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).

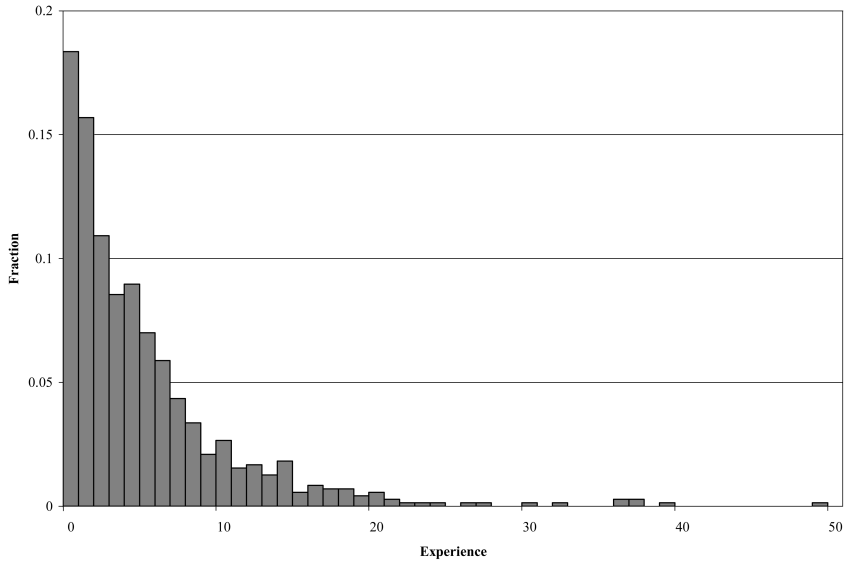

Figure 1. Distribution of judges' prior antitrust experience ($N = 714$ cases)

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.

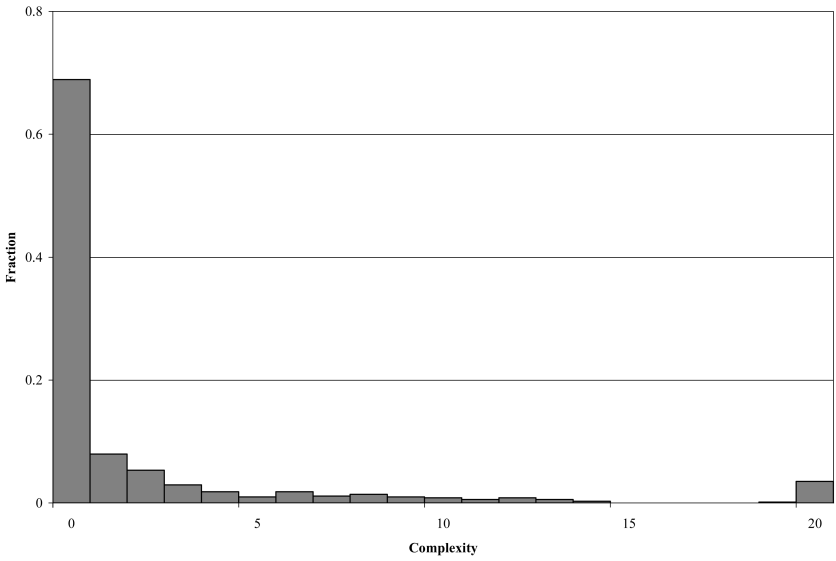

Figure 2. Distribution of economic complexity ($N = 714$ cases)

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.

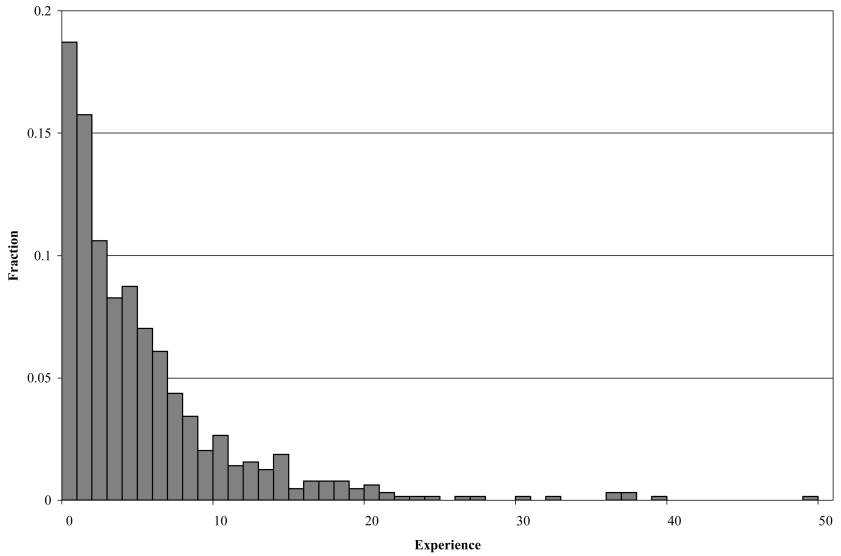

Figure A1. Distribution of federal district court judges' prior antitrust experience ($N = 641$ cases).

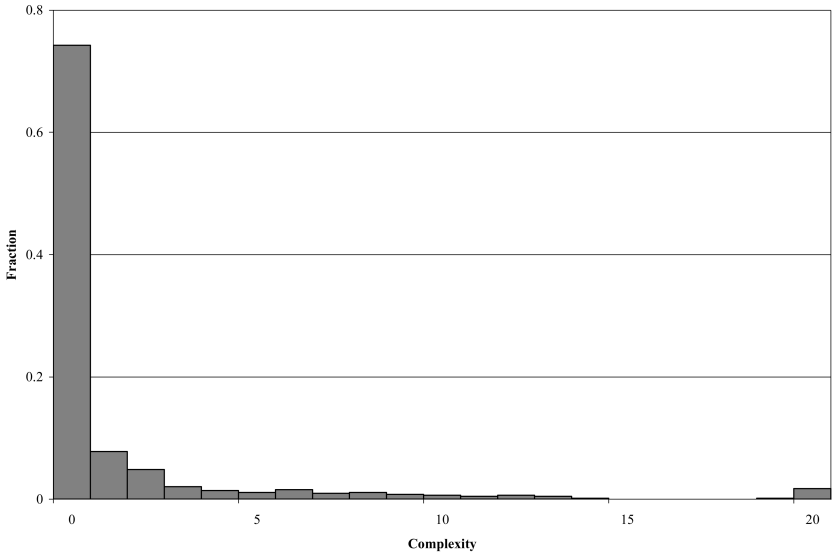

Figure A2. Distribution of economic complexity of 641 antitrust cases (sample of federal district court judges).

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Do expert agencies outperform generalist judges? Some preliminary evidence from the Federal Trade Commission

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In the context of US antitrust law, many commentators have recently called for an expansion of the Federal Trade Commission's (FTC's) adjudicatory decision-making authority pursuant to Section 5 of the FTC Act, increased rulemaking, and carving out exceptions for the agency from increased burdens of production facing private plaintiffs. These claims are often expressly grounded in the assertion that expert agencies generate higher quality decisions than federal district court judges. We call this assertion the expertise hypothesis and attempt to test it. The relevant question is whether the expert inputs available to generalist federal district court judges translate to higher quality outputs and better performance than the Commission produces in its role as an adjudicatory decision-maker. While many appear to assume agencies have courts beat on this margin, to our knowledge, this oft-cited reason to increase the discretion of agencies and the deference afforded them by reviewing courts is void of empirical support. Contrary to the expertise hypothesis, we find evidence suggesting the Commission does not perform as well as generalist judges in its adjudicatory antitrust decision-making role. Furthermore, while the available evidence is more limited, there is no clear evidence the Commission adds significant incremental value to the administrative law judge decisions it reviews. In light of these findings, we conclude there is little empirical basis for the various proposals to expand agency authority and deference to agency decisions. More generally, our results highlight the need for research on the relationship between institutional design and agency expertise in the antitrust context.

