Most mergers filed at the enforcement agencies are conglomerate in nature with only minor horizontal overlaps. An enforcement agency may challenge the merger, if any overlap is believed to be adversely affected by the transaction. While the merging firm is entitled to a hearing in federal court, the delay would impose additional costs, thereby giving the enforcement agency the ability to hold up the bulk of the transaction, pending resolution. Hence, the agency’s actions are not fully checked by the threat of litigation. This paper explores whether the FTC’s decisions to challenge conglomerate transactions that could feasibly settle differ from the controlling case law. We find that the representative enforcement regimes are remarkably similar, though the FTC is more accepting of efficiencies arguments, while the court considers buyer sophistication as a mitigating factor. In aggregate, the differences generally cancel out leaving comparable levels of enforcement. In depth analysis identifies some minor enforcement differences, almost none of which are statistically significant.

* The analyses and conclusions set forth in this paper are those of the authors and do not necessarily represent those of the Federal Trade Commission, any individual Commissioner or any Commission Bureau. We would like to thank Jeffrey Fischer, Arthur J. Del Buono, and anonymous referees for helpful comments on the paper; Glenn Hoetker for his Stata code; and Brian Murphy for research assistance.
I. INTRODUCTION

Most merger enforcement activity involves investigations of horizontal overlaps in conglomerate transactions. Although the merging conglomerates may compete in only a few relevant markets, the Federal Trade Commission (FTC) may challenge the entire deal based on a “reason to believe” that the merger will substantially lessen competition in any one of them. Because the aggregate size of the two firms triggers the Hart-Scott-Rodino (HSR) reporting process, even the smallest market affected by the merger may be subject to an FTC investigation. If challenged, a conglomerate firm can oppose the FTC’s action in federal court, but the entire deal would be delayed pending the outcome of the litigation (Coate and Kleit (2003)). In effect, the pro-competitive or competitively neutral portion of the transaction is held up, until the firm either prevails in court or enters into a settlement with the FTC, resolving the alleged competitive problem. Consequently, the Commission’s standard of “reason to believe” is not fully checked by the threat of litigation.

Using two models, one focused on FTC actions and the other on court judgments, we measure, for conglomerate transactions that could feasibly settle, the extent to which the FTC’s decisions to challenge a transaction differ from the legal standards of the court. We use two methods: (1) an Oaxaca decomposition and (2) an estimate of how often the court would have agreed with the FTC decisions. These approaches are useful because

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1 An overview of the FTC’s merger enforcement process is available in Bergman et al. (2007). Simply put, most large mergers must be filed (under HSR) and reviewed by either the Department of Justice or the FTC. If the relevant agency concludes that the merger is likely to be anticompetitive (i.e., has a “reason to believe,” in the jargon of the law) in any relevant product or geographic market, the agency must challenge the deal by filing a lawsuit to enjoin the merger. Firms may resolve the lawsuit prior to trial by entering into a settlement to divest assets sufficient to negate the expected anticompetitive effect. If the Agency does not file suit, the investigation is closed. If the firm prevails in court, it may consummate the merger.
they surpass a hypothesis test of equal regimes and measure the size of any differences. \(^2\) Overall, the court and FTC policies turned out remarkably similar, signifying that, on average, the FTC decisions closely track court precedent. We identified minor regime differences for unilateral effects investigations when the market definition is clear and the administration is Bush’s, though the disparities lack statistical significance and hence preclude a differential policy conclusion. We find two significant differences in the underlying regimes: (1) The FTC takes into account efficiencies, whereas the court does not. (2) The court considers customer sophistication, but the FTC does not. Though enforcement decisions on specific mergers may differ, we estimate an overall agreement rate of 79.7 percent.

The remainder of this paper is organized as follows. Section II explains the data. Section III discusses and estimates the probit regression models. Section IV introduces the decomposition procedure. Results of the decomposition are in Section V. Section VI further compares FTC and court regimes by predicting the overall level of agreement of the FTC and court, had they faced the same cases. Conclusions are in the final section.

II. OVERVIEW OF THE DATA

To compare policies at the regulatory and court levels, it was first necessary to assemble two comparable datasets, one for each regime. Because we focus on the potential for hold up, the FTC data consists of conglomerate mergers ending in a settlement or having the feasibility of one. \(^3\) We started with the 166 HSR investigations used in the Bergman et al. (2007) dataset (running from 1993 to 2003) and added 10

\(^2\) See McCloskey and Ziliak (1996) for discussion of why size (not just statistical significance) is important.

\(^3\) This data was readily available, given the Commission’s recent data collection project (see, Coate (2005); Coate and Ulrick (2006); Coate and Ulrick (2008); and Bergman, et al. (2007)).
matters decided in 2004. For the investigations that the FTC closed, we dropped all matters in which the FTC did not have a reasonable expectation that the firm would be willing to settle in response to a challenge of the overlap. We used the following rule: We dropped any matter in which the estimated value of all acquired business units participating in a market of concern accounted for over 67 percent of the transaction’s value. Among matters that were challenged, we removed litigated and abandoned transactions, leaving only settled matters. Our final dataset contains 125 mergers, of which 93 settled and 32 closed.

Data on court decisions were obtained by reading the controlling court opinions. As in Coate (1995), we abide by the conclusions in the highest court decision (i.e., the appellate, when available). We extended the data in Coate (1995) to match the FTC variables as discussed below. Then, we updated the sample by collecting information from 29 merger decisions reviewed between 1992 and 2004, so that our final sample ran from 1989 to 2004 and consisted of 51 court decisions. Twenty-six of these matters were enjoined; twenty-five were not.

