

The Evolution of Judicial Standards: Evidence from Litigated Merger Trials*

JEFFREY T. MACHER

Georgetown University
McDonough School of Business
Washington, DC 20057
jeffrey.macher@georgetown.edu

JOHN W. MAYO

Georgetown University
McDonough School of Business
Washington, DC 20057
mayoj@georgetown.edu

DAVID E.M. SAPPINGTON

University of Florida
Department of Economics
Gainesville, FL 32611
sapping@ufl.edu

MARK WHITENER

Georgetown University
McDonough School of Business
Washington, DC 20057
markdwhitener@gmail.com

ABSTRACT

A popular narrative suggests that, in the wake of the rise of the Chicago School, the judiciary has grown increasingly lax, making it difficult for antitrust agencies to successfully challenge mergers in court. We develop a theoretical framework to yield hypothesis regarding merger challenges, settlements and outcomes under varying judicial standards. We then undertake an empirical investigation of all mergers, challenges, and litigated outcomes in the U.S. between 1979 and 2021 to test for the presence of shifting judicial standards. Contrary to the popular narrative, we find that judicial standards have become increasingly pro-enforcement during the past four decades.

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1. Introduction

Monday-morning assessments of merger trial outcomes are common. Critics of a particular merger that is successfully blocked at trial will opine that the outcome is an indication that the judiciary has become overzealous in its interpretation of the Clayton Act's prohibition of mergers whose effects "may be substantially to lessen competition, or to tend to create a monopoly."¹ At the same time, a different adjudicated merger that is allowed to proceed will motivate critics to argue that the judiciary has become too lax in its interpretation of the Clayton Act.

Less common are attempts to systematically assess the body of merger outcomes to determine actual judicial trends and tendencies. This void has become more salient in recent years as a vocal narrative has emerged that suggests the judiciary, stimulated by the rise of the Chicago School, has become increasingly lax in its oversight of potentially anticompetitive mergers over time. To illustrate, Baker and Shapiro (2008) criticize what they interpret to be "the too-ready acceptance by some courts and enforcers of unproven noninterventionist economic arguments about concentration, entry and efficiencies" (p. 266). Similarly, Ashenfelter, Hoskens and Weinberg (2014) argue that "[b]y the late 1980s, the burden of proof required of the U.S. antitrust agencies to challenge horizontal mergers had dramatically increased" (footnote omitted) and that "[t]his increase in the evidentiary burden placed on the government has substantially limited its ability to challenge horizontal mergers" (p. S68). Kwoka (2018) opines that "[t]he judiciary is demanding ever greater proof of predicted anticompetitive outcomes. Ideological forces outside the agency have fostered an anti-antitrust view." And, more recently, *The Economist* (2019) suggests that "American antitrust regulators and courts have been unforgivably lax."²

Despite this narrative, activities undertaken by the Department of Justice (Antitrust Division) and the Federal Trade Commission (hereafter, the Agencies) as well as opinions of the courts have in a number of ways strengthened the hands of the Agencies in their prosecution of—and likely success with—merger challenges. Over the years, the successive iterations of the Horizontal

¹ 15 U.S. Code § 18

² See <https://www.economist.com/weeklyedition/2019-11-30> (p. 13). Also see Yale University's Thurman Arnold Project, which suggests that "interpretations of U.S. antitrust laws have been too lax toward consolidation" and that "Courts have lowered the burden on defendants to rebut structural presumption" (footnote omitted). See <https://som.yale.edu/centers/thurman-arnold-project-at-yale/modern-antitrust-enforcement>.

Merger Guidelines (HMGs) have introduced a variety of new analytical tools that may have significantly enhanced the ability of the Agencies to prevail in court. For instance, the earliest HMGs relied primarily on coordinated effects as the basis for challenging mergers. In 1992, the HMGs expanded the Agencies' toolkit to include challenges based on the potential for mergers to harm competition by the unilateral post-merger behavior of the surviving firm.³ In 2010, the HMGs further strengthened the Agencies' hands by broadening unilateral effects to include auction settings, capacity manipulation, and harm resulting from reduced innovation and product variety.⁴

Shapiro and Shelanski (2021) review all judicial decisions in merger cases between 2000 and 2020, and find that the 2010 HMGs have been “well accepted by the courts ... In particular, we find that the richer explanation of how the Agencies use qualitative and quantitative evidence to assess competitive effects has favorably influenced the case law and strengthened merger enforcement” (p. 53). They observe that “The case law now exhibits much greater receptivity to a government showing that the merger will lead to higher prices due to the loss of direct competition between the two merging firms” (p. 51).

Court opinions themselves have also evolved in ways that may strengthen the hands of the Agencies in merger challenges. For example, in *FTC v. HJ Heinz Co.*, 116 F. Supp. 2d 190 (D.D.C. 2000), the Court of Appeals for the District of Columbia indicated that generic claims of structural market barriers to collusion (such as the need for cartel members to agree on price and output, post-cartel incentives for participants to defect from such agreements by expanding output, and difficulties in sustaining collusive prices when firms cannot readily identify other firms' prices) are insufficient to rebut the “normal presumption” that increased market concentration raises both the incentive and ability for post-merger behaviors to increase prices. The courts have also increasingly embraced the proposition that mergers that substantially reduce or eliminate direct competition between close competitors often result in a lessening of competition.⁵ Furthermore, the Supreme Court has made it more difficult for merging parties to successfully claim a “state

³ Baker (1997) observes that this expansion was “a paradigm shift in merger analysis” (p. 21).

⁴ See <https://www.justice.gov/atr/horizontal-merger-guidelines-08192010>.

⁵ See *United States v. Anthem, Inc.*, 855 F.3d 345 (D.C. Cir. 2017), which cites both *Staples I*, 970 F. Supp. 1066, 1083 (D.D.C. 1997) and *Staples II*, 190 F. Supp. 3d 100, 131 (D.D.C. 2016) in finding that “the elimination of a particularly aggressive competitor in a highly concentrated market [is] a factor which is certainly an important consideration when analyzing possible anti-competitive effects.” And, as affirmed in *Anthem*, “this is true even where the merging parties are not the only two, or even the two largest, competitors in the market” (citations omitted).

action” immunity defense for an otherwise anticompetitive merger.⁶ In sum, while a narrative of easing judicial standards for merging firms has emerged, Agency developments and court opinions suggest something else. Whether and how judicial standards applied to mergers have shifted is thus an open question,⁷ which we examine both theoretically and empirically in this paper.

Our paper is set against the backdrop of an influential article by Priest and Klein (1984) which establishes an expectation that litigated outcomes in legal disputes will exhibit an even split—with plaintiffs winning half the cases and defendants winning half the cases. This conclusion reflects the fact that litigated cases are not a random draw of all lawsuits filed. Instead, selection effects can narrow cases that actually proceed to the litigation stage to cases that are most likely to “go either way.” Importantly, this 50/50 result has been interpreted to be independent of the stringency of – or shifts in – the judicial standard. To the extent that the Priest and Klein prediction holds, any informational value from trends in litigated case results is lost. Rather, deviations from 50/50 win-loss rates or trends in litigated outcomes over time arise from different litigant views of likely court outcomes, asymmetries in benefits and costs to litigants, or random errors. In contrast to Priest and Klein (1984), our theoretical model yields clear inferences regarding shifts in judicial standards from observable litigation challenges and outcomes.

Our analysis also bears some resemblance to Waldfogel (1995), in that we both draw conclusions about court standards from observed trial rates and court outcomes. However, our analyses have important differences. Waldfogel assumes that a plaintiff and a defendant receive distinct, imperfect information about the quality of the plaintiff’s case and the prevailing court standard. The two parties then announce their proposed terms of settlement. Settlement occurs if and only if the settlement payment the defendant offers exceeds the settlement payment the plaintiff demands.⁸ In Waldfogel’s model, variations in trial rates and court outcomes are driven by asymmetries in the payoffs the two potential litigants face if they proceed to trial and by differences in their assessments of the likelihood of prevailing in court. In our model, variations in

⁶ See *FTC v. Phoebe Putney Health Sys.*, 568 U.S. 216 (2013): the “state action” doctrine provides, subject to limitations, antitrust exemption to states and municipalities in situations in which they clearly articulate and actively supervise firms’ behaviors which would otherwise be anticompetitive. For a more complete discussion of the state action doctrine, see Areeda, Kaplow and Edlin (2013).

⁷ For a more detailed consideration of Agency and court developments that have led to this ambiguity, see Macher, Mayo, Sappington and Whitener (forthcoming).

⁸ Waldfogel (1995) shows that Priest and Klein’s (1984) model does not necessarily predict that plaintiffs and defendants typically will prevail in court with equal likelihood, regardless of the prevailing court standard.

trial rates and court outcomes are determined by the known judicial standard, by the known efficacy of the court in assessing the social harm a proposed merger would impose, and by the relationship between this social harm and the private profitability of the merger. Furthermore, in our model, a third party investigates proposed mergers and fashions remedies that would eliminate the associated social harm.⁹

Klerman and Lee (2014), Lee and Klerman (2016), and Klerman, Lee and Liu (2018) show that in the presence of incomplete settling, empirically-observed judicial outcomes can provide information about shifts in judicial standards. We derive a corresponding conclusion in a related but distinct model that is designed to capture key elements of merger review policies, which differ both structurally and practically from canonical models of settlement. We modify the standard settlement model in three primary respects to render it more relevant in the study of antitrust policy. First, unlike the standard settlement model, the plaintiff in our model (*viz.*, the Agency) must decide whether to legally challenge a proposed merger. That is, rather than taking the primary action (e.g., an accident) as given, we allow the action (whether and how to pursue a merger challenge) to be endogenous.¹⁰ Second, we depart from the standard presumption of directly opposing interests of two potential litigants by introducing an antitrust authority that seeks to maximize social welfare whereas the merging parties seek to maximize profit. Third, we allow the antitrust authority to identify merger remedies (e.g., asset divestitures) that mitigate the social harm from proposed mergers.

