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

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Abstract

The recent increase in the demand for expert economic analysis in antitrust litigation has improved the welfare of economists; however, the law and economics literature is silent on the effects of economic complexity or judges' economic training on judicial decision making. We use a unique data set on antitrust litigation in federal district and administrative courts during 1996–2006 to examine whether economic complexity impacts antitrust decisions and provide a novel test of the hypothesis that antitrust analysis has become too complex for generalist judges. We also examine the impact of basic economic training on 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 analysis supports the hypothesis that some antitrust cases are too complicated for generalist judges.

1. Introduction

Antitrust analysis is becoming increasingly complex. Modern antitrust litigation and agency practice typically involve judicial evaluation of economic and econometric analysis. The battle of the experts has become a standard, and critical,

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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, the law and economics literature is silent on the empirical effects of economic complexity on decision making 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 mathematically rigorous and technically demanding field. A second, not mutually exclusive, explanation relies on 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 toward 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.[1]

The effects-based structure of modern antitrust law requires economic expert testimony in large part because the Sherman Antitrust Act's (15 U.S.C. 1–7 [2006]) broad language delegates to the judiciary the task of identifying 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 horizontal mergers, or vertical restraints and evaluating conflicting econometric analyses. For instance, Judge Richard Posner (1999, p. 96) 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 a study to a lay audience." This paints a bleak picture for those with hopes that the antitrust enterprise will continue 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 a historical perspective, economically incoherent decisions are now relatively rare compared to the state of affairs that led 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

[1] This shift in federal courts toward incorporating economics in antitrust analysis was not sudden (Kaplow 1987). But there is no doubt that what Posner (2001, p. viii) describes as a "revolutionary change in law" increased the demand for economic testimony concerning the competitive effects of business practices.

upon 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 successful challenge by the Federal Trade Commission (FTC) to the proposed merger of Staples and Office Depot, which relied on complex econometric testimony showing that the merger would result in higher prices to consumers, Posner (2001, p. 158) announced that "[e]conomic 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 American Bar Association (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, p. 2) reached a 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."[2]

Indeed, modern critiques of important antitrust decisions frequently amount to a claim that the judge misunderstood or misapplied the relevant economics, failed to recognize the critical economic issue, or relied on the opinions and analysis of the wrong expert. But while claims that the federal judiciary is not equipped to competently evaluate complex economic or econometric evidence in antitrust cases are often made, and motivate many of the proposed reforms designed to improve judicial accuracy, such claims have not to date been subjected to formal empirical testing.

A recent ABA task force survey of 42 antitrust economists did reveal, however, that only 24 percent believe that judges "usually" understand the economic issues in a case (Baker and Morse 2006, app. II, p. 2). 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 the use of court-appointed experts pursuant to Federal Rule of Civil Procedure 706(a) to expanding the use of *Daubert* (*Daubert v. Merrell Dow Pharm., Inc.,* 509 U.S. 579 [1993]) to deter unsupported economic testimony, introducing concurrent

[2] Some economists suggest that modern antitrust enforcement actually harms consumers (Crandall and Winston 2003), but this is a source of some debate (Baker 2003).

evidence procedures, creating specialized courts, and supplying basic economic training to judges (Posner 1999).[3]

The benefit of providing economic training to judges who handle antitrust matters is obvious (at least to economists). 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, p. 77) 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." Similarly, 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" (Baker and Morse 2006, p. 6).

Judges also perceive economic training to be beneficial; as discussed below, hundreds of judges have already sought out basic economic training. One reason judges might take time away from heavy dockets to receive such training is because doing so improves their decisions, thereby reducing appeals, reversals, or other potentially deleterious effects of economic complexity that could damage their reputations.

Training judges in antitrust economics is not without controversy, however. Some have even criticized educational programs designed to teach judges basic economics. The George Mason University Law and Economics Center (LEC) has been the focus of much of the criticism, at least in some part because it is the largest 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 the height of its activity 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.[4] Critics claim that the programs amount to 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 training programs for federal judges (Teles 2008).

This paper represents a first attempt to empirically examine the effects of economic complexity and basic economic training on judicial decisions in antitrust. We find that economic complexity significantly increases the likelihood

[3] Gallini (2002), for example, provides an excellent discussion of how the creation of specialized courts has impacted patent litigation.

[4] The Law and Economics Center (LEC) claims that "[b]y 1990, approximately forty percent of the sitting federal judges had completed . . . the Economics Institute for Federal Judges" (Butler 1999, p. 352).

that a judge's decision is appealed.[5] This effect is statistically and practically significant; the appeal rate for economically complex decisions is about 10 percent greater than for simple cases in our most general specifications.[6] We also find that the decisions of judges with basic economic training are appealed in simple cases at significantly lower rates than those of their untrained counterparts. We find no evidence that a judge's basic training in economics has an impact on appeals in economically complex cases, which is consistent with the intuition that basic economics is helpful in deciding simple antitrust cases but not cases involving complex economic or econometric evidence. These results are robust across two data sets and different specifications that control for a judge's political ideology, level of antitrust experience, and postgraduate education—and other controls that include fixed effects for the type of plaintiff (for example, the FTC or the Department of Justice [DOJ]), the type of case (for example, merger or monopolization), and the circuit in which the case is litigated.

We believe these 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. We argue that the parties—who have typically invested in expert economists and thus are in a strong position to understand the strengths and weaknesses of complex economic arguments—can assess relatively well whether the initial 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 signals that (at least it believes) the judge made a potentially reversible error. 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 lower rate than those of their untrained counterparts as evidence that supports the view that some antitrust cases are too complex for generalist judges.

