Watchdog or Showdog? Spotlight Chasing in Antitrust Enforcement

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Abstract

We study how an acquirer's public visibility affects antitrust enforcement in mergers and its real economic consequences. Using a novel dataset linking FTC and DOJ inspection outcomes to news coverage of merger parties, we find that a 10 percent increase in the acquirer's share of industry news coverage raises the likelihood of being flagged by 1.5 to 3 percent. We establish causality using geographical proximity to media outlets as an exogenous source of variation in news coverage. We interpret these results through a political-accountability framework: higher visibility raises the reputational stakes of enforcement, leading regulators to challenge more visible acquirers. Consistent with this accountability framework, the visibility gradient strengthens during congressional appropriations hearings and disappears in lame-duck periods. It is also substantially stronger in consumer-facing industries, where accountability pressures are highest. Finally, visibility-driven scrutiny has real effects: overlooked low-visibility deals gain market power, while flagged acquirers expand inefficiently and face a higher financial burden. Our results highlight how public salience distorts merger review and generates persistent post-merger inefficiencies.

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1 Introduction

Why do some mergers face regulatory scrutiny while others, seemingly similar, proceed unchallenged? In December 2017, CVS Health announced its $69 billion acquisition of Aetna, merging a major pharmacy chain and pharmacy benefit manager with one of the nation's largest health insurers. Three months later, Cigna unveiled a comparable $67 billion bid for Express Scripts. Both deals involved large vertical integration in the healthcare sector and raised concerns about reduced competition and increased market power. Yet the Department of Justice (DOJ)'s antitrust division challenged only the CVS–Aetna deal, despite ultimately not succeeding in court, while the Cigna–Express Scripts merger proceeded without objection.

This divergence is puzzling. The two deals were highly similar in structure, size, and timing, yet only one prompted regulatory action. One key difference, however, was the public visibility of the acquirers. In the year before the transaction, CVS received significantly more media coverage than Cigna across a wide range of business and policy topics unrelated to the deal itself. This contrast raises a broader question: to what extent do regulators respond to the underlying public visibility of merging firms, even when such coverage is unrelated to the transaction, and does this influence the likelihood of antitrust enforcement?

A growing body of economic literature shows that regulatory decisions are often shaped not only by underlying fundamentals but also by the broader political environment in which agencies operate. In settings where enforcement actions are publicly observable and agency leadership is accountable to elected officials, public visibility can heighten external scrutiny of a regulator's decisions. Applied to merger review, this suggests a potential channel through which background media attention, although unrelated to the transaction's competitive merits, may alter the perceived reputational stakes of challenging or clearing a deal. Understanding whether such visibility shapes early-stage antitrust enforcement, and if so, through what mechanism, is central to interpreting patterns like the CVS–Aetna versus Cigna–Express Scripts contrast above.

In this paper, we investigate whether *public visibility* – measured by firm-level media coverage unrelated to the merger – systematically influences the likelihood that a transaction is flagged for in-depth antitrust review under the Hart-Scott-Rodino (HSR) Act. The HSR process requires the Federal Trade Commission (FTC) and the Department of Justice (DOJ) to quickly determine which mergers warrant closer scrutiny. Because statutory filing thresholds are low and most flagged deals are ultimately approved, enforcement decisions at this stage involve substantial discretion. We examine whether acquirers that attract greater media attention face disproportionately higher regulatory scrutiny, holding economic fundamentals constant.

To answer this question, we build a novel deal-level panel linking regulatory outcomes to firm-level public visibility using RavenPack Dow Jones data. Our main measure is the acquirer's share of non-M&A, non-regulatory news coverage within its 4-digit NAICS industry in the year prior to the deal announcement. Public visibility strongly predicts the likelihood of being flagged for antitrust scrutiny, even after controlling for extensive firm and deal characteristics and comparing deals within the same industry and presidential administration. The effects are economically meaningful: a 10-percentage-point increase in visibility raises the probability of being flagged by 0.67 to 1.46 percentage points, relative to baseline rates of 2.2% in all HSR-reportable deals and 3.9% in mandatory filings.

While these results are suggestive, they may still reflect firm-level factors correlated with both visibility and enforcement risk. To address this concern, we adopt a within-firm design that compares how the same acquirer is treated across different transactions as its visibility fluctuates over time. This approach accounts for all time-invariant firm characteristics, such as reputation, scale, or lobbying posture, that may jointly shape visibility and regulatory scrutiny. We find that increases in visibility within the same firm lead to substantially higher scrutiny: a 10 percent increase in visibility raises the likelihood of being flagged by 2 to 3 percent. These estimates reinforce that visibility independently drives enforcement outcomes.

Although the within-firm specification accounts for time-invariant acquirer characteris-

tics, it cannot address deal-specific factors that vary over time and may jointly affect visibility and enforcement. To isolate plausibly exogenous variation in media exposure, we implement an instrumental variables strategy based on geographic proximity to Dow Jones news bureaus. Firms located nearer to major bureaus receive more general business coverage because reporting costs are lower and journalists have easier access—forces unrelated to the competitiveness of any particular transaction.[1] We construct a hybrid instrument that interacts an indicator for being within 300 miles of a bureau with the standardized distance to that location, capturing both extensive and intensive margins of exposure. First-stage estimates confirm that proximity strongly predicts acquirer visibility. In the second stage, instrumented visibility has an even larger effect on enforcement probability than benchmark OLS estimates. We interpret this amplification as evidence that the instrument isolates the component of visibility most salient to regulators.

To further disentangle informational from non-informational drivers of regulatory scrutiny, we use the market-implied probability of deal approval, inferred from the target's stock price at announcement relative to the offer price. If visibility conveys economically relevant soft information, such as latent anti-competitive risk, forward-looking investors should price this in, and controlling for the market-implied approval probability should weaken the visibility effect. By contrast, if visibility continues to predict regulatory scrutiny conditional on market expectations, it suggests that regulators respond to dimensions of exposure that investors have not fully internalized. Empirically, we find that visibility remains a strong predictor of regulatory flagging after conditioning on approval probabilities, indicating that regulators react to aspects of public prominence—such as the reputational cost of clearing a highly visible acquirer—that markets do not fully incorporate.

Motivated by these patterns, we develop a simple accountability framework in the spirit of Maskin and Tirole (2004). In the model, visibility increases Congress's confidence in inferring whether the regulator's enforcement decision is appropriate. Because agency leadership

[1] This strategy follows prior work showing that geographic distance limits media attention due to monitoring costs and logistical frictions (e.g., Malloy, 2005; Engelberg and Parsons, 2011; Dyck et al., 2008).

is evaluated through appropriations and oversight hearings, higher visibility raises the reputational stakes of appearing overly lenient on salient mergers. Anticipating this, regulators place greater weight on challenging highly visible acquirers, even when the underlying competitive harm is identical. The model explains the baseline visibility gradient and yields a clear comparative static: the effect of visibility should strengthen when congressional oversight pressures are high and weaken when they are low.

We test this comparative-static prediction using both institutional variation in congressional oversight intensity and cross-sectional differences in the likelihood that a merger attracts public attention. During appropriations hearings—when agency leadership testifies before Congress and negotiations over future budgets occur—the reputational cost of appearing insufficiently vigilant is highest. In contrast, during strict lame-duck periods preceding a party-changing transition and the appointment of a new FTC Chair, forward-looking accountability pressures largely recede. Consistent with the model, the visibility gradient is strongest during appropriations hearings and nearly disappears during lame-duck periods. A similar logic applies in a cross-industry test: mergers involving consumer-facing acquirers are more salient to voters and congressional staff, heightening the reputational cost of underenforcement. Empirically, the visibility gradient is substantially larger for consumer-facing firms and is indistinguishable from zero for non-consumer-facing firms. These patterns are difficult to reconcile with deal composition, seasonality, or unobserved fundamentals, but follow directly from the accountability mechanism in the framework.

We then study whether this visibility-driven enforcement has real economic consequences. Specifically, we examine firms' post-merger outcomes for two sets of firms. One analysis examines acquirers whose deals should be flagged but are overlooked by the FTC due to low visibility, and the other examines the regulatory treatment effect of being flagged on acquirers. In the first analysis, we use propensity score matching to identify the acquirers of "false negative" deals, that is, transactions that scored high on enforcement criteria but were not flagged due to low visibility. We compare these overlooked deals to other unchallenged

deals and find that their gross profit margins and markups increase after the deal, suggesting they can exercise greater market power.