This paper makes three contributions. First, it provides a theoretical framework within which shifts in judicial standards can be inferred from the actions of firms, agencies, and the courts. Second, on the empirical front, the paper overcomes an historical impediment to making inferences regarding shifts in judicial standards caused by sample selection hurdles. In the case of mergers, interpretations of judicial outcomes as reflective of shifts in judicial standards can be confounded by the possibility that litigated merger cases might be a nonrandom set of all challenged mergers. Moreover, the observed set of challenged mergers is not likely to be a random sample of all mergers reported to the Agencies. Our empirical analysis addresses these sample selection issues

⁹ Waldfogel (1998) finds that models of divergent expectations with settlement (e.g., Priest and Klein, 1984; Waldfogel, 1995) better explain observed data than do models in which one potential litigant has better information than the other about the likelihood the plaintiff will prevail in court.

¹⁰ See also Shavell (1982), who extends the basic settlement model by endogenizing the plaintiff's decision about whether to bring suit.

directly to provide consistent estimates of merger challenges, litigated cases, and judicial outcomes. Third, the paper provides systematic rather than anecdotal evidence on shifts in judicial standards based upon the population of all mergers reported to the Agencies between 1979 and 2021. Contrary to the popular narrative, the empirical results indicate that the pattern of merger challenges that have proceeded to trial and the evolution of trial outcomes reflect judicial standards that have become increasingly pro-enforcement during the past four decades.

The paper proceeds as follows. Section 2 provides a conceptual framework that produces propositions that link actions by the Agencies, merging firms, and the courts to shifts in judicial standards. Section 3 presents the empirical analysis, which utilizes the population of all proposed mergers, agency challenges, fully-litigated merger trials, and trial outcomes in the United States between 1979 and 2021. Section 4 provides concluding comments.

2. Model

As described in the previous section, a narrative has arisen regarding the influence of the Chicago School on merger enforcement outcomes in the United States. However, the evolution of antitrust tools, presumptions, and case law make the ultimate veracity of the narrative an open question for at least two reasons. First, the primary variable of interest – shifts in judicial standards – is not directly measurable. Second, the most direct potential indicator of changes in judicial standards – courts’ decisions to allow or disallow mergers – is only the last stage in a series of actions, which complicates the identification of changes in judicial standards.

Figure 1 identifies several steps that ultimately produce the observed outcome of litigated merger challenges. First, among the set of all conceivable mergers, only some mergers are proposed. Second, among the set of proposed mergers, the Agencies challenge some and decline to challenge others. Third, among the set of challenged mergers, only a subset proceed to litigation. Finally, among the set of litigated mergers, the Agencies win some and lose others.

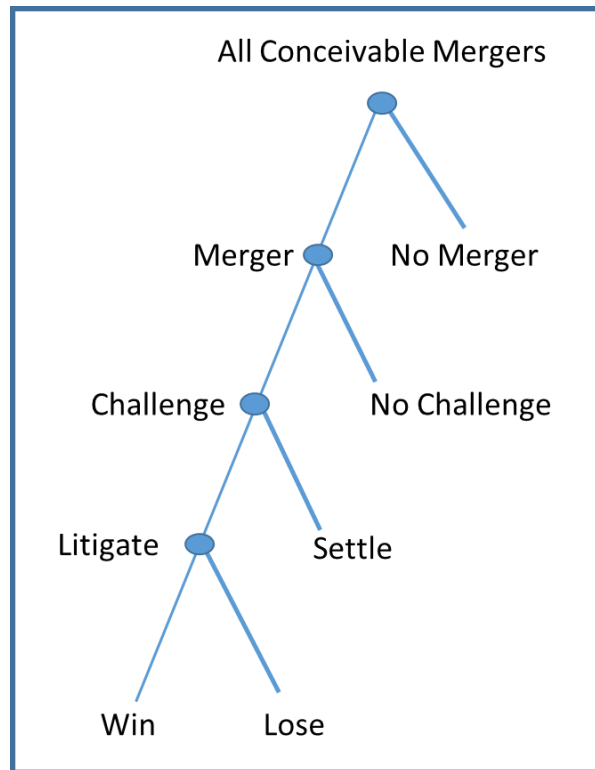


Figure 1. Antecedents to Outcomes of Litigated Merger Challenges

Changes in judicial standards can potentially affect each of the steps identified in Figure 1, which confound simple interpretations of observed outcomes. For instance, an increasing Agency win rate for litigated mergers might invite the simple interpretation that the courts' judicial standards have become more pro-enforcement (more stringent) over time. This interpretation, however, may be too simplistic. For instance, the pool of litigated mergers may itself be affected by, among other things, the evolution of judicial standards. In this event, Agency win rates may be determined in part by the altered mix of innocuous and anticompetitive mergers that proceed to trial, confounding the interpretation of changing Agency win rates as attributable to changing judicial standards. Similarly, to the extent that merger challenges are not exogenously determined but rather reflect Agency choices, merger enforcement intensity may rise or fall depending not on the stringency of judicial standards but on changing governmental policies. It is also conceivable that the observed set of mergers itself is endogenous to judicial standards, with more mergers being proposed under lax judicial standards and fewer proposed under tight standards. The presence of these stages creates sample-selection challenges that we address.

Our formal model considers the activities of a court, an antitrust authority (A), and the parties to a potential merger (M). We describe the model in three steps. First, we review the court's operation. Second, we describe A 's activities. Finally, we characterize M 's behavior.

If a proposed merger is challenged in court, the court proceeding generates a signal $s \in [\underline{s}, \bar{s}]$ about the social harm $h \in [\underline{h}, \bar{h}]$ the proposed merger would generate. This harm might reflect, for example, the amount by which the proposed merger would reduce consumer welfare by facilitating higher prices for M 's products. h can be negative, reflecting the fact that synergies can enable merging parties to set lower prices or deliver higher levels of product quality.¹¹

Higher realizations of the court signal indicate that the proposed merger is likely to impose relatively pronounced social harm. Consequently, the court prohibits a merger if the signal generated during the court proceeding exceeds a specified threshold, $\hat{s}$. Otherwise, the court permits the proposed merger to proceed. Thus, $\hat{s}$ can be viewed as the stringency of the court's standard in reviewing proposed mergers. A smaller value of $\hat{s}$ denotes a more stringent standard (or a more "pro-enforcement" standard) in the sense that the court is more likely to prohibit a proposed merger as $\hat{s}$ declines, *ceteris paribus*.

The association between the court signal and the actual social harm from a merger is captured by $F(s|h)$, the distribution function for s , given h . $f(s|h)$ is the corresponding conditional density function.¹² We assume $\frac{\partial F(\hat{s}|h)}{\partial h} < 0$ for all $\hat{s} \in (\underline{s}, \bar{s})$ and $h \in [\underline{h}, \bar{h}]$, so the probability of a court signal below the threshold $\hat{s}$ declines as h increases. Intuitively, as the actual harm a proposed merger would cause increases, the likelihood the court will approve the proposed merger declines, holding the prevailing judicial standard constant. In this sense, the court signal is informative (but not perfectly so) about the social harm the proposed merger would actually cause if it were consummated.

$g(h)$ denotes the density function for h and $G(h)$ denotes the corresponding distribution function. Variation in h can arise from several factors, including the prevailing industry structure, the price elasticity of demand for M 's products, and potential synergies in M 's operations. We

¹¹ A merger might also increase welfare by motivating non-merging parties to compete more vigorously.

¹² $f(\cdot)$ is assumed to have strictly positive support on $[\underline{s}, \bar{s}]$ for all $h \in [\underline{h}, \bar{h}]$. $F(\cdot)$ is assumed to be continuously differentiable for all $s \in [\underline{s}, \bar{s}]$ and $h \in [\underline{h}, \bar{h}]$.

assume $g(h)$ and $G(h)$ are not affected by the court's choice of $\hat{s}$. Thus, the variation in the social harm from potential mergers is presumed to be driven primarily by industry and firm characteristics (e.g., market structure and potential synergies) rather than by the strategic choices of potential merging parties, which in turn might be affected by $\hat{s}$.¹³

The mergers that entail the greatest social harm are the most profitable for M . This positive association between h and $\pi(h)$ arises naturally when, for example, a merger enhances the ability of the merging parties to increase prices profitably. Formally, we assume $\pi'(h) \geq 0$ for all $h \in [\underline{h}, \bar{h}]$, where $\pi(h)$ is the incremental profit M would derive from a merger that generates social harm h in the absence of any action to mitigate the harm (e.g., asset divestiture).

We now describe the operations of the antitrust authority (A). A first performs an initial review of all proposed mergers and ranks them according to the social harm (h) they would cause if they were implemented without mitigation. A then performs a more detailed investigation of as many of the proposed mergers as possible, given the resource constraint it faces. A 's detailed investigations focus on the mergers that would cause the most social harm, i.e., those with the highest values of h . For each proposed merger it investigates in detail, A identifies a remedy that would eliminate the social harm from the merger. A does not contest a proposed merger if M implements the identified remedy, thereby eliminating the social harm from the proposed (modified) merger.¹⁴ In contrast, A challenges any proposed merger for which it has identified a remedy if M declines to implement the remedy.

In the absence of a binding resource constraint, A conducts a detailed review of and identifies a remedy for each proposed merger that would impose social harm, i.e., for each merger with $h \in (0, \bar{h}]$.¹⁵ A conducts a detailed review of fewer proposed mergers when its resource constraint

¹³ This assumption is supported by the empirical finding of Clougherty and Seldeslacht (2013) that changes in the likelihood of prevailing in court do not affect the frequency or composition of mergers. Furthermore, if judicial standards materially affected the distribution of social harm from proposed mergers, we would expect more lax standards to increase the propensity for competitively harmful mergers (often taking the form of horizontal mergers). In fact, Agency-available data at the three-digit NAICS level since 2000 report a substantial decline in the ratio of horizontal mergers to total mergers (from 59 percent in 2000 to 13 percent in 2020). Although not dispositive, these data suggest that our assumption is conservative for the establishment of our findings below.