JEL codes: K21, K23, L40 and L51

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I have never heard anyone argue that [the FTC] has displayed superior expertise to the courts when it comes to deciding antitrust cases.[1]

Introduction

Governments and scholars have been increasingly willing to evaluate the performance of their competition and consumer protection agencies worldwide. Within the last few years alone, China,[2] India,[3] Brazil,[4] and the European Union[5] have undergone substantial institutional restructuring aimed at improving agency performance. At the same time, antitrust scholars have recently increased their focus upon the structure of competition enforcement institutions, giving rise to a burgeoning body of scholarly work.[6]

One critical dimension of the institutional design research agenda is how decision-making ought to be delegated between courts and agencies to best achieve the goals of competition policy. While antitrust scholars have long focused upon the importance of errors and the design of substantive legal rules to minimize error costs, relatively little attention has been paid to the myriad ways in which institutional design in general, and decision-making within expert competition agencies specifically, can improve the quality of these institutions. The organization of leadership and staff within a competition agency affects the structure of the decision-making process it undertakes. For example, the number of economists, the quality of their inputs, and the nature of their authority within a competition agency could affect agency enforcement decisions.[7] Indeed, throughout its history the Federal Trade Commission (FTC) has experimented with various organizational designs in hopes of incorporating the optimal level of economic influence to achieve the agency's goals.[8] Similarly, the European Commission (EC) has responded to calls for more coherent economic analysis through the addition of a team of PhD economists to aid the EC's Competition Directorate in improving its decision-making quality.[9]

[1] Richard A Posner, *Antitrust Law* (2nd edn, Chicago U Press 2001) 280.

[2] See Eleanor M Fox, 'Antitrust and Institutions: Design and Change' (2010) 41 Loy U Chi LJ 473, 476 (describing China's institutional design choices for enforcing its first comprehensive competition law in 2008); see also Daniel A Crane, *The Institutional Structure of Antitrust Enforcement* (Oxford U Press 2011) 211.

[3] See Crane (ibid) 211.

[4] ibid.

[5] See Luke M Froeb and others, 'The Economics of Organizing Economists' (2009) 76 Antitrust LJ 569, 571.

[6] See, eg Crane (n 2); Daniel A Crane, 'Technocracy and Antitrust' (2008) 86 Tex L Rev 1159; Fox (n 2); Froeb and others (n 5); Michael S Gal, 'When the Going Gets Tight: Institutional Solutions When Antitrust Enforcement Resources are Scarce' (2010) 41 Loy U Chi L Rev 417; Calvin S Goldman, QC and Navin Joneja, 'The Institutional Design of Canadian Competition Law: The Evolving Role of the Commissioner' (2010) 41 Loy U Chi L Rev 535; William E Kovacic, 'Lessons of Competition Policy Reform in Transition Economies for U.S. Antitrust Policy' (2000) 74 St John's L Rev 361.

[7] ibid.

[8] ibid.

[9] ibid.

The institutional design literature has identified a number of potential factors influencing decision-making, including whether the agency should be led by a single director or a collegiate body,[10] the experience held by agency heads,[11] the structure of enforcement,[12] and methods of ensuring transparency in agency decision-making.[13] There is no debate that theoretical potential for superior agency performance lies in its ability to harness its expertise. In practice, however, there is also little doubt that the observed design and structure of competition agencies in the USA bears little resemblance to the theoretical optimum. Holding aside the obvious and oft-discussed inefficiencies of multiple overlapping competition agencies, there appear to be other fundamental structural impediments to optimal agency performance.

To take but one example, former FTC Chairman William Kovacic has written at length about the disappointing overall quality of appointments of FTC commissioners.[14] While Congress envisioned a Commission comprising lawyers, business managers, and economists with superior achievements and substantial, diverse experience,[15] what it got was—in no small part due to political interference[16]—a history and pattern of appointments evidencing a systematic failure to meet those expectations.[17] Obviously, this is not to say that those appointed to lead the FTC are not talented professionals; it simply means the historic composition of the Commission has failed to encompass the qualities necessary to make it the leading authority in US antitrust law.[18]

Predicate to the question of precisely how to design competition agencies to improve their performance is the issue of precisely what locus of authority should be allocated to the expert agency. The answer to that question lies at the heart of many antitrust debates. Dissatisfied with recent changes in Sherman Act jurisprudence, some commentators have called for a shift in responsibility for shaping antitrust law from the courts to the agencies, reasserting the original vision of the FTC as an expert agency.[19]

[10] In the USA, the FTC is led by a five-member commission, whereas the Department of Justice's (DOJ's) Antitrust Division is led by a single Assistant Attorney General.
[11] Kovacic (n 6) 364–69.
[12] ibid 374–83.
[13] ibid 383–91.
[14] William E Kovacic, 'The Quality of Appointments and the Capability of the Federal Trade Commission' (1997) 49 Admin L Rev 915, 951.
[15] ibid 919.
[16] ibid 939.
[17] ibid 934–35.
[18] ibid 930.
[19] Kovacic writes:

> Congress assumed that: (1) presidents would appoint, and Congress would confirm, commissioners who were true experts in disciplines relevant to forming competition policy; (2) the agency's leadership would fully exploit the FTC's institutional potential to synthesize economic and legal learning; (3) federal judges would defer to the FTC as it designed new rules of business conduct; and (4) the FTC's analysis and reputation would command respect from business officials and their advisors.

A recurring and related issue in the debate over an expanded role for enforcement agencies—especially the FTC—in antitrust decision-making is whether Article III courts are sufficiently equipped to handle complex antitrust cases.[20] Evidence indicates that complex antitrust cases involve economic analysis that is sometimes too complicated for courts to consistently decide properly.[21] This is due in large part to the fact that courts are unable (some suggest unwilling[22]) to incorporate expert economic analysis into their antitrust decisions. Some commentators have argued, based upon courts' imperfect decision-making abilities, that the FTC should have greater decision-making authority to offset courts' shortcomings in understanding the complex economic analysis required to accurately assess modern antitrust issues.[23]

Which institution is better equipped to analyse complex modern antitrust cases? The debate is occasionally framed in unfair terms. There is no doubt the agency comprises antitrust and economic experts well equipped to analyse all modes of business dealings; in this sense, agencies certainly have greater economic expertise than the Article III judges as a general rule. But neither the expert economists in the Bureau of Economics nor the Bureau of Competition's lawyers make decisions for the agency. Both ultimately provide inputs to the five-person Commission in a complex decision-making process. Economic and legal expertise are not the only inputs. Commissioners are political appointees that may or may not begin their terms with substantial antitrust experience.[24] As the ultimate decision-makers in administrative litigation, the Commission is the body to which relevant analytical information must be transmitted. Comparing the expert Commission staff to combined expertise of the Article III judge and his law clerks is not the appropriate comparison; it also misses the point.[25] The issue remains whether the expert

ibid 920; see eg C Scott Hemphill, 'An Aggregate Approach to Antitrust: Using New Data and Rulemaking to Preserve Drug Competition' (2009) 109 Colum L Rev 629 (endorsing nearly unprecedented antitrust rulemaking based upon the FTC's information-gathering authority and purported expertise); J Thomas Rosch, Comm'r, Fed Trade Comm'n, The Great Doctrinal Debate: Under What Circumstances is Section 5 Superior to Section 2? (27 January 2011) (advocating for enlarging the scope of conduct falling within s 5 of the FTC Act).

