examine whether appeal rates for federal judges receiving economics training from the LEC are different than their “untrained” colleagues on the federal bench.³

Section 2 describes our data. Section 3 discusses methodological issues regarding our approach, as well as some important caveats and limitations of our analysis. Section 4

³ There is related literature on the impact of technical complexity on claim construction decisions in patent law finding that the Federal Circuit reverses district court decisions at a relatively high rate and implies poor performance by the district courts. See, e.g. Moore (2001), Chu (2001), see also Wagner (2004). This literature generally does not control for individual judicial characteristics such as technical scientific background, though Moore (2001) finds no difference in reversal rates between Federal Circuit judges with technical backgrounds and those without.

presents our empirical results, while Section 5 concludes with a discussion of some potential policy implications of our findings.

2 Data

There are four main categories of data. The first category involves information extracted from judicial opinions. We have attempted to collect every reported decision in which an administrative law judge or federal district court judge published a ruling on the merits of a substantive antitrust claim.⁴ Our sample includes a total of 73 decisions on substantive antitrust issued by administrative law judges and 641 by Article III federal district court judges from 1996-2006 for a total of 714 decisions. A number of decisions involving antitrust issues were excluded from the sample because they did not involve a decision on the merits of a substantive antitrust claim.⁵

Each decision was coded to include information describing the type or types of antitrust claims litigated (e.g. merger, monopolization, price-fixing, Robinson-Patman, or multiple claims), the procedural stage of the decision (e.g., motion to dismiss, summary judgment, and post-trial motion), plaintiff (FTC, DOJ, private party, state attorney general), and the date of decision. Our data also includes an indicator for whether one of the parties appealed the district court's decision.

⁴ We used Westlaw to collect these decisions with the following search term in the district court database (DCT): (antitrust & ("Sherman Act" "Clayton Act" "Robinson-Patman Act")).

⁵ These decisions were most commonly related to venue and class certification issues. It should also be noted that some cases involved multiple decisions. For example, *Smith v. Jones* might involve separate opinions on the same underlying issues at different procedural stages, or alternatively, separate opinions addressing different antitrust claims.

The second category of data includes observable characteristics of the judge. In order to be in a position to attempt to disentangle politics from economic training and other factors that might influence appeals, where available we collected data on the political ideology of the decision-maker as measured by the party of the appointing President.⁶ We also collected data involving court characteristics, including the federal court of appeals to which each district court judge belonged (thus allowing us to control for potential variation among circuits). This is potentially valuable if one believed, for example, that district court judges within the D.C. Circuit are more competent in handling complex antitrust cases litigated by the nearby enforcement agencies. Additionally, data on circuits permits us to control for potential inter-circuit variation including the political composition and economic sophistication of the appellate court and differences in the substantive antitrust law which might influence the appeal rate.

We also include data measuring judge's antitrust experience, measured by the number of antitrust opinions authored prior to the decision in our database. One might hypothesize that judicial experience in complex antitrust cases would be an important predictor of the quality of decision-making in such cases, and perhaps a substitute for economic training. Indeed, the argument that experience in the form of repetition results in specialization and higher quality decisions in complex litigation is the motivation for proposals for specialized antitrust courts. Figure 1 displays the distribution of our

⁶ Party of the appointment was recorded for each district court judge. We also coded age, race, gender and number of years on the bench. Administrative law judges are not appointed by the President and we thus do not record ideology measures for them.

measure of experience. Figure A1 presents the distribution with only Article III federal district court judges included and excluding administrative law judges.

The third category of data involves measures of economic complexity. We constructed a variable designed to capture whether the disposition of the underlying substantive antitrust issue involved analysis of sophisticated economic or econometric evidence. Specifically, we examined the decisions in each case and recorded the number of times key terms were referenced that one might expect to see in a complex economic case. These terms are summarized in Table 1.

We then constructed an aggregate summary statistic of the overall economic complexity of each decision by computing the total number of times these fourteen terms appeared in a given decision. Figure 2 displays the distribution of this measure of economic complexity. Figure A2 presents the same information for the Article III subsample. In light of the fact that the majority of the decisions were “simple” cases in that none of these fourteen terms were referenced in the decisions, we created an indicator variable that divided cases into two types: hard and simple. Simple cases generated opinions that did not use these terms at all, while a hard case was defined as one in which one or more of the terms in Table 1 were referenced. Our sample includes 222 hard cases and 492 simple cases.

The fourth category of data involves economic training for judges. Using publicly available sources, we recorded the identity of each federal judge attending basic economic

training sessions at the LEC and the date they attended.⁷ A total of **128 judges** in our sample attended LEC economics training seminars during the relevant time period, with some attending multiple programs. The purpose of this variable is to measure a judge's ability to analyze economic evidence in an antitrust case. A judge was considered "trained" for the purpose of our analysis only if the judge attended an LEC training session *before* the date the decision was issued.⁸

We believe the LEC data are a potentially useful measure of economic expertise for several reasons. First, because judges who attend basic economic training sessions are the least likely to have any economic sophistication or skills to begin with,⁹ it is likely that any impact of training on appeals can be attributed to a judge acquiring basic economic skills. Second, LEC training is just one form of judicial economic education. Our results may shed some light on many of the proposed institutional reforms, such as more liberal use of court appointed experts, designed to "train" judges with respect to some relevant technical

⁷ We used a number of data sources to compile this information. The primary source is the searchable database at <http://www.tripsforjudges.org/search.asp>. The database is the project of the Community Rights Counsel, a small environmental group that has been a vocal critic of the LEC and other judicial education programs, such as the Foundation for Research on Economics and the Environment (FREE) and the Liberty Fund (Adler, 2005). The database compiles judges' financial disclosure forms from 1992-2004. We've supplemented this information with more recently published financial disclosures and records available at the George Mason University Law and Economics Center.

⁸ LEC training is a proxy for a judge's ability to comprehend basic economic evidence. To the extent one is interested in measuring the effect of more generally acquired economic skills, a more accurate measure of technical competency would be the judge's educational background. For example, one could record whether the judge has an undergraduate or graduate degree in economics, math, or science as a proxy for possessing the technical skills relevant to analyzing complex economic or econometric evidence. This data is currently unavailable.

⁹ Programs consisted of a two and a half week course in basic microeconomics taught by instructors including Armen Alchian, Harold Demsetz, Martin Feldstein, Milton Friedman, Paul McAvoy, and Paul Samuelson (Teles, 2008). Charles Goetz, an instructor in LEC training programs, describes the content as "pretty much straight economics . . . the competitive model, capital values, discounting to present value, that sort of thing." (Teles, 2008).

skill. Finally, the merits of the judicial economic training programs (and the LEC in particular) have been the subject of intense debate.

Table 2 presents summary statistics sorted by circuit, type of case, and type of plaintiff. The summary statistics reveal a number of interesting trends. In terms of inter-circuit variation in appeal rates, the Seventh Circuit, home of antitrust experts Judge Richard Posner and Frank Easterbrook, claims the lowest appeal rate at 17.02% percent approximately half of the sample average. The FTC “Circuit” is home to the highest appeal rate at approximately 90 percent. With respect to LEC training, there is significant variation between appellate circuits. Zero cases in the First and Federal Circuits were decided by judges with LEC training prior to the decision while the over 30 percent of the Fourth Circuit’s substantive antitrust decisions were authored by trained judges.