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4 The data were collected from the internal files of the Federal Trade Commission’s legal and economic staff. In the Bergman et al. paper, the most serious competitive overlap was reviewed for all matters filed between fiscal years 1993 and 2003 in which (1) staff addressed a horizontal merger; (2) the Commission issued a “second request” requiring that the merging parties submit substantial information; and (3) the staff completed a comprehensive investigation on the merits of the merger action, ending with a recommendation to challenge or close. This final sampling structure excluded mergers in which (1) the staff closed the investigation after being quickly satisfied that no competitive concern existed and (2) the merging parties abandoned the transaction prior to the completion of the investigation. While eliminating these investigations from the sample would bias the estimate of the sample challenge rate, the lost observations should not materially affect the estimation of model of FTC merger activity, because they are generally outliers with either extraordinarily low or high enforcement probabilities.

5 A slightly higher cut-off would not have added closed investigations to the sample of potential settlements. While settlements are rare when an overlap is a large part of a deal, a review of the data showed that the FTC settled six matters with overlaps between 50 and 67 percent.

6 The Coate (1995) data ran from 1982 through 1992 and surveyed final decisions on the merits on federal, state and private merger challenges. We omitted data from before 1989, leaving only 22 observations reasonably contemporaneous to the FTC sample. A total of 31 decisions were reviewed for the 1993-2004 time period. Two post-1992 court decisions had no information available to encode, because the court rejected the plaintiff’s market out-of-hand. Those two matters could not be integrated into the sample.
A complete list of the variables appears in Table 1. The binary dependent variable, Enforce, indicates the outcome of a merger decision. It equals one if a court enjoins a transaction or the FTC enters into a structural settlement; it is zero if the court or FTC allows the merger to proceed unobstructed. (Throughout this paper, the term “enforce” will mean that an outcome involved either an FTC settlement or court injunction. “Settle” will be used only for an FTC decision, and “enjoin” only for a court decision.)

Coding the independent variables was only a little more complicated. To start, it was necessary to assume that the court and FTC define markets similarly. This assumption is reasonable in light of the generally accepted proposition that the Merger Guidelines are compatible with case law (Werden (2003)). With this assumption, comparable structural statistics could be computed. Although the level of detail varied by FTC investigation or court decision, both datasets contained sufficient information to estimate the Herfindahl index. Many matters contained actual Herfindahls, while others listed market shares. Occasionally, only competitors were identified, in which case number equivalent assumption was made. Two structural variables required some thought when assembling the court dataset: Unil -- a dummy variable equal to one when the theory of harm is unilateral effects (monopolization, dominant firm activity, etc.) --

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7 There were 10 matters reviewed by both the FTC and the courts during the relevant sample period; the same basic market was applied in 8 of them.
8 The numbers equivalent Herfindahl statistic assumes equal shares for all the significant rivals in the market. Thus, a market with four rivals has a Herfindahl of 2500 and one with three has a Herfindahl of 3333. The change caused by a merger of 4 to 3 rivals is 833. A few other adjustments were needed in the court cases. For five matters, the Herfindahl was imputed to be 1000, as the court rejected the narrow plaintiff’s market and the decision implied numerous firms competed in the broader market. The estimated Court model is robust to the deletion of these observations. When evidence enabled the direct estimation of the broad market Herfindahl, it was calculated from the facts given by the court. In two other matters, share data was used for the rejected narrow market, because the court included an “in the alternative” finding of no competitive concern even in the plaintiff’s market.
and Rivals -- the number of significant competitors affecting competition. Nonetheless, these variables were relatively easy to create using the court opinions’ market share data and competitive effects discussions as a guide. The FTC data directly identified these structural factors, as the Guidelines mandate a full competitive effects analysis.

From the court opinions, we also created Entry -- a dummy indicating when entry barriers were present in court matters. We set Entry equal to one whenever the court opinion claimed that at least one of the Guidelines’ timeliness, likelihood, and sufficiency barriers existed. If the opinion failed to discuss barriers, the entry variable was set to zero. This variable had already been created for the FTC, in prior research.

Court opinions easily begat two more dummy variables: buyer sophistication (Soph) and efficiencies (Eff). We let buyer sophistication equal one when the court found evidence that customers affected the competitive process enough to possibly disrupt anti-competitive behavior. The efficiencies variable was set to one if the court opined that the parties showed merger-specific cost savings. Similar information had already been collected for the FTC in Bergman et al. (2007).

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9 Coate and Ulrick (2007 at 566) define significant rivals as firms “whose independence could affect the ability of the merged firms to achieve an anticompetitive outcome.”
10 These allocations were easy because, in general, the discussion in a decision trends towards either unilateral effects (often with obvious monopoly/dominant firm conclusions) or collusion (with references to Posner’s checklist). If a relevant market an opinion evaluated both theories, the theory discussed in the principle finding was categorized as the relevant one.
11 The FTC structural indices were defined by reviewing both the Bureau of Competition (BC) and Bureau of Economics (BE) analyses and picking the structural indices most compatible with a Guidelines-based approach to market definition. In most cases, the analysis was very similar. The bulk of the differences were resolved by accepting the BC data (as the lawyers adhered more closely to the letter of the Merger Guidelines). This system ensured the decision making process was as standardized as possible. As discussed in Section IV, the methodology discussed in Coate and Fischer (2007) was used to determine which markets were relatively easy to define on both the demand and supply side. Decompositions are repeated separately for clear and complex market definitions to check the robustness of the results.
12 An entry index had been created in Bergmann et al. (2007). The entry index ranged from zero to three, depending on the number of entry impediments. For this paper, we mapped the Bergman et al. index into a binary entry variable equal to one if the Bergman et al. index was greater than or equal to one.
13 Both binary variables were coded to zero if the relevant evidence was not discussed.
Numerous other variables could be coded for either the court or the FTC datasets but not for both. Natural experiments, hot documents, and customer complaints, commonly addressed in FTC staff memos, played a role in some court decisions, but courts did not systematically discuss these factors. Similarly, a number of court decisions highlighted a range of factors from the Posner (1976) checklist (such as conditions compatible or incompatible with collusion), whereas some FTC cases did not. These variables cannot be used in our analysis.14