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

[5] 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, which suggests poor performance by the district courts. See, for example, Moore (2001); Chu (2001); see also Wagner and Petherbridge (2004). This literature generally does not control for individual judicial characteristics such as technical scientific background, with the exception of Moore (2001), who finds no difference in reversal rates between Federal Circuit judges with technical backgrounds and those without.

[6] In this context, "simple" describes only the absence of economic complexity. Like most other forms of civil commercial litigation, antitrust litigation can be highly complex as the result of legal and procedural considerations unrelated to technical economic sophistication.

2. Data

There are four main categories of data. The first category involves information extracted from judicial opinions. We 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 between 1996 and 2006.[7] Our sample includes 73 decisions on substantive antitrust issues by administrative law judges and 641 by Article III federal district court judges, for a total of 714 decisions.[8]

Each decision was coded to include information describing the type or types of antitrust claims litigated (merger, monopolization, price fixing, Robinson-Patman, or multiple claims), plaintiff (FTC, DOJ, private party, or state attorney general), and the date of the decision. Our data also include an indicator for whether at least one of the parties appealed the court's decision and an indicator for whether the appeal resulted in a reversal.

The second category of data consists of judge and court characteristics. In order to be in a position to attempt to disentangle political ideology from economic training and other factors that might influence appeals, we collected data on the political party of the judge as measured by the party of the appointing president.[9] In addition, we obtained data on the postgraduate education and the prior antitrust experience of judges. One might hypothesize that prior antitrust experience improves judicial decisions in complex cases and may be 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 motivates proposals for specialized antitrust courts. We use a proxy for judicial antitrust experience in the form of a count of the total number of antitrust opinions a judge authored prior to issuing a decision in each case. Figure 1 displays the distribution of this measure of experience. Notice that experience tends to be clustered around zero, which indicates that a large fraction of judges had little or no prior antitrust experience at the time the decision was made.

We also collected data on other court characteristics, including the federal circuit to which each district court judge belonged (thus allowing us to control for potential variation among circuits). This is potentially valuable if one believes, for example, that district court judges in the D.C. Circuit are more competent in handling complex antitrust cases litigated by the nearby enforcement agencies.

[7] 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")).

[8] A number of decisions involving antitrust claims are excluded from this sample because they did not involve a decision on the merits of a substantive antitrust issue. These decisions were most commonly related to venue and class certification issues. In cases generating multiple opinions, each opinion is treated as a distinct observation.

[9] Party of the appointing president is available for each district court judge. Administrative law judges are not appointed by the president, and thus political ideology data are unavailable for them. While there is a substantial body of literature on the influence of ideology in appellate courts and the Supreme Court (Cross 2007), the evidence of political effects in federal district courts is mixed (Posner 2008; Sisk, Heise, and Morriss 1998; Ashenfelter, Eisenberg, and Schwab 1995).

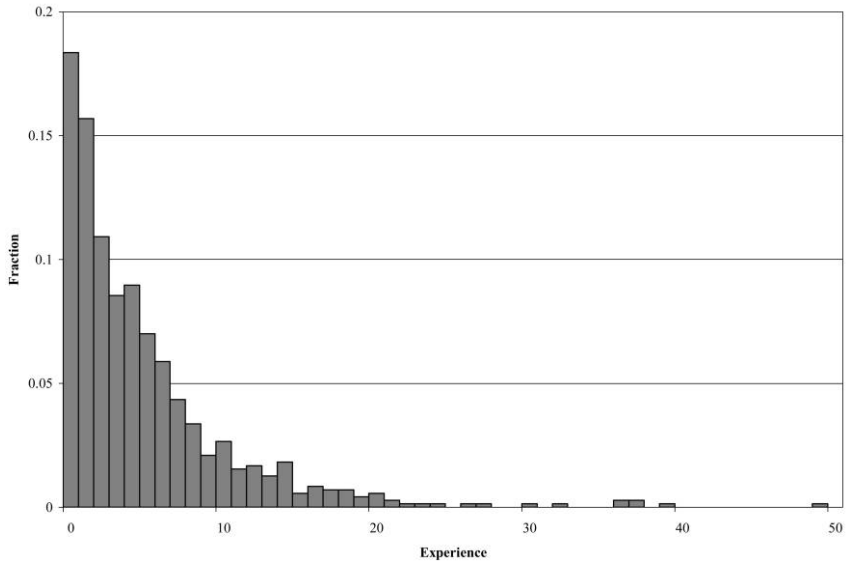

Figure 1. Distribution of judges' prior antitrust experience ($N = 714$ cases)

In addition, data on circuits permit us to control for potential intercircuit variation, including the political composition and economic sophistication of the appellate court and differences in the substantive antitrust law that might influence the appeal rate.

The third category of data involves measures of economic complexity. We selected 14 key terms that one would expect to arise in a complex antitrust case involving sophisticated economic or econometric evidence. We then performed an electronic search of the decisions in each case and recorded the number of times each of the key terms was referenced. These terms are summarized in Table 1. Finally, we constructed an aggregate summary statistic representing the overall economic complexity of each case by computing the total number of times these 14 terms appeared in a given decision. Figure 2 displays the distribution of this measure of economic complexity. In light of the fact that the majority of the decisions were in simple cases, in that none of these 14 terms were referenced in the decisions, we created an indicator variable that divides cases into two types: complex and simple. Simple cases generated opinions that did not use these terms at all, while a complex case is defined as one in which one or more of the terms in Table 1 was referenced. Our sample includes 222 complex cases and 492 simple cases.