Merger cases are the most complex in the sample, and therefore have a significantly higher appeal rate than other types of cases. Interestingly, these more complex merger cases are decided by judges with LEC training only 2.56 percent of the time, far less frequently than any other type of case. In addition to a particularly high rate of appeal when the FTC is a plaintiff, which is driven primarily by the fact that most administrative law decisions are appealed to the Commission, it is also interesting to note that an LEC trained judge has never authored an antitrust decision in a case where the FTC is plaintiff. By way of contrast, cases in which the DOJ is plaintiff are appealed 41.67 percent of the time and while 26.44 of decisions where private parties are plaintiffs are appealed.

3 Methodology and Caveats

The primary challenge related to this project is identifying a reliable measure of decision quality. Many studies attempting to measure the quality of lower court performance have relied on reversal rates rather than the decision to appeal, presuming that a reversal by the “higher” court warranted the inference that the initial decision was wrong. For a number of reasons, we believe that appeals are a superior measure of the “accuracy” or quality of lower court decision-making in this context. Thus, we estimate the probability of appealing a specific district court decision as a function of economic complexity, the judge’s economic training, and a variety of controls.

Our first reason for using appeals to measure the quality of lower court performance derives from a revealed preference argument. That is, the appeal rate is a signal generated by the actual costs incurred by the parties rather than the ex post decision of the appellate court. If the appellate court makes decisions with error, the ex ante decision to appeal can be a more accurate measure of reversibility related to judicial error than the ex post appellate decision to reverse. In many antitrust cases, the parties, informed by their economic experts, may well have better information about whether the district court committed (or is sufficiently likely to have committed) “reversible error” than the appellate court will have. While there are reasons for a party to appeal any given district court decision that are unrelated to its quality,¹⁰ it should generally be the case,

¹⁰ While a high appeal or reversal rate could indicate a failure of generalist judges to appreciate the technical economic complexity underlying antitrust analysis it might also, for example, merely reflect a period of

ceteris paribus, that observing an appeal signals that at least one party believes that it can convince a higher court that the initial decision contains reversible error. A reduction in the appeal rate is likely to mean that judges issued fewer opinions that left at least one party feeling strongly enough to invest in the opportunity to persuade an appellate court that the district court committed reversible error. It is true that an appeal can also indicate that at least one party wishes to invest in the opportunity to persuade an appellate court that the district committed a *legal* error, such as applying the wrong standard, unrelated to the type of antitrust fact-finding involving economic analysis that is the subject of our study. However, modern antitrust law is unique in the overlap between legal and economic inquiries, suggesting a large fraction of appeals will be motivated by the view that the district court “got the economics wrong.”¹¹

Moreover, parties typically invest in hiring economic experts, and are likely to be well-informed about the strengths and weaknesses of complex economics. In contrast, for all of the cases in our sample, judges did not utilize a court appointed expert, and thus were on their own to evaluate the evidence produced through any “battle of the economic experts.” In short, there are reasons to believe that the parties (through their experts) are in

uncertainty in the law that may decrease over time or a disagreement between district court and appellate judges on the substantive merits (Wagner, 2004).

¹¹ For example, it is difficult to conceive of an exclusively “legal” challenge to a district court decision in a merger case where the legal standard is whether the merger is likely to “substantially lessen competition,” a test that has taken on almost exclusively economic meaning. More generally, Posner (2001) has explained that the subtitle “An Economic Perspective,” was dropped from his influential antitrust treatise because “the other perspectives have largely fallen away,” and that there is now “a consensus that guidance must be sought in economics.”

a better position to evaluate whether a judge missed an important economic point than an appellate judge.

While the appeal rate is an imperfect measure of whether the initial court got the economics “wrong,” given our goal of examining the impact of complexity and training on the quality of district court decisions, we believe it is better than merely presuming that appellate courts do not err. There is no evidence that appellate panels are more likely than federal district court judges to possess the technical skills to evaluate complex economic and econometric evidence. Thus, using reversal rates to justify negative inferences about the quality of the district court decision is problematic in this context. Additionally, because the characteristics of the district court judge might influence both the decision to appeal and the conditional probability of a reversal given the decision to appeal, estimating reversal rates runs into an endogeneity problem.

The second reason to prefer appeals over reversals is methodological in terms of better statistics. While appeal rates and reversal rates are inextricably intertwined signals, one reason to prefer the appeal rate is simply that it provides a stronger measure of the same underlying concept – possibly reversible error. Assume for example, that an appeal is made only when reversal is a significant possibility, say a 1/3 or greater chance in the appellant’s opinion. Assume, too, that appellants are perfectly accurate in their judgments, and that half of all appeals are successful. Under these assumptions, both appeal rate and reversal rate would provide measures of quality, but we would lose half of

the signal by using reversal rates. Hence, we would have lower statistical reliability. Our findings are consistent with this analysis in that our reversal rate regressions produce similar, but less reliable, results.¹²

Finally, reversals of district court decisions involve the decisions of multiple decision-makers, each with potentially different political ideologies and economic training. Personal interactions among these decision-makers make it difficult to control for the effects of the characteristics of individual appellate judges (such as party or LEC training) on the appellate court's reversal decision. To the extent that an LEC trained appellate judge might use his or her expertise to influence the decisions of his or her untrained colleagues, it is far more difficult to control for judge characteristics using the reversal approach. For these reasons, and other things equal, we view a party's appeal of a district court decision as an indicator of potential judicial error by the district court.

Our analysis is, of course, not without limitations. As discussed earlier, the majority of cases in our sample are economically "simple," and there is not sufficient thickness in the data to separately control for each of the terms in Table 1. Thus, we have classified a decision as "hard" if it includes one or more of the terms in Table 1 and "simple" if it does not.¹³ Importantly, however, it is possible that decisions including these

¹² For instance, opinions authored by LEC trained judges are reversed only 13.6% of the time, while their untrained counterparts' decisions are reversed 23.9% of the time. Similarly, complex cases in our sample are reversed 28.2% of the time while simple cases are reversed only 18.6% of the time. While these results are similar in direction and magnitude to our appeal results reported in Table 2.1, the differences are not statistically significant.

¹³ We also ran other specifications that treated complexity as a continuous variable. Our finding that complexity increases the probability of appeals and that LEC training reduces appeals is robust to these

terms could involve very little sophisticated economic or econometric analysis.

Economically simple cases can be legally challenging, and vice versa. It is also possible that decisions are economically complex despite the absence of any of these terms. An informal review of our sample suggests that the “hard” cases consistently involve at least some evaluation of expert economic evidence, “simple” cases do not, and false negatives (e.g. cases coded as “simple” but involving evaluation of expert economic evidence) are more common than false positives (“hard” cases that do not involve any evaluation of economic or econometric evidence). Nonetheless, we acknowledge that our measure of economic complexity is a proxy for an admittedly nebulous concept.