¹⁴ When M implements the remedy, its incremental profit from the (modified) merger is $\pi(0)$. This profit can be positive, perhaps because the merger as modified by the remedy still generates synergies that reduce costs, thereby increasing profit even in the absence of price increases.

¹⁵ No remedies are specified for a proposed merger with $h \leq 0$. A does not contest any such merger.

binds. Formally, in this case, A conducts a detailed review of and identifies remedies for proposed mergers with $h \in (h_r, \bar{h}]$, where $h_r \in [0, \bar{h}]$.¹⁶ h_r increases as A 's resource constraint becomes more binding. A does not contest the proposed mergers it is unable to review in detail. These are the mergers that entail relatively small or no social harm, i.e., those with $h \in [\underline{h}, h_r]$.

We now analyze M 's choice between implementing the remedy that A identifies or proceeding to court when $h \in (h_r, \bar{h}]$.¹⁷ To do so, let $B(h, \hat{s})$ denote M 's expected private benefit from proceeding to court, given h and $\hat{s}$. This expected benefit is the difference between: (i) the product of the probability the merger is approved (because the court signal does not exceed $\hat{s}$) and M 's incremental profit from the merger; and (ii) M 's cost of participating in a court proceeding, $K_M > 0$. Formally:

$$B(h, \hat{s}) = F(\hat{s} | h) \pi(h) - K_M. \quad (1)$$

Let $\tilde{h} \in (h_r, \bar{h})$ denote the value of h at which M is indifferent between proceeding to court and implementing the remedy identified by A .¹⁸ Formally, $\tilde{h}$ is defined by:¹⁹

$$B(\tilde{h}, \hat{s}) = \pi(0). \quad (2)$$

To determine how a change in the court standard ($\hat{s}$) affects M 's decision about whether to proceed to court, we differentiate equation (2) to obtain:

$$\frac{\partial B(\tilde{h}, \hat{s})}{\partial \tilde{h}} d\tilde{h} + \frac{\partial B(\tilde{h}, \hat{s})}{\partial \hat{s}} d\hat{s} = 0 \quad \Rightarrow \quad \frac{d\tilde{h}}{d\hat{s}} = -\frac{\partial B(\tilde{h}, \hat{s})/\partial \hat{s}}{\partial B(\tilde{h}, \hat{s})/\partial \tilde{h}}. \quad (3)$$

Because M only pursues mergers for which $\pi(\cdot) > 0$, equation (1) implies:

$$\frac{\partial B(\tilde{h}, \hat{s})}{\partial \hat{s}} = f(\hat{s} | \tilde{h}) \pi(\tilde{h}) > 0; \text{ and} \quad (4)$$

¹⁶ The setting in which A faces no resource constraints is the special case of our model in which $h_r = 0$.

¹⁷ M consummates any proposed merger (with $\pi(\cdot) \geq 0$) that A does not challenge.

¹⁸ We assume $\tilde{h} \in (h_r, \bar{h})$ to avoid the uninteresting and empirically irrelevant settings in which the merging parties always or never agree to implement the remedies A identifies.

¹⁹ Equation (2) considers the case in which $\pi(0) \geq 0$, so M 's incremental profit from the modified merger as proposed by A (with $h = 0$) is at least as great as the profit M would secure if it decided not to pursue the merger. If $\pi(0) < 0$, then M prefers to abandon the merger rather than proceed with the modified merger. In this case, $\tilde{h}$ can be defined by $B(\tilde{h}, \hat{s}) = 0$ and interpreted as the value of h at which M is indifferent between proceeding to court and abandoning the modified merger. More generally, $\tilde{h}$ can be defined by $B(\tilde{h}, \hat{s}) = \max \{0, \pi(0)\}$ and interpreted as the value of h at which M is indifferent between proceeding to court and either abandoning the modified merger or implementing the modified merger, whichever is more profitable. The key qualitative conclusions drawn below persist under this more general formulation. Merger abandonment is not modeled formally for expositional ease.

$$\frac{\partial B(\tilde{h}, \hat{s})}{\partial \tilde{h}} = \frac{\partial F(\hat{s} | \tilde{h})}{\partial \tilde{h}} \pi(\tilde{h}) + F(\hat{s} | \tilde{h}) \pi'(\tilde{h}). \quad (5)$$

Equation (4) states that M 's expected benefit from proceeding to court ($B(\cdot)$) declines as the court standard becomes more stringent (i.e., as $\hat{s}$ declines), holding h constant. The more stringent (i.e., the more pro-enforcement) standard reduces the likelihood that M prevails in court, which reduces $B(\cdot)$.

Equation (5) indicates that h has two effects on $B(\cdot)$. First, an increase in h reduces the likelihood that M prevails in court because court signals below $\hat{s}$ become less likely. Second, if $\pi'(h) > 0$, an increase in h increases M 's profit from the merger (if approved). These two countervailing effects imply that, in principle, $B(\cdot)$ can either increase or decrease as h increases.

The ensuing analysis focuses on the setting in which $B(\cdot)$ increases with h (so $\frac{\partial B(h, \hat{s})}{\partial h} > 0$ for all $h \in [\underline{h}, \bar{h}]$).²⁰ This setting prevails when, for example, M 's incremental profit from the merger is highly correlated with the social harm from the merger (so $\pi'(h)$ is relatively large) and the outcomes of court proceedings exhibit considerable randomness (so $\left| \frac{\partial F(\hat{s} | h)}{\partial h} \right|$ is relatively small). As h increases under these circumstances, M 's payoff when it prevails in court increases more rapidly than its probability of prevailing in court declines. Consequently, M 's expected private benefit from proceeding to court increases as h increases.

To rule out the uninteresting (and empirically irrelevant) case in which no proposed merger ever proceeds to court, we assume $B(\bar{h}, \hat{s}) > \pi(0)$. This assumption simply states that for the most profitable potential merger, M 's expected private benefit from proceeding to court exceeds its profit from implementing the remedy.²¹

The timing in the model is as follows. After $\hat{s}$ is established, parties to potential mergers learn how profitable the mergers would be. Parties with profitable potential mergers ($\pi(\cdot) \geq 0$) then announce their intention to merge. Next, A reviews the announced mergers and launches detailed investigations of the mergers that would impose the most social harm (h) if implemented as proposed, i.e., those with $h > h_r$. A declines to challenge mergers with $h \leq h_r$. For proposed mergers with $h > h_r$, A identifies a remedy that would eliminate the social harm from the merger.

²⁰ Macher, Mayo, Sappington and Whitener (2022) analyze the setting in which $\frac{\partial B(h, \hat{s})}{\partial h} \leq 0$ for all $h \in [\underline{h}, \bar{h}]$. M always implements the remedy and never proceeds to court in this (empirically irrelevant) setting.

²¹ If $B(\bar{h}, \hat{s}) < \pi(0)$, M accepts the proposed remedy for all $h \in [h_r, \bar{h}]$.

A also signals its intention to challenge the merger in court if M does not implement the remedy. Next, M decides whether to implement the remedy (and thereby secure profit $\pi(0)$) or proceed to court. Finally, if M proceeds to court, the court proceeding generates signal s . The merger is approved if $s \leq \hat{s}$. The merger is precluded if $s > \hat{s}$.

Because M 's expected private benefit from proceeding to court increases with h , M proceeds to court rather than implement the remedy once h (and thus $\pi(h)$) becomes sufficiently large such that the expected payoff from proceeding to court outweighs the litigation costs (K_M). This observation is stated formally in Finding 1.

Finding 1. M implements the remedy if $h \in (h_r, \tilde{h}]$ whereas M proceeds to court if $h \in (\tilde{h}, \bar{h}]$.

Proof. Because M 's expected private benefit from proceeding to court increases as h increases, equations (1) and (2) imply:

$$\begin{aligned} B(h, \hat{s}) &< B(\tilde{h}, \hat{s}) = \pi(0) \quad \text{for } h \in (h_r, \tilde{h}) \text{ and} \\ B(h, \hat{s}) &> B(\tilde{h}, \hat{s}) = \pi(0) \quad \text{for } h \in (\tilde{h}, \bar{h}]. \end{aligned} \quad (6)$$

Equation (6) implies that M 's expected payoff is higher: (i) when it implements the remedy than when it proceeds to court if $h \in (h_r, \tilde{h})$; and (ii) when it proceeds to court than when it implements the identified remedy if $h \in (\tilde{h}, \bar{h}]$. ■

Figure 2 depicts M 's expected payoff, $E\{\Pi(h)\}$, as a function of h . The figure reflects the facts that: (i) the proposed merger is not challenged, so M 's payoff is $\pi(h)$, if $h \in [\underline{h}, h_r]$; (ii) $\pi'(h) \geq 0$; (iii) M implements the remedy and thereby secures payoff $\pi(0)$ if $h \in (h_r, \tilde{h}]$; and (iv) M proceeds to court if $h \in (\tilde{h}, \bar{h}]$; and (v) $B(h, \hat{s})$ increases with h .

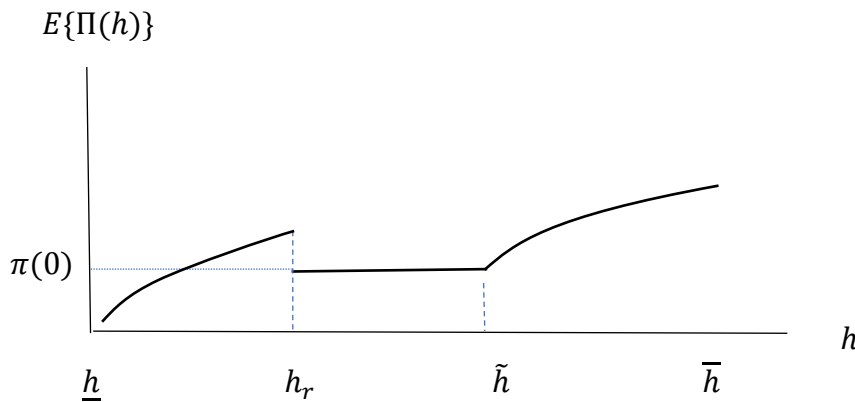


Figure 2. M's Expected Payoff, $E\{\Pi(h)\}$, as Social Harm h Varies

We seek to determine how changes in the stringency of the court standard ($\hat{s}$) affect the likelihood that M proceeds to court and the likelihood that M prevails in court. Finding 2 reports that M becomes less likely to proceed to court (i.e., $\tilde{h}$ increases) as the court standard becomes more stringent (i.e., as $\hat{s}$ declines).