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

Table 1
Instances per Case of Terms Identifying Economic Complexity ($N = 714$ Cases)

Term	Mean	SD	Max
Professor of economics	.049	.346	5
Econometrics	.052	.652	15
Economist	.387	1.637	26
Economic analysis	.071	.416	8
Industrial organization	.059	.502	10
Game theory	.003	.053	1
Statistical evidence	.041	.275	4
Statistics	.406	1.739	29
Regression	.158	2.051	46
Statistical significance	.010	.135	3
Expert witness	.322	1.285	18
Expert report	.465	2.203	26
Economic expert	.269	1.849	36
Economic report	.029	.573	15

Note. Min = 0 for all terms.

basic economic training sessions at the LEC and the date they attended.[10] 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 received basic economic training before the date the decision was issued.

These data are a potentially useful measure of economic expertise and are of interest for several reasons. First, to the extent that judges who attend basic economic training sessions are the least likely to have any economic sophistication or skills to begin,[11] it is likely that any impact of training on appeals can be attributed to a judge's acquiring basic economic skills. Second, since 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 tech-

[10] We used a number of data sources to compile this information. The primary source is the searchable database at the Web site Trips for Judges (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 and the Liberty Fund (Adler 2005). The database compiles judges' financial disclosure forms from the period 1992–2004. We supplemented this information with more recently published financial disclosures and records available at the LEC.

[11] Programs consisted of a 2-and-a-half-week course in basic economics 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, p. 112). Butler (1999) provides a detailed account of the LEC programs.

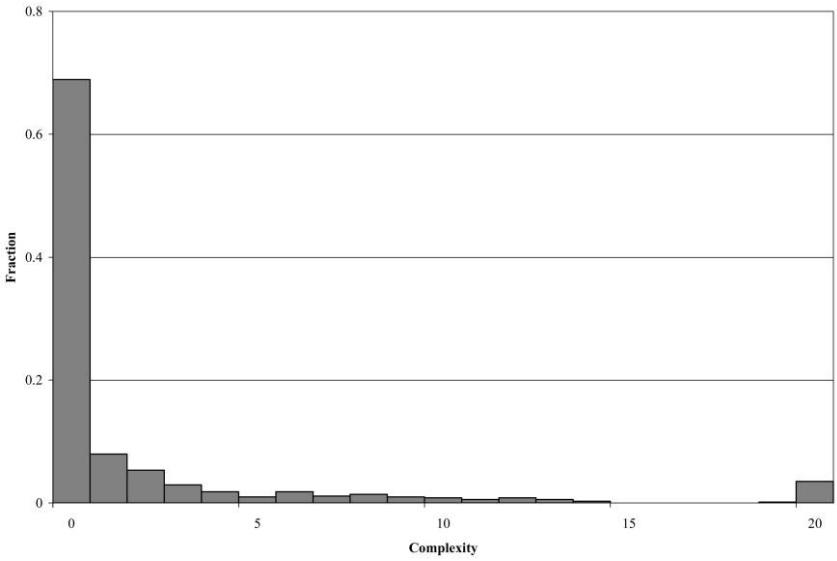

Figure 2. Distribution of economic complexity ($N = 714$ cases)

nical 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 patterns. In terms of intercircuit variation in appeal rates, the Seventh Circuit, home of antitrust expert judges Posner and Frank Easterbrook, claims the lowest appeal rate—approximately half of the sample average. Federal Trade Commission administrative litigation, where initial decisions are made by the FTC's administrative law judges (and appeals are made directly to the commission), has the highest appeal rate. With respect to LEC training, there is significant variation between circuits. No cases in the First and Federal Circuits were decided by judges with LEC training prior to the decision, while about 30 percent of the Fourth Circuit's substantive antitrust decisions were authored by trained judges. We note that while there is large variation in the percentage of trained judges across circuits and types of cases, random assignment of district court judges to cases suggests that this variation reflects a composition effect (different circuits have different types of judges and different types of cases) rather than nonrandom assignment (which would lead to case characteristics' being correlated with unobserved judge characteristics).

Merger cases are the most complex in the sample and have a significantly higher appeal rate than other types of cases. Interestingly, these more complex cases are decided by judges with LEC training only 2.56 percent of the time, far

Table 2
Selected Summary Statistics ($N = 714$ Cases)

	Cases (N)	Appealed (%)	Complex (%)	With Trained Judge (%)	With Trained Judge at Time of Decision (%)
Circuit:					
First	48	27.08	18.75	2.08	.00
Second	131	23.66	16.03	16.79	12.21
Third	75	22.67	20.00	16.00	14.67
Fourth	46	36.96	36.96	32.61	30.43
Fifth	30	33.33	20.00	13.33	3.33
Sixth	47	23.40	27.66	34.04	23.40
Seventh	47	17.02	27.66	34.04	25.53
Eighth	22	36.36	31.82	18.18	18.18
Ninth	60	35.00	28.33	20.00	16.67
Tenth	42	28.57	30.95	30.95	26.19
Eleventh	54	25.93	27.78	22.22	12.96
Federal	39	30.77	48.72	2.56	.00
FTC administrative litigation	73	91.78	78.08	.00	.00
Type of case:					
Merger	78	61.54	73.08	7.69	2.56
Monopolization	235	24.26	27.23	19.57	15.74
Robinson-Patman	33	18.18	33.33	12.12	9.09
Multiple claims	146	34.93	25.34	16.44	10.96
Price fixing or conspiracy	222	35.59	23.87	21.62	17.57
Plaintiff:					
Private	571	26.44	21.89	20.84	16.29
FTC	112	72.32	74.11	3.57	.00
DOJ	12	41.67	58.33	8.33	8.33
State attorney general	19	21.05	36.84	21.05	15.79
All data	714	33.75	31.09	17.93	13.59

Note. FTC = Federal Trade Commission; DOJ = Department of Justice.