III. ECONOMETRIC MODELS OF ENFORCEMENT

A. Introduction to the Models

In this section, we introduce the econometric models used to examine how closely the FTC follows court precedent. Because the outcomes we measure are binary, we model enforcement with a probit function. To develop the first model, note that institutionally both regimes should be related to the case law derived from *Philadelphia National Bank* and *Baker Hughes*.15 Simply put, the plaintiff (i.e., the government) has the burden to define a market and show it is concentrated. If the plaintiff meets this burden, the respondents (the merging firms) have an obligation to produce evidence that the merger is unlikely to harm competition. If the respondents produce credible evidence, the plaintiff must prove that the merger is likely to substantially lessen competition. This legal structure suggests Model (1). In it, the Herfindahl index ($HHI$), its change ($\text{Change}$), the interaction between $HHI$ and $\text{Change}$ ($\text{Inter}$), and the number of

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14 Though some variables might not affect both the FTC and court decisions, it is necessary for the variables to be defined for both, for us to predict how one entity would have decided the other’s cases.

significant rivals (Rivals) represent the government’s structural evidence. The entry
(Entry) and sophistication (Soph) variables account for well-known rebuttals. Another
variable, efficiencies (Eff), completes the model, though its weight in case law is weak.

\[(1) \text{Enforce} = f(HHI, \text{Change}, \text{Inter}, \text{Rivals}, \text{Entry}, \text{Soph}, \text{Eff}).\]

While the Philadelphia National Bank decision remains the controlling legal
authority, a case can be made that FTC settlement policy follows the spirit of the Merger
Guidelines (1992). While the Guidelines are closely related to the case law, the
structural variables depend on the theory of concern (Coate (2005)). This gives rise to
Model (2), wherein mergers with a focus on unilateral concerns are identified by the
dummy variable, \textit{Unil}. For unilateral matters, the number of significant rivals defines the
best structural index (Coate (2005)). Hence, model (2) includes \textit{Rivals-Unil}, which is
\textit{Rivals} interacted with the unilateral indicator. Coordinated interaction investigations are

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16 We integrate the structural variables in log form. This choice modifies the nonlinearities of the probit
model and improves fit. \textit{Inter} is included, because it substantially contributes to model fit. This interaction
term is calculated as the product of the logged \textit{HHI} and \textit{Change} variables.

17 See for example, U.S. v. Country Lake Foods, 1990-2 Trade Cas. (CCH) ¶ 69,113 (D.Minn. June 1,
1990) (for customer sophistication) and U.S. v. Long Island Jewish Medical Center 983 F. Supp. 121
(E.D.N.Y. 1997) (for efficiencies).

18 Philadelphia National Bank, supra note at 371 rejects the basic idea of an efficiencies defense. More
recently, courts have been willing to entertain efficiencies, but for mergers that substantially increase
concentration, these savings must be extraordinary. See FTC v. Heinz 1116 F. Supp. 2d. 190 rev’d 246 F.

19 \textit{HHI, Change and Entry} are expected to have positive coefficients, while \textit{Rivals, Soph, and Eff} are likely
to have negative effects. Given previous research, \textit{Inter} is expected to have a negative coefficient (Coate
and Ulrick (2006)).

20 The Merger Guidelines (1992) define two theories of competitive concern used by the enforcement
agencies in merger enforcement. Coordinated interaction is simply a new name for the standard theory of
collusion, although the Guidelines detail the evidentiary requirements of the analysis. Simply pointing to a
merger in a concentrated market is no longer enough. Unilateral effects is the other theory and covers all
situations in which the merged firm is able to substantially lessen competition through independent
conduct. While monopoly and dominant firm situations are the most obvious, unilateral effects extend to
cover situations in which the merger’s change in structure affects the competition “game” played in the
market. If prices are expected to materially increase, the merger can be challenged. For one approach to
the problem, see Werden and Froeb (2004).
identified by a collusion indicator \((\text{Coll})\), which is defined as the complement of \(\text{Unil}\) \((i.e., \text{Coll} + \text{Unil} = 1)\). Three variables model structure in collusion cases: the Herfindahl interacted with collusion \((\text{HHI-Coll})\); the change in the Herfindahl interacted with collusion \((\text{Change-Coll})\); and the interaction of \(\text{HHI}, \text{Change}, \) and \(\text{Coll} \) \((\text{Inter-Coll})\). The three previously introduced control variables -- \(\text{Entry}, \text{Soph}, \) and \(\text{Eff} \)-- complete Model \((2)\). Although the Commission recognizes the importance of efficiencies, it does not always consider customer sophistication in enforcement decisions.\footnote{Merger Commentaries, 2006. For efficiencies consider pages 49-59; for customer sophistication see pages 17-18.}

\[
(2) \text{Enforce} = f(\text{Unil}, \text{Rivals-Unil}, \text{HHI-Coll}, \text{Change-Coll}, \text{Inter-Coll}, \text{Entry}, \text{Soph}, \text{Eff}).
\]