Another limitation of our analysis is that we do not have data to control for some other potentially important predictors of the appeal rate. The most important of these omitted variables is the stakes of the underlying litigation which is likely to be a significant predictor of the appeal rate. Two of our control variables can be interpreted as controlling for litigation stakes. First, our control for type of case distinguishes merger cases from price-fixing or monopolization allegations and is a reasonable proxy for stakes in the antitrust litigation context.¹⁴ Second, one might interpret our complexity measure as capturing the presence of an expert report and thus correlated with litigation stakes. To the extent that our measure of complexity and litigation stakes are highly correlated, one

alternative specifications as well as specifications using decision length (number of words) as an alternative measure of complexity.

¹⁴ Bizjak and Coles (1995) find litigation involving horizontal conspiracy allegations to be associated with larger negative wealth effects than vertical allegations involving monopolization, and that Clayton Act merger litigation has larger effects than other forms of litigation in stock market event studies.

might have less confidence that our complexity results are solely attributable to the technical economic complexity of the decision rather than both complexity and stakes effects. However, our basic economic training results are robust to specifications including both our measure of complexity and controls for the type of case, which is also correlated with litigation stakes.

We do not have access to data on other important predictors of the appeal rate. For example, we do not have data on the quality of the lawyers on either or both sides. Judges might also rely on unobserved methods, unrelated to economic training or education, to signal their grasp of the economic issues to the parties. This would reduce the likelihood of appeal for any given level of economic training or complexity. In addition, district court judges' abilities to evaluate complex economic evidence might improve with repeated exposure to antitrust cases over time, and there could be judge-specific effects. Unfortunately, the data are not rich enough to permit us to control for all of these possibilities.

Finally, our sample of cases is not random. Since our sample consists of only litigated cases, it is a well known result that these cases are likely to include cases that are "close calls" (Priest and Klein, 1984). For our purposes, this sample selection does not impact our ultimate research question: how well do generalist judges evaluate economic evidence in modern antitrust litigation? In other words, we are interested in how well judges evaluate these close calls, not the substantive merits of the underlying allegations.

To the extent that the mix of litigated cases changes over time in important ways that might correlate with decision quality or complexity, we include controls such as a time trend and type of case.

4 Economic Complexity, Judicial Training and Appeal Rates

4.1 Complexity, Economics Training and Appeal Rates

We begin with some simple comparisons of means to explore differences in the appeal rates in hard and simple decisions, as well decisions by LEC-trained and untrained judges. Table 2.1 reports results. Economically hard cases in our sample are 24.2 percent more likely to be appealed than simple cases. The difference is statistically significant at the 1 percent level, and in practical terms, quite large. In just over 50 percent of cases involving evaluation of complex economic or econometric evidence, the decision is appealed. In contrast, only 26.2 percent of the decisions in economically simple cases are appealed. With respect to basic economic training, decisions authored by judges who have attended an LEC training session are appealed at a rate 12.8 percent lower than decisions authored by their colleagues lacking economic training. This difference is also both statistically (at the 1 percent level) and practically significant. District court judges who have attended LEC basic economic training programs prior to authoring an antitrust decision have that opinion appealed only 22.7 percent of the time.

These means tests provide preliminary evidence that both complexity and judicial economic training are important predictors of appeal rates in antitrust cases. However, it

is possible that the correlations between complexity, LEC training and appeals may be the result of omitted variable bias confounding their true impact. In the remainder of the paper, we use a probit regression framework to control for possible influences and isolate the impact of economic complexity and judicial training on antitrust appeals.

4.2 Baseline Probit Regressions

In each of our regressions the dependent variable is *appeal*, an indicator set equal to 1 if the district court decision is appealed and zero otherwise. Our primary independent variable of interest is *hard*, a dummy variable taking the value of one when the district court opinion included at least one of the terms in Table 1 (indicating the presence of complex economic or econometric evidence) and zero otherwise. A second independent variable of interest is *LEC*, a dummy variable that equals one if the district court judge issuing an antitrust opinion attended an LEC training course in basic economics prior to the decision, and zero otherwise. To further explore the impact of judicial training on appeals, we generated two interaction terms: *hard_LEC* and *simple_LEC*. The interaction terms allow us to isolate the marginal impact of LEC training on appeals in “hard” cases involving economic and econometric evidence as well as “simple” cases where basic economic training is more likely to have a greater marginal effect.

To explore the effect of these variables on the appeal rate, we estimated a series of probit regressions that include the above key variables along with a set of controls that are potentially predictive of the appeal rate. These controls include a time trend, dummy

variables indicating the type of claim, the type of plaintiff, and the circuit in which the decision was litigated.¹⁵ Table 3 reports marginal effects along with robust z-statistics.

Specification (1) is our baseline model, which is similar to the means comparisons in Table 3 except that it simultaneously controls for both economic complexity and LEC training. The results are similar in magnitude and significance to the mean tests reported in Table 2.1, with complex cases being appealed 23.6 percent more often than simple cases and LEC training reducing the probability of appeal by approximately 10.7 percent. This is consistent with our expectation that economically complex cases are more likely to result in larger zones of reasonable factual disagreement on substantive issues and divergent expectations with respect to the likelihood of success on appeal. A more cynical interpretation of this finding would be that more complex cases raise more difficult fact-finding determinations and therefore, more opportunities to commit reversible error in the eyes of the reviewing court.

Specification (2) uses interaction terms to determine whether LEC training has a differential impact on appeals rates in hard and simple cases, and adds a control for the political affiliation of the judge.¹⁶ As before, decisions involving complex economics or econometrics are more likely to be appealed than simple cases: Hard cases are 22.7 percent more likely to be appealed than simple cases, and the effect is statistically significant at the

¹⁵ We also controlled for the procedural stage of the decision (e.g. motion to dismiss, motion for summary judgment, post-trial motion). Results in these specifications are similar to those reported here and are available from the authors upon request.

¹⁶ Specifications including LEC, HARD and one interaction term (HARD_LEC) generated similar results.

1 percent level. Interestingly, basic LEC training does not have a statistically significant effect on complex cases (the coefficient of HARD_LEC is statistically insignificant at conventional significance levels), but reduces the appeals rate in simple cases by a statistically significant 12.5 percent (the coefficient of SIMPLE_LEC). This result is consistent with what one might expect: Basic economic training is not enough to help judges get the economics right in complex cases, but has a high marginal return in simple cases.

Specifications (3), (4), (5) and (6) in Table 3 reveal that these results are robust to, respectively, the addition of a simple time trend, dummy variables to control for the type of case (merger, monopolization, price discrimination, or price-fixing), the type of plaintiff, and dummy variables to control for the circuit in which the case was litigated. In all specifications with these controls, hard cases are 11-17 percent more likely to be appealed than simple cases, and arming judges with basic economic skills reduces the appeal rate in simple cases by about 10-11 percent. Except for the circuit coded for administrative litigation, the type of claim, the circuit court of appeals within which the case is filed, and whether the plaintiff is a government agency are not significant predictors of appeals.¹⁷

¹⁷ We discuss the impact of administrative litigation in Section 4.4.