[20] See eg ibid 673–74 (questioning the ability of courts to identify anticompetitive conduct in ambiguous circumstances and finding the FTC 'essentially by definition, is less likely to make mistakes identifying' such conduct).

[21] See Michael D Baye and Joshua D Wright, 'Is Antitrust Too Complicated for Generalist Judges? The Impact of Economic Complexity and Judicial Training on Appeals' (2011) 54 JL Econ 1.

[22] See eg Hemphill (n 19) 674–75 ('In a key case brought by the FTC, the appeals court largely ignored the analysis employed by the agency, granted essentially no deference to its findings of fact, and indeed berated the Agency for failing to follow the appeals court's earlier rule. For the most part, courts have also ignored the results of the FTC's extensive 2002 study and its subsequent annual summary updates, as well as its amicus recommendations based on this data.'). But see Posner (n 1) 277 ('American courts are accustomed to dealing with technical questions . . . by having technical experts present evidence at trial that the judge and jury . . . is expected somehow to assimilate. This system does not work so badly as its critics maintain . . . .').

[23] Rosch (n 19) 4.

[24] ibid. Kovacic (n 14) 950.

[25] Article III judges also receive economic inputs in the way of expert testimony through the adversarial process. On the tradeoffs between adversarial and inquisitorial regimes of judicial decision-making, see Luke M

inputs available to the Commission's decision-makers manifest themselves in the context of administrative decision-making compared to generalist judges.

This article focuses upon a narrow, but important, aspect of FTC activity: agency decision-making in administrative litigation. Beyond litigation, the FTC is also vested with information-gathering, reporting, and advisory functions.[26] Recent arguments that the antitrust agencies should be permitted to expand its litigation authority, however, suggest closer evaluation of this particular agency function is warranted. There is a dearth of empirical evidence to support or oppose an argument for expanding the FTC's authority, or more generally, increasing the enforcement powers delegated to competition agencies. This article seeks to fill that gap by conducting a comparative analysis of Commission and Article III judicial decisions to test what we refer to as the 'expertise hypothesis', the assumption that Commission decision-making is superior due to its greater expertise. The answer to this question implicates a number of critical issues concerning institutional design and the optimal roles of administrative agencies and courts in modern antitrust enforcement, including the proper scope of Section 5 of the FTC Act, FTC rulemaking, and the appropriate level of deference afforded to the FTC as plaintiff in federal court.

The expertise hypothesis: courts versus agencies

In establishing an administrative agency, Congress evidences its decision to delegate resolution of given issues to a specialized body that is presumptively superior to the other branches of the federal government in interpreting laws and guiding policies. Congress may choose to delegate initial adjudicative authority to the agency, or it may leave the authority to the courts. A decision to vest an agency with administrative adjudicatory power is an indication that Congress believes the agency is better equipped than courts to resolve issues in which it specializes.

The primary justification for empowering agencies to adjudicate is that they possess the expertise to resolve technical questions more efficiently than if those questions were left to the judicial system.[27] Accordingly, for an agency to fulfil this purpose, it must systematically outperform courts in adjudicating legal issues in which it specializes. Determination of whether this hypothesis is

Froeb and Bruce H Kobayashi, 'Evidence Production in Adversarial vs. Inquisitorial Regimes' (2001) 70 Econ Letters 267.

[26] Marc Winerman, 'The Origins of the FTC: Concentration, Cooperation, Control, and Competition' (2003) 71 Antitrust LJ 1, 93, 97.

[27] Matthew C Stephenson, 'Legislative Allocation of Delegated Power: Uncertainty, Risk, and the Choice Between Agencies and Courts' (2006) 119 Harv L Rev 1035, 2042. Other justifications for delegation to agencies over courts have been proffered. For discussions of alternative justifications, see generally Margaret H Lemos, 'The Consequences of Congress's Choice of Delegate: Judicial and Agency Interpretations of Title VII' (2010) 63 Vand L Rev 363, 372–80; Stephenson, this note, at 1042–49.

true is ultimately an empirical question; however, no studies have been conducted to test the hypothesis.

There are several methods by which scholars have attempted to measure judicial performance. Citation-based studies appear commonly;[28] however, they have often been employed to compare the performance and reputation of individual judges and are therefore unsuitable for our purpose, which focuses upon measuring the performance of institutions rather than the individuals comprising them. Other studies use metrics that hew more closely to the performance we seek to measure in this article. These metrics seek to measure the efficiency and speed of judicial decision-making.[29] Some of these studies discuss the performance of specialized courts as compared to courts of general jurisdiction. For example, Jonathan Nash and Rafael Pardo have sought to compare the quality of decisions made by appellate bankruptcy judges with those of federal district courts.[30] They compared reversal rates of bankruptcy appellate panels (BAPs) to the reversal rates of bankruptcy decisions in federal district courts,[31] and they concluded courts of appeals place more weight upon the decisions of BAPs.[32] Jay P Kesan and Gwendolyn G Bell have sought to measure judicial performance in the context of patent cases.[33] They concluded increased experience with patent law is associated with increased accuracy of patent decisions.[34] Kesan and Bell concluded their results provided 'a real but modest case' for establishing a specialized patent court comprising experts in the field.[35]

Very little has been done to measure the performance of administrative agencies. Evaluations have been conducted both qualitatively and quantitatively. Richard Posner, in a 1969 article, argued that FTC hearing examiners were less efficient than federal district court judges in part due to a misconception about the virtues and vices of federal regulation.[36] Kovacic

[28] See eg Gregory A Caldeira, 'On the Reputation of State Supreme Courts' (1983) 5 Pol Behav 83, 83; Lawrence Friedman and others, 'State Supreme Courts: A Century of Style and Citation' (1981) 33 Stan L Rev 773, 773; William M Landes, Lawrence Lessig and Michael E Solimine, 'Judicial Influence: A Citation Analysis of Federal Courts of Appeals Judges' (1998) 27 J Legal Stud 271, 272–76; Rodney L Mott, 'Judicial Influence' (1936) 30 Amer Pol Sci Rev 295, 295.

[29] See eg Arie Y Lewin, Richard C Morey and Thomas J Cook, 'Evaluating the Administrative Efficiency of Courts' (1982) 10 Int'l J Mgmt Sci 401 (1982); Robert K Christensen and John Szmer, *Examining the Efficiency of the U.S. Court of Appeals: Pathologies and Prescriptions*, 1 (IEL Paper in Comparative Analysis of Institutions, Economics & Law No 4, 2011) <http://polis.unipmn.it/pubbl/RePEc/uca/ucaiel/iel004.pdf> accessed 2 December 2012.

[30] Jonathan Nash and Rafael Pardo, 'An Empirical Investigation into Appellate Structure and the Perceived Quality of Appellate Review' (2008) 61 Vand L Rev 1745.

[31] Parties appealing from decisions of bankruptcy courts have the option to seek review in a district court or a BAP. ibid 1746. BAPs are assumed to have more expertise in bankruptcy law than district courts. ibid 1759. Appeals from both district courts and BAPs are taken to federal courts of appeals. ibid 1747.

[32] ibid 1807.

[33] Jay P Kesan and Gwendolyn G Ball, 'Judicial Experience and the Efficiency and Accuracy of Patent Adjudication: An Empirical Analysis of the Case for a Specialized Patent Trial Court' (2011) 24 Harv JL & Tech 393.

[34] ibid 437.

[35] ibid 444.