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 by the fact that about two-thirds of the FTC's cases were administrative law cases (in which appeals are made to the commission), it is also interesting to note that an LEC-trained judge has never authored an antitrust decision in a case in which the FTC is plaintiff. By way of contrast, cases in which the DOJ is plaintiff are appealed 41.67 percent of the time, while only 26.44 percent of decisions involving private-party plaintiffs are appealed.

3. Methodology and Caveats

Our primary measure of the quality of an initial court's decision is a party's decision to appeal. Thus, we estimate the probability of a specific initial court

decision's being appealed as a function of the economic complexity of the case, the judge's economic training, and a variety of other controls.[12]

Our primary rationale for using appeals as an indicator for whether the initial court made an error of economics derives from a revealed-preference argument. The appeal rate is a signal generated by actual costs incurred by parties who, informed by their economic experts, are in a good position to evaluate whether the initial court committed (or is sufficiently likely to have committed) reversible error. While there are reasons for a party to appeal any given initial court decision that are unrelated to its quality, ceteris paribus, an appeal signals that at least one party believes that it can convince a higher court that the initial decision contains reversible error. A lower appeal rate likely means that a judge issued fewer opinions that left at least one party feeling strongly enough to invest in the opportunity to persuade an appellate court that the initial 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 initial court 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's effects-based approach creates unique overlap between legal and economic inquiries, relative to other areas of the law. For example, the legal inquiry under section 7 of the Clayton Antitrust Act (15 U.S.C. 18 [2006]) is whether the proposed transaction will "substantially lessen competition," a test that has taken on an exclusively economic interpretation that equates a violation of this standard with a reduction in consumer welfare. The fact that modern legal and economic antitrust analyses are inextricably intertwined suggests that an especially large fraction of appeals will be motivated by the view that the initial court made an economic error.[13]

Moreover, parties in antitrust cases frequently invest in hiring economic experts and are likely to be well informed about the strengths and weaknesses of complex economic evidence. In contrast, judges did not use a court-appointed expert for any of the cases in our sample and thus were on their own to evaluate the evidence produced through any battle of the economic experts.

We also report results based on an alternative indicator of the quality of the initial court's decision: a reversal by the appellate court.[14] Unfortunately, because appellate reversals involve the decisions of a panel of multiple decision makers, each with potentially different political ideologies and economic training, per-

[12] As discussed below, we also examine models involving the probability of a specific initial court decision's being reversed, conditional on that decision's being appealed.

[13] This feature of modern antitrust analysis is not limited to mergers. More generally, Posner (2001, pp. vii, 35) explains 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."

[14] This measure is sometimes used in the literature on patent litigation; see Gallini (2002). More recently, Duso, Neven, and Röller (2007) use an event study methodology to examine the impact of European Union merger decisions on stock performance.

sonal interactions among these decision makers preclude us from controlling for the effects of the characteristics of individual appellate judges (such as political party or basic economic training) on the appellate court's reversal decision when this alternative indicator is used. In addition, reversals are necessarily conditioned on the decision's being appealed in the first place, which significantly reduces the sample size in specifications that use it to measure the quality of an initial court's decision. For these reasons, we primarily use a party's appeal of an initial court decision to measure potential economic error by the initial 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 complex if it includes one or more of the terms in Table 1 and as simple if it does not. Importantly, however, it is possible that decisions including these terms could involve very little sophisticated economic or econometric analysis. It is also possible that decisions are economically complex despite the absence of any of these terms. An informal (ex post) review of the decisions in our sample suggests that the complex cases consistently involve at least some evaluation of expert economic evidence, and simple cases do not. Nonetheless, we acknowledge that our measure of economic complexity is a proxy for a nebulous concept.

Another limitation of our analysis is that we do not directly observe some potentially important predictors of the appeal rate. The most important of these potentially omitted variables is the stakes of the underlying litigation, which could be a significant predictor of the appeal rate. However, two of our control variables can be interpreted as controlling for litigation stakes. First, our control for the type of case distinguishes merger cases from price-fixing or monopolization allegations, and there is some evidence that the type of case is correlated with stakes in the antitrust litigation context.[15] Second, even with this control, it is possible that our measure of complexity is a confluence of economic complexity and the presence of high litigation stakes (since an expert report is presumably more likely in cases in which litigation stakes are high). If this is the case, the results we report for the impact of complexity on appeals should be interpreted as capturing the impact of both economic complexity and high stakes on appeals.

Unfortunately, we do not have access to data on some potentially important predictors of the appeal rate, such as the quality of legal representation. 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

[15] Bizjak and Coles (1995) find that litigation involving horizontal conspiracy allegations is 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.

complexity. There may also be judge-specific effects. Unfortunately, the data are not rich enough to permit us to control for these possibilities.

Finally, our sample consists of only litigated cases generating published opinions, and it is well known that these cases are more likely to be close calls (see, for instance, Block, Nold, and Sidak 1981; Carlton 2008; Priest and Klein 1984). Likewise, some cases may show up in the data as "not appealed" because they are settled prior to an appellate opinion. In this case, a decision to appeal may indicate heterogeneous beliefs regarding initial judicial error. This sample selection does not impact our ultimate research question but means that our analysis should be interpreted as examining how well judges evaluate close calls or cases where beliefs are heterogeneous. To account for the possibility that the mix of cases that are litigated rather than settled changes over time in ways that correlate with decision quality or complexity, we include controls such as a time trend and dummy variables for the type of case, plaintiff, and circuit.