4.3 Economic Training Versus Experience

The story that emerges from our baseline specification is that economic complexity increases the appeal rate while basic economic training reduces it in simple cases but has little or no effect in the more complex cases. This simple story that basic economic training arms generalist judges with enough economic knowledge to resolve simple antitrust cases accurately provides some support for antitrust litigation reform efforts designed to arm judges with greater economic expertise with training and court appointed experts. However, a frequently discussed alternative to increasing judicial economic competency is the creation of specialized antitrust tribunals which would benefit from allowing judges repeated exposure to complex antitrust issues. In Table 4, we add our judicial experience measure to our baseline specification in order to explore the effects of experience on decision-making quality.

The results in Table 4 suggest that the story emerging from the baseline specifications in Table 3 is robust to the addition of controls for judicial experience. Economic complexity results in an 11-15 percent increase in the appeal rate and LEC training decreases the appeal rate by approximately 11-12 percent in specifications with fixed effects for year, type of case, plaintiff and circuit. Both results are similar in magnitude and significance to the baseline results. Judicial exposure to antitrust issues has the expected sign in all specifications, reducing the appeal rate, but is both small in magnitude and statistically insignificant. Thus, one might interpret the results in Table 4

as suggesting that repeated exposure to antitrust issues is a poor substitute for economic training in terms of influence on the quality of judicial output.

4.4 Robustness Check: Federal District Court Judges Only

One possible explanation of our results in Table 3 and Table 4 is that the inclusion of administrative law judges in the sample is driving our results. While the specifications with dummy variables for the Circuit controls for fixed effects for administrative law judges, it does not control for the fact that the underlying appeals model (and the impact of LEC training, experience, and complexity differs for administrative law judges and district court judges. As shown in Table 2, none of the administrative law judges in our sample received any LEC training in our sample, their decisions involve a higher fraction of complex cases, and the appeal rate of administrative law judges to the Commission in Part III litigation is significantly higher than the overall sample. While this may be driven by a lack of experience or a lack of LEC training, it is also possible that these differences stem from institutional differences in Federal Trade Commission Part III administrative litigation and federal district court. Thus one might expect federal court judges who are more likely to receive LEC training to have lower appeal rates.¹⁸ At a minimum, the parties' decision to appeal from administrative litigation might be driven by different

¹⁸ Consistent with this concern, the only "circuit" which has a significant influence on the appeal rate in Tables 4 and 5 is our dummy variable for decisions originating in administrative litigation. The coefficient (.580) suggests that the appeal rate in administrative litigation in our sample is nearly 60 percent higher than that for decisions originating in federal district court and the difference is significant at the 1 percent level.

considerations than the decision to appeal a federal judge's decision in district court.¹⁹

These factors combine to generate the possibility that our "LEC effect" might be an artifact of the data and institutional differences between federal district court and Part III litigation rather than improving accuracy of the judicial decisionmaking process.

In order to address these concerns, we replicate our analysis with a subsample including only Article III judges. Tables A1 and A2 present our complexity measures and summary statistics for the restricted sample. Figures A1 and A2 display distributions of judicial experience and economic complexity. Specification (1) in Table 5 is the equivalent to specification (6) in Table 3, which includes fixed effects for year, type of case, plaintiff, and circuit. Our basic results in Tables 3 and 4 are robust to this restricted sample in magnitude and statistical significance, suggesting that our results are not an artifact of the inclusion of administrative law judges. Specification (2) in Table 5 tests the possibility that differences in antitrust experience levels between generalist federal judges and administrative specialists explains our results by adding experience as a control. Again, our basic results are robust to this specification.

¹⁹ In a sample of Sherman Act disputes litigated in front of administrative law judges at the Federal Trade Commission from 1983-2008, Melamed (2008) presents evidence that the respondents prevailed in only 4 of 16 cases. All 16 of these cases were appealed to the full Commission, which affirmed all 12 decisions decided against respondents and reversed all 4 decisions decided in favor of respondents. Melamed suggests that the disparate appeal rates and respondent win rates are likely explained, at least partially, by the fact that "Commissioners inherently and unavoidably lack the independence that we expect from adjudicative fact-finders.

4.5 Robustness Check: Judicial Training or Ideology?

Thus far we have not examined the role of political ideology in influencing antitrust decisionmaking. One related concern with the results thus far is that judges receiving LEC training are not randomly assigned. For example, LEC training might be correlated with another characteristic impacting the appeal rate. One such selection hypothesis is that judges attending LEC training programs may be more politically conservative or otherwise more pre-disposed to economics and market oriented thinking than their untrained counterparts. If that were so, LEC attendance might be capturing some pre-existing differences in economic sophistication or orientation of the judges rather than the effect of basic LEC training. Consistent with this view, much of the controversy surrounding the LEC training programs have involved allegations that the programs teach a unique “free market” oriented version of economics that would be more likely to appeal to conservative judges.

As a preliminary matter it is worth noting that the LEC effect is not an artifact of the selection into these programs by predominantly Republican judges. 322 of the opinions in our federal court database are authored by Democrats and 319 by Republicans. Approximately 13 percent of the Democrats and 17 percent of the Republicans in our sample received LEC training. We present a more rigorous test of the influence of ideology on the appeal rate in Table 5. Specification (3) adds political party of the appointing judge as a control and runs the same regressions on the restricted sample of

federal district court judges. Both the economic complexity and LEC effects are robust to the inclusion of this measure of political ideology. The political ideology of the district court judge is not a significant predictor of the appeal rate in any of the specifications, while the complexity and LEC training results are roughly similar in magnitude and significance to those in the baseline specifications reported in Tables 3, 4 and 5.

In addition to this analysis, we also separately examined the possibility that LEC training impacts Republican and Democrat judges differently based on these underlying and unobserved differences by estimating variants of the model that allow differential effects of LEC training on republican and democratic judges. The results are similar to those reported in Table 5. For instance, in a specification analogous to that in specification (1) in Table 3, economic complexity increases the likelihood of appeal by 23 percent (significant at the 1 percent level), and the impact of LEC training for both Republican and Democrat judges is similar in both magnitude and direction: -10.6 percent for Republican judges and -10.1 percent for Democrats.²⁰ Thus, it does not appear that the reduction in appeals associated with LEC training is an artifact of the ideology of those opting to take such training in the first place.²¹

²⁰ Neither interaction term is significant at the 10 percent level, with p-values of .106 and .181, respectively. Similarly, LEC training is not a significant predictor of the defendant win rate in our sample. These results are available from authors upon request.

²¹ Moore (2001) also finds that the political party of the appointing president does not predict reversal rates in district court patent claim construction cases. But see Sag et al (2008) who find that political ideology is a significant predictor of outcomes in Supreme Court intellectual property cases, including the subset of cases involving the intersection of intellectual property and antitrust.