In the second analysis, we study the regulatory scrutiny treatment effect by comparing flagged deals to their similar counterparts that are not flagged. We show that flag decisions by the FTC/DOJ are effective in reducing market power but have side effects for the acquirer. Regulatory scrutiny reduces focal firms' market power, resulting in lower profit margins and markups. But they increase their capital and labor expansion after the deal's completion, despite weak investment opportunities. This inefficient expansion leads to higher debt burdens for the acquirers and increases their financial constraint risk. Moreover, the effectiveness of the FTC challenge in addressing the anticompetitive environment remains ambiguous, despite its effectiveness against the focal firm. If a competitor takes advantage of regulatory scrutiny of the focal firm to gain market share, consumer welfare may still worsen due to reduced market competition. We plan to evaluate the effects on market competition in future analyses.

These findings have important policy implications. The fact that overlooked deals gain market power underscores the benefits of clearer standards and greater transparency in pre-merger disclosures. The implications of the regulatory treatment effect are more nuanced. Being challenged by the FTC reduces the focal firm's market power, which aligns with the regulator's objective. However, it imposes additional risk on the firm, and the effect on market-wide competition is ambiguous. Moreover, this effect also raises concerns about over-enforcement: deals that should not be flagged under merger guidelines are flagged due to high visibility. If the regulatory treatment effects are similar on those deals, getting flagged will be costly to these "false positive" deals. While the exact effect remains to be further analyzed, costly over-enforcement suggests that closely monitored private antitrust litigation may be a helpful complementary mechanism to public regulators.

Literature Review

Our paper contributes to three strands of literature.

First, we contribute to the literature on antitrust enforcement and product-market competition by identifying public visibility as a systematic determinant of merger scrutiny, operating through political accountability rather than through traditional structural criteria. While Wollmann (2019, 2020); Barrios and Wollmann (2024), Kepler et al. (2023, 2024), and show that firms can avoid antitrust scrutiny through threshold manipulation, accounting treatments, and strategic deal structuring that enable consolidation to proceed undetected, our paper shows that enforcement decisions are also systematically influenced by the media exposure of the acquirer. In contrast to structural screens or reporting exemptions, visibility affects regulatory attention through informational and accountability channels, even among deals with similar economic fundamentals.

Moreover, our study contributes to the broader empirical literature on how mergers and acquisitions shape firm outcomes and market competition (e.g., Andrade et al. (2001); Healy et al. (1992); Hitt et al. (1991); Seru (2014)). While some studies emphasize productivity gains and efficiency improvements, others document adverse effects such as reduced innovation, increased market power, and investment distortions (Gutiérrez and Philippon (2017); Cunningham et al. (2021)). Shapiro (2019) highlights growing concerns about underenforcement in merger control, particularly in markets characterized by rising concentration and limited labor mobility. Complementing these concerns, Bhattacharya et al. (2023) show that variation in antitrust enforcement materially affects prices and competitive outcomes in U.S. consumer packaged goods markets. In this context, our findings on the downstream consequences of visibility-driven enforcement suggest that selective oversight contributes to heterogeneity in post-merger firm growth and ultimately influences the distribution of market power and the allocation of resources.

Second, we provide a novel channel through which media outlets affect the financial mar-

ket. We argue that media coverage distorts regulatory decisions, which in turn affects firm outcomes in merger deals. In this regard, we are closely related to the growing literature on the effect of media in corporate events such as mergers and acquisitions (M&A). Ahern and Sosyura (2014) demonstrate that media coverage of M&A deals can influence market perceptions and the likelihood of deal completion by shaping investor beliefs and public narratives. In our paper, we focus only on non-M&A news that affects the *regulators'* decisions, not market participants. Similarly, Gurun and Butler (2012) document that local media coverage facilitates access to external finance by increasing investor familiarity. Beyond capital markets, we also relate to the literature that media coverage affects strategic corporate decisions. Dai et al. (2015) show that managers respond to negative media attention by curtailing risk-taking, consistent with the media serving as an external governance mechanism. Related work links coverage with investment policy, payout decisions, corporate governance (Dyck et al., 2008), and executive compensation (Core et al., 2008; Joe et al., 2009), arguing that heightened visibility increases reputational costs, thereby influencing managerial incentives. Earlier, Tetlock (2007), Barber and Odean (2008), and Engelberg and Parsons (2011) show that shifts in media tone and attention can predict short-term returns and trading volume, highlighting the role of salience in asset pricing.

Third, our paper contributes to research on heterogeneous decision-making in high-stakes institutional environments and to the political economy of bureaucratic accountability. A large empirical literature shows that outcomes in judging, medicine, and financial supervision vary systematically across decision-makers, reflecting differences in preferences, incentives, or information (e.g., Lim et al., 2015; Angelova et al., 2023; Chan et al., 2022; Agarwal et al., 2024). A complementary theoretical literature emphasizes that bureaucrats operate under political oversight and are evaluated by external audiences who observe their actions imperfectly (Holmström, 1999; Maskin and Tirole, 2004; Prat, 2005; Besley and Prat, 2006). Classic work in political science highlights the mechanisms through which Congress monitors agencies—most notably, appropriations and fire-alarm oversight— that create incentives

for regulators to avoid publicly visible errors (Weingast and Moran, 1983; McCubbins and Schwartz, 1984). Building on these institutional foundations, Carpenter (2002, 2014) argue that agencies cultivate reputations among multiple audiences and make enforcement choices with an eye toward preserving legitimacy and political support. Our paper connects these strands by showing that public visibility—measured through non-merger media coverage—modulates the accountability pressures faced by antitrust regulators. We document that highly visible acquirers are systematically more likely to be challenged, and that this pattern strengthens when congressional scrutiny is most salient. In doing so, we highlight an accountability channel through which public attention shapes the implementation of merger policy, distinct from idiosyncratic discretion or random error.

The paper proceeds as follows. Section 2 provides institutional background on U.S. merger enforcement and outlines the conceptual framework. Section 3 describes the construction of the deal-level panel and the media visibility measures. Section 4 presents the core empirical results linking acquirer visibility to enforcement, including OLS, within-firm, and IV strategies, and explores whether the effect of visibility operates through informational or non-informational channels. Section 5 develops an accountability framework that formalizes how visibility interacts with congressional oversight to shape enforcement incentives. Section 6 examines the downstream consequences of visibility-driven enforcement on post-merger firm behavior and market outcomes. Section 7 concludes with implications for institutional design and antitrust policy.

2 Institutional Background and Details

Antitrust regulation in the United States has evolved over more than a century, shaped by political movements, industrial consolidation, and changing regulatory philosophies. While early enforcement relied on ex post litigation under the Sherman and Clayton Acts, modern merger policy is anchored in the premerger review system created by the Hart–Scott–Rodino

(HSR) Act of 1976. This section outlines the legal and institutional foundations of U.S. merger enforcement, with particular attention to the design and evolution of the HSR framework. We then turn to how enforcement operates in practice, drawing on insights from former agency officials and industry participants. These institutional perspectives highlight the central role of discretion in merger review and motivate our central question: how do regulators decide which deal requires further scrutiny?

2.1 Early Antitrust Enforcement: Sherman and Clayton Acts

The Sherman Antitrust Act of 1890 was the first federal law aimed at tackling monopoly power and promoting competition. It was passed in response to public outrage over powerful industrial trusts like Standard Oil. The law banned "*every contract, combination... or conspiracy in restraint of trade*" and made monopolization illegal. However, enforcing the Act was challenging because it lacked clear rules. Courts interpreted it differently – some required clear proof of collusion, while others used a broad "rule of reason" to judge whether business practices unfairly limited competition. This led to uneven enforcement and limited effectiveness in deterring anticompetitive behavior.

To address these gaps, Congress passed the Clayton Act in 1914. Section 7 specifically prohibited mergers and acquisitions that "*may substantially lessen competition*," allowing regulators to act before a company gained too much market power. The Act also prohibited practices like exclusive dealing and interlocking directorates. Additionally, it created the Federal Trade Commission (FTC) to help enforce antitrust laws alongside the Department of Justice (DOJ).