Finding 2. $\frac{d\tilde{h}}{d\hat{s}} < 0$ for all $\tilde{h} \in (\underline{h}, \bar{h})$.

Proof. The conclusion follows directly from equations (3) and (4) because $\frac{\partial B(h, \hat{s})}{\partial h} > 0$ for all $h \in [\underline{h}, \bar{h}]$. ■

Finding 2 holds because a more stringent (or more pro-enforcement) standard reduces the likelihood that M prevails in court, holding h constant. Therefore, as $\hat{s}$ declines, M will only proceed to court if it anticipates a larger profit when it prevails in court. M anticipates a larger such profit when h (and thus $\pi(h)$) increases.

Figure 3 illustrates Finding 2. The $B(h, \hat{s}_c)$ curves in the figure slope upward because $B(h, \hat{s})$ increases with h . Equation (4) implies that $B(h, \hat{s})$ declines as the court standard becomes more stringent, holding h constant. Therefore, the $B(h, \hat{s})$ curve shifts downward as $\hat{s}$ declines in Figure 3. Consequently, as $\hat{s}$ declines from $\hat{s}_0$ to $\hat{s}_1$, the critical value of h at which M is indifferent between proceeding to court and implementing the remedy increases from $\tilde{h}_0$ to $\tilde{h}_1$ in Figure 3.

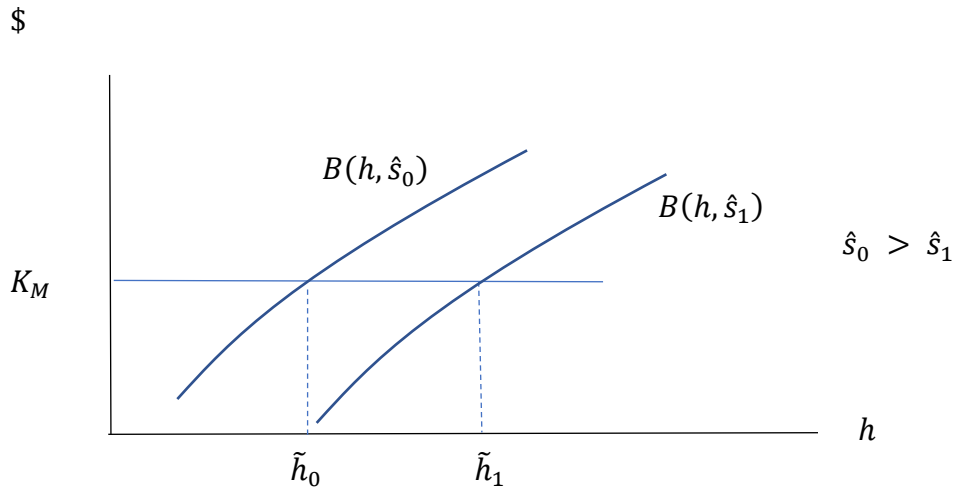


Figure 3. The Impact of a More Stringent Court Standard.

Findings 1 and 2 imply that a more stringent (or more pro-enforcement) court standard has two effects. First, fewer proposed mergers proceed to court (because M proceeds to court only if $h \in (\tilde{h}, \bar{h}]$ and $\tilde{h}$ increases as $\hat{s}$ declines). Second, the expected social harm from potential mergers that proceed to court increases (i.e., $E\{h \mid h \in (\tilde{h}, \bar{h}]\}$ increases as $\hat{s}$ declines). This second effect arises because a more stringent court standard discourages M from proceeding to court unless the potential gain from prevailing in court is sufficiently pronounced. This potential gain ($\pi(h)$) will be large precisely when the merger would cause substantial social harm. Therefore, as the court standard becomes more stringent, the expected social harm from proposed mergers that proceed to court increases.

Findings 1 and 2 also imply that a more stringent court standard reduces the probability that M prevails in court for two reasons. First, holding h constant, the more stringent standard reduces the probability that a proposed merger is approved by the court. Second, the more stringent standard increases the expected social harm from proposed mergers that proceed to court. The increased expected harm also reduces the probability that a proposed merger is approved by the court. These observations (and Finding 2) underlie the following propositions.

Proposition 1. *As the court standard becomes more (less) stringent, the probability that M accepts the identified remedy increases (declines).*

Proof. A identifies a remedy for all $h \in (h_r, \bar{h}]$ and M accepts the identified remedy if $h \in (h_r, \tilde{h}]$. (Recall Finding 1.) Therefore, the probability that M accepts an identified remedy is $\frac{G(\tilde{h}) - G(h_r)}{1 - G(h_r)}$.

This fraction is less than 1 (because $\tilde{h} < \bar{h}$). Observe that:

$$\frac{d}{d\hat{s}} \left(\frac{G(\tilde{h}) - G(h_r)}{1 - G(h_r)} \right) = \left[\frac{g(\tilde{h})}{1 - G(h_r)} \right] \frac{d\tilde{h}}{d\hat{s}} < 0. \quad (7)$$

The inequality in expression (7) holds because $\frac{d\tilde{h}}{d\hat{s}} < 0$, from Finding 2. Expression (7) implies that the probability M accepts an identified remedy increases as $\hat{s}$ declines. ■

Proposition 2. *As the court standard becomes more (less) stringent, the probability that M prevails in court declines (increases).*

Proof. The probability that M prevails in court, given h and $\hat{s}$, is $F(\hat{s}|h)$. Therefore, because M proceeds to court if and only if $h \in (\tilde{h}, \bar{h}]$ (recall Finding 1), the probability that M prevails in court, given $\hat{s}$, is $\int_{\tilde{h}}^{\bar{h}} F(\hat{s}|h) g(h) dh$. Because $g(h)$ is not affected by $\hat{s}$:

$$\frac{d}{d\hat{s}} \int_{\tilde{h}}^{\bar{h}} F(\hat{s}|h) g(h) dh = \int_{\tilde{h}}^{\bar{h}} f(\hat{s}|h) g(h) dh - F(\hat{s}|\tilde{h}) g(\tilde{h}) \frac{d\tilde{h}}{d\hat{s}} > 0. \quad (8)$$

The inequality in expression (8) holds because $\frac{d\tilde{h}}{d\hat{s}} < 0$, from Finding 2. Expression (8) implies that the probability M prevails in court declines as $\hat{s}$ declines ■

In summary, M proceeds to court if h (and thus $\pi(h)$) is sufficiently large. M implements the remedy for smaller values of h . A more stringent court standard (i.e., a reduction in $\hat{s}$) reduces M 's expected payoff in court, holding h constant. Therefore, M implements the remedy more often (i.e., for a broader range of relatively low h realizations) as $\hat{s}$ declines. Because the expected social harm from a proposed merger that proceeds to court increases as $\hat{s}$ declines, the probability that M prevails in court declines as the court standard becomes more stringent (or more pro-enforcement).

3. Empirical Estimation: Agency Challenges, Litigation and Judicial Outcomes

Data

Our theoretical framework informs our empirical test of whether and in what direction judicial standards applied to merger challenges have evolved. To conduct this test, we first assemble data on the population of all mergers reported to the Agencies over 1979-2021. These data are drawn from the set of annual competition reports produced by the Agencies pursuant to the Hart-Scott-Rodino Antitrust Improvement Act of 1976 (HSR).²² Beginning in 1979, the annual reports include data on all merger filings. Firms reported 84,386 HSR mergers between 1979 and 2021. We construct an annual measure (*HSR MERGERS*) of the number of reported HSR mergers. Figure 4 indicates substantial heterogeneity in the number of HSR mergers over the sample period.

²² These Annual Reports are available at: <https://www.ftc.gov/policy/reports/policy-reports/annual-competition-reports>.

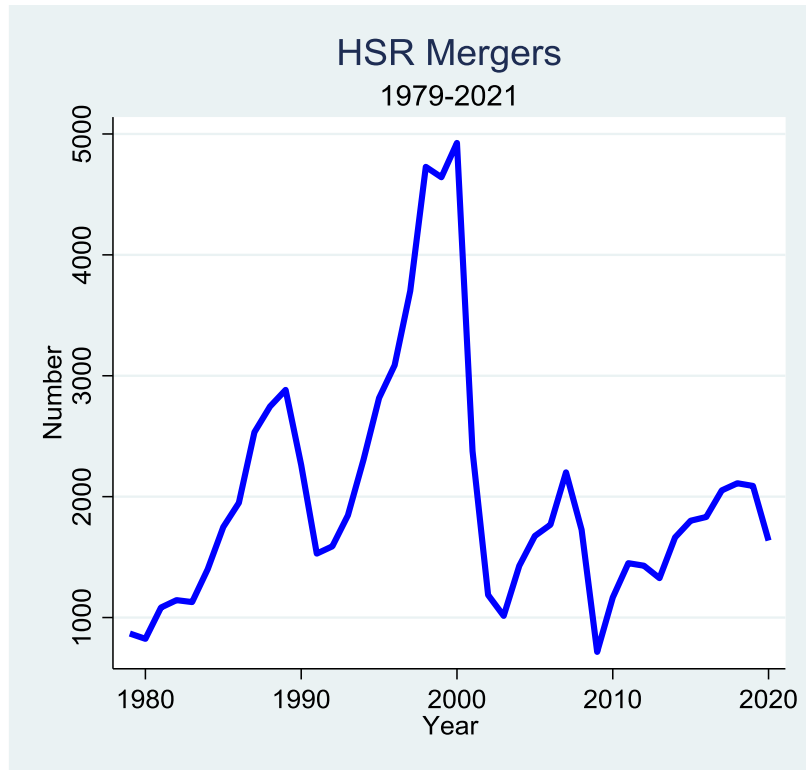


Figure 4 — Reported HSR Mergers

Notes: This figure shows the annual number of reported HSR mergers. It uses information in the FTC and Department of Justice Annual Competition Reports. HSR merger data are available over 1979-2021. The fiscal year is reported on the horizontal axis. The large changes in HSR mergers appear mainly due to economic recessions, which subsequently reduce the number of (HSR-eligible or otherwise) mergers. For instance: (1) in 1982, a global economic recession and stagflation resulted from the 1973 oil crisis and 1979 energy crisis; (2) in 2003, a shallow recession emerged from the dot-com bubble, falling investments, and 09/11; and (3) in 2009, the “great recession” resulted from the subprime mortgage and global financial crisis.