5 Conclusions

Modern antitrust litigation involves more economic sophistication than it did even 20 or 30 years ago. The evolution of antitrust into an economically coherent body of doctrine from its notoriously incoherent and non-economic origins has been nearly universally heralded as a favorable development. As economists, we certainly consider the evolution of antitrust doctrine over the past 30 years to be a positive development for consumers. Our inquiry examined whether the incorporation of economic sophistication into modern antitrust analysis may have reached the point of diminishing marginal returns. While commentators have discussed the challenges facing generalist judges charged with the task of sifting through competing expert economic evidence in complex antitrust cases, and their failures in individual cases, we present the first empirical evidence of the relationship between technical economic complexity and antitrust litigation. We also are able to examine whether basic economic training for judges mitigates the negative impact of this form of complexity on judicial decision-making.

The evidence here suggests that economic complexity and training influence the appeal rate in opposite directions. Economic complexity significantly increases the probability of appeal, while judicial training reduces it. The effects are of similar magnitude and robust to a host of controls and specifications. More specifically, our first finding is that there is a statistically and practically significant positive relationship between economic complexity and appeals. Decisions involving some evaluation of

economic or econometric evidence are appealed approximately 11-17 percent more frequently than cases demanding less economic skill.

That a decision was appealed suggests that at least one party is willing make a costly investment for the opportunity to persuade an appellate court that the district court judge erred. This should be more likely in cases involving complex economic evidence because there are likely to be reasonable fact-finding dispute and thus, more room to persuade an appellate court that a reversible error has been committed by the lower court. This result provides some empirical support for the claim that economic complexity is having an important impact on antitrust litigation. Our finding also provides some support for the oft-raised claim that the ever increasing levels of economic sophistication and complexity in modern antitrust litigation are now generating negative marginal returns. While one may reasonably dispute whether this relationship between complexity and appeals is strong evidence of a divergence between the technical demands of contemporary antitrust analysis and the technical economic skills of the federal bench, it is clear that economic complexity is an important part of the modern antitrust litigation landscape.

Our second finding is that the decisions of judges who attended LEC programs to learn basic economic skills are appealed at the same rate as their untrained counterparts in complex cases, but 10-11 percent less often in cases that do not involve the evaluation of sophisticated economic or econometric evidence. This effect of economic training has

interesting implications for understanding the impact of economic complexity in antitrust litigation. One interpretation of these findings is that while adding basic economic skills does not enhance the accuracy of judicial fact-finding in technically complex cases, these skills do allow trained judges to identify and “correctly” decide simple cases. This interpretation is consistent with the “entry level” content of LEC programs. Exposure to basic economics is not likely to prepare a district court judge to evaluate the type of complex econometric testimony seen in many modern antitrust cases. However, these skills are more likely to enable a district court judge to identify and correctly decide simple antitrust claims not requiring sophisticated economic analysis. Further, we find that repeated exposure to complex antitrust issues is not an adequate substitute for economic training.

Judicial economic training appears to be demonstrating both its promise and limits. On the one hand, the reduced appeal rate in simple cases supports the claim that basic economic training improves judicial decision-making. The primary benefit of basic economic training is that judges are able to reach more accurate decisions in simple cases, at least in the sense that the parties are less willing to appeal. Further, the improvement comes in a form that critics of judicial education programs would not predict. Rather than influencing judicial-decision making in a manner that is consistent with political ideology (for example, LEC training resulting in a higher defendant win rate), LEC training appears to have no impact on which party ultimately prevails. To the contrary, the impact of

training is similar in magnitude and direction regardless of the political party of the deciding judge.

However, the results also demonstrate that economic training alone is not likely to improve outcomes in the increasing number of complex cases. Improving accuracy in complex antitrust cases involving significant amounts of economic and econometric evidence might require more alternative strategies, such as more liberal use of court appointed experts, or perhaps more drastic measures such as creating specialized antitrust courts. However, our results suggest that the type of repeat exposure to antitrust litigation contemplated by proposals for specialized courts is not as likely to improve outcomes as basic economic training.

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FIGURE 1: Distribution of Experience

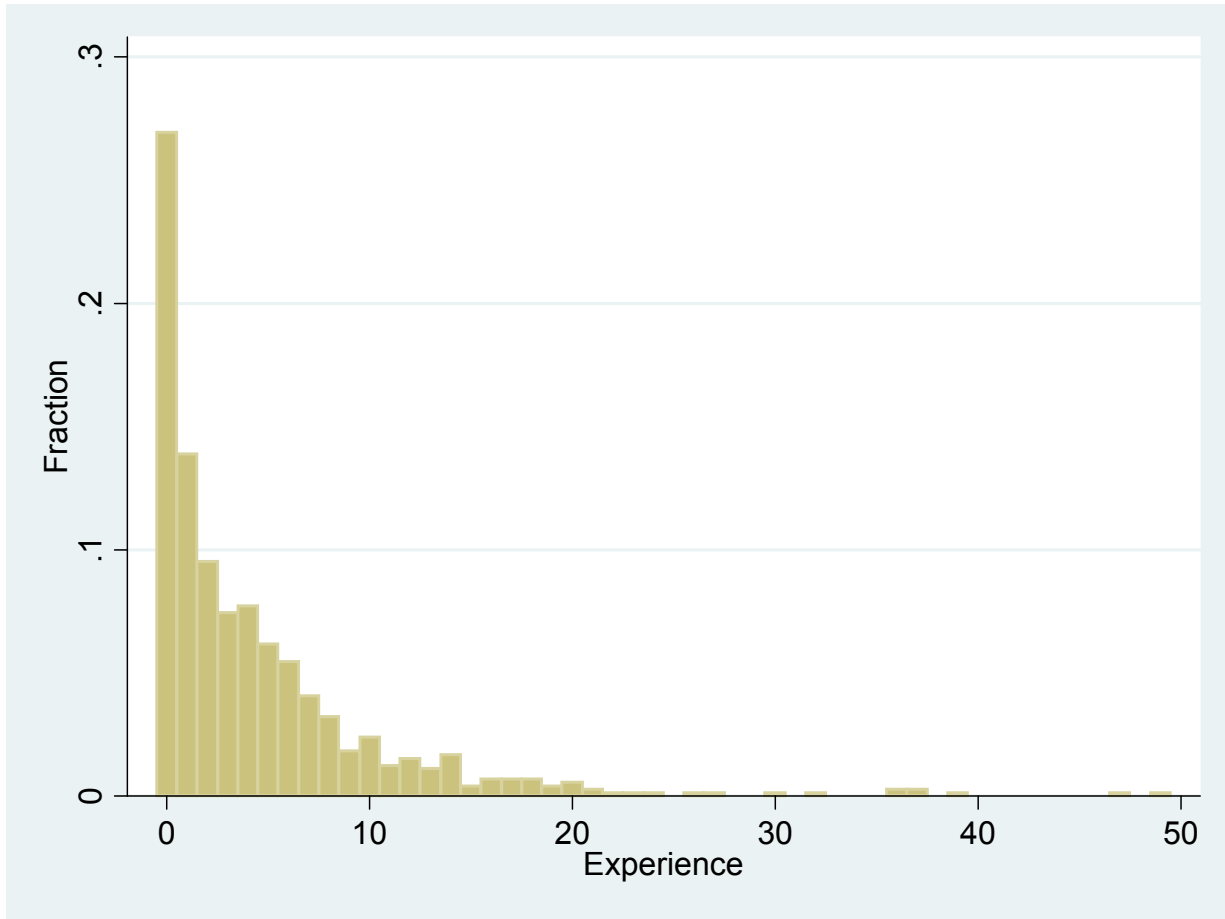


TABLE 1. Summary of Terms Used to Identify Economic Complexity in Sample of 714 Cases