[36] Richard Posner, 'The Federal Trade Commission' (1969) 37 U Chi L Rev 47.

has argued that appointed FTC commissioners have systematically failed to meet the qualifications Congress expected when establishing the FTC.[37] Such failure has contributed to the perception that the FTC is underqualified when it comes to specialized antitrust decision-making.[38] In a quantitative study, Gene A Brewer sought to measure agency performance through survey results from over eight thousand federal government employees.[39] As a proxy for overall agency performance, the study was designed to measure management practices within federal agencies; Brewer concluded management is deficient in federal agencies and efforts should be made to improve performance by retaining high-quality management.[40]

Although some work has been done to measure both judicial and agency performance, none has been done to examine whether the expertise hypothesis is true. The FTC provides an apt subject to test because of recent calls for expansion of its authority and the accompanying debate over whether to permit it. We discuss the expertise hypothesis as it relates to the FTC in the next section.

The FTC and the expertise hypothesis

Congress envisioned the FTC as an agency with superior business and economic knowledge that could use its expertise to influence competition policy and guide the public in its business endeavours. Congress vested the FTC with exclusive enforcement authority of Section 5 of the FTC Act, which prohibits 'unfair methods of competition'. This language indicates congressional intent for Section 5 to apply more broadly than the Sherman Antitrust Act and the Clayton Antitrust Act.

One of the FTC's many responsibilities is to adjudicate the legality of conduct it believes violates Section 5. In theory, Section 5 was to encompass conduct that falls outside the scope of the Sherman and Clayton Acts but that nevertheless harms competition and consumers. In practice, the FTC's Section 5 authority has historically been held applicable to a very narrow range of conduct in competition cases.[41] Though the FTC has attempted to enforce a more expansive version of Section 5, it has, in large part, failed in its endeavours.[42] The legacy of the FTC's Section 5 competition enforcement

[37] Kovacic (n 14).
[38] ibid 951.
[39] Gene A Brewer, 'In the Eye of the Storm: Frontline Supervisors and Federal Agency Performance' (2005) 15 J Pol Admin & Res Theory 505.
[40] ibid 520.
[41] This authority is to be distinguished from the FTC's consumer protection authority under s 5, through which it has promulgated numerous rules and brought a broad array of administrative cases.
[42] William E Kovacic and Marc Winerman, 'Competition Policy and the Application of Section 5 of the Federal Trade Commission Act' (2010) 76 Antitrust LJ 929, 933–34 ('One would be hard-pressed to come up with a list of ten adjudicated decisions that involved the FTC's application of Section 5 in which the FTC prevailed and the case can be said to have had a notable impact, either in terms of doctrine or economic effects.').

agenda is underwhelming at best. Its most notable attempt to influence antitrust doctrine occurred in the early 1980s when it brought and lost three cases based upon Section 5 theories.[43]

The debate concerning the desirability of expanding Section 5's scope returns to the fundamental issue of whether the Commission's expertise renders it better situated than generalist courts to decide modern antitrust cases. Proponents of an expanded Section 5 contend Congress expected the FTC to possess the expertise necessary to overcome the Sherman Act's flaws via Section 5 enforcement.[44] A broad authority would allow the Commission to use its expertise to prohibit conduct having ambiguous competitive effects and to permit the FTC to fill the gaps between the Sherman Act and incipient anticompetitive conduct.

Additionally, cases decided under Section 5 are more susceptible to judicial adoption. 'The entire reason that agency interpretations receive any deference is that specialized agencies are presumed to have greater subject matter expertise than generalist judges.'[45] Sole enforcement authority allows courts to give more deference to the Commission on appeal. The FTC is permitted to use its superior knowledge of competitive conditions to enforce Section 5, and courts are 'more likely to trust an agency's prediction based on its superior familiarity with the type of conduct at issue.'[46]

The argument against judicial resolution of complex or novel antitrust cases can be summarized simply: 'The problem [with generalist federal judges] is that they're not required to be experts in antitrust law.'[47] Because Article III judges handle a wide array of cases, they are provided with little opportunity to refine their antitrust knowledge. Antitrust cases comprise only a small percentage of a district court docket. In contrast, the Commission was created to specialize in competition law and considers competition issues on a regular basis.

There is evidence supporting the view that antitrust cases involving complex economic issues are too difficult for Article III judges to analyse properly.[48] Professors Michael Baye and Joshua Wright recently conducted a study of antitrust cases in Article III courts, and they concluded that, even where judges have some economic training, they are no better at deciding antitrust cases

[43] See *EI du Pont de Nemours and Co v FTC*, 729 F 2d 128 (2d Cir 1984); *Official Airline Guides, Inc v FTC*, 630 F 2d 920 (2d Cir 1980); *Boise Cascade Corp v FTC*, 637 F 2d 573 (9th Cir 1980); see also Kovacic and Winerman (n 42) 942 (explaining that in each case 'the court found that the Commission had failed to make a compelling case for condemning the conduct in question').

[44] Rosch (n 19) 14 ('Congress enacted Section 5 of the FTC Act at the same time it created the Federal Trade Commission because it anticipated that the FTC would serve as an expert appellate body in Section 5 cases.').

[45] Tad Lipsky, Workshop on Section 5 of the FTC Act as a Competition Statute (17 October 2008).

[46] Daniel Crane, Reflections on Section 5 of the FTC Act and the FTC's Case Against Intel (19 January 2010).

[47] Rosch (n 19) 14.

[48] Baye and Wright (n 21).

involving complex economic analysis than judges with no economic training.[49] Relying upon conclusions such as this one, commentators have called for the FTC to exercise its Section 5 authority in complex cases more frequently. The essence of the argument is that more-advanced economic training is necessary to successfully resolve complex cases. Because a low level of economic training adds no value in such cases, commentators conclude judicial disposition is inadequate; therefore, they turn to the FTC on the basis of its expertise.

An empirical study of the FTC's expertise

Data and methodology

Our primary data are information extracted from judicial opinions. We attempted to collect every reported decision in which an ALJ published a ruling on the merits of a substantive antitrust claim between 1976 and 2010. This sample includes 74 cases. We also attempted to gather every reported decision in which an Article III federal district court judge published a ruling on the merits of a substantive antitrust claim between 1977 and 2007. This sample included 644 cases,[50] bringing the total to 718 cases altogether. For each case, we record the original decision of the ALJ or federal district court judge, whether the decision was appealed, and whether the decision was reversed. For the ALJ decisions that were appealed, we also included an indicator for whether the subsequent decision made by the Commission had been appealed and whether the Commission decision resulted in a reversal.

We coded a number of characteristics about each of these 718 cases, including the type or types of antitrust claims involved (merger, monopolization, price fixing, Robinson-Patman, or multiple claims), identity of the plaintiff (FTC, DOJ, private party, or state attorney general), which party prevailed, the year the decision was issued, and for cases brought in federal district court, the procedural stage in which the case was decided (motion to dismiss, summary judgment, trial, or any post-trial motion). We also include data regarding additional information for the decisions made by the ALJ, the Commission, and Article III judges that was not universally applicable to all three types of decisions. For the Article III decisions, we determined whether the issuing judge had received specialized training on economics or antitrust by the Law and Economics Center (LEC). For the ALJ and the Commission decisions, we determined the political party of the President when the case was decided.

[49] ibid.

[50] The data including antitrust opinions of federal district court judges was originally compiled and analysed in Baye and Wright (n 21).

These data are potentially useful for measuring the performance level of the ALJ and the Commission compared to the performance level of Article III judges. The data can also be useful for determining how the Commission performs relative to the ALJ and whether subsequent review by the Commission adds value to the ALJ decisions. 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 whether the decision-maker is the Commission, an Article III judge, or an ALJ, the type of case, the judge's economic training where applicable, the political control of the FTC, and the year.