The annual reports also describe the merger enforcement activities of the Agencies. In particular, data on formal complaints filed as well as preliminary injunctions, agencies’ declarations of intentions to challenge mergers, and agencies’ filings of consent decrees that reflect adjustments designed to remove anticompetitive concerns regarding as-proposed mergers—more indirect manifestations of merger enforcement—are included.²³ We aggregate these formal and

²³ This measure is consistent with the Baker and Shapiro (2008) enforcement action measure which includes “court cases, consent settlements, and transactions abandoned or restructured prior to filing a complaint as a result of an announced challenge” (p. 245). Our measure is also akin to the Leary (2002) merger enforcement intensity measure, although the author’s narrative description indicates that post-second request merger abandonments are included as an indicator of merger challenges. We more conservatively exclude post-second request merger challenge abandonments in our measure for two reasons. First, such abandonments can occur for reasons independent of merger challenges. See, e.g., Coate (2018) who indicates that “[n]ot all matters [are] withdrawn from review due to antitrust concerns, as any other impediment to the transaction can force the withdrawal

informal complaints into a measure of the number of merger enforcement challenges (*CHALLENGES*). Over 1979-2021, the Agencies challenged 1,509 of the 84,386 HSR mergers. We then construct an annual merger challenge rate measure (*MC RATE*), which is the ratio of Agency challenges to reported HSR mergers. Figure 5 indicates that, following initial declines in the 1980s, the merger challenge rate has increased from under one percent in the late-1980s to more than two percent in recent years.

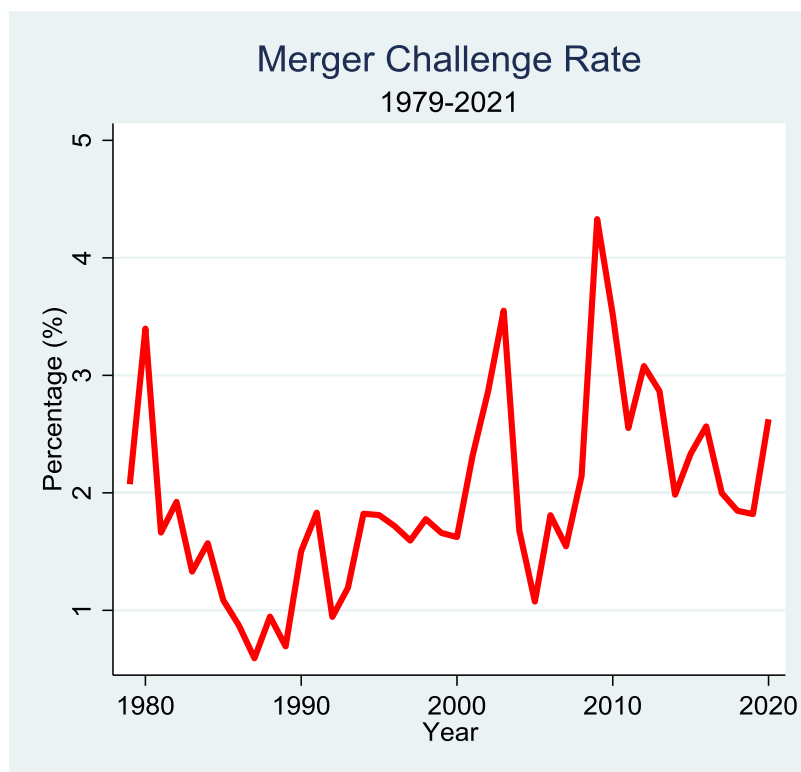


Figure 5 — Agency Merger Challenge Rate

Notes: This figure presents the annual merger challenge rate (MC Rate) stated as a percentage, reflecting the ratio of Agency challenges to reported HSR mergers. It uses information in the FTC and Department of Justice Annual Competition Reports. HSR merger and challenge data are available over 1979-2021. The fiscal year is reported on the horizontal axis.

We also rely on the annual competition reports to identify whether challenged mergers are fully-litigated. Merging parties resolve challenges either by: (1) restructuring the merger to

decision. Examples include an inability to obtain other regulatory approvals, losing a bidding war for the target, or failure to obtain financing. Antitrust concerns appear to be the most likely reason for abandonment, although non-antitrust concerns regularly lead to the abandonment of the merger” (p. 4). Second, as a practical matter, the annual reports do not consistently provide data on the number of mergers abandoned in the wake of second requests by the Agencies.

eliminate the competitive concerns; or (2) abandoning the merger altogether. Settlements constitute the vast majority of all challenges: 1,415 of the 1,509 challenges (approximately 94 percent) over the sample period were resolved or abandoned. We construct annual measures of the number of litigated cases (*LC NUM*) and the litigated case rate (*LC RATE*), which represents the ratio of litigated cases to Agency challenges. Figure 6 indicates that the litigated case rate by decade has decreased markedly: from roughly 14 percent during 1980-1989 to between four and five percent in subsequent decades.

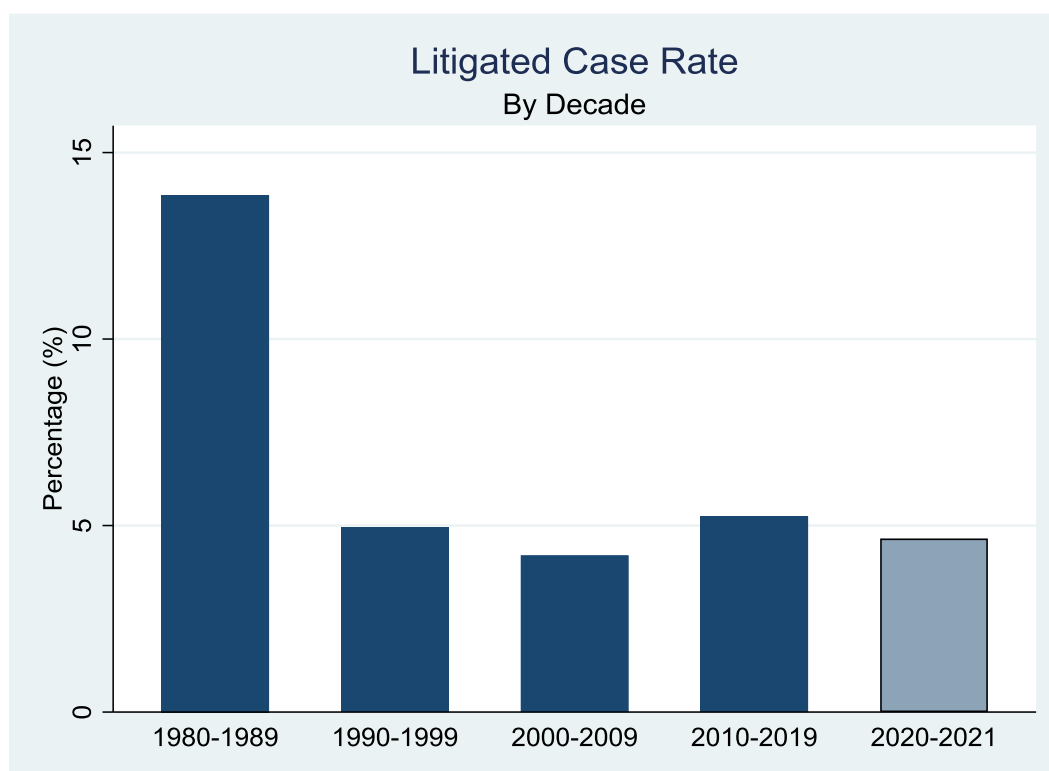


Figure 6 — Litigated Case Rate

Notes: This figure shows the percentage of reported HSR mergers that are challenged by the Agencies and proceed to trial by decade. It uses information in the FTC and Department of Justice Annual Competition Reports. The fiscal year is shown on the horizontal axis. There are several fiscal years where challenges did not result in a fully-litigated trial (viz., 1983, 1987, 1999, 2001, 2006, and 2010). The current decade (viz., 2020-2021) reflects only two years of information (43 challenges and two trials) and is color-coded to denote its small sample size.

We also employ the annual reports to identify both the population of fully-litigated mergers and the outcome of each such merger. In instances where complete information is not available in the HSR Annual Competition Reports, we use the Westlaw database to determine the litigation outcome. We review each case to ensure that it was fully litigated and judged on the antitrust

merits. To be considered “litigated,” a Federal District or Appellate Court must have evaluated the challenge and rendered a verdict. We do not include challenges where the court only issued a final judgment reflecting that a settlement was reached by the parties.

We identify 94 unique mergers litigated by the DOJ or the FTC that meet these criteria during the sample period. For each litigated merger, we record the fiscal year (*FISC YEAR*) in which the merger was officially challenged by the Agencies; the decision year (*DEC YEAR*) in which the case completed; the case name (e.g., “Federal Trade Competition v. The Coca-Cola Co.”); the agency involved (*FTC* or *DOJ*); the judges involved; and the trial outcome (i.e., *TRIAL WON* is one if the Agency prevailed at trial and is zero otherwise).