However, enforcement remained reactive. Firms were not required to notify authorities before closing a deal, and post hoc challenges were costly and often ineffective. For example, the government spent 17 years and made seven trips to the Supreme Court attempting to reverse El Paso Natural Gas's acquisition of its only regional rival.[2]

[2]See FTC speech: "Reflections on 20 Years of Merger Enforcement under the Hart–Scott–Rodino

2.2 The HSR Act and Its Modernization

By the 1960s and 1970s, merger activity had accelerated, driven in part by conglomerate expansions that blurred traditional industry boundaries. Antitrust agencies faced mounting political pressure to respond, but lacked tools for timely intervention. Firms increasingly exploited these limitations by closing transactions quickly, often before enforcement action could be considered. These so-called "midnight mergers" frequently escaped scrutiny, as agencies only learned of them after completion.

In response, Congress passed the Hart–Scott–Rodino Antitrust Improvements Act (HSR Act) in 1976. Co-sponsored by Senator Philip Hart, Senator Hugh Scott, and Representative Peter Rodino, the legislation established the first mandatory premerger notification system in the United States. Covered transactions were required to be reported to both the FTC and the DOJ in advance, and were subject to a statutory waiting period—typically 30 calendar days—before closing. If no action was taken before the waiting period expired, the transaction was automatically cleared.

Most filings were resolved during this initial review phase. Agency staff conducted a preliminary assessment based on the materials submitted and internal market analysis. If no significant competitive concerns emerged, the transaction proceeded without delay. If concerns arose, the agency could issue a formal "Second Request" for additional documents and data, pausing the clock until the parties substantially complied. This extended investigation could result in clearance, remedies (such as divestitures), or litigation to block the deal.

The HSR Act applied only to transactions exceeding both a size-of-transaction threshold and a size-of-person test. At enactment, the transaction threshold was $15 million. The size-of-person test required one party to have at least $100 million in total assets or sales and the other at least $10 million. These thresholds remained fixed in nominal terms for over two decades, meaning that as inflation and deal volumes increased, a growing number

Act," https://www.ftc.gov/news-events/news/speeches/reflections-20-years-merger-enforcement-under-hart-scott-rodino-act.

of transactions became subject to review.

By the late 1990s, the number of reportable mergers had risen sharply. Antitrust agencies faced growing administrative burdens, with staff attention increasingly devoted to transactions unlikely to raise substantive concerns. In response, Congress passed the HSR Modernization Act of 2000, signed into law by President Clinton and implemented in early 2001. The amendment introduced three major changes: it raised the transaction-size threshold from $15 million to $50 million and indexed it to inflation; it mandated reporting for all transactions above $200 million, regardless of party size; and it simplified filing fees and reduced reporting requirements for smaller transactions.

Rather than expanding agency capacity, the amendment restructured the scope of regulatory oversight. The number of HSR filings fell by roughly 70 percent in the year following implementation, even as overall merger activity remained high. Many deals that would previously have required notification – particularly in local or fragmented markets – were now exempt. The policy change redefined the boundary between observed and unobserved merger activity in the United States.

Figure 1 summarizes the structure of the current review process. Transactions below the filing thresholds are not subject to notification. Transactions above the thresholds trigger a waiting period and, if warranted, a more detailed investigation. The reviewing agency may allow the deal to proceed, request remedies, or initiate enforcement action.

While the HSR framework determines which transactions are subject to mandatory review, it leaves open how agencies decide which of those merit closer scrutiny. With fewer deals reaching regulators after the 2000 amendment, the selection problem within the reviewed pool became more salient. Staff must determine, often under significant time pressure and with limited information, whether a transaction raises competitive concerns. These judgments are formally guided by the Merger Guidelines, which set out structural benchmarks and other indicators used to identify potentially problematic deals. The next section examines how these guidelines are applied in practice.

2.3 Merger Guidelines and the Limits of Rule-Based Enforcement

Once a transaction is filed, agency staff assess whether it raises competitive concerns. This process is formally structured around the DOJ and FTC's Merger Guidelines, which specify structural thresholds and other indicators of potential harm. While the Guidelines aim to promote consistency and transparency in enforcement, many of the inputs – such as market definition, concentration measures, and potential entry – are difficult to define or observe in practice. As a result, discretion remains central even within this nominally rule-based framework.

The 2023 Guidelines, for example, identify several structural triggers that may raise concern. These include high post-merger market concentration—measured by the Herfindahl-Hirschman Index (HHI) – particularly when the post-merger HHI exceeds 1800 and the change in HHI is greater than 100. Agencies also flag transactions in which the merged firm would hold more than a 30 percent market share alongside a significant HHI increase. Additional concerns include substantial pre-merger competition between the parties, heightened risks of coordination, the elimination of a "maverick" firm, the loss of a potential entrant, and the ability to foreclose rivals in vertical supply chains.

Applying these criteria in practice requires judgments that are often subjective or data-intensive. Defining the relevant market – a prerequisite for calculating market shares or HHI – is frequently disputed, especially in differentiated product industries or dynamic sectors. Concentration measures rely on granular and sometimes proprietary data, which may be unavailable or incomplete at the time of filing. Even when data are accessible, results may vary depending on modeling assumptions and how markets are delineated.

Other guideline concepts present similar challenges. Identifying a "maverick" firm depends on qualitative assessments of behavior and internal strategy. Evaluating the risk of future entry involves forward-looking speculation. Vertical theories of harm, such as foreclosure or raising rivals' costs, rely on counterfactual reasoning and often require internal

documents to support claims. As a result, different reviewers may reach different conclusions about the same transaction depending on how they interpret incomplete or ambiguous signals.

Therefore, enforcement discretion remains even in cases that nominally satisfy structural thresholds. Some transactions flagged by the Guidelines receive little scrutiny, while others that do not meet any specific benchmark may trigger extended review. In practice, the Guidelines serve more as heuristics than rules. They structure the initial review, but they leave significant scope for agency judgment, especially when applied to incomplete or case-specific information. This flexibility contributes to variation in enforcement across deals and over time – even among transactions that appear similar on paper.

2.4 Political Oversight Under the HSR Act

While the Merger Guidelines structure how competitive concerns are evaluated, the antitrust agencies operate within a broader political environment. The FTC and DOJ Antitrust Division are subject to routine oversight by Congress, most notably through the annual appropriations process. Agency leadership appears at public hearings to justify recent enforcement activity, respond to questions about specific mergers, and defend the adequacy of the agency's vigilance. In these settings, legislators observe *enforcement actions*—that is, which deals were challenged or allowed to proceed—but not the underlying confidential evidence or the staff's private assessments. As a result, public attention surrounding a transaction can shape the context in which Congress evaluates the agency's choices.

A natural implication is that some mergers attract more external scrutiny than others. Transactions involving acquirers with substantial public visibility, strong brand recognition, or intense media coverage tend to receive more attention from constituents and interest groups, increasing the likelihood that legislators take notice. In contrast, otherwise similar deals involving less visible firms may unfold without meaningful external commentary. This

imbalance was evident in the contrasting public profiles of the CVS–Aetna and Cigna–Express Scripts mergers as mentioned earlier, where only the former became a focal point of public and congressional debate despite both involving large industry participants in the U.S. healthcare sector.

Interviews with former enforcement officials echo this asymmetry. One former DOJ antitrust official explained that, "*When the buyer is a big, well-known company, you think twice before clearing it—because everyone will be watching if it goes wrong.*" Such remarks do not imply political determinism, but they suggest that the agencies operate in an environment where the potential for congressional or public scrutiny is uneven across deals. Because external audiences observe outcomes rather than investigative nuance, the reputational stakes associated with clearing a merger may vary with its public prominence.

These institutional features indicate that merger review does not occur in a vacuum: the salience of a transaction and the prospect of congressional attention shape the external environment in which enforcement decisions are ultimately judged. This asymmetry in public visibility motivates examining, in a systematic way, whether more visible acquirers indeed face different enforcement outcomes—and whether such patterns are consistent with the oversight dynamics embedded in the HSR framework.

3 Data and Measurement

We compile multiple data sources to construct a comprehensive deal-level dataset to analyze the determinants of merger enforcement under the HSR regime. Our key data sources include merger transactions from SDC Platinum, enforcement outcomes from FTC and DOJ records, media visibility measures from RavenPack, and firm-level characteristics from Compustat. Below, we describe each component in detail.