Appeals are an imperfect but useful indicator for whether the initial court made an economic error. Baye and Wright explain the value of the appeals measure with a revealed preference argument—that is, the appeal rate is a signal generated by actual costs incurred by the parties who, informed by their own economic experts, have determined the initial court committed a reversible error.[51] There are, of course, many reasons for a party to appeal any initial court decision. However, ceteris paribus, an appeal signals that at least one party believes it can convince a higher court an error has occurred. In other words, a higher appeal rate implies the decision-maker has issued more opinions that leave at least one party feeling strongly enough to invest in the opportunity for another decision-maker to decide that he has committed a reversible error.[52] For these reasons, we use appeal as our primary quality measure.[53]

Reversal rates are also commonly relied upon in the judicial performance literature.[54] Reversal rates also contain some information on the quality of the underlying decision, but there are several drawbacks to using reversals rather than appeals.[55] Perhaps most importantly, because reversals are necessarily

[51] ibid 5.

[52] The appeal rate's value as an indicator of quality may be greater in antitrust than other substantive fields of law. One potential concern with its value as a proxy for quality decision-making is that the 'error' in the underlying decision can be legal or procedural, rather than economic in nature and thus not allow proper inference concerning the economic expertise of the underlying decision. 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 s 7 of the Clayton Antitrust Act, 15 USC 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.

[53] Other proxies for judicial performance are sometimes used, including publication rate, citations, invocations, time to issue a decision and reversal rates. For a summary of this literature, see Stephen J Choi, Mitu Gulati and Eric A Posner, *How Well Do Measures of Ability Predict Judicial Performance?: A Case Study Using Securities Class Actions* (Univ of Chi Law Sch Law & Econ, Olin Research Paper No 519 & NY Univ Sch of Law, Law & Econ Research Paper No 10-18, 2011), <http://scholarship.law.duke.edu/cgi/viewcontent.cgi?article=3001&context=faculty_scholarship> accessed 2 December 2012.

[54] See eg Baye and Wright (n 21); Nash and Pardo (n 30); Kimberly A Moore, 'Are District Court Judges Equipped to Resolve Patent Cases?' (2010) 15 Harvard JL & Tech 1; R Polk Wagner and Lee Petherbridge, 'Is the Federal Circuit Succeeding? An Empirical Assessment of Judicial Performance' (2004) 152 U Pennsylvania L Rev 1105; Christian A Chu, 'Empirical Analysis of the Federal Circuit's Claim Construction Trends' (2001) 16 Berkeley Tech LJ 1075.

[55] See Baye and Wright (n 21) (discussing the relative merits of appeals as opposed to reversal rates).

conditioned on the decision's being appealed in the first place, sample size is reduced significantly in specifications that use it to measure the quality of an initial court's decision. Nonetheless, we report results using both appeals and reversals.

It is also important to highlight an important limitation of our analysis. Our sample contains only litigated cases generating published opinions. It is well known these cases are not representative of the population of underlying disputes.[56] 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: how well do courts and agencies decide the cases in front of them? However, cases are not randomly assigned to courts and agencies. Systematic differences between cases the FTC chooses to litigate in federal district court versus administrative proceedings could influence both appeal and reversal rates. Furthermore, as we shall discuss, differences in standards of review between courts and agencies may bias our comparisons between Commission and judicial decision-making.

Empirical strategy

Our goal is to provide some empirical evidence testing the expertise hypothesis, namely, that expert agency decision-making will be superior to decision-making by generalist judges. Advocates have relied upon the expertise hypothesis to justify increased delegations of power to administrative agencies and increased judicial deference to those agencies' decisions. In the antitrust context specifically, the expertise hypothesis has provided the primary intellectual basis for arguments for aggressive and expansive use of the FTC's Section 5 authority outside the bounds of the Sherman Act,[57] agency rulemaking,[58] and increased deference to FTC decisions in federal court.[59] We are not aware of any empirical studies comparing the relative performance of judges and agencies; there is, however, a relatively small but growing literature focusing upon the relationship between judicial specialization and performance.[60] We test the expertise hypothesis by way of comparing the adjudicatory decisions of two different sets of decision-makers.

We first compare the decisions of federal district court judges and FTC Commissioners. This comparison has a number of intuitively appealing

[56] George L Priest and Benjamin Klein, 'The Selection of Disputes for Litigation' (1984)13 JLS 1.
[57] See eg Rosch (n 19).
[58] See eg Hemphill (n 19).
[59] See eg Daniel A Crane, 'Technocracy and Antitrust' (2008) 86 Tex L Rev 1159, 1206–10.
[60] Rochelle Cooper Dreyfuss, 'The Federal Circuit: A Case Study in Specialized Courts' (1989) 64 New York L Rev 1; Rochelle C Dreyfuss, 'Forums of the Future: The Role of Specialized Courts in Resolving Business Disputes' (1995) 61 Brooklyn L Rev 1; Jeffrey W Stempel, 'Two Cheers for Specialization' (1995) 61 Brooklyn L Rev 67; Nash and Pardo (n 30).

features. First, both sets of decisions are appealed to federal courts of appeals. Second, most variants of the expertise hypothesis in the competition context appear to have precisely this comparison in mind.[61] Congress intended and designed the FTC to be an expert agency with specialized knowledge and resources unavailable to generalist judges; it is that expertise and specialized knowledge that Congress and proponents of the expertise hypothesis presume will increase the quality of inputs into the Commissioners' decision-making processes and thus also increase the quality of the outputs. Third, the Commission reviews ALJ decisions *de novo*, and thus its own decisions, like the district courts, are not bound by prior fact-finding.[62] Comparison of judicial and Commission decisions allows a fairly intuitive and direct test of the expertise hypothesis.

This comparison also suffers from some important limitations. Perhaps the most important is that Commission decisions are afforded greater deference than district court decisions by federal courts of appeal on review.[63] Furthermore, cases come to the Commission after a full administrative trial. While the Commission need not afford ALJ decisions significant deference, the fact that cases must go through a full trial before they can be appealed to the Commission, and perhaps ultimately to a federal court of appeals, is an important difference between the two sets of decisions. Administrative cases in which defendants are willing to incur the costs of a full administrative trial and Commission review, including the costs of delay, may be systematically more likely to contain reversible error than federal district court decisions in the sample.

Our second comparison takes a different approach, ignoring federal court decisions and focusing upon differences between ALJ and Commission decisions. The intuition of this approach is to try to estimate the 'marginal product' of Commission decision-making. We attempt to isolate the incremental impact of Commission input into the agency decision-making relative to ALJ decision-making without Commission input. Put simply, our sample of

[61] See eg Crane (n 2) 132–43 (discussing the increased influence of antitrust juries and generalist trial judges, *inter alia*, and methods by which the FTC can restore its norm-creative role and harness its 'substantial advantages over the institutional realities of private litigation'); Rosch (n 19) 15 ('In [*Polygram* and *Indiana Federation of Dentists*, the appellate courts] agreed and adopted the FTC's analysis. Had these questions been presented to a federal district court in the first instance, it's unlikely that the court would have been open (let alone equipped) to apply a more novel form of analysis in the first instance.'). But cf, eg Kovacic (n 14) 942–43 (discussing the failure of reviewing courts to adopt novel Commission analyses due to its perceived lack of expertise).

[62] See 16 CFR s 3.54 (2011) (FTC Rule of Practice permitting the Commission to, upon appeal from an initial decision, 'exercise all the powers which it could have exercised if it had made the initial decision'); see also *In re* NC Bd of Dental Exam'rs, 2011 WL 6229615, at *14 (FTC 7 December 2011) ('The Commission reviews the ALJ's findings of facts and conclusions of law *de novo*.')