4. Results

4.1. Economic Complexity, Basic Economic Training, and Appeals

We begin with some simple comparisons of means to explore differences in the appeal rates for complex and simple decisions and decisions by trained and untrained judges. Table 3 reports the results. Economically complex cases in our sample are 24.2 percent more likely to be appealed than are 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 trained judges are appealed at a rate 12.8 percent lower than decisions authored by their untrained colleagues. This difference is also both statistically (at the 1 percent level) and practically significant. Judges who have previously attended economic training programs have their antitrust opinions appealed only 22.7 percent of the time, compared to 35.5 percent for decisions by untrained judges.

While we prefer comparisons based on appeals rather than reversals, we note that similar results obtain when we use reversals. Conditional on being appealed, opinions authored by trained judges are reversed by a higher court only 13.6 percent of the time, while their untrained counterparts' decisions are reversed 23.7 percent of the time. Similarly, complex cases in our sample are reversed 27.7 percent of the time, while simple cases are reversed only 18.6 percent of the time.[16]

These means tests suggest that economic complexity and basic economic train-

[16] While these results are similar in direction and magnitude to the results based on appeals and reported in Table 3, the use of reversals significantly reduces the sample size; only the difference in reversal rates for complex and simple cases is statistically significant at the 5 percent level.

Table 3
Economic Complexity and Basic Economic Training: Impact on Appeals

	N	Mean	SE
Complex cases	222	.505	.034
Simple cases	492	.262	.020
Combined	714	.338	.018
Difference		.242	.037
Trained judges	97	.227	.043
Untrained judges	617	.355	.019
Combined	714	.338	.018
Difference		−.128	.051

Note. In two-sample *t*-tests with equal variances, $t = 6.51$ for cases and $t = -2.49$ for judges.

ing for judges are important predictors of appeal (and reversal) rates in antitrust cases. However, it is possible that the correlations between complexity, basic economic training, and appeals may be the result of omitted variable bias confounding their true impact. In the sequel, we use a probit regression framework to control for other possible influences and isolate the impact of economic complexity and basic economic training on antitrust appeals.

4.2. Baseline Probit Regressions

In each of our regressions, the dependent variable is APPEAL, an indicator that equals one if the initial decision is appealed and zero otherwise. Our primary independent variable of interest is COMPLEX, a dummy variable that equals one when the initial court's 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 TRAINED, a dummy variable that equals one if the judge issuing the initial opinion received training in basic economics prior to the decision and zero otherwise. To further explore the impact of basic economic training on appeals, we generated two interaction terms: COMPLEX × TRAINED and SIMPLE × TRAINED. These interaction terms allow us to isolate, respectively, the marginal impact of training on appeals in complex cases that involve economic or econometric evidence and simple cases that do not.

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 (YEAR) and dummy variables for the type of claim, the type of plaintiff, and the circuit in which the decision was litigated. Table 4 reports marginal effects and robust *z*-statistics.

Specification 1 is our baseline model, which is similar to the mean comparisons in Table 3 except that it simultaneously controls for both economic complexity and basic economic training. The results are similar in magnitude and signifi-

Table 4
Baseline Probit Regressions Reporting Marginal Effect on Appeal Rate ($N = 714$ Cases)

	(1)	(2)	(3)	(4)	(5)	(6)
COMPLEX	.236**	.227**	.152**	.166**	.131**	.107*
	(6.05)	(5.54)	(3.52)	(3.72)	(2.79)	(2.17)
TRAINED	−.107*					
	(2.06)					
COMPLEX × TRAINED		−.053	.072	.061	.093	.088
		(.51)	(.64)	(.55)	(.83)	(.73)
SIMPLE × TRAINED		−.125*	−.105+	−.110+	−.097	−.108+
		(2.06)	(1.69)	(1.76)	(1.54)	(1.68)
YEAR			−.021**	−.021**	−.015**	−.013**
			(7.13)	(6.43)	(3.56)	(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

Note. Robust z-statistics are in parentheses.
+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

cance to those reported in Table 3, with complex cases being appealed 23.6 percent more often than simple cases and basic economic training reducing the probability of appeal by 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. In addition, complex cases raise more difficult fact-finding determinations and, therefore, greater opportunities for a judge to commit potentially reversible errors that might trigger an appeal by one of the parties.

Specification 2 uses interaction terms to examine whether basic economic training has a differential impact on appeals rates in complex and simple cases. As before, decisions involving complex economics or econometrics are more likely to be appealed than simple cases: complex cases are 22.7 percent more likely to be appealed than simple cases, and the effect is statistically significant at the 1 percent level. Interestingly, basic economic training does not have a statistically significant effect on complex cases (the coefficient of COMPLEX × TRAINED 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 × TRAINED). This result is consistent with intuition: 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 4 reveal that the results in specification 2 are robust to, respectively, the addition of a simple time trend and dummy variables to control for the type of case, the type of plaintiff, and the circuit in which the case was litigated. In the specifications with these controls, complex

cases are 11 to 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 percent.[17]

4.3. Economic Training versus Prior Antitrust Experience

The story that emerges from Table 4 is that economic complexity increases the appeal rate, while basic economic training reduces appeals in simple cases but has little or no effect in the more complex cases. This evidence that basic economic training arms generalist judges with enough economic knowledge to more accurately resolve simple antitrust cases provides some support for antitrust litigation reform efforts designed to equip judges with greater economic expertise through training and court-appointed experts. However, a frequently discussed alternative to increasing judicial economic competency is the creation of specialized antitrust tribunals that would give judges repeated exposure to complex antitrust issues. In Table 5, we add EXPERIENCE to control for a judges' prior antitrust exposure and thus to explore the effects of experience on the quality of decisions.