Although it is well known that adverse selection issues will affect statistical estimates of average enforcement probabilities \((\text{Priest and Klein (1984)}; \text{Froeb (1993)})\), our research design tends to minimize the concern, because we explicitly measure the difference in the enforcement rates attributable to the heterogeneity in case mixes. Sample selection issues would still cause problems if the selection mechanism biases the coefficient estimates. For selection to be a problem there must be omitted variables determining whether or not the case appears in the data, and these omitted variables must be correlated with (a) the enforcement decision and (b) the variables already in the model.\footnote{See, \textit{e.g.}, Kennedy (2003), § 16.3, pp. 284 - 285.} The most obvious problem could occur for the court cases, as firms may be reluctant to litigate very strong cases, while plaintiffs may avoid very weak ones.\footnote{The same effect might also exist with the FTC sample, as very weak cases are closed prior to the completion of the investigation. A few strong cases might also fail to reach the settlement stage if the firm abandons the transaction once it is clear the investigation will be lengthy. Large selection issues could exist for litigated cases, but these FTC investigations are all excluded from the sample by design.}
Because selection focuses on only the most extreme matters, the effects of omitted variables are likely to be small.24

B. Comparing the FTC’s and Court’s Estimated Models

Because we use probit models, we cannot compare FTC settlements with court injunctions by comparing the coefficients of the respective models. As Allison (1999) observes “Differences in the estimated coefficients [of a logit or probit model] tell us nothing about the differences in the underlying impact of $x$ on the two groups,” even if those differences are statistically significant. This is because the parameters in a probit model are normalized by the latent variable’s unobserved variance. Allison presents a modified test accounting for this fact, but it is limited to testing if (a) at least one coefficient differs, without identifying which or (b) a single, specific coefficient differs. Neither is particularly useful in a comprehensive comparison of policy. Moreover, Hoetker (2004) shows that Allison’s test suffers from low power, which further limits its use in our analysis.25 In later sections, we use other, more comprehensive methods to explore the differences between the regimes.26

Table 2 reports the estimated models. For the FTC data, it was possible to obtain reasonable estimates of Model 1. These results are listed under the column titled

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24 Because we use a probit model, omitted variables-based selection arguments appear moot, as long as the selection mechanism focuses on either very strong or very weak cases. Probits focus on probabilities, not levels, and therefore missing observations with enforcement probabilities close to 1 (strong cases) or 0 (weak cases) have minimal effects on the optimization routine.

25 We ran Allison’s test on our data (using Hoetker’s code and hence a logit model without robust standard errors) and found no significant differences between court and FTC coefficients. Given Hoetker’s findings and our small sample sizes, this result is not particularly informative. Nevertheless, even a significant difference would tell us little because it would not tell us the extent to which the regimes differ.

26 Because we use a nonlinear model, juxtaposing the marginal effects implied by the FTC and court models does not allow an overall evaluation of whether the FTC follows court precedent. This is because the marginal effects in a probit are different for each possible point in the theoretical data distribution, and it would be infeasible to compare marginal effects at a sufficiently exhaustive set of points to comprehensively compare the enforcement regimes. Our methods consider the entire distribution.
“FTC1.” The Herfindahl-related variables border on statistical significance. We posit that the low t-values are driven by multicollinearity (the various structural variables are highly correlated, ranging from .60 to .97). Estimating Model 2 generates significant results that are reported in the column titled “FTC2.” These parameters imply that the probability of a settlement depends on the number of significant rivals in unilateral effects cases and the three Herfindahl-related variables for collusion cases.27 In both specifications, a finding of barriers to entry significantly increases the chance of a settlement, an efficiency finding makes closing the investigation somewhat more likely, and the customer sophistication variable has no significant effect.28 To gauge the magnitude of the impact of efficiencies, note that, if entry equals one, a finding of efficiencies offsets the increase in the settlement probability associated with a change in rivals from 5 to 4.

The court results allude to a different enforcement regime. There are too few observations to estimate a reliable version of the full Model 1 for the court data. Thus, in the column headed “Court1a,” we report a slightly different version that excludes Change and Inter. The coefficient of HHI is significant, whereas the coefficient of Rivals is not. A simpler structural presumption focused just on the Herfindahl represents a better proxy

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27 An unreported regression adds (a) Coll interacted with Rivals and (b) Unil interacted separately with each of HHI, Change, and Inter to the model reported as FTC2. We cannot reject the hypothesis that all of these new variables are statistically insignificant, suggesting that we can leave them out of the model. Moreover, none of their individual t-stats show significance. However, in this unreported model, we reject the null that HHI-Coll, Change-Coll, Inter-Coll, and Rivals-Uni are all zero, suggesting we should not remove them from the model. (Note that all other control variables in the unreported model are the same as in FTC2.) In another unreported regression, a variable for the share of the transaction potentially subject to competitive concerns was included in the model to check for a continuous “hostage” effect. The smaller the share, the more one might hypothesize that the FTC could hold up a transaction. The variable proved insignificant in both specifications, suggesting that any FTC effect that exists applies whenever settlement is a feasible option.

28 The insignificant Soph variable contrasts with its significant effect in Bergman et al. (2007) possibly because the current sample excludes all matters in which litigation is likely. Without the threat of litigation, the FTC may not consider buyer sophistication to be an important consideration.
for court decisions. This model is estimated in the column labeled “Court1b.” In both specifications, entry and buyer sophistication have significant effects on the likelihood of an injunction, while efficiency findings do not. According to the Court1b model, a finding of buyer sophistication offsets the effect of an increase in the Herfindahl from 2400 to 4200.

The initial modeling suggests that FTC settlements and court injunctions depend on some similar and some different variables. More detailed analysis is necessary to determine how the two enforcement regimes compare. Evidence that the FTC settles matters at a higher rate than the court enjoins them, holding the merits of cases constant, would imply that the FTC sets “reason to believe” well above the legal standard.