Among the set of litigated mergers, most were decided at the District Court level but some were ultimately decided by an Appeals Court or the Supreme Court. In those instances, we use the highest court that decided the litigated case on the merits for purposes of determining which party won. Appeals that were remanded back to a lower court for a decision on the merits or appeals that only determine the court’s own jurisdiction are not considered. We also do not include proceedings entirely contained within the FTC’s administrative procedures (i.e., trials in front of an administrative law judge and appeals to the full FTC heard by the FTC Commissioners) that are not appealed to a Circuit Court. We include both preliminary injunction hearings and trials on the merits. We do not include the issuance of a temporary restraining order because such issuances typically are far less burdensome for the government to secure.

Figure 7 shows the percentage by decade of fully-litigated merger cases won by the Agencies, categorized by case decision year. The Agency win rate has increased markedly: from 34.6 percent during 1990-1999 to 71.4 percent during 2000-2009 and 81.0 percent during 2010-2019. The current decade reflects only two years of information (i.e., four trials) and is color-coded to denote its small sample size.

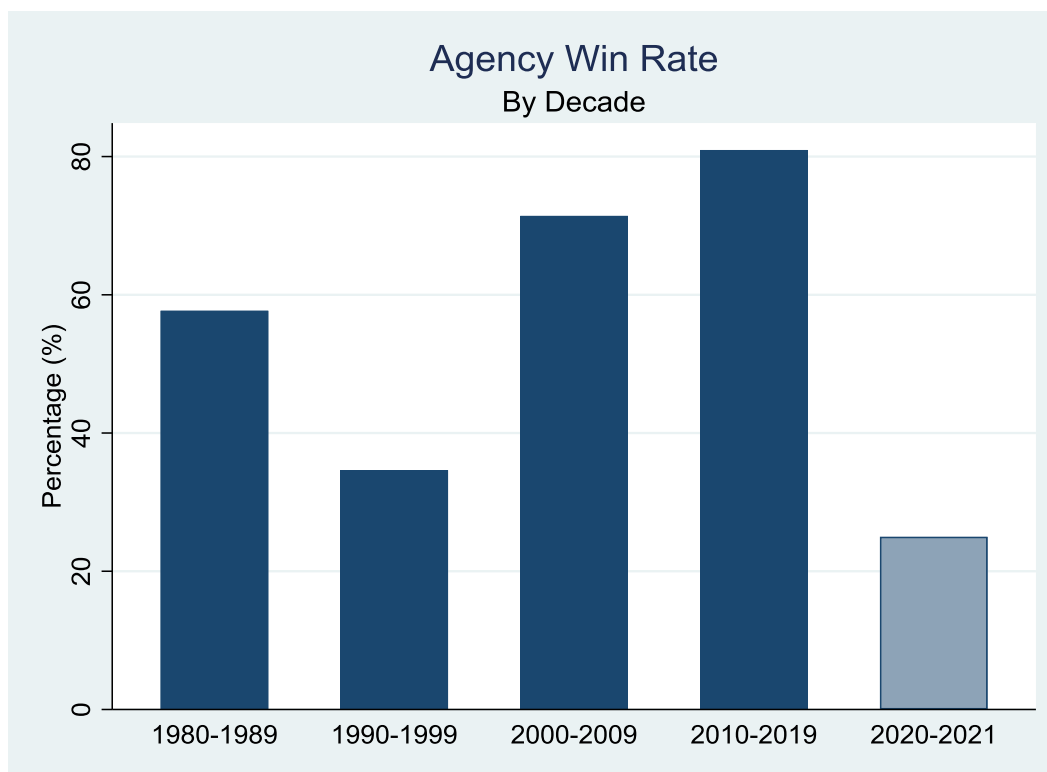


Figure 7 — Agency Win Rate

Notes: This figure shows the percentage of fully-litigated merger cases won by the Agencies (*TRIAL WON*) in litigated mergers by decade. It uses information in the FTC and Department of Justice Hart-Scott-Rodino (HSR) Annual Reports and the WestLaw database. The decision year is shown on the horizontal axis. The current decade (viz., 2020-2021) reflects only two years of information and is color-coded to denote its small sample size.

All federal judges are appointed by the president and confirmed by the United States Senate. To account for the possibility that judge ideology may independently influence merger trial outcomes, we collect data on the political party of the president who appointed each judge that rendered one or more merger decisions.²⁴ These data are drawn from the Federal Judicial Center’s Biographical Directory of Article III Federal Judges.²⁵ *DEM JUDGE* is an indicator variable that equals one if the judge writing the majority opinion for the merger trial decision was appointed by a Democratic President and is zero otherwise. Where there are multiple judges (appellate decisions), the party of the president that appointed the judges issuing each majority opinion is

²⁴ Our measure follows the standard approach for capturing judicial ideology (Goldman, 1999).

²⁵ See <https://www.fjc.gov/history/judges>.

listed (regardless of whether the opinion was written by a judge appointed by a Republican or Democrat president).

Per our theoretical model, we collect data on the annual budgets of the Antitrust Division of the DOJ and FTC to control for any binding resource constraint.²⁶ We aggregate these data into a single measure (*BUDGET*) and adjust it for inflation using the Consumer Price Index.

Finally, we incorporate data on the potential influence of each presidential administration. We create a count variable (*ADMIN*) that increments by one for each successive administration during the 1979-2021 time period.²⁷ We utilize a set of indicator variables to reflect each administration in the estimation. Following Leary (2002) and Baker and Shapiro (2008), we account for the fact that it takes time for a new administration to put its political leadership team in place at the Agencies. To do so, we count (the influence of) each administration as beginning one year after its formal start date and ending one year after its official end date.

Descriptive Statistics

Table 1 provides descriptive statistics of key variables during the 1979-2021 sample period. The number of mergers that meet HSR reporting requirements ranges from 716 to 4,926, with a mean of just more than 2,000 per fiscal year. Of these proposed mergers, the Agencies collectively challenge roughly 36 per fiscal year on average. As is apparent from this statistic and as shown in Figure 5, the number of Agency challenges is small relative to the number of HSR mergers. Of these Agency challenges, slightly more than two per year on average (roughly eight percent) proceed to trial. Inflation-adjusted agency budgets average \$380 million over the sample period. Table 2 provides correlation statistics at the fiscal year-level for these measures. Agency challenges of HSR mergers (*CHALLENGES*) and the merger challenge rate (*MC RATE*) are positively correlated with fiscal year, while fully-litigated cases (*LIT CASES*) and the litigated case rate (*LC RATE*) are negatively correlated. Fiscal year and reported HSR mergers are uncorrelated. Agency budgets (*BUDGET*) increase over time.

²⁶ Appropriation data for the DOJ are found at <https://www.justice.gov/atr/appropriation-figures-antitrust-division>. Similar appropriation data for the FTC are found at <https://www.ftc.gov/about-ftc/bureaus-offices/office-executive-director/financial-management-office/ftc-appropriation>.

²⁷ Because the Carter administration is only available for two years, our main estimation combines these data with the Reagan administration in creating an administration baseline. We examine this further in empirical robustness tests.

Table 3 provides descriptive statistics of the set of fully-litigated trials. The average annual number of trials ranges from 0.07 during the Trump administration to 0.34 during the “Reagan administration” (which includes the last two years of the Carter administration). On average, the Agencies win 56 percent of the fully-litigated trials, and a Democrat-appointed judge presides in 41 percent of the trials. Table 4 provides correlation statistics for these variables. The likelihood of an Agency prevailing at trial increases over time and shows an increasingly positive correlation across successive administrations. The judge writing the majority opinion is less likely to be a Democrat over time.

The summary and correlation statistics reflect general patterns of potential interest. However, the statistics do not fully address the subtleties highlighted in our theoretical model. We thus turn to an empirical analysis that examines the evolution of judicial standards in more detail.

Empirical Specification

Proposition 1 indicates that variations in the likelihood that challenged mergers proceed to trial can be indicative of shifting judicial standards. Proposition 2 indicates that shifts in the likelihood that Agencies prevail in court also can reflect shifts in judicial standards. Our data on proposed mergers, Agency challenges, and litigated outcomes allow us to examine these predictions.

Our empirical analysis examines whether reported HSR mergers were challenged or allowed (first stage), whether challenges proceeded to trial or were settled (second stage), and whether the Agency won or lost fully-litigated trials (third stage). We examine these stages by working backward. Our third-stage estimation predicts whether Agencies prevail in court using a fully-litigated “trial” sample of 94 observed outcomes. Our second-stage estimation predicts whether an Agency challenge proceeds to trial. We observe the total number of Agency challenges in each year, and thus expand the fully-litigated trial outcome sample into a “challenge” sample that includes all Agency challenges (i.e., those that proceed to trial and those that settle).²⁸ Our first-stage estimation predicts whether a reported HSR merger is challenged. We observe the total number of HSR mergers in each year, and thus expand the challenge sample into a “merger” sample that includes all reported HSR mergers (i.e., those that are challenged and those that are

²⁸ For example, in 1990, the Agencies challenged 34 HSR mergers and brought 6 to trial. The data are expanded for this year by the difference between the number of HSR mergers challenged and the number of HSR mergers fully-litigated (28 additional observations).

allowed).²⁹ From 94 observed trial outcomes, the dataset thus expands twice: first, to 1,509 observations that represent the 94 challenges that went to trial and the 1,415 challenges that were settled; and second, to 84,386 observations that represent the 1,509 reported HSR mergers that were challenged and the 82,877 reported HSR mergers that were allowed.