The results in Table 5 suggest that the baseline specifications reported in Table 4 are robust to the addition of this control. In the most general specification, economic complexity increases the appeal rate by 10.7 percent, while basic economic training decreases appeals in simple cases by 10.7 percent. All results are similar in magnitude and significance to the results in Table 4. Judges' prior exposure to antitrust cases 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 5 as suggesting that repeated exposure to antitrust cases is a poor substitute for economic training.

4.4. Robustness Check: Federal District Court Judges Only

One possible explanation of the results in reported in Tables 4 and 5 is that they are driven by the inclusion of FTC administrative litigation in the sample. While the specifications with circuit fixed effects control for the fact that decisions in FTC administrative litigation are made by administrative law judges rather than district court judges, they do not control for the fact that the underlying appeals model (and the impact of basic economic training, experience, and complexity) may differ for FTC administrative litigation and litigation in federal district courts. As shown in Table 2, none of the FTC administrative law judges received any LEC training, their decisions involve a higher fraction of complex cases, and the rate at which their decisions are appealed (to the commission) is significantly higher than the rates at which the decisions of federal district judges

[17] We also ran these specifications using reversal rather than appeal as the dependent variable. Conditional on appeal, economic complexity increases the likelihood of a reversal, and LEC training reduces the likelihood of a reversal in specifications analogous to those in Table 4. As discussed earlier, conditioning on appeal reduces the overall sample size such that these effects are not statistically significant in all specifications.

Table 5
Probit Regressions Reporting Marginal Effect on Appeal Rate, with Controls for Antitrust Experience of Judges ($N = 714$ Cases)

	(1)	(2)	(3)	(4)	(5)	(6)
COMPLEX	.235**	.227**	.152**	.166**	.13**	.107*
	(6.03)	(5.52)	(3.52)	(3.71)	(2.78)	(2.17)
TRAINED	−.103+					
	(1.96)					
EXPERIENCE	−.002	−.002	−.001	−.002	−.002	−.001
	(.78)	(.79)	(.44)	(.61)	(.66)	(.23)
COMPLEX × TRAINED		−.047	.075	.065	.099	.090
		(.46)	(.66)	(.59)	(.88)	(.75)
SIMPLE × TRAINED		−.121*	−.103	−.107+	−.094	−.107+
		(1.98)	(1.64)	(1.70)	(1.48)	(1.65)
YEAR			−.021**	−.021**	−.015**	−.013**
			(7.10)	(6.40)	(3.54)	(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

+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

are appealed (to federal appellate courts). While the high appeal rate may be driven by the greater complexity or a lack of basic economic training, it is more likely that these differences stem from procedural and institutional differences between FTC administrative litigation and litigation in federal district courts. Consistent with this concern, the estimated circuit fixed effect for FTC administrative litigation in Tables 4 and 5 implies an appeal rate in FTC administrative litigation that is about 60 percent higher than that for decisions originating in federal district court. Indeed, others have argued that the lack of independence in FTC administrative litigation provides an incentive for parties to appeal FTC administrative litigation decisions more often than those generated by federal district court judges.[18]

In order to address these concerns, we replicate our analysis with a sample that includes only initial decisions issued by Article III federal district court judges.[19] Specification 1 in Table 6 corresponds to specification 6 in Table 4, which includes fixed effects for the type of case, plaintiff, and circuit. Specification

[18] Using a sample of Sherman Act disputes litigated before administrative law judges at the FTC from 1983 to 2008, Melamed (2008) presents evidence that the respondents prevailed in only four 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 four decisions decided in favor of respondents. Melamed (2008, p. 20) suggests that the disparate appeal rates and respondent win rates are likely explained, at least partially, by the fact that "[c]ommissioners inherently and unavoidably lack the independence that we expect from adjudicative fact-finders."

[19] Tables A1 and A2 provide summary statistics for these data and the complexity measures, while Figures A1 and A2 display the corresponding distributions of judicial experience and economic complexity.

Table 6
Probit Regressions Reporting Marginal Effect on Appeal Rate: Sample of Federal District Court Judges ($N = 641$ Cases)

	(1)	(2)	(3)	(4)
COMPLEX	.096*	.096*	.096*	.096*
	(2.06)	(2.06)	(2.06)	(2.06)
COMPLEX × TRAINED	.080	.082	.080	.076
	(.73)	(.75)	(.73)	(.70)
SIMPLE × TRAINED	−.095+	−.094+	−.094+	−.095+
	(1.69)	(1.66)	(1.66)	(1.67)
YEAR	−.010*	−.010*	−.010*	−.010*
	(2.11)	(2.11)	(2.11)	(2.11)
EXPERIENCE		−.001	−.001	−.001
		(.2)	(.22)	(.17)
PARTY			.005	.001
			(.14)	(.02)
QUALITY				−.061
				(.78)

Note. Robust *z*-statistics are in parentheses. All specifications include case type, plaintiff, and circuit fixed effects.
+ Significant at 10%.
* Significant at 5%.

2 in Table 6 corresponds to specification 6 in Table 5, which includes a control for the antitrust experience of the judge as well as fixed effects.