Term	<i>Mean</i>	<i>Std. Dev.</i>	<i>Min</i>	<i>Max</i>
Professor of Economics	0.049	0.346	0	5
Econometrics	0.052	0.652	0	15
Economist	0.387	1.637	0	26
Economic Evidence	0.071	0.416	0	8
Industrial Organization	0.059	0.502	0	10
Game Theory	0.003	0.053	0	1
Statistical Evidence	0.041	0.275	0	4
Statistics	0.406	1.739	0	29
Regression	0.158	2.051	0	46
Statistical Significance	0.010	0.135	0	3
Expert Witness	0.322	1.285	0	18
Expert Report	0.465	2.203	0	26
Economic Expert	0.269	1.849	0	36
Economic Report	0.029	0.573	0	15

FIGURE 2. Distribution of Economic Complexity

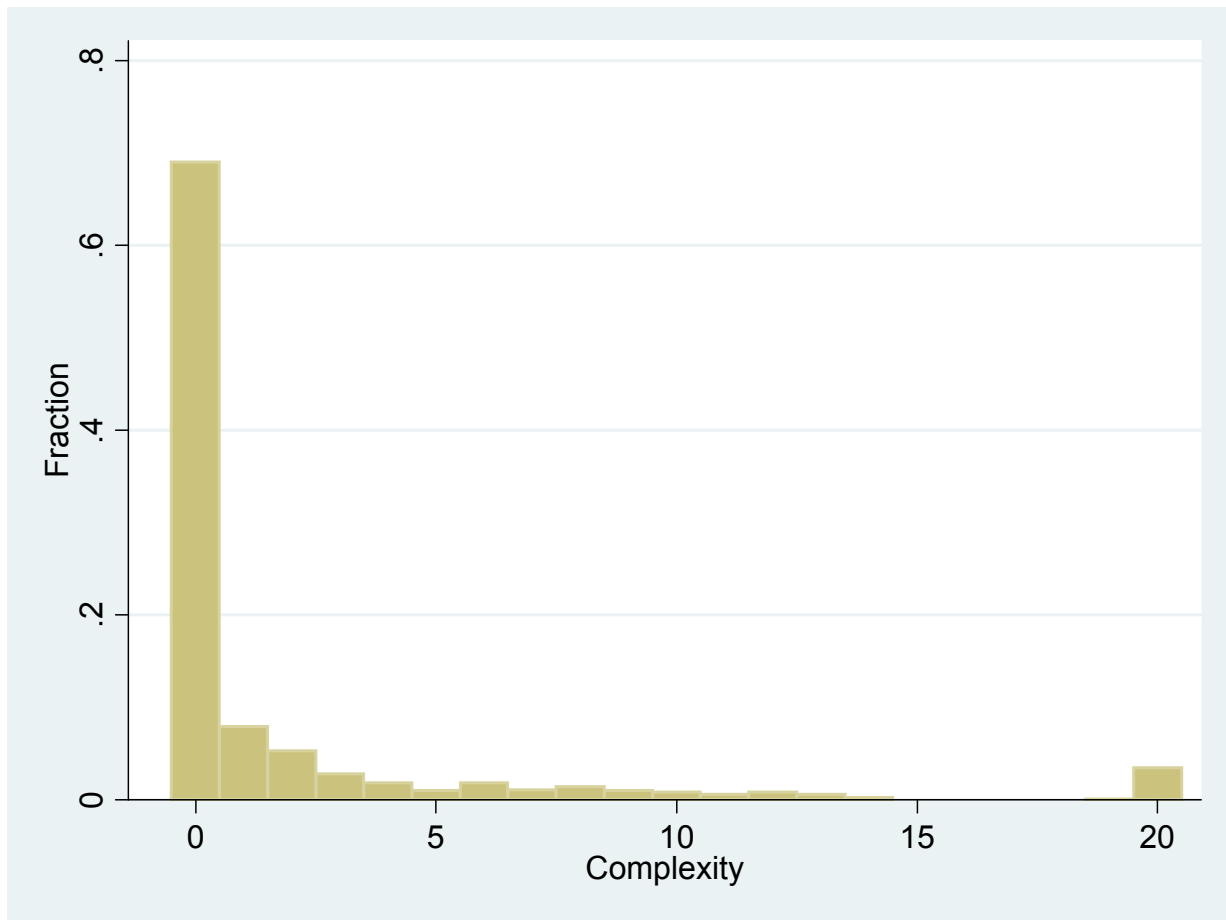


TABLE 2. Selected Summary Statistics, By Circuit, Type of Case, and Plaintiff

	Identifier	Number of Cases	Percent Appealed	Percent Hard	Percent with LEC Trained Judge	Percent with LEC Trained Judge at Time of Decision
By Circuit						
1	First Circuit	48	27.08%	18.75%	2.08%	0.00%
2	Second Circuit	131	23.66%	16.03%	16.79%	12.21%
3	Third Circuit	75	22.67%	20.00%	16.00%	14.67%
4	Fourth Circuit	46	36.96%	36.96%	32.61%	30.43%
5	Fifth Circuit	30	33.33%	20.00%	13.33%	3.33%
6	Sixth Circuit	47	23.40%	27.66%	34.04%	23.40%
7	Seventh Circuit	47	17.02%	27.66%	34.04%	25.53%
8	Eighth Circuit	22	36.36%	31.82%	18.18%	18.18%
9	Ninth Circuit	60	35.00%	28.33%	20.00%	16.67%
10	Tenth Circuit	42	28.57%	30.95%	30.95%	26.19%
11	Eleventh Circuit	54	25.93%	27.78%	22.22%	12.96%
13	Federal Circuit	39	30.77%	48.72%	2.56%	0.00%
14	FTC	73	91.78%	78.08%	0.00%	0.00%
By Type of Case						
1	Merger	78	61.54%	73.08%	7.69%	2.56%
2	Monopolization	235	24.26%	27.23%	19.57%	15.74%
3	Robinson-Patman	33	18.18%	33.33%	12.12%	9.09%
4	Multiple Claims	146	34.93%	25.34%	16.44%	10.96%
5	Price Fixing/Conspiracy	222	35.59%	23.87%	21.62%	17.57%
By Plaintiff						
1	Private	571	26.44%	21.89%	20.84%	16.29%
2	FTC	112	72.32%	74.11%	3.57%	0.00%
3	US DOJ	12	41.67%	58.33%	8.33%	8.33%
4	State Attorney General	19	21.05%	36.84%	21.05%	15.79%
ALL DATA		714	33.75%	31.09%	17.93%	13.59%