IV. INTRODUCTION TO THE DECOMPOSITIONS

We assess the similarity of the regimes with an Oaxaca (1973) decomposition. The Oaxaca decomposition is commonly used in labor economics to analyze wage gaps (for a summary, see Berndt (1991)). Applied in the present paper, it isolates the portion of the gap in the FTC settlement and the court injunction rates that is due to a difference in enforcement regimes as opposed to case selection. Though the Oaxaca decomposition is usually applied to a linear model, we extend it in a manner similar to that in Ulrick (2005) and Bergman et al. (2007) to handle the nonlinear probit model. See those papers for the mathematical theory; we discuss only the intuition here.

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29 Estimating Model 2 for the court data showed no significant Herfindahl-related variables (though the court sample size is smaller). This result is not surprising, as the courts are not constrained by the Merger Guidelines.
The decomposition is implemented as follows. The difference between the FTC settlement regime and court injunction process is divided into two parts: difference due to regimes and difference due to case mix. That is,

\[(3) \quad [\text{difference between FTC settlement and court injunction}] = [\text{difference due to regimes}] + [\text{difference due to case mix}].\]

We estimate each of the components on the right hand side of (3). The first term is estimated as

\[(4) \quad [\text{difference due to regimes}] = [\text{Actual FTC settlement rate on FTC matters}] - [\text{The hypothetical court injunction rate on FTC matters}].\]

The hypothetical court injunction rate on FTC matters is computed by plugging the FTC cases into the estimated court injunction model and calculating the average predicted settlement rate had the FTC used the court process.\(^{30}\) Note that in (4), the distribution of cases is held constant; only the regime is changed. Therefore, a positive or negative value in (4) represents only the difference in enforcement activity that can be attributed to the regimes. Analogously, the second term in the decomposition (3) is estimated with

\[(5) \quad [\text{difference due to case mix}] = [\text{The hypothetical court injunction rate on the FTC matters}] - [\text{Actual court injunction rate on court matters}].\]

In (5), the regime is held constant. Therefore, any non-zero value in (5) represents only the difference in enforcement activity that can be attributed to a difference in case mixes.

\(^{30}\) The mathematics of the decomposition call for the actual expected FTC enforcement rate in (4) and the actual expected court rate in (5). (See Ulrick (2005) and Bergman et al. (2007).) It is econometrically more efficient to directly estimate the actual expected FTC enforcement rate with the sample average enforcement rate than with the probit’s predicted average.
Finally, note that there is an indexing issue in (4) and (5). That is, in (4), we can just as well fix the cases at the sample of court matters instead of at the FTC matters, and in (5) we can hold the regime fixed as the FTC’s and not the court’s.

V. RESULTS OF THE DECOMPOSITIONS

The FTC settles 74.4 percent of its investigations in the sample. In contrast, the court enjoined 51.0 percent of its sampled cases, for a difference of 23.4 percentage points. This section presents the decomposition results and estimates what portion of this gap can be explained by differences in either policy or case selection. The analysis uses the FTC2 and Court1b regressions, as these models outperform the alternative models on the statistical tests discussed in Section III.

For the first step, we examine how the court would have enforced the FTC cases. This hypothetical is listed in the upper portion of Table 3. Using the estimated court model, we predict that, were the court faced with FTC cases, the court would have enjoined 72.9 percent of them. This rate is slightly less aggressive than the FTC’s actual settlement rate of 74.4 percent on the matters. The difference is only 1.5 percentage points and statistically insignificant.31 Similarly, if we hold the case mix constant at the court matters, the difference between the hypothetical FTC and actual court enforcement rates is a statistically insignificant 3.5 percentage points.

The comparisons between the actual and hypothetical enforcement rates are inputs into the decompositions; they are listed in the lower portion of Table 3. The decomposition that uses the FTC matters as the index implies that a statistically

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31 We bootstrapped this and all other hypothesis tests involving hypothetical predictions or decompositions using 10,000 bootstrap replications and Stata’s bsample sampling procedure.
insignificant 1.5 percentage points of the 23.4 point difference in the enforcement rate is due to dissimilarity in the regimes, and a statistically significant 21.9 percentage points of the difference are due to heterogeneity in the case mixes. The results change very little if we use the alternative index based on the hypothetical situation wherein the FTC faces the court cases. The difference in enforcement still appears attributable mostly to the case mix (a significant 19.9 percentage points out of the 23.4 percentage point difference), and very little is linked to heterogeneous regimes (only an insignificant 3.5 percentage points). Thus, the initial decomposition suggests that the overall effect of the FTC’s “reason to believe” standard matches the relevant legal standards.

It is possible to partially control for the difference in the case mix by splitting the samples into unilateral effects cases (relatively more prevalent in the FTC sample, with 81 of 125 matters) and coordinated interaction cases (relatively more prevalent in the court sample, with 38 of 51 cases). The subsample means are much more comparable, with only a 7.1 percentage point difference for the unilateral cases and a 14.7 percentage point gap for the collusion cases. These differences are not statistically significant, so we cannot reject that the FTC and court’s enforcement rates are the same.\(^{32}\)

The decomposition procedure is applied separately to the unilateral and coordinated interaction cases in Table 3. In both the unilateral and collusion subsets, the difference due to the regime is very small: For unilateral matters, an insignificant 1.8 or 3.1 percentage points of the 7.1 point gap can be attributed to the enforcement regime, depending on whether the FTC or court data are used as weights. For the collusion cases, the portion attributable to regime is, depending on weighting, an insignificant 1.3 or 3.7

\(^{32}\) The similar means in these categories give raise to less similar overall means, because the FTC sample contains relatively more of the high enforcement unilateral cases than does the court sample.
points of the 14.7 point gap. In both subsets, the difference due to case mix is larger, though also insignificant (perhaps because the subsamples are small).