We employ three dichotomous dependent variables. *CH_DV* takes the value one if the merger is challenged by the Agencies and is zero otherwise. *TR_DV* takes the value one if the challenge proceeds to a fully-litigated trial and is zero otherwise. *OUT_DV* takes the value one if the Agency prevails in the fully-litigated trial and is zero otherwise. Our general specification entails three equations:

$$y_1^* = x_1\beta + \mu_1; \quad y_1 = 1 \text{ if } y_1^* > 0; \quad 0 \text{ otherwise} \quad (9)$$

$$y_2^* = x_2\gamma + \mu_2; \quad y_2 = 1 \text{ if } y_2^* > 0; \quad 0 \text{ otherwise} \quad (10)$$

$$y_3^* = x_3\delta + \mu_3; \quad y_3 = 1 \text{ if } y_3^* > 0; \quad 0 \text{ otherwise} \quad (11)$$

In equations (9) – (11), $y_1^* = CH_DV$, $y_2^* = TR_DV$, and $y_3^* = OUT_DV$; β, γ , and δ represent parameters to be estimated; x_1, x_2 , and x_3 are vectors of exogenous explanatory variables; and μ_1, μ_2 , and μ_3 are random error terms. We additionally specify that:

$$\begin{aligned} E[\mu_1|x_1, x_2, x_3] &= E[\mu_2|x_1, x_2, x_3] = E[\mu_3|x_1, x_2, x_3] = 0; \\ Var[\mu_1|x_1, x_2, x_3] &= Var[\mu_2|x_1, x_2, x_3] = Var[\mu_3|x_1, x_2, x_3] = 1; \\ \text{and } Cov[\mu_1, \mu_2, \mu_3|x_1, x_2, x_3] &= \rho \end{aligned} \quad (12)$$

where $E[\cdot]$ is the expectations operator and $Var[\cdot]$ and $Cov[\cdot]$ represent the variance and covariance, respectively.

Because each of the dependent variables in equations (9) – (11) is dichotomous, probit (or logit) estimation is appropriate. Conceivably, these equations might be estimated separately. However, the set of Agency challenges and the set of fully-litigated trials may not constitute random samples of, respectively, all HSR mergers and all Agency challenges. Therefore, single equation estimation could produce biased parameter estimates. Accordingly, equations (9) – (11) are structured as a system involving trivariate probit with double sample selection. We jointly

²⁹ For example, in 1990, 2,262 HSR mergers were reported and 34 of these were challenged by the Agencies. The data are expanded for this year by the difference between the number of HSR mergers reported and the number of HSR mergers challenged (2,228 additional observations).

estimate the system of equations (including the selection process) using Roodman's (2011) conditional mixed-process (cmp) regression procedure. This model allows simultaneous estimation of all three equations and employs a simulated maximum likelihood algorithm (Geweke, 1989; Hajivassiliou and McFadden, 1998; Keane, 1994) to directly estimate the cumulative higher-order likelihood function.

We allow merger challenges to be affected by the natural log of Agency budgets (*BUDGET*) in the first-stage (challenge) estimation.³⁰ We also control for the number of merger filings (*HSR*), which might affect Agency workloads and thereby influence the propensity for cases to be challenged. For the second-stage (trial) estimation, we examine the time-varying propensity of the parties to proceed to litigation. The estimating equation includes as controls a set of indicator variables that connote each presidential administration during the 1979-2021 sample period. The base category represents the Reagan administration and the last two years of the Carter administration. We also control for the total number of Agency challenges (*CHALLENGES*), which might affect Agency workloads and thereby influence the likelihood that cases proceed to trial. For the third-stage (outcome) estimation, we examine the time-varying propensity for the Agency to win using a simple time trend based on decision year (*DEC YEAR*). We also control for the political party of the president who appointed the presiding judge (*DEM JUDGE*).³¹ This measure permits us to examine whether the courts—potentially influenced by the Reagan era emergence of the Chicago School—have subsequently imposed increasingly lax standards for approving litigated mergers.

Empirical Results

Table 5 provides the estimation results. Coefficient estimates and standard errors are reported for each explanatory variable. Standard errors are robust and clustered (by *FISC YEAR*). Univariate probit (upper panel) and trivariate probit with double sample selection (lower panel) results are

³⁰ See Macher and Mayo (2021) for a detailed discussion of the evolution of merger challenge rates.

³¹ Additional empirical variables suggested by the theoretical literature proved infeasible to employ as controls. For example, Waldfogel (1995) identifies the degree of uncertainty facing the litigants, trial costs, and the prospective monetary judgment upon victory at trial as theoretical determinants of whether private parties in civil litigation proceed to trial. As he observes, however, direct data on the uncertainty facing the respective litigants is “difficult to obtain” (p. 458). For a large set of civil cases filed in the Southern District of New York, he proxies such uncertainty by the fraction of pro se plaintiffs, the tendency for litigation parties to be repeat players, and the fraction of institutional rather than individual litigants. These variables are not applicable in our antitrust setting. Waldfogel observes that data on litigation costs “are not available” (p. 462). This is also true in our context. Similarly, the magnitude of a monetary judgment is inapplicable in the case at hand.

shown for comparison purposes. We emphasize the latter results in the ensuing discussion. The results from the trivariate probit estimation include the correlations among the error terms in the three equations. We report Fischer's z transformations of ρ in the atanhrho statistics, which indicate moderate correlation between the fully-litigated trial and outcome stages.

Overall, the model estimation performs well, with statistically significant likelihood ratio tests for both the trivariate system estimation and the single equation estimations. An additional measure of the goodness of fit of our trial outcome estimation can be derived from comparing the predictions of fully-litigated trial outcomes to actual trial outcomes.³² We dichotomize the predicted probability of an agency win as one if it is equal to or greater than the median predicted probability, and as zero otherwise. The results of this approach are provided at the bottom of Table 5. We correctly predict 60 out of 94 (approximately 64 percent) of the actual litigated trial outcomes, with slightly better accuracy in Agency losses than in Agency wins.

The first-stage results indicate that the probability a reported HSR merger is challenged increases with the (logged) Agency budget. Moreover, the probability that a HSR merger is challenged declines as the number of HSR reported mergers increases during the fiscal year. These results suggest that the Agencies are more likely to challenge a reported HSR merger when resources are readily available and relatively unconstrained. The univariate and trivariate probit estimation results are nearly identical in this stage.

The second-stage results indicate a reduced likelihood that a challenged merger proceeds to trial after the Carter and Reagan administrations, although statistical significance is achieved only for the Clinton administration. Framed by Proposition 1, this finding provides modest support for the premise that judicial standards have changed over time, while providing no indication that judicial standards have become more lax over time. Notably, the second-stage univariate probit estimation indicates a statistically significant pattern of decline across successive administrations, reflecting an increasing propensity for firms to settle cases rather than proceed to trial. These findings are consistent with a shift toward more pro-enforcement judicial standards over time.

The third-stage results indicate that, after controlling for sample selection from the challenge/no challenge stage and the litigate/settle stage, the probability that the Agency prevails in fully litigated mergers has increased over time (using *DEC YEAR*). There is an increasing

³² See, e.g., Perloff, Rubinfeld and Ruud (1996).

propensity for the Agencies to win at trial—a result that is of moderate statistical significance and consistent with changing judicial merger standards. Informed by Proposition 2, the increasing probability of an Agency win at trial is consistent with the judiciary adopting more stringent standards for merger approval during our sample period. The third-stage estimation also reveals that *DEM JUDGE* is not statistically significant. That is, the party of the president that appointed federal judges to the bench does not appear to influence the outcome of fully-litigated merger trials. Although this finding does not support the narrative that changes in the composition of judges hearing merger cases have systematically altered judicial outcomes, it is consistent with earlier empirical analysis of litigated outcomes in other contexts.³³ The univariate and trivariate probit estimation results are comparable in this stage.³⁴

In summary, we find evidence of a decreasing propensity for challenged mergers proceed to trial and an increasing propensity for Agencies to prevail in court during the 1979-2021 sample period. These findings provide support for the conclusion that, contrary to the popular narrative, judicial standard have not become increasing lax over the past four decades.

Before concluding, we review some of the robustness checks of our empirical analysis that we undertook. We considered alternative estimation approaches. Our results are robust to estimation that examines only two stages (e.g., bivariate probit models), and to estimation that assumes continuous dependent variables (i.e., linear probability models) in some or all stages. We also considered alternative variables in particular stages. Our results are robust to the replacement of Agency budget with administration dummy variables in the first stage (challenge) estimation. Our results are also robust to the replacement of decision year with fiscal year in the third-stage (outcome) estimation. We also considered the inclusion of additional variables in particular stages. Our results are robust to the inclusion of (logged) FTC full-time employee (FTE) counts³⁵ and to the inclusion of economic controls (e.g., GDP price index, real (logged) corporate profits, consumer confidence, etc.) in the first stage (challenge) estimation. Our results are mostly robust to the inclusion of (logged) Agency budget in the second stage (trial) estimation, with some loss

³³ See, e.g., Ashenfelter, Eisenberg and Schwab (1995) who reach a similar conclusion in an extensive study of federal civil rights and prisoner cases.

³⁴ Ideally, we would incorporate as control variables additional data on relevant firm and market characteristics associated with each merger in our sample. However, these data are not included in the Agencies' Annual Competition Reports or other available data sources.

³⁵ Department of Justice (Antitrust Division) full-time employee data are not available.

of statistical significance in the third stage and some gain of statistical significance in the second stage. The trivariate and univariate estimations both indicate that the probability a challenge proceeds to trial is unaffected by the number of challenges in the fiscal year. We also allowed for the possibility that other, more macroeconomic indicators might affect the probability of a challenged merger proceeding to trial. To do so, we included, alternatively, annual measures of the national unemployment rate, the Conference Board measure of Consumer Confidence and the Purchasing Manager Index. The estimations provided no indication that these variables affect the probability that proposed mergers proceed to trial. Furthermore, including these variables in our regressions did not materially affect the results reported in Table 5.

We also considered alternative variable permutations. Our key results are invariant to omitting the last two years of the Carter administration, again with some loss of statistical significance in the third stage and some gain of statistical significance in the second stage. Our results are also robust to separating the Carter and Reagan administrations (with the former administration serving as the baseline) in the second-stage (trial) estimation.

4. Conclusion

The prosecution of merger challenges and judicial assessments of these challenges have long been sources of controversy in antitrust enforcement circles. Too often, inferences regarding the judicial standards that are applied to merger challenges have been drawn from limited data and casual observations rather than systematic analysis. In this vein, a narrative has developed; in particular, in the wake of the rise of the Chicago School, the judiciary has grown increasingly lax, with Agencies facing increasing burdens in court to challenge mergers successfully.