TABLE 2.1
Mean Testing: Economic Complexity and LEC Training

Two-sample t-test with equal variances

Variable	N	Mean	Std. Err
Hard Appeal	222	0.505	0.034
Simple Appeal	492	0.262	0.020
Combined	714	0.338	0.018
Difference		0.242	0.037
T-Statistic	6.51		
LEC Appeal	97	0.227	0.043
No LEC Appeal	617	0.355	0.019
Combined	714	0.338	0.018
Difference		-0.128	
T-Statistic	2.49		

TABLE 3. Probit Regressions Reporting Marginal Effect on Appeal Rate: Baselines
714 Antitrust Cases

	(1)	(2)	(3)	(4)	(5)	(6)
HARD	0.236*** (6.05)	0.227*** (5.54)	0.152*** (3.52)	0.166*** (3.72)	0.131*** (2.79)	0.107** (2.17)
LEC	-0.107** (2.06)					
HARD_LEC		-0.053 (0.51)	0.072 (0.64)	0.06 (0.55)	0.093 (0.83)	0.087 (0.73)
SIMPLE_LEC		-0.125** (2.06)	-0.105* (1.69)	-0.109* (1.76)	-0.097 (1.54)	-0.108* (1.68)
YEAR			-0.021*** (7.13)	-0.021*** (6.43)	-0.015*** (3.56)	-0.012*** (2.79)
FIXED EFFECTS:						
Type of Case	No	No	No	Yes	Yes	Yes
Plaintiff	No	No	No	No	Yes	Yes
Circuit	No	No	No	No	No	Yes

Robust z statistics in
parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 4. Probit Regressions Reporting Marginal Effect on Appeal Rate
Controls for Antitrust Experience of Judges, 714 Cases

	(1)	(2)	(3)	(4)	(5)	(6)
HARD	0.235*** (6.03)	0.227*** (5.52)	0.152*** (3.52)	0.166*** (3.71)	0.130*** (2.78)	0.107** (2.17)
LEC	-0.103* (1.96)					
EXPERIENCE	-0.002 (0.78)	-0.002 (0.79)	-0.001 (0.44)	-0.002 (0.61)	-0.002 (0.66)	-0.001 (0.23)
HARD_LEC		-0.047 (0.46)	0.075 (0.66)	0.065 (0.59)	0.099 (0.88)	0.09 (0.75)
SIMPLE_LEC		-0.121** (1.98)	-0.103 (1.64)	-0.107* (1.70)	-0.094 (1.48)	-0.107* (1.65)
YEAR			-0.021*** (7.10)	-0.021*** (6.40)	-0.015*** (3.54)	-0.012*** (2.78)
FIXED EFFECTS:						
Type of Case	No	No	No	Yes	Yes	Yes
Plaintiff	No	No	No	No	Yes	Yes
Circuit	No	No	No	No	No	Yes

Robust z statistics in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

TABLE 5. Probit Regressions Reporting Marginal Effect on Appeal Rate
641 Cases (Excludes Decisions by Administrative Law Judges)

	(1)	(2)	(3)
HARD	0.096** (2.06)	0.096** (2.06)	0.096** (2.06)
HARD_LEC	0.08 (0.73)	0.082 (0.75)	0.08 (0.73)
SIMPLE_LEC	-0.095* (1.69)	-0.094* (1.66)	-0.094* (1.66)
YEAR	-0.010** (2.11)	-0.010** (2.11)	-0.010** (2.11)
EXPERIENCE		-0.001 (0.20)	-0.001 (0.22)
PARTY			0.005 (0.14)
FIXED EFFECTS:			
Type of Case	Yes	Yes	Yes
Plaintiff	Yes	Yes	Yes
Circuit	Yes	Yes	Yes

Robust z statistics in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

**FIGURE A1. Distribution of Experience
(Sample Excluding ALJs)**

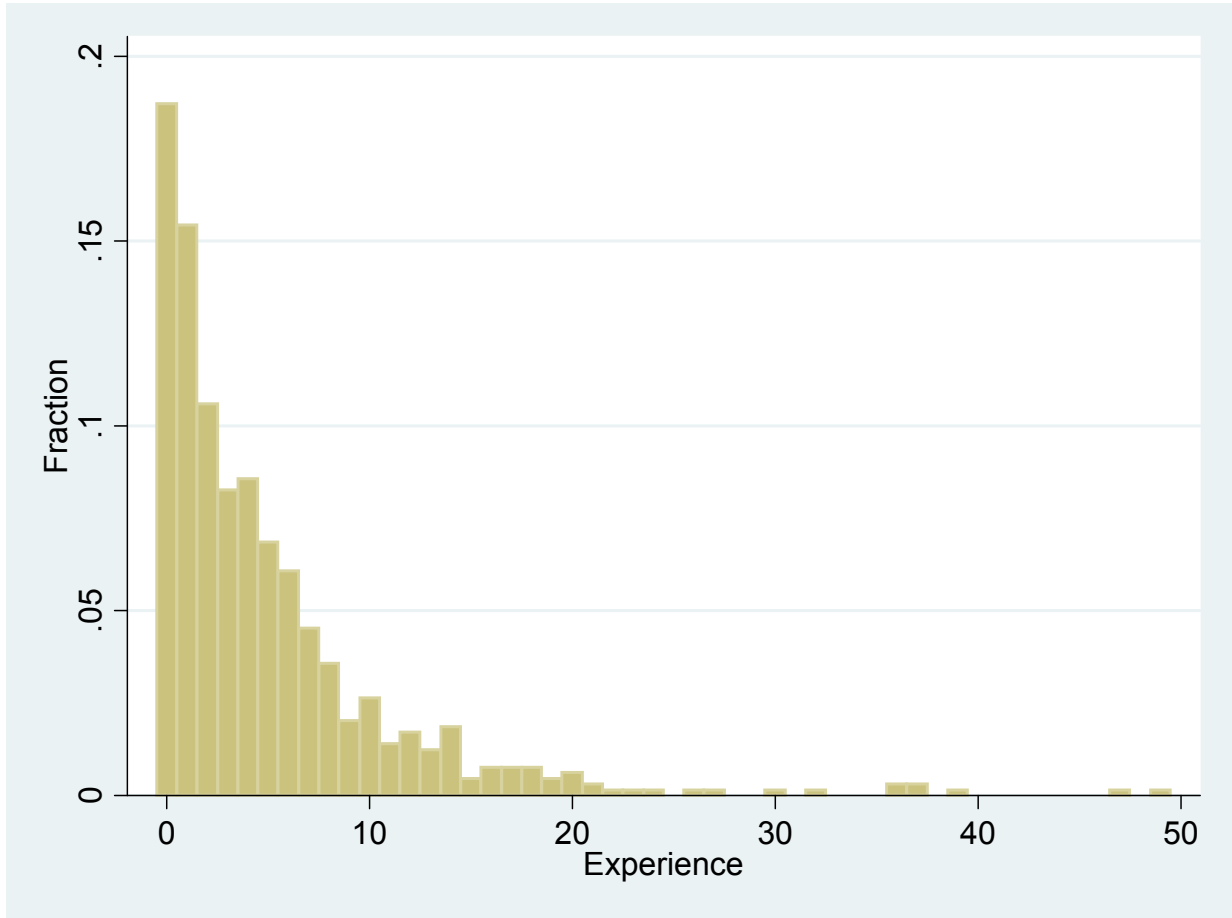


TABLE A1. Summary of Terms Used to Identify Economic Complexity (641 Observation Sample Excluding ALJ's)