Specifications 1 and 2 in Table 6 reveal that the results in Tables 4 and 5 are not driven by FTC administrative litigation. In these specifications, appeal rates are 9.6 percent higher in complex cases, and basic economic training reduces the likelihood of appeal in simple cases by about 9.5 percent.

4.5. Robustness Check: Judicial Training or Ideology?

One related concern with the results thus far is that judges receiving basic economic training are not randomly assigned. One such hypothesis is that judges attending training programs are more politically conservative or otherwise more predisposed to economics and business-oriented thinking than their untrained counterparts. If that were so, our training measure might be capturing some preexisting differences in the economic sophistication or orientation of the judges rather than the effect of basic economic training. Consistent with this view, much of the controversy surrounding the LEC training programs has 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 does not appear that the training effect is an artifact of selection into these programs by Republican judges. Of the opinions in our federal court database, 321 are authored by Democrats and 320 by Republicans. Approximately 13 percent of the Democrats and 17 percent of the Republicans in our sample received basic economic training.

To more formally explore the possibility that the effects of training are being

driven by political ideology, specification 3 in Table 6 includes PARTY—a dummy variable for the political party of the appointing president—as a control. The results of this specification reveal that the political ideology of the district court judge is not a significant predictor of the appeal rate, and the effects of economic complexity and basic economic training are similar in magnitude and significance to those reported in Tables 4 and 5, as well as specifications 1 and 2 in Table 6. Thus, it does not appear that the reduction in appeals associated with basic economic training is an artifact of the ideology of those opting to take such training in the first place.[20]

4.6. Robustness Check: Judicial Training or Judicial Quality?

While our finding that basic economic training significantly reduces appeal rates in simple cases is robust to a variety of controls and the use of alternative data sets, it is of course possible that the actual effects are driven by other unobserved factors that are merely correlated with training. For instance, higher quality judges may be more adept at sorting through complex economic issues. To the extent that such judges may be more intellectually curious, they may be more likely to seek out training. If this is the case, training is merely serving as a proxy for intellectual curiosity or judicial quality. It is of course impossible to entirely rule out these sorts of arguments, but the fact that the results presented in Tables 4 and 5 are robust to the exclusion of administrative law judges, as well as controls for the antitrust experience and the political party of judges (specifications 1–3 in Table 6), suggests that training does have an effect.

As an additional robustness check, we obtained data to construct an additional measure of judicial quality based on the postgraduate education of the district court judges in our sample. This measure, QUALITY, is a dummy variable that equals one if the judge holds an M.A., M.S., or Ph.D. and zero otherwise. As shown in specification 4 of Table 6, our results are robust to this additional control. The estimated coefficient of QUALITY implies that decisions of judges with advanced degrees are about 6 percent less likely to be appealed, although the effect is not statistically significant at conventional levels. More important, however, even with this and all of the other controls, complex cases are 9.6 percent more likely to be appealed than simple ones, and the appeal rate in simple cases for judges with basic economic training is 9.5 percent lower than for their untrained counterparts.

[20] We also ran specifications allowing for the possibility that basic economic training impacts Republican and Democratic judges differently and found that the effects of training are similar for both. These results are consistent with those of Moore (2001), who finds that the political party of the appointing president does not predict reversal rates in patent claim construction decisions in district court. But see Sag, Jacobi, and Sytch (2009), who find that political ideology is a significant predictor of outcomes in Supreme Court intellectual property cases.

5. Conclusions

Modern antitrust litigation involves considerably greater economic sophistication than it did even 25 years ago. While numerous 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 offer the first empirical evidence on the relationship between technical economic complexity and the quality of antitrust decisions. The evidence here suggests that economic complexity and judicial economic training influence the appeal rate in opposite directions: economic complexity significantly increases the probability of appeal, while judicial training reduces it. The estimated effects are similar across two data sets, in a variety of specifications, and with a host of controls.

More specifically, our first finding is that decisions involving some evaluation of economic or econometric evidence are appealed approximately 10 percent more frequently than cases demanding less economic skill. An appeal indicates that at least one party is willing to make a costly investment for the opportunity to persuade an appellate court that the district court judge erred. This is more likely in cases involving complex economic evidence because in such cases there are likely to be reasonable fact-finding disputes and, thus, more room to persuade an appellate court that a reversible error was committed by the lower court. While one may reasonably dispute whether the relationship between economic complexity and appeals identified here is strong evidence of a divergence between the technical demands of contemporary antitrust analysis and the technical economic skills of generalist judges on the federal bench, it is clear that economic complexity does impact the modern antitrust litigation landscape.

Our second finding is that the decisions of judges who attended programs to learn basic economic skills are appealed at the same rate as those of their untrained counterparts in complex cases but about 10 percent less often in cases that do not involve the evaluation of sophisticated economic or econometric evidence. One interpretation is that, while basic economic training does not prepare a district court judge to evaluate the complex economic testimony seen in many modern antitrust cases, such training does help judges get the economics right in simple antitrust cases. Our results also suggest that repeated exposure to complex antitrust issues is not a close substitute for economic training.

Our empirical results highlight both the promise and the limits of training judges in basic economics. On the one hand, the primary benefit of basic economic training is that judges are more likely to get the economics right in simple cases. On the other hand, our results suggest that basic economic training alone does not improve judicial decisions in complex antitrust cases. Improving the quality of decisions in modern antitrust cases involving complex economic and econometric evidence may require more drastic institutional changes. Our estimates suggest that the type of repeat exposure to antitrust litigation contemplated by proposals for specialized courts is not as likely to improve decisions

as is more advanced economic training for judges or the use of court-appointed experts.