[63] See *FTC v Ind Fed'n of Dentists*, 476 US 447, 454 (1986) ['The legal issues presented . . . are . . . for courts to resolve, although even in considering such issues the courts are to give some deference to the Commission's informed judgment that a particular commercial practice is to be condemned as unfair.' (internal quotation marks omitted)]. However, greater deference to Commission decisions should bias estimates of the impact of Commission decision-making on appeal and reversal rates downward.

FTC administrative litigation involves three types of cases: cases where the ALJ decision was not appealed to the Commission, cases where the ALJ decision was simply affirmed by the Commission, and cases where the ALJ decision was reversed or significantly modified by the Commission. We test whether, controlling for other potential factors, Commission decisions changing ALJ opinions have different appeal or reversal rates than those ALJ decisions the Commission simply affirms or leaves untouched.

This second comparison indirectly tests the expertise hypothesis. It does not evaluate Commission decisions relative to those issued by district court judges. Rather, this approach tests the expertise hypothesis from a different perspective, attempting to identify evidence of the Commission's expertise over ALJs in its decisions. While this approach avoids some of the limitations inherent in comparing administrative adjudication to litigation in federal court, it does not completely avoid limitations associated with selection effects because the FTC chooses the cases it brings in administrative litigation as opposed to federal court.

Results

In this section, we present simple differences in means followed by probit regression analysis for each of our two comparisons.

Federal Trade Commissioners versus Generalist Judges

Means comparisons

We begin with some simple comparisons of the means to explore the differences in the appeal rates for Commission decisions and Article III judicial decisions. Figure 1 reports the results. Aside from including the appeal rate for the Article III judges and the Commissioners, the appeal rate for the Article III judges, conditional on the plaintiff winning, is also included. In our sample, cases decided by the Commission are 14 per cent more likely to be appealed than are cases decided by Article II judges. The difference is statistically significant at the 5 per cent level. The contrast between the Commission's appeal rate and the Article III judges' appeal rate conditional on the plaintiff winning the Article III case is greater and more statistically significant than the unconditional comparison. Commission decisions are 27 per cent more likely to be appealed than are the conditional cases by the Article III judges. The difference is statistically significant at the 1 per cent level and, in practical terms, quite large. The parties involved in FTC litigation are 25 per cent more likely to be disgruntled enough to appeal their case to the circuit court of appeals.

Figure 2 compares Commission appeal rates with those of Article III judges with basic economic training. LEC-trained judges' opinions are appealed at a

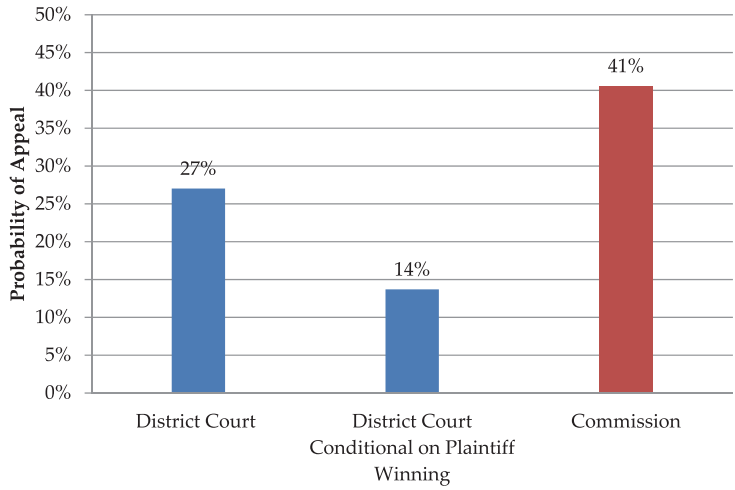

Figure 1. District court and Commission appeal rates (unconditional and conditional on plaintiff prevailing).

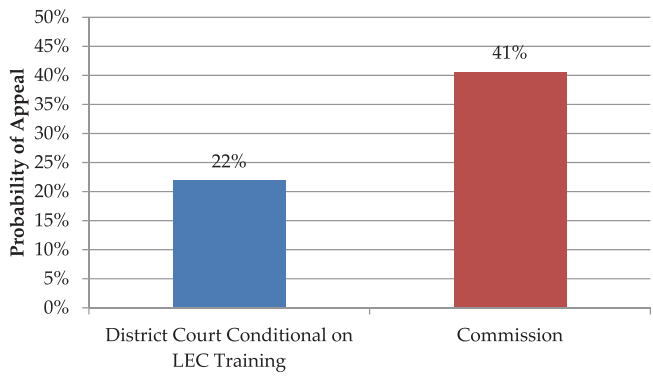

Figure 2. District court and Commission appeal rates (conditional on LEC training).

rate 5 percentage points lower than the decisions of their untrained Article III colleagues and a full 19 percentage points less frequently than those of the Commission. This difference is statistically significant at the 1 per cent level.

One important difference between Commission decisions and decisions authored by district court judges is that, as discussed above, the Commission's *de novo* review takes place after a full administrative trial in front an ALJ. Thus, it might be the case that different stages of factual development drive differences in appeal rates. For a preliminary examination of this possibility,

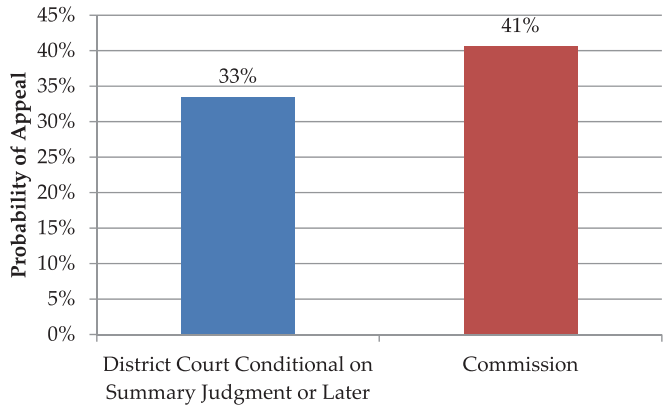

Figure 3. District court and Commission appeal rates (summary judgment or later).

Figure 3 reports appeal rates of Commissioners and Article III judges in antitrust cases conditioned upon limiting the judicial sample to decisions at or after the summary judgment stage. Judicial appeal rates are only 8 percentage points lower than the Commission's, and the difference is not statistically significant.

While we prefer comparisons based upon appeals rather than reversals, we note that we obtain similar, though less drastic, results when we use reversals. Figure 4 shows that Commission opinions are reversed 20 per cent of the time and decisions by Article III judges are reversed only 5 per cent of the time. The Article III judges' reversal rate is nearly identical to a subset of Article III judge decisions conditional on the plaintiff winning at trial. This 15 per cent point difference is statistically significant at the 1 per cent level.

Figures 5 and 6 report comparative reversal rates when we condition judicial reversal rates on economic training and decisions at or after the summary judgment stage. The differences remain stable at approximately 15 per cent; judicial reversal rates are substantially lower, providing some preliminary evidence contrary to the expertise hypothesis. The difference is statistically significant at the 1 per cent level.

