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GENERALIST JUDGES?  
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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

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 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 analysis supports the hypothesis that some antitrust cases are too complicated for generalist judges.

**Keywords:** Political Economy, Antitrust, Competition Law

**JEL Classifications:** A2, A11, K21, K41, L4

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## 1. Introduction

Antitrust analysis is becoming increasingly complex. Modern antitrust litigation and agency practice typically involves judicial evaluation of 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, 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 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.<sup>1</sup>

The effects-based structure of modern antitrust law requires economic expert testimony in large part because the Sherman Act’s broad language delegates to the judiciary the task of

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<sup>1</sup> This shift in federal courts toward incorporating economics into antitrust analysis was not sudden (Kaplow 1987). But there is no doubt that what Posner (2001) describes as a “revolutionary change in law” increased the demand for economic testimony concerning the competitive effects of business practices.

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) 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 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.”<sup>2</sup>

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/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. 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), 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).<sup>3</sup>

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

<sup>3</sup> Gallini (2002), for example, provides an excellent discussion of how the creation of specialized courts has impacted patent litigation.

The benefit of providing economic training to judges that 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) 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."

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 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.<sup>4</sup> Critics claim that the programs amount to junkets designed to influence

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<sup>4</sup> 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

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 that a judge's decision is appealed.<sup>5</sup> 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.<sup>6</sup> We also find that the decisions of judges with basic economic training are appealed in simple cases at significantly lower rates than 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 and/or econometric evidence. These results are robust across two datasets and different specifications that control for a judge's political ideology, level of antitrust experience, and post-graduate education—and other controls that include fixed effects for the type of plaintiff (for example, FTC or DOJ), type of case (for example, merger or monopolization), and the circuit in which the case is litigated.

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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).

<sup>5</sup> 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, suggesting poor performance by the district courts. See, for example Moore (2001), Chu (2001), see also Wagner (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.

<sup>6</sup> 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.

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—are in a relatively good position to assess 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 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.

## **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 between 1996 and 2006.<sup>7</sup> Our sample includes a total of 73 decisions on

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<sup>7</sup> 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")).

substantive antitrust issues by administrative law judges and 641 by Article III federal district court judges for a total of 714 decisions.<sup>8</sup>

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, state attorney general), and the date of decision. Our data also includes an indicator for whether at least one of the parties appealed the court's decision, as well as 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.<sup>9</sup> Additionally, we obtained data on both the post-graduate education and 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 proxy judicial antitrust experience with 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 their decision was made.

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<sup>8</sup> 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.

<sup>9</sup> Party of the appointment is available for each district court judge. Administrative law judges are not appointed by the President and thus political ideology data are unavailable for these judges. 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 et al. 1998, Ashenfelter et al. 1995).

We also collected data on other 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.

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 of the overall economic complexity of each case 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. 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: complex and simple. Simple cases generated opinions that did not use these terms at all, while a complex case was defined as one in which one or more of the terms in Table 1 were 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 basic economic training sessions at the LEC and the date they attended.<sup>10</sup> A total of 128 judges in our sample attended LEC

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<sup>10</sup> 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

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 with,<sup>11</sup> it is likely that any impact of training on appeals can be attributed to a judge 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 technical 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 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. FTC 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

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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 supplemented this information with more recently published financial disclosures and records available at the George Mason University Law and Economics Center.

<sup>11</sup> Programs consisted of a two 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). Butler (1999) provides a detailed account of the LEC programs.

rate of 91.78 percent. With respect to LEC training, there is significant variation between circuits. Zero 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 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 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 by the fact that about two-thirds of the FTC's cases were administrative law decisions (where appeals are 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, 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 appealing a specific initial court decision as a function of the economic complexity of the case, the judge's economic training, and a variety of other controls.<sup>12</sup>

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<sup>12</sup> As discussed below, we also examine models involving the probability of reversing a specific initial court decision, conditional on that decision being appealed.

Our primary rationale for using appeals as an indicator for whether the initial court “got the economics wrong“ 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 Act is whether the proposed transaction will “substantially lessen competition,” a test that has taken on an exclusively economic interpretation which 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 “got the economics wrong.”<sup>13</sup>

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<sup>13</sup> This feature of modern antitrust analysis is not limited to mergers. 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.”

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, 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.”