More subtle differences may be hidden in the data if artificially narrow FTC market definitions disguise the enforcement structures. Further analysis can address this point. Specifically, we contrast matters wherein the market definition is very clear to those in which it is complex. Coate and Fischer (2007) present a market definition tabulation to identify industries in which both the product and geographic market definitions are relatively clear. In effect, these markets define themselves; no real economic analysis is needed to define them. Therefore, it is reasonable to conclude that both the FTC and the courts would adopt the same market. When market definition requires substantial economic analysis, we label the market definition as complex and expect that the FTC and court market definition regimes might differ.

Table 4 presents the enforcement probability differences for four classifications: unilateral clear market, unilateral complex market, coordinated interaction clear market, and coordinated interaction complex market. Injunction probabilities predicted by Court1b are again compared to actual FTC decisions. No clear differences are observed for either coordinated interaction or unilateral effects cases; the enforcement rates are very close in each category.33 (The differences in the first two columns are not significant. Sample size prevents testing the last two columns in the table; however, the

33 Further work on the unilateral case subsample limited to the 15 matters with four or more significant rivals showed the FTC was willing to more aggressively enforce a unilateral effects theory in the few cases that involved clear markets (FTC actual enforcement rate exceeded the one estimated by court model by 11.9 percentage points) than complex markets (Court enforcement rate exceeded the FTC actual enforcement rate by 26.4 percentage points. While this difference was huge, the limited number of observations limits its relevance. For insight into the problems with showing a unilateral effects case, see U.S. v. Oracle, 331 F. Supp. 2nd 1098 (N. D. Cal. 2004).
differences are so small that, even if they were statistically significant, they would not be economically significant.\textsuperscript{34}

The hypothetical predictions allow another analysis, which explores whether the policy differences depend on political regime.\textsuperscript{34} The FTC data can be organized into three eras: those associated with the Bush II administration (June 2001 through 2004), the Clinton administration (April 1995 through May 2001), and the Bush I administration (January 1993 through March 1995). Dates fail to match presidential terms, because they are based on the Chairmanship of the specific Presidential appointees. The Bush II regime settled 73.3 percent of the relevant matters, the Clinton team settled 80.0 percent, and Bush I regulators resolved 65.7 percent of their sampled cases with settlements. Standing alone, these numbers are meaningless, unless we can claim that our settlement samples were random. However, the numbers can be compared after being normalized with the predictions of the court model. Here we find the Bush II administration settled at a six percentage point higher level than would be predicted by the court model (settled 73.3 percent of the sample, prediction of 67.3 percent), while the two other administrations were within 1.5 percentage points of the expected number. Further analysis showed a 10 percentage point gap for unilateral cases (settled 88.2 percent of the sample, prediction of 78.2 percent). None of these differences are statistically significant; thus, we can not conclude that the Bush II administration pushed a more aggressive interpretation of the wording of the unilateral effects model, relative to other administrations.

\textsuperscript{34} Coate and Ulrick (2006) discuss the possibility of a lower enforcement rate for the Bush era (at 539-540), but the statistical tests reject the hypothesis (Table 3).
The enforcement differences can be further tested by modeling the over-enforcement associated with the FTC investigations. Two approaches are possible. First, it is possible to subtract the fitted court value (from the Court1b regression) from the actual FTC outcome (either 0 or 1) to compute a measure of FTC aggressiveness \((Diff1)\). Second, the difference between the fitted value from the FTC equation (from the FTC2 regression) and the calculated prediction of the FTC settlement outcome from the Court1b regression defines a second measure of FTC aggressiveness \((Diff2)\). Based on the results of the decompositions in Table 4, the analysis of the two prediction error variables is limited to the 81 unilateral effects cases (oligopoly analyses show no difference what-so-ever between the FTC and court regimes and thus need no further analysis). We regress both \(Diff1\) and \(Diff2\) on an indicator for the Bush administration, controlling for clear markets via a dummy indicating that the market definition is clear.

No relevant results are observed for \(Diff1\). Using \(Diff2\) gives slightly more interesting results. The indicator for a clear market shows that the FTC is 7 to 10 percentage points more aggressive, when the market is easy to define, while the constant suggest less aggressive enforcement when the market is complex. This effect borders on significance using a bootstrap confidence interval. The Bush administration effect remains insignificant for the alternative prediction variable. Thus, while we can find differences in enforcement underneath the aggregate similarities, they are not statistically significant (though small samples handicap the tests). No matter how we look at the decomposition, the FTC does not systematically apply a more aggressive enforcement regime than the courts.
VI. HOW OFTEN WOULD THE COURTS AND FTC AGREE?

If the court exhibits a high rate of agreement with FTC settlements, it is evidence that the FTC’s decisions are closely related to the court’s, *i.e.* , that the FTC does not regularly exercise its ability for holding up cases. Therefore, for our final analysis, we measure how often the court would be expected to agree with the FTC’s outcomes. The method is simple. We estimate the expected value of the number of times the court would have agreed with the FTC on its 125 matters. To elaborate, suppose that the FTC settled a particular matter. Suppose further that the predicted court outcome on the matter is an 89 percent chance of an injunction. Then, there is an 89 percent chance that the court would agree with the FTC on the matter. On the other hand, if the FTC closed a matter and the court outcome predicted a 35 percent chance of injunction, there is a 65 percent chance that the court would agree, *i.e.*, also close the investigation. The average probability of agreement across all FTC matters gives the expected rate of agreement.