These means comparisons provide preliminary evidence suggesting the Commission's decisions are more likely to be appealed and reversed than those of Article III generalist judges. Taken at face value, the comparison implies that on this particular margin of performance—adjudicatory decision-making—Commissioners do not perform as well as district court generalists. However, these differences in appeal and reversal rates may be the result of omitted variables or sample selection. In the next section, we use a probit regression

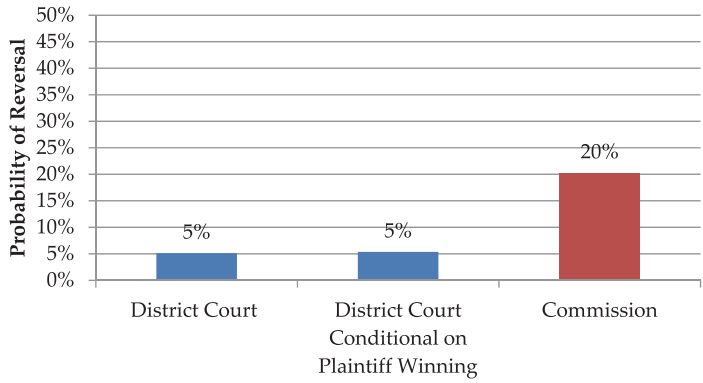

Figure 4. District court and Commission reversal rates (unconditional and conditional on plaintiff prevailing).

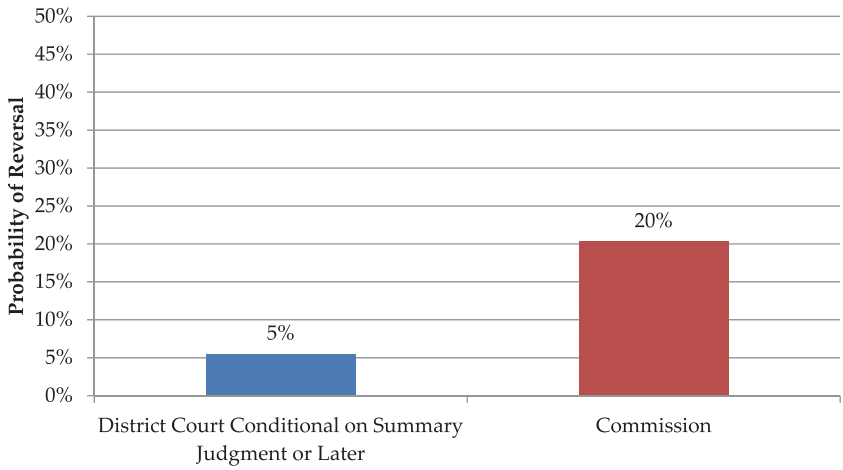

Figure 5. District court and Commission reversal rates (summary judgment or later).

framework to control for other factors that may reasonably influence the appeal and reversal rates of the Commission and Article III judges.

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. We also run each specification using REVERSAL rather than appeal as the dependent variable. Our primary independent variable of interest is COMMISSION, a dummy variable that equals one when the Commission is the relevant

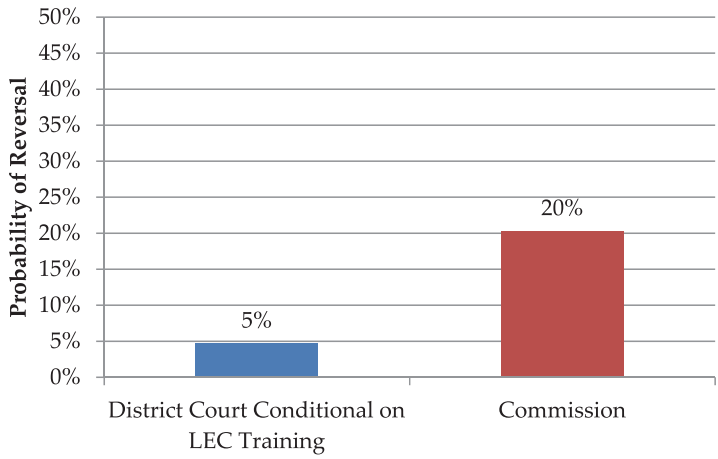

Figure 6. District court and Commission reversal rates (conditional on LEC training).

Table 1. Baseline probit regression probability of appeal or reversal ($N = 688$)

	Appeal				Reversal	
	(1)	(2)	(3)	(4)	(5)	(6)
Commission	0.135**	0.180*	0.166	0.141**	0.152***	0.153***
	(2.32)	(1.65)	(1.50)	(2.39)	(4.21)	(4.16)
Type			0.123	0.009		0.013
			(0.01)	(0.75)		(0.21)
Year dummies	No	Yes	Yes	No	No	No

* denotes statistical significance at the 10% level
** denotes statistical significance at the 5% level
*** denotes statistical significance at the 1% level

decision-maker and zero otherwise (in this case, an Article III federal district court judge issued the decision). The regressions also include a set of controls that are potentially predictive of the appeal and reversal rates, including a time trend (YEAR) and dummy variables for the type of claim (eg price-fixing, merger, or monopolization). Table 1 reports marginal effects and robust z-statistics.

Specification 1 is our baseline model, which is similar to the unconditional mean comparisons in Figure 1, except that it controls for time trends and type of case. Recall that Commission appeal rates were approximately 14 percentage points higher than that for district judges. The results are similar in magnitude and significance, with Commission decisions being appealed 13.5 percentage

Table 2. Subsample probit regression probability of appeal or reversal conditioned on the plaintiff winning sample includes only Federal District Court decisions where plaintiff prevailed ($N = 237$)

	Appeal				Reversal	
	(1)	(2)	(3)	(4)	(5)	(6)
Commission	0.268***	0.344***	0.343***	0.270***	0.149***	0.149***
	(4.39)	(2.80)	(2.77)	(4.34)	(3.32)	(3.43)
Type			0.001	0.012		0.012
			(0.03)	(0.07)		(1.12)
Year dummies	No	Yes	Yes	No	No	No

*** denotes statistical significance at the 1% level

points relative to the baseline appeal rate of Article III judges. Point estimates range from 13.5 to 18.0 percentage differences, depending upon the specification; results are robust to including controls for either type of case or time trends, but fall just outside conventional levels of significance when controlling for both simultaneously. Reversal rate regressions are consistent, with Commission decisions resulting in a reversal rate 15 percentage points higher than the baseline for federal district court judges. The basic story that emerges from Table 1 is that the Commission, contrary to the expertise hypothesis, has a significantly higher appeal and reversal rate than federal district court judges.

There are a number of potential differences between the FTC as an administrative agency and federal courts that could bias estimates of the difference in appeal and reversal rates. One critical difference is that an overwhelming majority of Commission decisions favour the plaintiff (ie the FTC). Thus, appeals from Commission decisions may be systematically different in quality or other dimensions from the distribution of cases from which appeals from district court opinions are drawn. One possibility is that the Commission is uniquely situated to select winning cases; another is that its record in this regard reflects exploitation of its substantive and procedural powers as an administrative agency rather than anything about quality of cases. In either event, one reasonable approach to dealing with this concern is to compare Commission decisions to a truncated sample of federal district court decisions including only those where the plaintiff has prevailed. Table 2 presents these results. The gap between Commission and judicial appeal rates becomes even larger in this limited sample, ranging from 26.8 to 34.4 percentage points. The gap in reversal rates remains stable at approximately 15 percentage points. Thus, it does not appear that systematic differences in case selection by plaintiffs in federal court and the FTC in administrative litigation drive the Commission's higher appeal and reversal rates.