We also report results based on an alternative indicator of the quality of the initial court’s decision: a reversal by the appellate court.<sup>14</sup> Unfortunately, appellate reversals involve the decisions of a panel of multiple decision-makers, each with potentially different political ideologies and economic training. Personal 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. Additionally, reversals are necessarily conditioned on the decision 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 “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

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<sup>14</sup> This measure is sometimes used in the literature on patent litigation; see Gallini (2002). More recently, Duso et al. (2007) used an event study methodology to examine the impact of EU merger decisions on stock performance

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.<sup>15</sup> 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 where 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 *both* the impact of 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 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 cases that are “close calls.”<sup>16</sup> Likewise, some cases may show up in the data as “not appealed” because they are settled prior to an appellate

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<sup>15</sup> 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.

<sup>16</sup> See, for instance, Block et al. (1981), Carlton (2008), and Priest and Klein (1984).

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 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 in complex and simple decisions, as well as decisions by trained and untrained judges. Table 3 reports results. Economically complex 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 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 attended economic training programs prior to authoring an antitrust decision have that opinion appealed only 22.7 percent of the time, compared to 35.5 percent for untrained judges.

While we prefer comparisons based on appeals rather than reversals, we note that similar results obtain when one uses reversals. Conditional on a decision being appealed, opinions authored by trained judges are reversed only 13.6 percent of the time, while their untrained

counterparts' decisions are reversed by a higher court 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.<sup>17</sup>

These means tests suggest that economic complexity and basic economic training 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 involving economic/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

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<sup>17</sup> While these results are similar in direction and magnitude to the results reported in Table 3 based on appeals, 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.

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 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 basic economic training. The results are similar in magnitude and significance to the mean tests 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. Additionally, 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, dummy variables to control for the type of case, the type of plaintiff, and the circuit in which the case was litigated. In all 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.<sup>18</sup>

#### 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

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<sup>18</sup> 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 were not statistically significant in all specifications.

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 that the rates at which the decisions of federal district judges are appealed (to federal appellate courts). While the high appeal rate may be driven by the higher 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.<sup>19</sup>

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.<sup>20</sup> 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 (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 pre-disposed to economics and business oriented thinking than their untrained counterparts. If that were so, training might be capturing some pre-existing differences in 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

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<sup>19</sup> In a sample of Sherman Act disputes litigated before 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.”

<sup>20</sup> Tables A1 and A2 in the appendix provide summary statistics for the complexity measures and summary statistics for these data, while Figures A1 and A2 display the corresponding distributions of judicial experience and economic complexity.

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 does not appear that the training effect is an artifact of selection into these programs by Republican judges. 321 of the opinions in our federal court database 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 impact 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.<sup>21</sup>

#### 4.6. *Robustness Check: Judicial Training or Judge 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 datasets, 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.

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<sup>21</sup> We also ran specifications allowing for the possibility that basic economic training impacts Republican and Democrat judges differently, and found that the effects of training are similar for Republican and Democratic judges. These results are consistent with Moore (2001), who 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.

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 through 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 post-graduate education of the district court judges in our sample. This measure, *QUALITY*, is a dummy variable that equals one if the judge holds a 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 importantly, 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 for judges with basic economic training is 9.5 percent lower in simple cases than their untrained counterparts.

## **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 datasets, 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 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 there are likely to be reasonable fact-finding disputes and thus, more room to persuade an appellate court that a reversible error has been 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 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 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 more *advanced* economic training for judges or the use of court appointed experts.