3.1 Merger and Acquisition Transactions

We extract all mergers and acquisitions announced between 2000 and 2023 from the Thomson Reuters SDC Platinum database. We restrict the sample to transactions in which both the acquirer and the target are U.S.-based entities and the disclosed deal value exceeds the inflation-adjusted HSR reporting thresholds in effect at the time of announcement. For each transaction, we observe the key deal-level characteristics, including announcement date, effective (or closing) date, acquirer and target firm names, 6-digit CUSIP identifiers, disclosed deal size, transaction status, and primary industry classifications using 6-digit NAICS codes. To calculate market-implied success probability, we also extract from SDC Platinum the initial offer price for cash deals and the exchange rate ratio for stock deals.

To align the sample with the scope of federal antitrust enforcement, we exclude deals involving financial institutions—specifically, transactions in which either party is classified as a commercial bank, thrift, insurance company, or real estate investment trust (REIT) — as these are generally subject to separate regulatory regimes. We also exclude deals in which the acquirer is identified as a leveraged buyout (LBO) or private equity firm, in order to focus on transactions initiated by strategic corporate acquirers.

3.2 Regulatory Scrutiny

To study how regulators screen mergers under the HSR framework, we aim to capture which transactions raise substantive antitrust concerns. Ideally, one would use the set of deals that received a Second Request following the initial HSR filing, as these represent the full pool of transactions identified by regulators as potentially problematic. However, neither the FTC nor the DOJ discloses a comprehensive list of Second Request recipients, and no systematic database exists for such cases.

In light of this data limitation, we focus on transactions that were eventually subject to formal enforcement actions—cases in which the FTC or DOJ publicly challenged the merger

through a complaint, consent decree, settlement, or court ruling. These enforcement actions are disclosed through litigation records posted on the agencies' official websites. We manually collect and match these cases to our SDC sample based on participating parties' names, deal value, and announcement year.

We define an indicator variable `Flagged` that equals one if a transaction was formally challenged by the FTC or DOJ and zero otherwise. While this captures only a subset of all transactions that may have raised concern, it has the advantage of clear, verifiable public disclosure. The interpretation is conservative: flagged deals represent cases where concern was strong enough to merit litigation or negotiated remedies.

To ensure that all deals in our analysis were subject to HSR review, we restrict the main sample to transactions exceeding the inflation-adjusted size-of-transaction threshold above which the size-of-person test no longer applies. These transactions are mandatorily reportable under the HSR Act, regardless of the size of the parties involved, and must therefore be submitted for agency review. As a robustness check, we also construct an alternative version of the review-eligible sample using our own implementation of the size-of-person test, based on financial data for both acquirers and targets. We compare the resulting sample counts to the FTC's official monthly disclosures of HSR filings and find close agreement over time (see Figure 2).

3.3 Firm Visibility Measures

To examine the role of public visibility in regulatory scrutiny, we construct firm-level measures of media attention using RavenPack's Dow Jones News Analytics database.[3]

This dataset provides daily coverage of financial news, with each article timestamped, linked to a proprietary entity identifier (`rp_entity_id`), and annotated with structured

[3]We use the Dow Jones edition of RavenPack, which focuses on high-quality financial news sources such as the *Wall Street Journal*, *Barron's*, *MarketWatch*, and Dow Jones Newswires. This version ensures consistent topical tagging and source attribution, minimizing noise from lower-tier or irrelevant outlets. See Hu (2024) and Boudoukh et al. (2019) for related applications of news-based firm salience using this dataset.

metadata on topics (e.g., "acquisition," "earnings," "regulatory"), sentiment, source, and relevance.

Our goal is to isolate visibility that reflects a firm's ambient public salience, rather than coverage mechanically linked to the deal itself or its regulatory implications. To this end, we exclude all articles tagged as involving mergers, acquisitions, joint ventures, antitrust investigations, regulatory approvals, or congressional testimony. These filters are designed to remove news events that may be directly triggered by the transaction or by expectations of scrutiny, ensuring that the visibility measure is not endogenously related to regulatory outcomes.

In some specifications, we further exclude earnings-related coverage—the most common type of financial news—to capture visibility driven by factors such as reputation, public profile, or political salience, rather than routine disclosure cycles. This stricter measure helps distinguish ambient media attention from standard informational flows.

For each firm, we construct raw media attention as the count of qualifying articles over a set of pre-merger windows: 24 months, 12 months, 9 months, 6 months, 3 months, 2 months, 1 month, and a "donut" window of 18 to 13 months before deal announcement. These multiple windows allow us to capture both persistent and transitory attention, and to test whether timing affects the regulatory response. The donut measure, in particular, helps mitigate concerns about reverse causality from deal anticipation.

Because firms differ in size and operate in industries with varying baseline levels of news activity, we construct a normalized visibility measure: the firm's share of all qualifying articles within its 4-digit NAICS industry, based on Compustat classifications. This "spotlight share" captures a firm's relative salience within its competitive landscape, adjusting for sector-level media volume and enabling consistent comparisons across deals and industries. A higher spotlight share reflects disproportionate public attention, which may influence agency priorities, political oversight, or external pressure.

We link the RavenPack data to Compustat firms using a historical crosswalk between `rp_entity_id` and each firm's 6-digit `ncusip`. We then merge the resulting visibility measures to the SDC deal-level data using the year-month of deal announcement. This yields firm-level spotlight shares for each acquirer in our sample, aligned to the relevant pre-merger window.

We focus on the acquirer's visibility rather than the target's for both conceptual and empirical reasons. First, under the HSR framework, the acquiring firm is typically treated as the initiating party: it files the premerger notification, interacts directly with regulators, and often bears the burden of regulatory concessions. Public and political scrutiny is therefore more likely to be directed at the acquirer, particularly in high-profile transactions. Second, acquirers are more likely to be large, publicly traded firms with broader media footprints and observable balance sheets, allowing for more precise measurement of pre-deal visibility and post-deal outcomes. In contrast, targets are often smaller or private firms with limited media coverage, making it difficult to construct meaningful and comparable spotlight measures across the sample.

3.4 Firm-Level Financial Data

To examine how antitrust scrutiny affects firms' post-merger behavior, we construct a panel of firm-level financial outcomes for publicly traded U.S. acquirers using annual data from Compustat North America. Our sample spans the years 2000 to 2023 and focuses on acquirers involved in transactions covered by our deal-level dataset. We track firm performance before and after deal completion across several dimensions, including inputs, outputs, investment, leverage, innovation, and valuation.

Firm inputs and outputs are measured using standard accounting proxies. Labor input is defined as the log number of employees, $\log H = \log(\texttt{emp})$. Capital input is the log of real capital stock, calculated by deflating property, plant, and equipment (`ppegt`) by the NIPA investment deflator: $\log K = \log(\texttt{ppegt}/P_K)$. Following Kogan et al. (2017), we

define output as real gross output, measured as total sales plus inventory changes, deflated by the Consumer Price Index (CPI): $\log Y = \log[(\texttt{sale} + \Delta\texttt{invt})/\text{CPI}]$. Operating profits are defined as revenues net of cost of goods sold (`cogs`), also deflated by CPI: $\log P = \log[(\texttt{sale} - \texttt{cogs})/\text{CPI}]$.

We also construct measures of firm investment, leverage, innovation, and valuation. The investment rate is defined as capital expenditures (`capx`) scaled by lagged total assets (`at`), consistent with Fama and French (2015). Net leverage is computed as the ratio of total liabilities net of cash and short-term investments to total assets, $(\texttt{lt} - \texttt{che})/\texttt{at}$, providing a measure of the firm's debt exposure relative to asset base. To proxy for innovative effort, we use R&D intensity, defined as research and development expenses (`xrd`) divided by total assets: $\texttt{red} = \texttt{xrd}/\texttt{at}$.

To capture firm valuation, we adopt the total q measure developed by Peters and Taylor (2017), which combines market and book values while accounting for the replacement cost of capital. This measure is merged onto our panel using firm identifiers (`gvkey`) and fiscal year.

We link firm-year observations to the M&A transaction data described in Section 3.1. Specifically, we focus on completed deals — those that either were not flagged by the FTC/-DOJ or were flagged but ultimately allowed to proceed. Withdrawn transactions are excluded, as they may reflect either voluntary abandonment or blocked deals. The deal completion date is defined as the effective date reported in the SDC database, and we evaluate firm outcomes in the years following that date.