We have attempted to provide a more systematic assessment of the extent to which judicial merger standards may have shifted in recent decades. To do so, we adapted popular models of settlement to account for key elements of the merger review and litigation processes, including merger remedies. Our model predicts that as the court standard for approving a merger becomes more (less) stringent: (i) the probability that merging parties accept a merger remedy (rather than proceed to trial) increases (decreases); and (2) the probability that merging parties prevail in court decreases (increases).

We also undertook an empirical analysis that employs the population of all mergers reported to the Agencies between 1979 and 2021. These data include 84,386 HSR mergers that are reported

to the Agencies, of which 1,509 were challenged and 94 proceeded to trial and were adjudicated. Given the presence of potential selection effects in the three stages of our analysis – the Agencies’ challenge decision, the merging firms’ litigate or settle decision, and the court’s outcome decision – we employed a trivariate probit model with double sample selection estimation. After controlling for sample selection, we found evidence that merging parties have become less likely to proceed to court and less likely to prevail in court over the sample period. These findings suggest that, contrary to the narrative of an increasingly lax judiciary, judicial merger standards have become more stringent (more pro-enforcement) during the past four decades.

In closing, we note possible opportunities for enhancing our understanding of the evolution of judicial standards. Our theoretical model implicitly assumes that all HSR merger notices arrive simultaneously and the Agencies quickly rank order all potential mergers according to their social harm before deciding which proposed mergers to examine in more detail. In practice, mergers are reported throughout the year and are addressed in a serial manner. Our model also assumes the Agencies proffer remedies, and merging firms either accept or reject the single proposed remedy. In practice, merging firms sometimes offer settlement terms that render an otherwise objectionable merger acceptable to the Agencies. Although incorporating these features into the theoretical model would seem to add considerable complexity without fundamentally altering the considerations reflected in our more tractable analysis, efforts to model actual merger enforcement policy more closely might prove fruitful going forward. On the empirical side, our ability to capture the role of changing judicial standards on merger challenges, settlements, and court outcomes was limited by a lack of granular data on the specific firm and market characteristics of the merging parties. Going forward, such data may offer substantially expanded insights into the nature of challenges, settlements, and court outcomes to provide an enhanced understanding of the role of shifting judicial standards.

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Table 1 — Summary Statistics (Fiscal Year-Level)

VARIABLE	OBS	MEAN	STD DEV	MIN	MAX
<i>FISC YEAR</i>	42	1999.50	12.27	1979.00	2020.00
<i>HSR MERGERS</i>	42	2009.19	1005.89	716.00	4926.00
<i>CHALLENGES</i>	42	35.93	17.05	15.00	84.00
<i>MC RATE [CHAL/HSR]</i>	42	0.02	0.01	0.01	0.04
<i>LIT CASES</i>	42	2.24	1.62	0.00	6.00
<i>LC RATE [LIT CASES/CHAL]</i>	42	0.08	0.07	0.00	0.29
<i>BUDGET</i>	42	380.42	103.37	231.54	540.22
<i>ADMIN</i>	42	3.17	1.67	1.00	6.00

Table 2 — Correlation Statistics (Fiscal Year-Level)

VARIABLE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) <i>FISC YEAR</i>	1.00							
(2) <i>HSR MERGERS</i>	0.00	1.00						
(3) <i>CHALLENGES</i>	0.38	0.75	1.00					
(4) <i>MC RATE [CHAL/HSR]</i>	0.44	-0.42	0.23	1.00				
(5) <i>LIT CASES</i>	-0.22	0.04	-0.10	-0.17	1.00			
(6) <i>LC RATE [LIT CASES/CHAL]</i>	-0.47	-0.19	-0.45	-0.30	0.85	1.00		
(7) <i>BUDGET</i>	0.87	-0.30	0.22	0.66	-0.26	-0.40	1.00	
(8) <i>ADMIN</i>	0.98	-0.05	0.40	0.50	-0.23	-0.48	0.89	1.00

Table 3 — Summary Statistics (Fully Litigated Trial-Level)

VARIABLE	OBS	MEAN	STD DEV	MIN	MAX
<i>FISC YEAR</i>	94	1997.59	12.78	1979.00	2020.00
<i>DEC YEAR</i>	94	1998.34	12.81	1979.00	2020.00
<i>CARTER-REAG ADMIN</i>	94	0.34	0.48	0.00	1.00
<i>BUSH (GHW) ADMIN</i>	94	0.13	0.34	0.00	1.00
<i>CLINTON ADMIN</i>	94	0.15	0.36	0.00	1.00
<i>BUSH (GW) ADMIN</i>	94	0.14	0.35	0.00	1.00
<i>OBAMA ADMIN</i>	94	0.17	0.38	0.00	1.00
<i>TRUMP ADMIN</i>	94	0.07	0.26	0.00	1.00
<i>TRIAL WON</i>	94	0.56	0.50	0.00	1.00
<i>DEM JUDGE</i>	94	0.41	0.50	0.00	1.00

Table 4 — Correlation Statistics (Fully-Litigated Trial-Level)

VARIABLE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) <i>FISC YEAR</i>	1.00									
(2) <i>DEC YEAR</i>	0.99	1.00								
(3) <i>CARTER-REAG ADMIN</i>	-0.76	-0.73	1.00							
(4) <i>BUSH (GHW) ADMIN</i>	-0.20	-0.22	-0.27	1.00						
(5) <i>CLINTON ADMIN</i>	-0.02	-0.03	-0.30	-0.16	1.00					
(6) <i>BUSH (GW) ADMIN</i>	0.24	0.25	-0.29	-0.15	-0.17	1.00				
(7) <i>OBAMA ADMIN</i>	0.59	0.59	-0.33	-0.17	-0.19	-0.18	1.00			
(8) <i>TRUMP ADMIN</i>	0.47	0.47	-0.20	-0.11	-0.12	-0.11	-0.13	1.00		
(9) <i>TRIAL WON</i>	0.24	0.22	-0.14	-0.05	-0.05	0.04	0.28	-0.08	1.00	
(10) <i>DEM JUDGE</i>	-0.08	-0.09	0.22	-0.13	-0.17	-0.09	0.08	0.01	0.04	1.00

Table 5 — Empirical Results

FIRST STAGE		SECOND STAGE		THIRD STAGE	
DEP VAR: <i>CH_DV</i>	UNI-PROBIT	DEP VAR: <i>TR_DV</i>	UNI-PROBIT	DEP VAR: <i>OUT_DV</i>	UNI-PROBIT
<i>LN(BUDGET)</i>	0.369*** (0.042)	<i>BUSH (GHW) ADMIN</i>	-0.113 (0.198)	<i>DEC YEAR</i>	0.023** (0.011)
<i>LN(HSR)</i>	-0.066*** (0.024)	<i>CLINTON ADMIN</i>	-0.860*** (0.336)	<i>DEM JUDGE</i>	0.169 (0.270)
		<i>BUSH (GW) ADMIN</i>	-0.563*** (0.183)		
		<i>OBAMA ADMIN</i>	-0.595*** (0.206)		
		<i>TRUMP ADMIN</i>	-0.515** (0.246)		
		<i>CHALLENGE</i>	0.000 (0.007)		
<i>CONSTANT</i>	-3.765*** (0.358)	<i>CONSTANT</i>	-1.058*** (0.173)	<i>CONSTANT</i>	-44.869** (21.074)
Observations	84,386	Observations	1,509	Observations	94
LR [$\chi(2)$]	118.06***	LR [$\chi(6)$]	38.03***	LR [$\chi(2)$]	4.83*
Pseudo R ²	0.008	Pseudo R ²	0.054	Pseudo R ²	0.038
FIRST STAGE		SECOND STAGE		THIRD STAGE	
DEP VAR: <i>CH_DV</i>	TRI-PROBIT	DEP VAR: <i>TR_DV</i>	TRI-PROBIT	DEP VAR: <i>OUT_DV</i>	TRI-PROBIT
<i>LN(BUDGET)</i>	0.370*** (0.062)	<i>BUSH (GHW) ADMIN</i>	-0.109 (0.103)	<i>DEC YEAR</i>	0.024* (0.014)
<i>LN(HSR)</i>	-0.067* (0.035)	<i>CLINTON ADMIN</i>	-0.564** (0.283)	<i>DEM JUDGE</i>	0.160 (0.284)
		<i>BUSH (GW) ADMIN</i>	-0.238 (0.1171)		
		<i>OBAMA ADMIN</i>	-0.230 (0.166)		
		<i>TRUMP ADMIN</i>	-0.210 (0.219)		
		<i>CHALLENGE</i>	0.000 (0.004)		
<i>CONSTANT</i>	-3.765*** (0.520)	<i>CONSTANT</i>	-2.767*** (0.234)	<i>CONSTANT</i>	-49.225* (27.603)
<i>atanhrho_12</i>			0.086 (0.647)		
<i>atanhrho_13</i>			0.355 (0.494)		
<i>atanhrho_23</i>			1.241* (0.712)		
Observations			84,386		
LR [$\chi(10)$]			62.53***		
	TOTAL TRIALS		AGENCY WINS		AGENCY LOSSES
	94		53		41
NUM PREDICTED CORECTLY	60		33		27
PCT PREDICTED CORRECTLY	63.8%		62.3%		65.9%

Notes: (1) * $p < .10$; ** $p < .05$; *** $p < .01$. (2) Likelihood Ratio (LR) tests reported for each estimation. (3) The first-stage estimation uses the merger sample. The dependent variable is one for all HSR reported mergers that are challenged, and zero otherwise. (4) The second-stage estimation uses the trial sample. The dependent variable is one for all challenged mergers that are fully-litigated, and zero otherwise. (5) The third-stage estimation uses the outcome sample. The dependent variable is one for those fully-litigated trials where the Agency wins, and zero otherwise. (6) Standard errors, which are reported below the coefficients, are robust and clustered (by fiscal year).