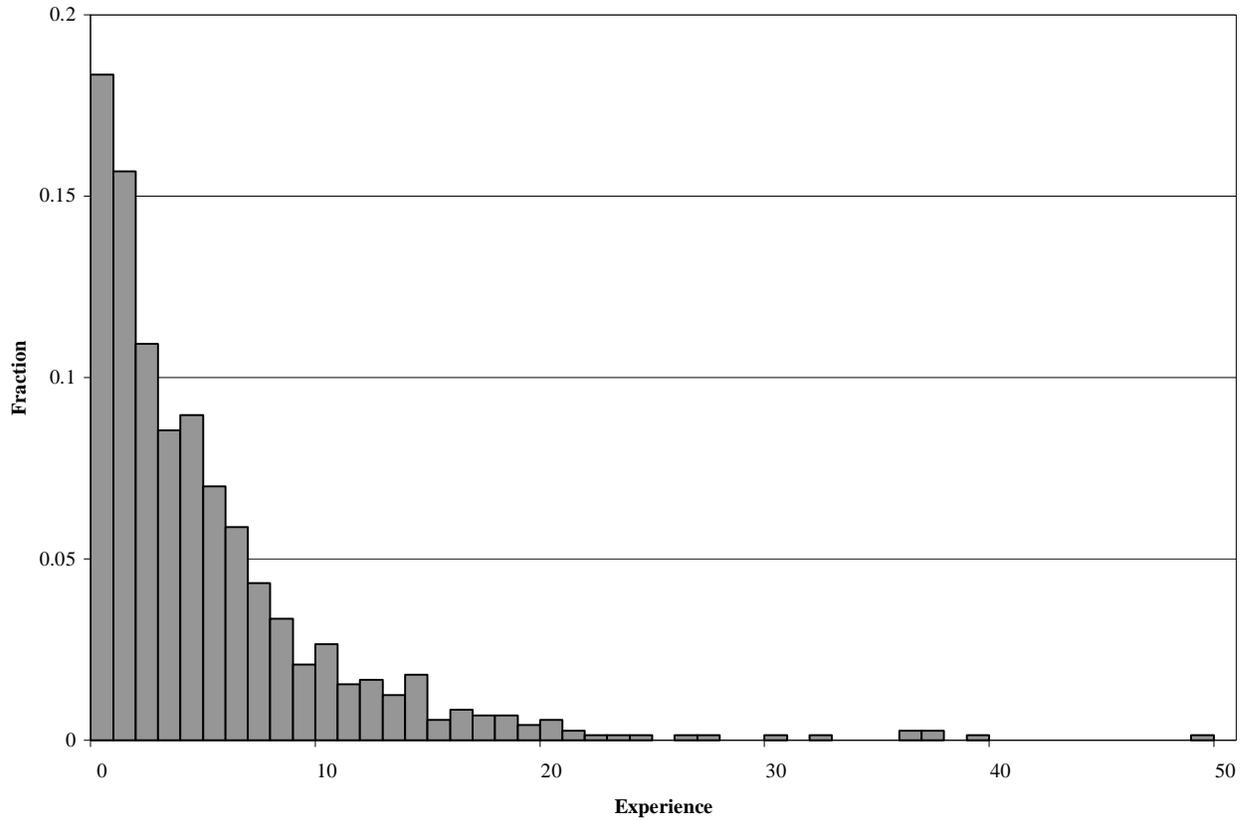
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**FIGURE 1.**