We estimate that, on average, the court would have agreed with the FTC 79.7 percent of the time. The 95 percent bootstrapped confidence interval for this agreement rate is (71.8%, 86.4%). For matters that were settled by the FTC, the agreement rate is even higher: 85.3% with a bootstrapped confidence interval of (75.7%, 95.6%). That is, the court would be expected to agree with the FTC’s challenge decisions relatively often, suggesting a close link between the FTC regime and the court precedent.35

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35 To explore what was likely to happen in court when the firms are unable to settle with the FTC, we reviewed the matters that were either litigated or abandoned. This supplementary data also comes from the original Bergman et al. sample and consists of ten matters which were litigated to a conclusion (ending with six injunctions), one that settled just prior to a verdict, and eight that were abandoned. Using the court model, we estimate that an expected seven of the abandoned deals would have been enjoined had the cases gone to trial. Thus, amongst these challenged cases, the courts would have upheld the FTC enforcement action in 14 of 19 cases, or 73.7 percent of the time. This rate is slightly less than the agreement rate for settled matters, though the small sample size of challenged matters prevents us from testing whether the difference is statistically different.
The agreement is not 100% for at least three reasons: First, the FTC model responds significantly to efficiency considerations, while the court model is more affected by customer sophistication. Second, factors that could only be measured for either the court or the FTC but not for both (such as hot documents or other factors listed at the end of section II) were excluded from the models. Third, a number of completely immeasurable characteristics can cause court and FTC decisions to differ. For example, merging parties could hire particularly skillful lawyers or economists, or a Judge may have a specific understanding of relevant aspects of the case law.

VII. CONCLUSION

The comparison of the FTC settlement regime and the court injunction process identifies only small differences, with the FTC interested in efficiencies and the courts in customer sophistication. Aggregate enforcement probabilities were remarkably similar. No material differences are observed for the average settled case, suggesting the FTC and the courts apply the same generic policy. For the Bush administration and unilateral effects cases in well-defined markets, the FTC appears slightly more aggressive than the comparable court analysis, but the result is not statistically significant in the sample.

Of course, the standards differ at the margin and uncertainty exists in all litigations, so some differences in prediction will be observed. Our statistics show the FTC and court decisions would be expected to match quite often: 79.7 percent of the time. A few firms may feel their mergers are challenged by the FTC when the evidence does not point to a violation. However, since the aggregate enforcement policies are comparable, our results do not identify a systematic problem in merger regulation.
BIBLIOGRAPHY


Table 1 – Overview of the Explanatory Variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Definition</th>
<th>Range</th>
<th>Means-FTC</th>
<th>Means-Court</th>
</tr>
</thead>
<tbody>
<tr>
<td>Enforce</td>
<td>Settlement by FTC / merger order by court</td>
<td>0/1</td>
<td>.744*</td>
<td>.509</td>
</tr>
<tr>
<td>Unil&lt;sup&gt;a&lt;/sup&gt; (Unilateral Effects)</td>
<td>Finding in a Guidelines analysis that the unilateral effects theory is most relevant to evaluation of merger / finding by court that suggests unilateral effects theory most relevant to evaluation of merger</td>
<td>0/1</td>
<td>.648*</td>
<td>.255</td>
</tr>
<tr>
<td>HHI</td>
<td>Herfindahl Index computed by summing the square of market share held by each firm in the post-merger market in Guidelines-based market by FTC staff /court decision (presumed Guidelines-based).</td>
<td>914/10000</td>
<td>5465*</td>
<td>3755</td>
</tr>
<tr>
<td>Change&lt;sup&gt;b&lt;/sup&gt;</td>
<td>Change in the Herfindahl index caused by the merger.</td>
<td>23/5000</td>
<td>1854*</td>
<td>1211</td>
</tr>
<tr>
<td>Rivals</td>
<td>Number of pre-merger “significant” rivals in market affected by merger identified in Guidelines-based market by FTC staff /court decision (presumed Guidelines-based).</td>
<td>2/16</td>
<td>3.34*</td>
<td>5.47</td>
</tr>
<tr>
<td>Entry</td>
<td>Dummy for finding at least one expected impediment to entry (i.e., timeliness, likelihood or sufficiency concern).</td>
<td>0/1</td>
<td>.840*</td>
<td>.706</td>
</tr>
<tr>
<td>Soph&lt;sup&gt;c&lt;/sup&gt; (Sophistication)</td>
<td>Finding by either BC or BE that suggests buyers are sophisticated / finding by court that suggests buyers are sophisticated.</td>
<td>0/1</td>
<td>.240</td>
<td>.314</td>
</tr>
<tr>
<td>Eff&lt;sup&gt;c&lt;/sup&gt; (Efficiencies)</td>
<td>Finding by either BC or BE that suggests efficiencies / finding by court that suggests efficiencies</td>
<td>0/1</td>
<td>.536*</td>
<td>.352</td>
</tr>
<tr>
<td></td>
<td>Number of investigations in sample</td>
<td></td>
<td>125</td>
<td>51</td>
</tr>
</tbody>
</table>

* Difference between court and FTC means significant at the 95% (two-tailed) level.

<sup>a</sup> Coll (collusion) is defined as the complement of Unil, such that Coll + Unil = 1, for all cases.