All nominal variables are deflated to constant dollars using either the Consumer Price Index (for flow variables) or the NIPA equipment deflator (for capital). We drop firm-year observations with non-positive total assets or sales. All variables are winsorized at the 1st and 99th percentiles within year to mitigate the influence of outliers. The resulting dataset is an unbalanced panel at the `gvkey` $\times$ year level and serves as the basis for our post-merger outcome analysis.

3.5 Other Data

We complement our firm-level data with two additional sources: (i) industry-level output data from the Bureau of Economic Analysis (BEA) to measure market concentration, and (ii) geographic data on Dow Jones newsrooms to support an instrument for firm visibility.

Industry Concentration. To proxy for each acquiring firm's competitive environment, we construct Herfindahl–Hirschman Index (HHI) measures at the 3-digit NAICS level. We begin by mapping each firm's Standard Industrial Classification (SIC) code in Compustat to its dominant NAICS counterpart using the concordance provided by the National Bureau of Economic Research (NBER).[4]

We then merge firm-level revenue with gross output data from the U.S. Bureau of Economic Analysis (BEA), aggregated at the 3-digit NAICS and calendar-year level. For each industry-year cell, we compute the firm's output share and construct the HHI as the sum of squared revenue shares.

To address incomplete coverage in Compustat—due to the absence of private and small firms—we implement an adjusted HHI approach. Specifically, if total observed sales fall short of BEA gross output, we assign the residual share to a synthetic "private sector" and include its squared share in the index. Formally, the adjusted concentration index is:

$$\texttt{HHI}_{\text{adj}} = \sum_{i \in \text{Compustat}} s_i^2 + s_{\text{private}}^2,$$

where s_i is firm i's share of industry output and $s_{\text{private}} = 1 - \sum_i s_i$ is the residual share attributed to unlisted firms. This approach ensures that the HHI reflects true market concentration, not merely coverage gaps in financial datasets.

This method is conceptually related to approaches in Gutiérrez and Philippon (2017) and

[4]See https://www.nber.org/research/data/nber-ces-manufacturing-industry-database. We retain the NAICS code with the highest estimated employment share within each SIC as the dominant match.

Decker and Williams (2023), who scale Compustat-based measures using national output benchmarks to correct for public-firm under-coverage. Our use of a residual-squared share follows the same logic and yields a measure that is more robust to variation in industry observability.

Dow Jones Office Proximity. As part of our instrumental variable strategy for firm visibility, we collect the geographic locations of all Dow Jones editorial offices in the United States from the company's official website. These include newsrooms affiliated with the *Wall Street Journal*, Dow Jones Newswire, and other affiliated publications. For each acquiring firm, we compute the great-circle distance between the firm's headquarters (from Compustat) and the nearest Dow Jones office.

The resulting variable captures exogenous variation in a firm's proximity to financial media infrastructure, which affects the probability of incidental coverage but is plausibly orthogonal to regulatory outcomes. We use this distance-based measure as an instrument for acquirer-level visibility in our later analysis.

3.6 Descriptive Statistics

To build intuition for the main variables used in our analysis, Table 1 reports summary statistics for the sample of merger transactions that meet the HSR size-of-transaction thresholds and are therefore subject to federal antitrust review. Panel A includes all qualifying deals involving public acquirers between 2000 and 2020[5], while Panel B focuses on the subset of transactions subject to mandatory HSR reporting, based on observed deal size and applicable thresholds in each year.

Approximately 2-4% of transactions are formally challenged by the FTC or DOJ, including

[5]We end the main sample in 2020 to exclude the period following the appointment of FTC Chair Lina Khan in mid-2021, which marked a substantial shift in merger enforcement priorities, procedures, and legal doctrine. Including these later years would introduce structural breaks in regulatory behavior that are orthogonal to the baseline institutional framework studied in this paper.

those that were ultimately withdrawn following agency intervention. One-third of deals involve horizontal mergers within the same 6-digit NAICS industry, a dimension central to traditional antitrust evaluation. Deal sizes vary widely, with medians above $230 million in Panel A and above $630 million in Panel B.

The acquirer's media visibility, captured through the spotlight share measure, also displays considerable dispersion across deals. The 75th percentile of the one-year spotlight share is roughly three times the median, indicating that a small set of firms attract disproportionate non-deal, non-regulatory media coverage in the year preceding a transaction. This heterogeneity supports the idea that certain acquirers may be more visible to regulators, interest groups, or the public, independent of deal characteristics.

Similarly, acquirers' roles in their respective industries differ substantially. While the typical firm accounts for less than 2% of total industry sales, some represent more than a quarter of their sector's output, particularly in concentrated markets. The adjusted HHI measure, which incorporates an imputed share for private firms based on BEA gross output, spans a wide range—from highly fragmented to near-monopoly industries—providing a natural baseline for competitive pressure.

These summary statistics highlight meaningful cross-sectional variation in acquirer characteristics across deals. While most transactions involve firms with modest industry shares and limited media presence, a nontrivial subset of acquirers commands significant sales shares and attracts disproportionate attention in the financial press. This heterogeneity—both in economic scale and public visibility—motivates our empirical analysis of how regulators prioritize merger review.

4 Does Visibility Influence Enforcement?

4.1 Visibility and Regulatory Scrutiny

Building on the institutional background and anecdotal evidence in the preceding sections, we now examine whether public visibility systematically shapes the likelihood that a proposed merger receives antitrust scrutiny. Although early-stage review is formally guided by structural benchmarks—market share, industry concentration, and transaction size—agencies must still determine, within a short statutory window, which transactions merit further investigation. Because only a small fraction of HSR-reportable mergers are ultimately challenged, this screening stage plays a central allocative role. We therefore ask whether, controlling for deal fundamentals and political conditions, acquirers with greater public visibility are more likely to have their transactions flagged for in-depth review.

This hypothesis is grounded in the institutional structure of early-stage merger review. As described in Section 2, the FTC and DOJ must decide quickly which transactions warrant further investigation, and these early decisions are observable to Congress and the broader public. Because transactions involving highly visible acquirers are more likely to attract external attention—whether from journalists, congressional staff, or stakeholders—they naturally carry greater external salience. In economics, one can view this through the lens of accountability: when actions are taken under heightened public scrutiny, the reputational stakes of each decision increase, even when the underlying policy objectives remain unchanged. Such institutional and economic forces create a setting in which public visibility may shape which mergers receive closer review, above and beyond structural indicators such as market share, industry concentration, and transaction size. This motivates a formal examination of whether, conditional on these fundamentals, acquirer visibility predicts the likelihood of enforcement.

As a first step toward evaluating this hypothesis, Figure 3 plots the distribution of acquirer visibility for flagged and non-flagged deals. Visibility is measured as the acquirer's share

of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the year preceding the transaction. Panel A shows the full sample of HSR-eligible transactions involving public acquirers, while Panel B restricts to the subset subject to mandatory HSR review. In both samples, flagged deals disproportionately involve firms in the upper tail of the visibility distribution. While consistent with our hypothesis, this pattern is descriptive: acquirer visibility may be correlated with other deal characteristics—such as size, overlap, or industry structure—that also affect enforcement likelihood.

We therefore turn to a regression framework to isolate the relationship between visibility and enforcement, controlling for core economic factors emphasized in the Merger Guidelines. Although we do not observe the confidential materials submitted in HSR filings, we construct empirical proxies that closely correspond to the formal screens described in the Guidelines. In particular, our baseline control variables include: deal size (to capture transaction scale), a horizontal merger indicator (to reflect competitive overlap), acquirer size and market position (proxied by log net sales and industry HHI), and recent acquisition activity (to flag patterns of serial consolidation). These variables are designed to reflect the information that regulators themselves prioritize in merger review, enabling us to assess whether public visibility adds predictive power beyond these structural determinants.

We measure enforcement using a binary indicator I(Deal Flagged)_d, equal to one if deal d was formally challenged by the FTC or DOJ. The main independent variable is Visibility_d, defined as the acquirer's share of qualifying media coverage—excluding articles related to mergers, acquisitions, and regulatory actions—within its 4-digit NAICS industry during the t-month period prior to announcement. We report results across four pre-merger windows: 12, 9, 6, and 3 months. This variation allows us to examine the role of both persistent and short-term attention while ensuring robustness to the choice of visibility horizon.