**Distribution of Judges' Prior Antitrust Experience, 714 Antitrust Cases**



**FIGURE 2.**

**Distribution of Economic Complexity, 714 Antitrust Cases**

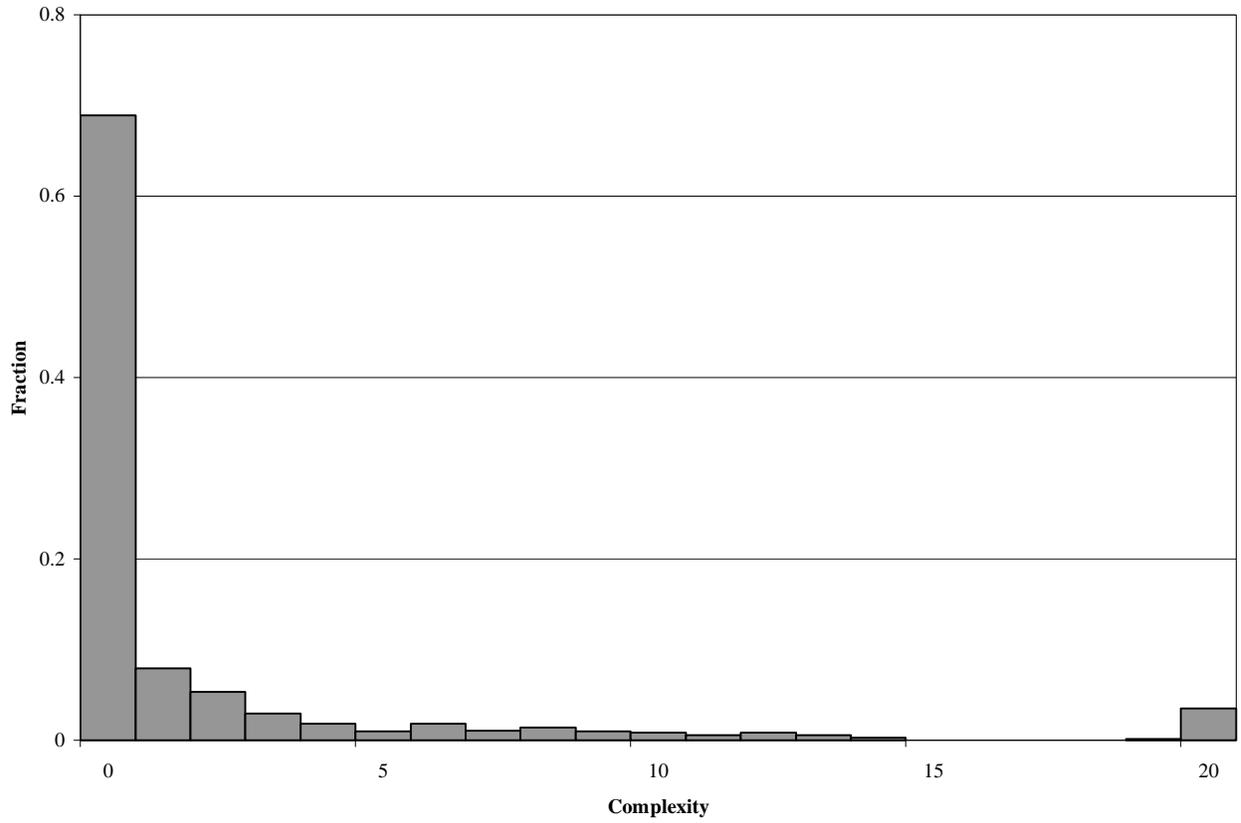


TABLE 1.

Terms Used to Identify Economic Complexity,  
714 Antitrust Cases

Term	Times Appearing Per Case			
	Mean	Std. Dev.	Min	Max
Professor of Economics	0.049	0.346	0	5
Econometrics	0.052	0.652	0	15
Economist	0.387	1.637	0	26
Economic Analysis	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

TABLE 2.  
Selected Summary Statistics, 714 Antitrust Cases

Identifier	Number of Cases	Percent Appealed (%)	Percent Complex (%)	Percent with Trained Judge (%)	Percent with Judge Trained 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 Admin Litigation	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 3.

Appeals: Impact of Economic Complexity and Basic Economic Training  
Two-sample t-test with equal variances

Variable	N	Mean	Std. Err
Complex Cases	222	0.505	0.034
Simple Cases	492	0.262	0.020
Combined	714	0.338	0.018
Difference		0.242	0.037
<i>t</i> -Statistic	6.51		
Trained Judges	97	0.227	0.043
Untrained Judges	617	0.355	0.019
Combined	714	0.338	0.018
Difference		-0.128	0.051
<i>t</i> -Statistic	-2.49		

TABLE 4.  
Baseline Probit Regressions Reporting Marginal Effect on Appeal Rate,  
714 Antitrust Cases

	(1)	(2)	(3)	(4)	(5)	(6)
COMPLEX	0.236** (6.05)	0.227** (5.54)	0.152** (3.52)	0.166** (3.72)	0.131** (2.79)	0.107* (2.17)
TRAINED	-0.107* (2.06)					
COMPLEX_TRAINED		-0.053 (0.51)	0.072 (0.64)	0.061 (0.55)	0.093 (0.83)	0.088 (0.73)
SIMPLE_TRAINED		-0.125* (2.06)	-0.105 <sup>+</sup> (1.69)	-0.110 <sup>+</sup> (1.76)	-0.097 (1.54)	-0.108 <sup>+</sup> (1.68)
YEAR			-0.021** (7.13)	-0.021** (6.43)	-0.015** (3.56)	-0.013** (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

<sup>+</sup> significant at 10%

\* significant at 5%

\*\* significant at 1%

TABLE 5.  
 Probit Regressions Reporting Marginal Effect on Appeal Rate,  
 Controls for Antitrust Experience of Judges (714 Antitrust Cases)

	(1)	(2)	(3)	(4)	(5)	(6)
COMPLEX	0.235 ** (6.03)	0.227 ** (5.52)	0.152 ** (3.52)	0.166 ** (3.71)	0.13 ** (2.78)	0.107 * (2.17)
TRAINED	-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)
COMPLEX_TRAINED		-0.047 (0.46)	0.075 (0.66)	0.065 (0.59)	0.099 (0.88)	0.090 (0.75)
SIMPLE_TRAINED		-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)	-0.013 ** (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