Table 3. Subsample probit regression probability of appeal or reversal conditioned on the plaintiff winning sample includes only decisions at or after summary judgment ($N = 237$)

	Appeal				Reversal	
	(1)	(2)	(3)	(4)	(5)	(6)
Commission	0.071	0.107	0.089	0.076	0.148***	0.141***
	(1.12)	(0.90)	(0.74)	(1.19)	(3.74)	(3.77)
Type			0.016	0.014		0.007
			(0.83)	(0.89)		(0.85)
Year dummies	No	Yes	Yes	No	No	No

*** denotes statistical significance at the 1% level

A second important institutional difference between administrative litigation and federal court decisions in our sample is that, because cases reaching the Commission have undergone a full trial on the merits whereas our sample of judicial decisions includes the full panoply of pre- and post-trial motions, the former enjoy a much more fully developed factual record. This difference could potentially impact observed appeal and reversal rates. For example, the Commission's access to a fully developed factual record could enable it to make more accurate decisions on average than district court judges operating with less information, thus lowering the Commission's relative appeal rate. To account for this possibility, we also run our baseline regressions on the Commission decisions and a subsample of district court decisions limited to those at the summary judgment stage or later. Table 3 presents results. Here, appeal rate results lose significance, suggesting that controlling for these differences in case development equalize Commission and judicial performance; however, the reversal rate gap remains constant with Commission reversal rates approximately 15 percentage points higher than that of the district court judges.

Table 4 reports regressions limiting the sample of judicial decisions to those authored by LEC-trained judges; in other words, each of the judges in the subsample have had at least some basic economic training. Previous research indicates basic economic training improves judicial performance in the form of lower appeal and reversal rates.[64] The appeal rate gap increases slightly in the most basic specification to 18.9 percentage points while the Commission's reversal rates compared to trained judges remains stable at approximately 15 percentage points.

[64] See Baye and Wright (n 21).

Table 4. Subsample probit regression probability of appeal or reversal conditioned on LEC-trained judges sample includes only decisions of LEC-trained judges ($N = 197$)

	Appeal				Reversal	
	(1)	(2)	(3)	(4)	(5)	(6)
Commission	0.187***	0.089	0.089	0.185***	0.156***	0.156***
	(2.73)	(0.59)	(0.58)	(2.64)	(3.28)	(3.12)
Type			−0.017	−0.003		0.000
			(−0.67)	(0.02)		(−0.01)
Year dummies	No	Yes	Yes	No	No	No

*** denotes statistical significance at the 1% level

While the expertise hypothesis predicts lower appeal and reversal rates for the Commission relative to generalist district judges ceteris paribus, we consistently observe higher appeal and reversal rates for the Commission that are robust to controls for type of case, time trends, and a variety of robustness checks designed to control for unobservable differences in cases brought through administrative litigation rather than in federal district court.

Commissioners versus ALJs

We now turn to our second approach to evaluate the expertise hypothesis—comparing appeal and reversal rates for ALJ decisions left untouched by the Commission with those the Commission modifies or reverses.

Mean comparisons

Once again, we will begin with a simple comparison of means to explore the differences in appeal rates when the Commission modifies the ALJ ruling and when it does not. Figure 7 reports the appeal rates. There is only a 3 per cent difference in the appeal rates when the Commission modifies the ALJ ruling and when it does not modify the ALJ ruling. There is an 11 per cent difference in the reversal rates conditional upon whether or not the Commission modified the ALJ decision. Neither difference is statistically significant.

These simple comparisons only weakly suggest value added from the Commission relative to ALJ decisions; the differences in appeal and reversal rates are not statistically significant, which is at least partially attributable to the relatively small sample size of Commission decisions. Once again, the relationship between the decision-maker (ALJ or Commission) and appeal and reversal rates may be the result of omitted variable bias or sample selection effects. In the next section, we use a similar probit regression framework to control for other possible influences and isolate the impact of Commission modification of ALJ rulings on appeal and reversal rates.

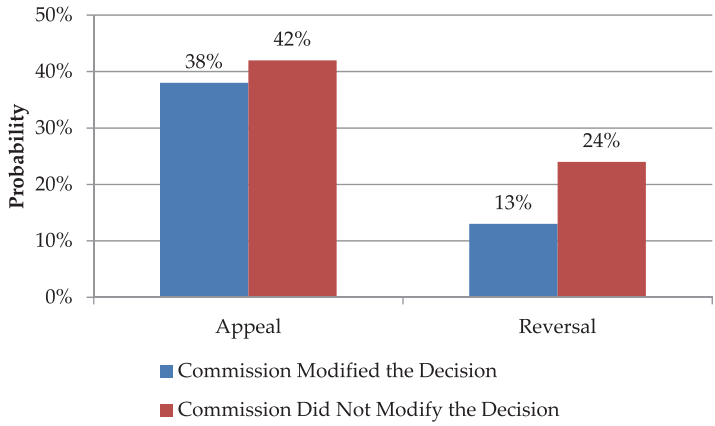

Figure 7. ALJ and Commission appeal and reversal rates

Table 5. Baseline probit regression ALJ versus commission decisions ($N = 69$)

	Appeal		Reversal			
	(1)	(2)	(3)	(4)	(5)	(6)
Commission	0.079	0.069	0.003	−0.296	−0.335	−0.003
	(0.79)	(0.69)	(0.02)	(−1.02)	(−1.13)	(−0.02)
Type		0.024			0.081*	0.028
		(0.91)			(1.79)	(0.87)
Year dummies	No	No	No	Yes	Yes	No

* denotes statistical significance at the 10% level

Baseline probit regressions

Our data for analysing administrative cases in a regression framework are naturally limited to the number of such cases brought by the Commission in the relevant time period ($n = 69$). Recall that the comparison of means reported in Figure 7 indicates the Commission's incremental impact on ALJ decisions is to reduce the appeal rate by 4 percentage points and the reversal rate by 9 percentage points. While neither of those differences is statistically significant at conventional levels, this is likely attributable to our small sample size. Thus, we consider the comparisons of means suggestive of a modest improvement in agency performance attributable to Commission-level expertise relative to ALJs. Applying the same baseline regression framework as in Tables 1–4, Table 5 reports results. Predictably, in light of sample size, the difference in appeal and reversal rate remains insignificant.

Conclusions

Expertise has long been the touchstone of administrative agency performance. In the context of antitrust agencies, like others, the expert inputs are translated into outputs including adjudicatory decisions, rulemaking, consents, advocacy, and amicus briefs. An often overlooked aspect of understanding agency performance and its relationship to expertise is institutional design. The so-called expertise hypothesis posits that the institution with more expert 'inputs' will consistently produce higher quality outputs. That assumption suffers from the Nirvana Fallacy as it lacks a basis without an analysis of the institutions and processes translating those inputs to outputs. Inability of an agency to translate its expertise into high-quality decision-making renders it at best ineffective and at worst costly to society, and institutional design has the potential to hinder the flow of information from an agency's staff to its decision-makers.

In the context of US antitrust law, many commentators have recently called for an expansion of the FTC's adjudicatory decision-making authority pursuant to Section 5 of the FTC Act, increased Commission rulemaking, and carving out exceptions for the agency from increased burdens of production facing private plaintiffs. These claims are often expressly grounded in the expertise hypothesis. The relevant question is whether the expert inputs available to generalist federal district court judges through expert evidence, amicus briefs, and economic training, among other sources of such expertise, translate to higher quality outputs and better performance than produced by the Commission in its role as an adjudicatory decision-maker.

Many appear to assume that agencies have courts beat on this margin. To our knowledge, while oft-cited as a reason to increase the discretion of agencies and the deference afforded them by reviewing courts, no one has provided empirical support for this claim. We seek to fill that gap, and contrary to the expertise hypothesis, we find the evidence suggests the Commission does not perform as well as generalist judges in its adjudicatory antitrust decision-making role. Furthermore, while the available evidence is more limited, there is no clear evidence the Commission adds significant incremental value to the ALJ decisions it reviews. In light of these findings, there is little empirical basis for the various proposals to expand agency authority and deference to agency decisions. More generally, our results highlight the need for research on the relationship between institutional design and agency expertise in the antitrust context.