Our empirical specification is a linear probability model of the form:

$$\text{I(Deal Flagged)}_d = \alpha + \beta_1 \cdot \text{Visibility}_d + \gamma' Z_d + \lambda_{j,p} + \delta_k + \varepsilon_d, \tag{1}$$

where Z_d denotes the set of baseline control variables described above. The fixed effects $\lambda_{j,p}$ represent the interaction of the acquirer's 4-digit NAICS industry j and the presidential administration p in office at the time of announcement, capturing variation in regulatory philosophy and policy enforcement across industries and administrations (e.g., Bush, Obama, Trump). The δ_k terms represent 6-digit NAICS fixed effects for the target industry, absorbing heterogeneity in product market structure and enforcement salience.

This empirical design exploits cross-sectional variation in visibility among deals announced within the same acquirer industry and administration. By comparing flagged and non-flagged transactions within these tightly defined institutional contexts, we aim to isolate the incremental role of acquirer visibility in predicting enforcement. Standard errors are clustered at the acquirer and announcement year levels to account for correlation in enforcement outcomes across deals involving the same firm or announced in the same period.

Table 2 presents estimates from our benchmark regressions testing whether acquirer visibility predicts antitrust enforcement. Across all specifications, visibility is positively and significantly associated with the probability that a deal is flagged. The coefficients imply that a ten percentage point increase in an acquirer's industry share of non-M&A, non-regulatory media coverage is associated with a 0.67 to 1.46 percentage point increase in the likelihood of regulatory challenge. These magnitudes are economically meaningful: the average flagging rate is 2.2% for all HSR-reportable deals and 3.9% for transactions subject to mandatory HSR review (as shown in Table 1). Thus, the visibility effect represents an increase in the likelihood of enforcement of 17% to 66% relative to these baselines.

The estimated effects are remarkably consistent across alternative constructions of the visibility measure, whether using 12-, 9-, 6-, or 3-month windows prior to deal announcement. This stability suggests that both persistent and more recent media exposure are predictive of early-stage scrutiny, and that the results are not sensitive to the specific horizon used to measure visibility. The inclusion of deal-level controls—such as transaction size, horizontal structure, industry concentration, and acquirer scale—further ensures that the estimates

isolate the role of public visibility from traditional economic screens.

Notably, the estimated coefficients are larger in the subsample of deals subject to mandatory HSR review (Columns 2, 4, 6, and 8), where the reporting threshold is higher and the size-of-person test no longer applies. This pattern is consistent with institutional features of the HSR framework: in the mandatory sample, all transactions are known to trigger agency screening under the statute. By contrast, the broader HSR-reportable sample includes deals that exceed the size-of-transaction threshold but may be exempt from review if they do not satisfy the size-of-person test. Unfortunately, because we do not observe party-level financials for non-public targets, we cannot cleanly identify which of these deals actually underwent review. We therefore include both samples in our benchmark results: the HSR mandatory sample serves as our preferred specification, offering a clean window into enforcement decisions among transactions that are definitively screened; the broader HSR-reportable sample offers a more inclusive view of the reporting universe, but may attenuate the estimated relationship due to unobserved screening exemptions. That the visibility effect remains significant in both cases reinforces its empirical relevance.

Robustness to Alternative Pre-Merger Windows One potential concern is that our benchmark estimates may be sensitive to the specific horizon used to construct the visibility measure. If media coverage fluctuates over time or reflects anticipated merger activity, then estimates based on a single window could conflate firm visibility with deal timing. To address this, we explore a broader set of pre-merger intervals: a 2-year window, a "donut" specification that excludes the 12 months immediately preceding announcement (using coverage from months 13–18), and a short-run 2-month window. Each alternative window targets a distinct dimension of potential bias. The 2-year measure captures longer-run firm salience, less affected by short-run news cycles. The donut specification is designed to rule out contamination from deal anticipation: if firms manage the timing of announcements to coincide with recent media exposure, or if press coverage responds to early deal rumors or leaks, this would bias

our baseline measures upward. By excluding the final 12 months before the transaction, the donut approach isolates visibility unrelated to contemporaneous deal activity. The 2-month window, by contrast, focuses on the most recent coverage, capturing the degree of short-run salience just prior to agency review.

As shown in Table A.1, all specifications yield positive and statistically significant coefficients on visibility. The magnitudes are comparable to those in our main results, suggesting that both long-run visibility and more immediate public attention are associated with increased regulatory scrutiny. The consistency of these findings across multiple windows reinforces the robustness of our baseline interpretation.

Excluding the Most Visible Acquirers A potential concern is that the positive relationship between visibility and regulatory scrutiny may be disproportionately influenced by a small number of extremely high-profile firms, such as Google or Amazon, whose transactions are likely to attract agency attention under any circumstances. While our baseline regressions control for deal size, acquirer scale, industry structure, and horizontal overlap, it remains possible that these "star firms" are flagged by factors beyond recent media coverage, such as longstanding public scrutiny or political salience. In such cases, visibility may be a proxy for institutional notoriety.

To address this concern, we re-estimate the main specification excluding deals in the top decile of the visibility distribution. As shown in Table A.2, the estimated coefficients remain statistically significant and, if anything, slightly larger in magnitude. This finding suggests that the relationship between visibility and enforcement is not driven solely by the most visible firms, but persists across a wide range of acquirers. By removing cases where scrutiny may be effectively automatic, this robustness check provides a stronger test of whether marginal differences in public attention affect regulatory decisions, even among firms that do not dominate headlines.

Excluding Earnings-Related Coverage Another potential concern is that media visibility around scheduled corporate events—such as earnings announcements—may drive spikes in coverage that are unrelated to the firm's broader public salience. If such events occur close to deal timing, they could confound the interpretation of visibility as an indicator of general prominence. To address this, we refine our measure by excluding articles related to earnings releases and financial reporting, in addition to the baseline filters that remove M&A- and regulation-related coverage.

Table A.3 reports the results using this narrower measure of public visibility. Across all time windows, the estimated coefficients remain positive, statistically significant, and comparable in magnitude to those in the baseline specification. This suggests that the relationship between pre-merger visibility and enforcement is not driven by routine coverage tied to earnings cycles or financial disclosure. Instead, it reflects broader public attention to the acquirer, consistent with the idea that regulators respond more strongly when a merger involves a firm operating under greater external visibility.

Extending the Sample Using Verifiable Size-of-Person Criteria As discussed in Section 2, the HSR Act requires pre-merger notification only when a deal exceeds both a size-of-transaction threshold and, for most transactions, a size-of-person test based on the financial characteristics of the parties involved. In our main analysis, we distinguish between the full set of HSR-reportable transactions and the subset subject to mandatory review, for which the size-of-person requirement is automatically satisfied by the transaction's scale. However, the full early-review universe includes additional deals that satisfy the size-of-person test, but for which this status is not directly observable in our data—particularly when the target firm is private or financial disclosures are incomplete. To better approximate the full set of screened transactions, we construct a supplemental sample that includes HSR-reportable deals for which we can verify compliance with the size-of-person test using available party-level financial data. While this approach does not capture the entire review-eligible

universe, it expands coverage beyond the mandatory sample and allows us to assess whether the visibility–enforcement relationship holds among a broader subset of deals that we know should have triggered agency scrutiny.

Table A.4 reports the results. Across all visibility windows, the coefficients remain positive, statistically significant, and similar in magnitude to our benchmark estimates. These findings provide further support for the empirical relevance of acquirer visibility in shaping regulatory outcomes and help address concerns that the results are driven by sample misclassification or selection into the mandatory review tier.

Alternative Political Environment Fixed Effects A potential concern is that our baseline fixed effects—acquirer industry interacted with presidential administration—may not fully capture the institutional environment shaping enforcement decisions. To address this, we consider two alternative measures of political context, each motivated by distinct oversight channels emphasized in the regulatory literature.

First, we replace presidential administration with the term of the *FTC Chair*[6]. The Chair plays a central role in directing enforcement priorities, setting agency tone, and initiating formal investigations. Unlike broader presidential alignment, Chair-specific leadership more directly governs early-stage screening behavior. Table A.5 presents the results using acquirer industry × FTC Chair fixed effects. The estimated coefficients on visibility remain positive, statistically significant, and of similar magnitude to those in our benchmark specifications. These findings suggest that our results are not sensitive to the particular leadership measure used to capture enforcement context.