<sup>b</sup> The variable Inter is included in some models, to allow for more complex effects. Inter is defined as the product of HHI and Change.
Table 2 – FTC/Court Results for Settlement/Enjoin Probability

<table>
<thead>
<tr>
<th></th>
<th>(FTC1)</th>
<th>(FTC2)</th>
<th>(Court1a)</th>
<th>(Court1b)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>HHI</strong></td>
<td>4.755 (1.64)</td>
<td>1.300* (1.92)</td>
<td>1.681*** (2.87)</td>
<td></td>
</tr>
<tr>
<td><strong>Change</strong></td>
<td>5.656 (1.63)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Inter</strong></td>
<td>-0.6494 (-1.56)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Rivals</strong></td>
<td>-1.775*** (-2.92)</td>
<td>-0.4628 (-.54)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>HHI-Coll</strong></td>
<td></td>
<td>7.054* (1.87)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Change-Coll</strong></td>
<td></td>
<td>8.846* (1.90)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Inter-Coll</strong></td>
<td></td>
<td>-1.005* (-1.78)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Rivals-Unil</strong></td>
<td></td>
<td>-2.737*** (-3.39)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Entry</strong></td>
<td>2.083*** (5.49)</td>
<td>2.200*** (5.76)</td>
<td>2.629*** (4.07)</td>
<td>2.728*** (3.64)</td>
</tr>
<tr>
<td><strong>Soph</strong></td>
<td>-0.1288 (-.31)</td>
<td>-0.156 (-.39)</td>
<td>-0.9390* (-1.88)</td>
<td>-0.9302* (-1.82)</td>
</tr>
<tr>
<td><strong>Eff</strong></td>
<td>-0.7323** (-2.23)</td>
<td>-0.8442** (-2.39)</td>
<td>-0.2583 (-.53)</td>
<td>-0.3309 (-0.61)</td>
</tr>
<tr>
<td><strong>Unil</strong></td>
<td></td>
<td>65.38** (2.13)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Constant</strong></td>
<td>-39.44* (-1.66)</td>
<td>-62.55** (-2.04)</td>
<td>-11.36* (-1.76)</td>
<td>-15.20*** (-3.03)</td>
</tr>
<tr>
<td><strong>Sum of Squared Residuals</strong></td>
<td>11.07</td>
<td>10.13</td>
<td>4.571</td>
<td>4.827</td>
</tr>
<tr>
<td><strong>Pseudo -R-Squared</strong></td>
<td>0.5167</td>
<td>0.5389</td>
<td>0.574</td>
<td>0.5689</td>
</tr>
<tr>
<td><strong>Predictions correct</strong></td>
<td>89.60%</td>
<td>88.80%</td>
<td>84.30%</td>
<td>84.30%</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>125</td>
<td>125</td>
<td>51</td>
<td>51</td>
</tr>
<tr>
<td><strong>Pseudo Log-likelihood</strong></td>
<td>-34.36</td>
<td>-32.79</td>
<td>-15.05</td>
<td>-15.24</td>
</tr>
</tbody>
</table>

a. Dependent variable: Enforce.
b. Robust t-statistics in parentheses. Two-tailed test is significant at *10%, **5%,
or ***1% level.
Table 3 – Hypothetical Enforcement Rates and the Oaxaca Decomposition

<table>
<thead>
<tr>
<th>Actual and Hypothetical Enforcement Rates (percent)</th>
<th>All Cases</th>
<th>Unil Cases</th>
<th>Coll Cases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Actual FTC rate on FTC cases:</td>
<td>74.4</td>
<td>84</td>
<td>56.8</td>
</tr>
<tr>
<td>Hypothetical court rate on FTC cases:</td>
<td>72.9</td>
<td>82.2</td>
<td>55.8</td>
</tr>
<tr>
<td>Difference (percentage points):</td>
<td>1.5</td>
<td>1.8</td>
<td>1.0</td>
</tr>
<tr>
<td>Actual court rate on court cases:</td>
<td>51</td>
<td>76.9</td>
<td>42.1</td>
</tr>
<tr>
<td>Hypothetical FTC rate on court cases:</td>
<td>54.5</td>
<td>80</td>
<td>45.8</td>
</tr>
<tr>
<td>Difference (percentage points):</td>
<td>-3.5</td>
<td>-3.1</td>
<td>-3.7</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Difference in Enforcement Rates and Decomposition (percentage points)</th>
<th>All Cases</th>
<th>Unil Cases</th>
<th>Coll Cases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Actual difference in enforcement rates:</td>
<td>23.4*</td>
<td>7.1</td>
<td>14.7</td>
</tr>
<tr>
<td>Decomposition of enforcement rates:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Holding cases fixed at FTC matters</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Amount due to regime:</td>
<td>1.5</td>
<td>1.8</td>
<td>1.0</td>
</tr>
<tr>
<td>Amount due to case mix:</td>
<td>21.9*</td>
<td>5.3</td>
<td>13.7</td>
</tr>
<tr>
<td>Holding cases fixed at court matters</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Amount due to regime:</td>
<td>3.5</td>
<td>3.1</td>
<td>3.7</td>
</tr>
<tr>
<td>Amount due to case mix:</td>
<td>19.9*</td>
<td>4.0</td>
<td>11</td>
</tr>
</tbody>
</table>

* Statistically different than zero at 5% level.

Values in this row were tested for significance with a bootstrap hypothesis test.

Values in this row were tested for significance with a t-test.

Note: Number of observations is 125 for the US data and 51 for the Court data.
Table 4 – Detailed Hypothetical Enforcement Rates

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Cases</td>
<td>47</td>
<td>34</td>
<td>21</td>
<td>23</td>
</tr>
<tr>
<td>Actual FTC Enforcement Rate</td>
<td>91.5</td>
<td>73.5</td>
<td>61.9</td>
<td>52.2</td>
</tr>
<tr>
<td>Hypothetical Court Rate on FTC Matters</td>
<td>86.7</td>
<td>75.9</td>
<td>60.6</td>
<td>51.5</td>
</tr>
<tr>
<td>Difference&lt;sup&gt;a&lt;/sup&gt;</td>
<td>4.8</td>
<td>-2.4</td>
<td>1.3</td>
<td>.7</td>
</tr>
</tbody>
</table>

<sup>a</sup> We tested the differences between the Actual FTC and Hypothetical Court enforcement rates for the unilateral effects categories, with a bootstrap hypothesis test. The differences were not statistically significant. We do not test the difference in the coordinated interaction categories because the samples are small.