TABLE 6.  
 Probit Regressions Reporting Marginal Effect on Appeal Rate,  
 Sample of Federal District Court Judges, (641 Antitrust Cases)

	(1)	(2)	(3)	(4)
COMPLEX	0.096* (2.06)	0.096* (2.06)	0.096* (2.06)	0.096* (2.06)
COMPLEX_TRAINED	0.080 (0.73)	0.082 (0.75)	0.080 (0.73)	0.076 (0.70)
SIMPLE_TRAINED	-0.095+ (1.69)	-0.094+ (1.66)	-0.094+ (1.66)	-0.095+ (1.67)
YEAR	-0.010* (2.11)	-0.010* (2.11)	-0.010* (2.11)	-0.010* (2.11)
EXPERIENCE		-0.001 (0.2)	-0.001 (0.22)	-0.001 (0.17)
PARTY			0.005 (0.14)	0.001 (0.02)
QUALITY				-0.061 (0.78)
FIXED EFFECTS:				
Type of Case	Yes	Yes	Yes	Yes
Plaintiff	Yes	Yes	Yes	Yes
Circuit	Yes	Yes	Yes	Yes

Robust z statistics in parentheses

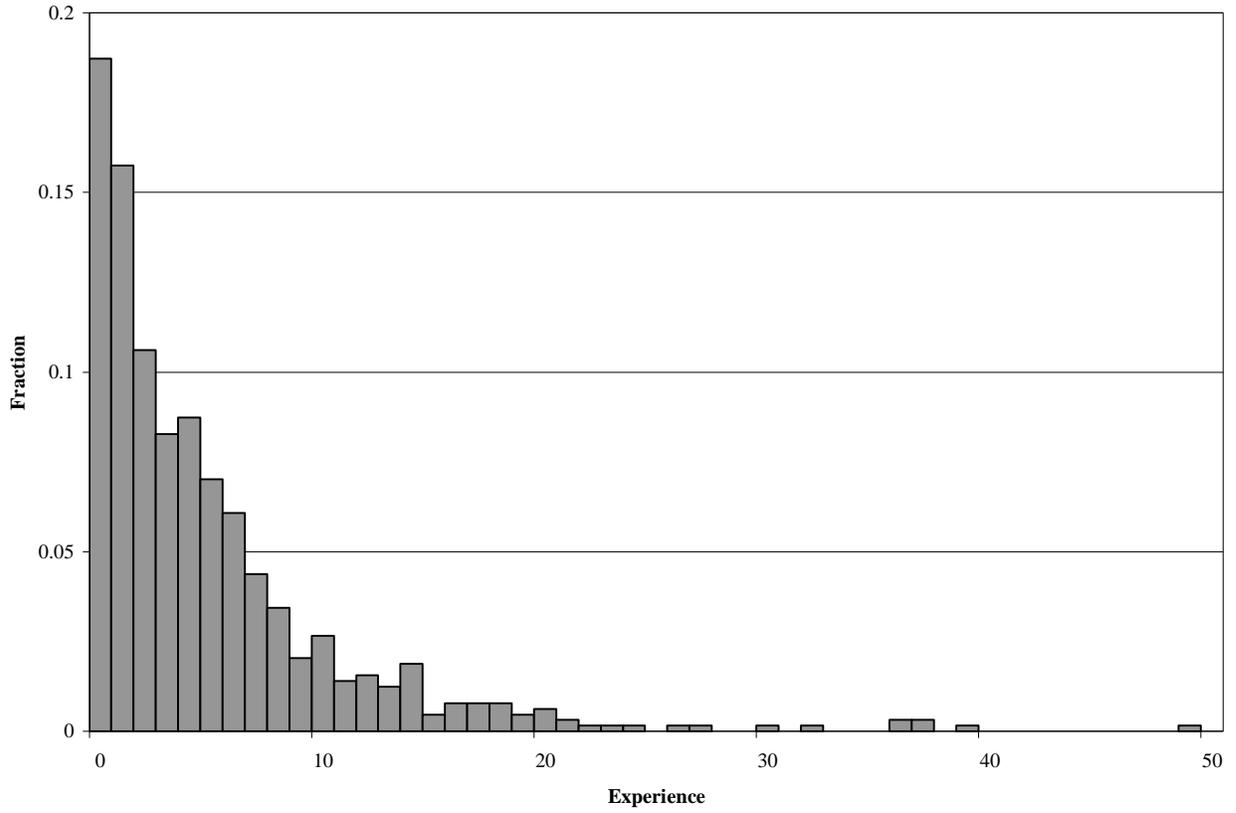
+ significant at 10%

\* significant at 5%

\*\* significant at 1%

**FIGURE A1.**

**Distribution of Prior Antitrust Experience, 641 Antitrust Cases  
(Sample of Federal District Court Judges)**