Appendix

Table A1
Selected Summary Statistics: Sample of Federal District Court Judges (N = 641 Cases)

	Cases (N)	Appealed (%)	Complex (%)	With LEC-Trained Judge (%)	With LEC-Trained Judge at Time of Decision (%)
Circuit:					
First	48	27.08	18.75	2.08	.00
Second	131	23.66	16.03	16.79	12.21
Third	75	22.67	20.00	16.00	14.67
Fourth	46	36.96	36.96	32.61	30.43
Fifth	30	33.33	20.00	13.33	3.33
Sixth	47	23.40	27.66	34.04	23.40
Seventh	47	17.02	27.66	34.04	25.53
Eighth	22	36.36	31.82	18.18	18.18
Ninth	60	35.00	28.33	20.00	16.67
Tenth	42	28.57	30.95	30.95	26.19
Eleventh	54	25.93	27.78	22.22	12.96
Federal	39	30.77	48.72	2.56	.00
Type of case:					
Merger	45	37.78	66.67	13.33	4.44
Monopolization	231	22.94	25.97	19.91	16.02
Robinson-Patman	31	12.90	29.03	12.90	9.68
Multiple claims	136	30.88	21.32	17.65	11.76
Price fixing or conspiracy	198	29.29	18.69	24.24	19.70
Plaintiff:					
Private	571	26.44	21.89	20.84	16.29
FTC	39	35.90	66.67	10.26	.00
DOJ	12	41.67	58.33	8.33	8.33
State attorney general	19	21.05	36.84	21.05	15.79
All data	641	27.15	25.74	19.97	15.13

Note. LEC = George Mason University Law and Economics Center; FTC = Federal Trade Commission; DOJ = Department of Justice.

Table A2

Instances per Case of Terms Identifying Economic Complexity: Sample of Federal District Court Judges (N = 641 Cases)

Term	Mean	SD	Max
Professor of economics	.022	.223	4
Econometrics	.050	.678	15
Economist	.198	.814	10
Economic analysis	.044	.246	2
Industrial organization	.011	.118	2
Game theory	.003	.056	1
Statistical evidence	.031	.242	4
Statistics	.231	1.094	12
Regression	.048	.689	14
Statistical significance	.009	.137	3
Expert witness	.201	.895	9
Expert report	.443	2.014	23
Economic expert	.101	.592	8
Economic report	.025	.594	15

Note. Min = 0 for all terms.

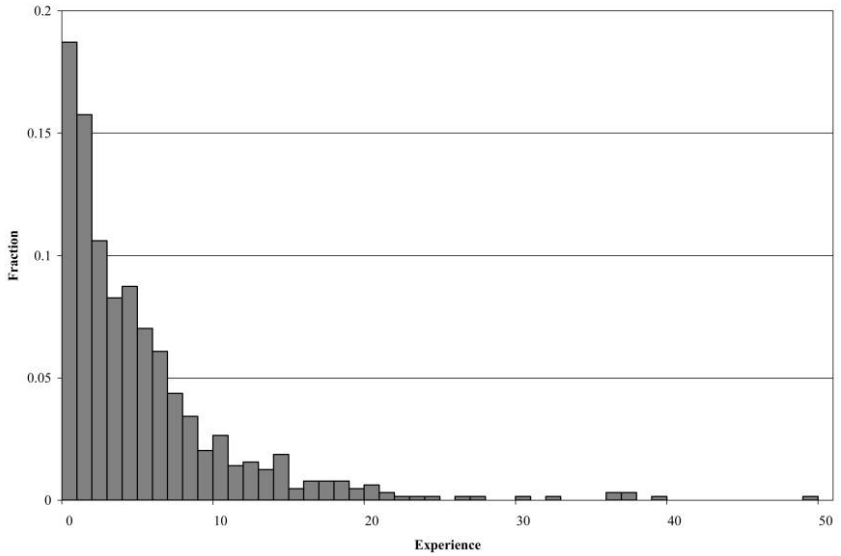

Figure A1. Distribution of federal district court judges' prior antitrust experience (N = 641 cases).

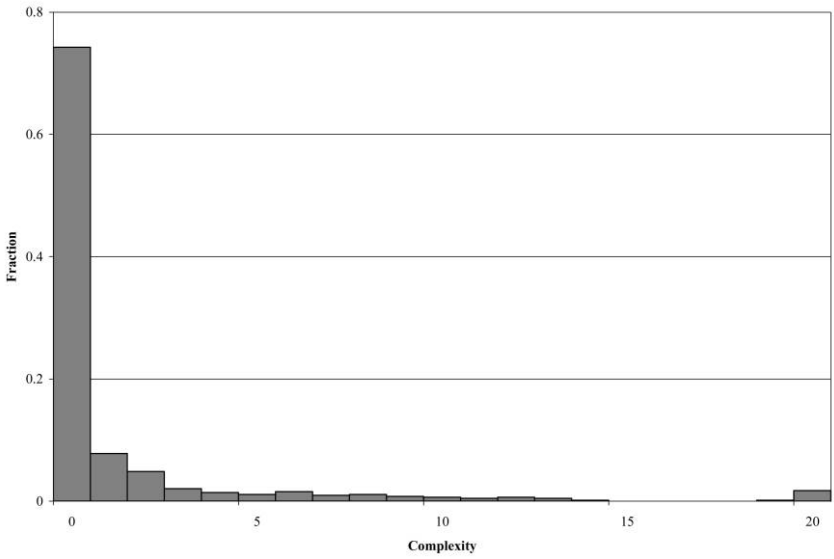

Figure A2. Distribution of economic complexity of 641 antitrust cases (sample of federal district court judges).

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