Second, we examine the role of *Congressional Control*, the interaction between the acquirer industry and indicators for unified Democratic, unified Republican, or divided government. Congress controls FTC funding, conducts public oversight hearings, and can directly

[6]We assign FTC Chair terms based on the following leadership periods: Timothy Muris (R): Jun 2001–Aug 2004; Deborah Platt Majoras (R): Aug 2004–Mar 2008; William Kovacic (R): Mar 2008–Mar 2009; Jon Leibowitz (D): Mar 2009–Mar 2013; Edith Ramirez (D): Mar 2013–Jan 2017; Maureen Ohlhausen (R): Jan 2017–May 2018; Joseph Simons (R): May 2018–Jan 2021.

pressure the agency to pursue or avoid high-profile enforcement actions. Table A.6 reports the results. Across all visibility windows, the coefficients remain statistically significant and stable in magnitude, reinforcing the conclusion that public salience predicts scrutiny regardless of short-run political alignment.

Taken together, these alternative specifications address the concern that our findings may be driven by unobserved shifts in the regulatory environment not captured by presidential terms alone. The results confirm that visibility remains a robust predictor of enforcement even after conditioning on narrower, more proximate measures of agency leadership and political pressure.

4.2 Addressing Endogeneity: Within-Firm Design and Instrumental Variables

While the baseline specifications control for an extensive set of observables, including industry concentration, acquirer size, transaction value, horizontal overlap, recent acquisition activity, and absorb fixed effects for acquirer industry by political regime, concerns about endogeneity remain. Specifically, the positive association between visibility and enforcement could reflect unobserved firm-level traits correlated with both media coverage and regulatory risk, or it could reflect reverse causality if impending scrutiny attracts preemptive press attention.

We address these concerns using two complementary strategies: a saturated within-firm design and a plausibly exogenous instrument for media visibility.

Within-firm variation under a common political regime. Our first strategy absorbs all acquirer-specific and regulator-specific fixed characteristics by including fixed effects for the interaction between each acquirer and presidential administration (e.g., Bush, Obama, Trump). This specification identifies the visibility effect from variation across deals conducted

by the same firm under the same political environment. It thus accounts for persistent firm traits, such as lobbying history, legal exposure, deal frequency, or press strategy, as well as broader changes in enforcement philosophy across administrations.

The results are reported in Table 3. Across all specification windows (12, 9, 6, and 3 months), the visibility coefficients remain statistically significant and economically large. Compared to the benchmark estimates, the magnitudes are slightly higher, suggesting that controlling for acquirer-specific unobservables does not attenuate the relationship. These findings confirm that the association between media visibility and enforcement persists even when comparing deals initiated by the same firm under the same political administration.

Importantly, this within-firm specification helps rule out many confounding explanations, such as persistent differences in firm reputation, regulatory exposure, or lobbying history, but it does not by itself distinguish between competing interpretations of the visibility effect. One possibility is that visibility captures deal-specific soft information that is observable to regulators but not to researchers. Another possibility is that visibility acts as a behavioral cue, drawing regulators' attention independent of underlying economic risk. Our empirical design cannot fully separate these channels. Instead, the key takeaway from this specification is that the observed relationship between visibility and scrutiny is not driven by unobserved heterogeneity at the firm or political level, reinforcing the interpretation that public salience plays an active role in early-stage enforcement.

Instrumental variable strategy based on proximity to news bureaus. While the within-firm specification addresses a wide set of confounders, it cannot rule out all forms of endogeneity. In particular, unobserved deal-specific salience or anticipatory press coverage, perhaps in response to political signals or regulatory leaks, could still bias the observed relationship between visibility and enforcement. To address these concerns, we implement an instrumental variables strategy based on geographic proximity to media infrastructure.

The core idea follows prior literature showing that firms located closer to major financial

news outlets are more likely to receive incidental media coverage, independent of fundamentals or firm behavior. Malloy (2005) documents geographic frictions in equity research coverage, while Engelberg and Parsons (2011) and Dyck et al. (2008) show that local proximity to journalists predicts visibility and market reaction. Related work by Dai et al. (2015) and Solomon et al. (2014) leverages similar spatial variation to identify the causal impact of news exposure on governance outcomes and capital flows.

Following this tradition, we construct a hybrid instrument that captures both the extensive and intensive margins of geographic proximity. For each acquirer, we identify its nearest Dow Jones (DJ) news bureau and compute the geodesic distance to the firm's headquarters. We then interact a binary indicator for being located within 300 miles of a DJ office with the standardized log distance to that office. This construction flexibly captures the idea that firms located within media coverage zones, but closer to bureau hubs, are more likely to receive consistent attention, regardless of transaction specifics or firm strategy.

The exclusion restriction requires that, conditional on deal observables, firm characteristics, and sector-specific time effects, geographic proximity to Dow Jones news bureaus affects enforcement outcomes only through its influence on general media visibility. A potential concern is that firms located near major media hubs may systematically differ in ways that also affect the probability of antitrust intervention, for example, through heightened political exposure, lobbying activity, or reputational salience. While this restriction cannot be directly tested, we mitigate these risks in two ways. First, our specifications include a rich set of deal-level controls, covering transaction size, horizontal overlap, acquirer scale, market concentration, and acquisition history, to account for standard structural triggers of enforcement. Second, we absorb fixed effects for acquirer 4-digit NAICS by announcement year, ensuring that identification arises solely from cross-sectional variation in geographic proximity within narrow sector-year cells. Under this design, identification is not driven by comparisons across broad geographic or political contexts, but rather by differences in proximity among observationally similar firms operating in the same product market and

year.

Table 4 presents the results from our instrumental variables analysis. Columns (2), (4), (6), and (8) report the first-stage regressions, confirming that geographic proximity to Dow Jones news bureaus strongly predicts acquirer visibility. The estimated coefficients on the proximity instrument are positive and statistically significant across all pre-announcement windows, with F-statistics well above conventional thresholds. Columns (1), (3), (5), and (7) report the corresponding second-stage estimates. Across all specifications, instrumented visibility is positively associated with the probability of regulatory challenge, and the estimated coefficients are substantially larger than those from the earlier OLS models.

We interpret the amplification in the IV estimates not as a correction for classical measurement error, but as evidence that the instrument isolates the component of media visibility most likely to influence regulatory scrutiny. Proximity to Dow Jones news bureaus plausibly predicts greater intensity of journalist-initiated coverage, which is more likely to be noticed by regulators, either because it surfaces soft information relevant to competitive risk or because it increases salience through media-driven attention dynamics. While our design cannot distinguish between these mechanisms, the IV estimates reflect variation in the type of coverage that is more likely to shape early-stage review. In this sense, proximity serves not merely as a shift in the volume of media exposure, but as a shift toward coverage more likely to register in the screening process.

Taken together, the IV and within-firm results provide consistent evidence that public visibility causally affects early-stage antitrust enforcement. The instrument isolates plausibly exogenous variation in exposure to general business coverage, while the within-firm design absorbs all time-invariant characteristics of the acquirer. Although these estimates do not fully disentangle informational from reputational components, the pattern is consistent with regulators responding more strongly when enforcement decisions are likely to be evaluated under greater public visibility.

4.3 Visibility and Market-Implied Success Probability

In this section, we assess whether financial markets internalize the increased regulatory scrutiny associated with public visibility and whether the predictive power of visibility persists after conditioning on market expectations of deal success. If the stock market efficiently incorporates information about the likelihood of antitrust intervention, then to the extent that visibility reflects economically relevant deal characteristics, its effect should already be captured by the market-implied probability of deal completion. In this case, controlling for the market-implied success probability should attenuate the predictive effect of visibility. Conversely, if visibility continues to predict enforcement after conditioning on market expectations, it suggests that visibility captures factors beyond fundamental deal risk, such as behavioral or institutional responses by regulators, which markets may underappreciate.