**FIGURE A2.**

**Distribution of Economic Complexity, 641 Antitrust Cases  
(Sample of Federal District Court Judges)**

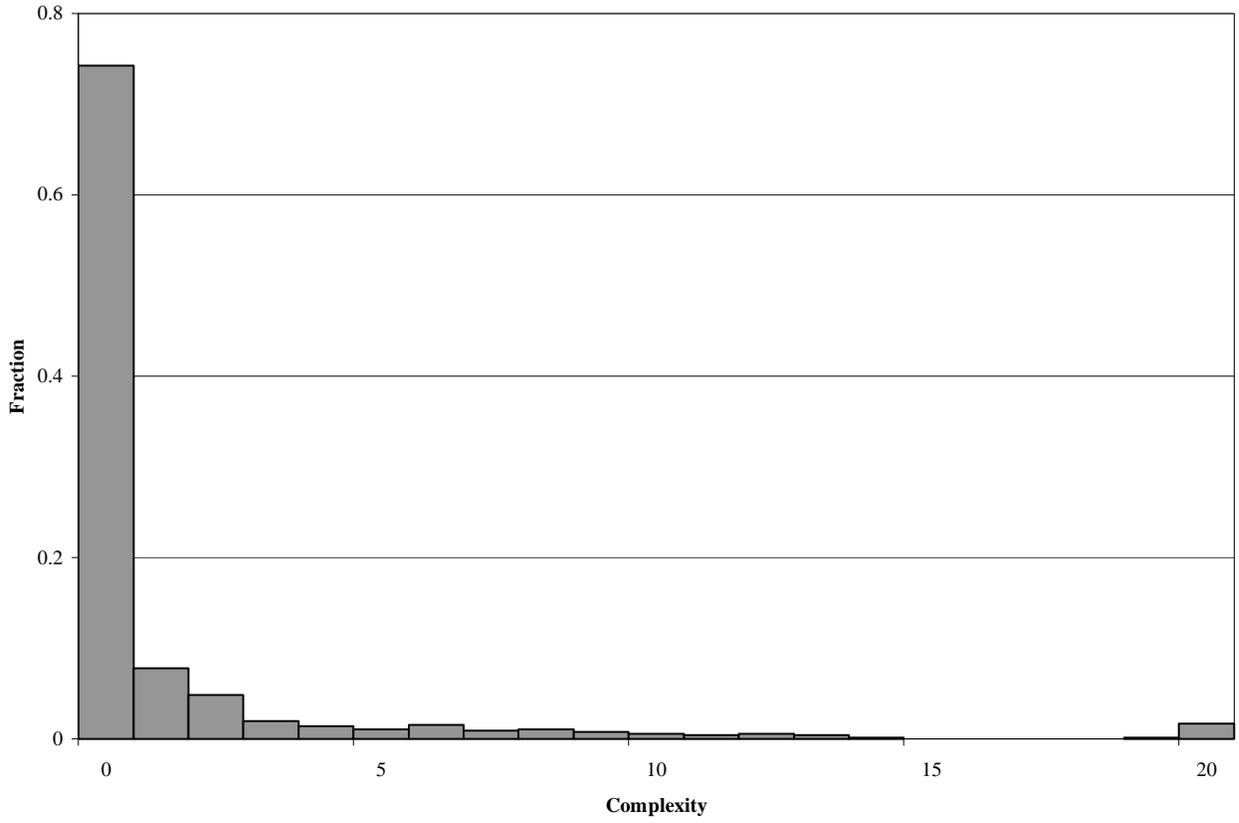


TABLE A1.

Terms Used to Identify Economic Complexity,  
641 Antitrust Cases  
(Sample of Federal District Court Judges)

Term	Times Appearing Per Case			
	Mean	Std. Dev.	Min	Max
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Econometrics	0.05	0.678	0	15
Economist	0.198	0.814	0	10
Economic Analysis	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

TABLE A2.  
Selected Summary Statistics, 641 Antitrust Cases  
(Sample of Federal District Court Judges)

Identifier	Number of Cases	Percent Appealed (%)	Percent Complex (%)	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
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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