Term	<i>Mean</i>	<i>Std. Dev.</i>	<i>Min</i>	<i>Max</i>
Professor of Economics	0.022	0.223	0	4
Econometrics	0.050	0.678	0	15
Economist	0.198	0.814	0	10
Economic Evidence	0.044	0.246	0	2
Industrial Organization	0.011	0.118	0	2
Game Theory	0.003	0.056	0	1
Statistical Evidence	0.031	0.242	0	4
Statistics	0.231	1.094	0	12
Regression	0.048	0.689	0	14
Statistical Significance	0.009	0.137	0	3
Expert Witness	0.201	0.895	0	9
Expert Report	0.443	2.014	0	23
Economic Expert	0.101	0.592	0	8
Economic Report	0.025	0.594	0	15

**FIGURE A2. Distribution of Economic Complexity
(Sample Excluding ALJs)**

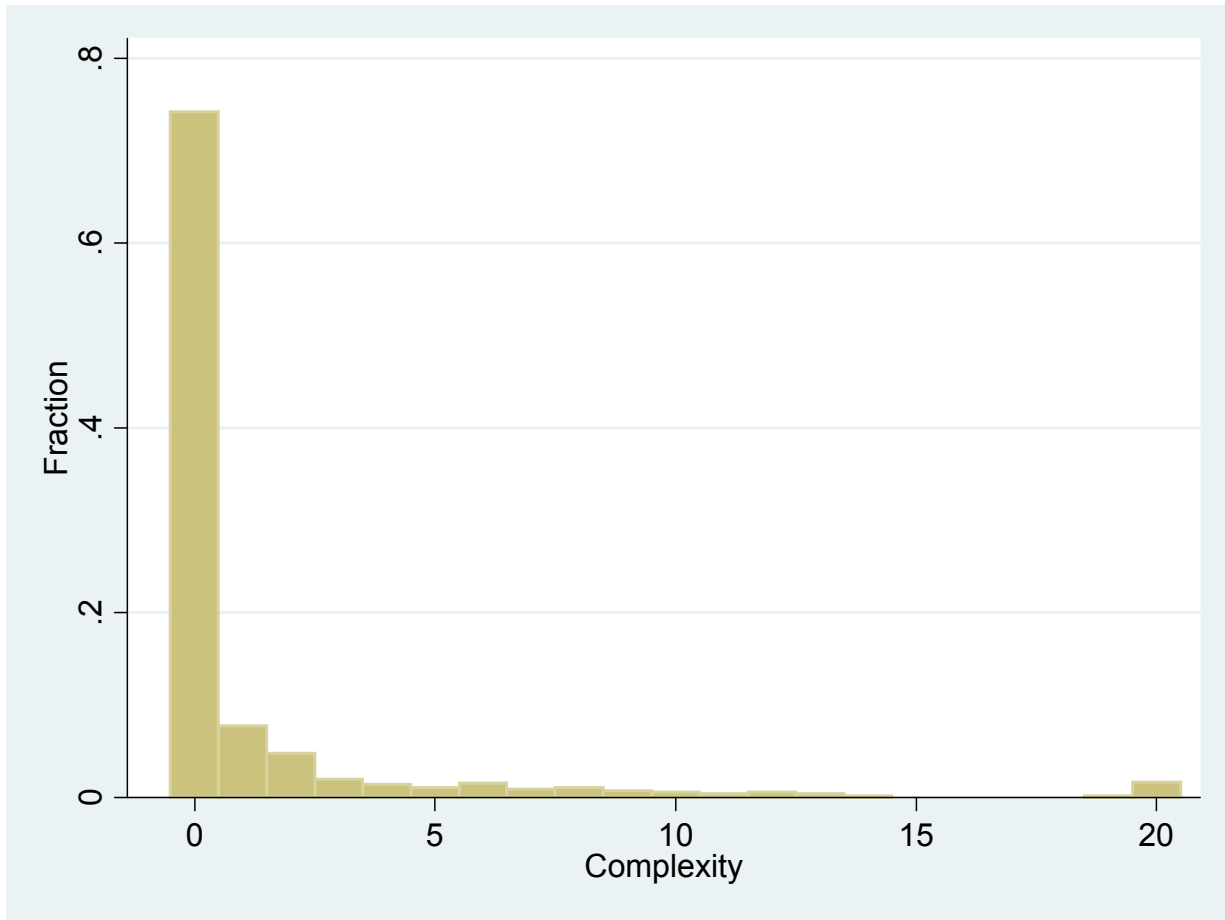


TABLE A2. Selected Summary Statistics, By Circuit, Type of Case, and Plaintiff
Sample Excluding ALJs

	Identifier	Number of Cases	Percent Appealed	Percent Hard	Percent with LEC Trained Judge	Percent with LEC Trained Judge at Time of Decision
By Circuit						
1	First Circuit	48	27.08%	18.75%	2.08%	0.00%
2	Second Circuit	131	23.66%	16.03%	16.79%	12.21%
3	Third Circuit	75	22.67%	20.00%	16.00%	14.67%
4	Fourth Circuit	46	36.96%	36.96%	32.61%	30.43%
5	Fifth Circuit	30	33.33%	20.00%	13.33%	3.33%
6	Sixth Circuit	47	23.40%	27.66%	34.04%	23.40%
7	Seventh Circuit	47	17.02%	27.66%	34.04%	25.53%
8	Eighth Circuit	22	36.36%	31.82%	18.18%	18.18%
9	Ninth Circuit	60	35.00%	28.33%	20.00%	16.67%
10	Tenth Circuit	42	28.57%	30.95%	30.95%	26.19%
11	Eleventh Circuit	54	25.93%	27.78%	22.22%	12.96%
13	Federal Circuit	39	30.77%	48.72%	2.56%	0.00%
By Type of Case						
1	Merger	45	37.78%	66.67%	13.33%	4.44%
2	Monopolization	231	22.94%	25.97%	19.91%	16.02%
3	Robinson-Patman	31	12.90%	29.03%	12.90%	9.68%
4	Multiple Claims	136	30.88%	21.32%	17.65%	11.76%
5	Price Fixing/Conspiracy	198	29.29%	18.69%	24.24%	19.70%
By Plaintiff						
1	Private	571	26.44%	21.89%	20.84%	16.29%
2	FTC	39	35.90%	66.67%	10.26%	0.00%
3	US DOJ	12	41.67%	58.33%	8.33%	8.33%
4	State Attorney General	19	21.05%	36.84%	21.05%	15.79%
ALL DATA		641	27.15%	25.74%	19.97%	15.13%