We calculate the market-implied success probability by comparing the target stock price to the deal offer price. The intuition is that immediately after a deal is announced, the target stock price will jump, but not enough to the price at which the acquirer will buy the target stock in deal execution. The gap between the target price after the merger announcement and the offer price reflects the market's belief that the deal will eventually close. To capture this idea, the market-implied success probability is defined as

$$\text{P(Implied Success)}_d = \frac{P_{d,target} - P^0_{d,target}}{P_{d,deal} - P^0_{d,target}} \tag{2}$$

For each deal d, $P_{d,target}$ is the target stock price immediately after the merger announcement, and $P^0_{d,target}$ is the estimated value of the target firm if the deal fails, which is generally assumed to be the target stock price before the announcement. The offer price, $P_{d,deal}$, is calculated differently depending on the deal structure. For cash deals, the offer price is directly provided on a per-share basis. For stock deals, the offer price is the acquirer's stock price multiplied by the exchange ratio. We match merger announcement dates to the most recent trading day, and use the announcement end-of-day price to calculate $P_{d,target}$ and

$P_{d,deal}$, and the end-of-day price on the previous trading day as $P^0_{d,target}$. For simplicity, we use only all-cash-financed or all-stock-financed deals (Giglio and Shue, 2014), and we keep only deals for which the offer price or exchange rate ratio is available in SDC Platinum, so the number of deals in this analysis is lower than in our main results.

Using the implied success probability from market prices, we test whether news coverage of the acquiring firm continues to predict deal flagging, controlling for the implied probability. For a deal d happening in year t where firm i is the acquirer, we run deal-level regressions as follows:

$$\text{I(Deal Flagged)}_d = \alpha_{FE} + \beta_1 \text{Visibility}_d + \beta_2 \text{P(Implied Success)}_d + \gamma Z_d + \varepsilon_d, \tag{3}$$

where Visibility_d is the acquirer's share of industry news from 3 months to 1 year prior to the deal announcement date. We control for deal size (in millions of USD), a horizontal merger indicator, and the acquirer's net sales share. We also control for acquirer industry, announcement year, and target industry fixed effects.

Table 5 presents the results. Controlling for the market-implied probability of deal success, firm visibility continues to significantly predict the likelihood of being flagged by the FTC or DOJ. This finding is notable because the market-implied success probability is forward-looking and plausibly incorporates a wide array of observables and unobservables, including regulatory risk, that investors perceive as relevant to deal completion. The residual predictive power of visibility, conditional on this proxy, suggests that public visibility plays an independent role in triggering regulatory scrutiny.

A complementary question to this is whether the market at least partially prices in the effect of spotlight chasing. To study this, for the deal d that occurs in year t where firm i is the acquirer, we run deal-level regressions as follows:

$$\text{P(Implied Success)}_d = \alpha_{FE} + \beta_1 \text{Visibility}_d + \gamma Z_d + \varepsilon_d, \tag{4}$$

where Visibility_d is the acquirer's share of industry news from 3 months to 1 year prior to the deal announcement date. We control for deal size (in millions of USD), a horizontal merger indicator, and the acquirer's net sales share. We also control for acquirer industry, announcement year, and target industry FEs. In Appendix Table A.7, we show that visibility negatively correlates with the market-implied probability of success. The negative correlation aligns with our hypothesis that acquirers with higher public visibility tend to get flagged by the FTC more often, which decreases the probability that the deal goes through. However, the coefficient is statistically insignificant, which could indicate that the market does not understand the role of spotlight chasing in regulator decisions, or that they do not think the effect of public visibility on being flagged matters enough to decrease the success probability.[7]

Taken together, these results indicate that visibility has an independent and economically meaningful effect on early merger review, above and beyond what is captured by observable deal fundamentals or by market-perceived enforcement risk. Because the implied success probabilities already embed information shared by both regulators and investors, the remaining visibility gradient is unlikely to be driven solely by soft signals observable to both sides, and instead points to an evaluative channel that investors do not fully price but nonetheless shapes enforcement. This motivates us to propose a formal framework that clarifies the mechanism by which visibility enters the regulator's decision process.

5 A Model of Visibility, Accountability, and Merger Enforcement

The set of empirical analyses in the previous section show that, conditional on deal fundamentals and the political environment, acquirer visibility is a strong and robust predictor of whether a merger is challenged by the FTC or DOJ. This relationship persists across a

[7]The lack of statistical significance could also be a lack of sufficient data to calculate market-implied probability. We are agnostic about why there is no statistical significance, but the negative coefficients reassuringly align with our hypothesis.

wide range of specifications—including within-firm designs that absorb acquirer-specific characteristics and an instrumental-variables strategy based on proximity to Dow Jones news bureaus—indicating that visibility plays an independent role in shaping enforcement outcomes, above and beyond the structural screens emphasized in the Merger Guidelines. These patterns naturally raise the question of why visibility matters for enforcement.

Our explanation builds on the political-accountability framework of Maskin and Tirole (2004). Modern U.S. antitrust institutions delegate substantial discretion to agency staff in determining whether a transaction should be challenged, but the agencies' ultimate overseer is Congress, whose influence operates through budget appropriations and public oversight hearings.[8] In this environment, the reputational stakes of an enforcement decision are greater when a merger involves a highly visible acquirer. Anticipating this, regulators place greater weight on challenging such transactions. The framework developed below formalizes this mechanism and clarifies how visibility alters the incentives underlying regulatory decisions. It not only helps rationalize the empirical visibility–enforcement relation documented earlier, but more importantly, it also delivers an additional testable implication: the strength of the visibility gradient should depend on the intensity of congressional accountability pressures.

5.1 Setup and Information Structure

We consider a regulator (FTC or DOJ Antitrust Division) deciding whether to challenge a proposed merger, which is characterized by a latent level of competitive harm $H \in \mathcal{H} \subset \mathbb{R}$, drawn from prior $g(H)$.[9] The regulator chooses an enforcement action $a \in \{0, 1\}$, where $a = 1$ denotes a decision to *challenge* the merger and $a = 0$ denotes a decision to *clear* it.

[8] Agency leadership regularly testifies before House and Senate committees during the appropriations cycle. Because members of Congress are themselves electorally accountable, they have incentives to react to mergers that attract public attention, heightening reputational costs for regulators who appear too lenient in salient cases.

[9] The latent variable H represents the true competitive harm associated with a proposed merger, aggregating all structural determinants emphasized in the Merger Guidelines (market concentration, overlap, entry conditions, the scale of the transaction, etc.). In the empirical analysis, we proxy for H using observable deal fundamentals, but the regulator observes additional non-public information.

Let $W(a, H)$ denote the social welfare when action a is taken in state H. The net social gain from challenging is defined as

$$\Delta W(H) \equiv W(1, H) - W(0, H),$$

which captures the welfare benefit of blocking a transaction with harm H.

Assumption 1 (Policy monotonicity)**.** *$\Delta W(H)$ is strictly increasing in H.*

Assumption 1 formalizes the idea that challenging is socially more desirable when the merger poses greater competitive harm. Challenging a merger entails a fixed process cost $C(1) > 0$ (representing administrative, litigation, and opportunity costs), whereas clearing is costless, that is $C(0) = 0$.

Private information. Before choosing an enforcement action, the regulator receives a private signal $s \in \mathcal{S} \subset \mathbb{R}$ drawn from density $f(s \mid H)$. This signal captures non-public investigative information, including internal market analyses, confidential documents submitted during the HSR review, and staff assessments of competitive conditions that do not enter the public record.[10]

Assumption 2 (Monotone likelihood ratio)**.** *For any $H_2 > H_1$, the likelihood ratio $f(s \mid H_2)/f(s \mid H_1)$ is weakly increasing in s.*

Assumption 2 ensures that higher private signals are associated with higher expected competitive harm. Let

$$m(s) \equiv \mathbb{E}[\Delta W(H) \mid s],$$

denote the regulator's posterior expected social gain from challenging. Under Assumptions 1–2, $m(s)$ is strictly increasing in s.

[10] In practice, the regulator observes substantially more information than external audiences. The private signal s represents this richer informational set.

Oversight audience and visibility. Enforcement decisions are publicly observable, and Congress—as the agency's formal overseer—evaluates these decisions through appropriations and oversight hearings.[11] Congress does not observe the true competitive harm H or the regulator's private signal s. Instead, it forms a judgment about whether the regulator's action appears *appropriate*.

Appropriateness. Let

$$\theta(a, H) \in \{0, 1\}$$

denote whether action a is normatively appropriate in state H. We assume:

$$\theta(a, H) = \begin{cases} 1 & \text{if challenging is appropriate given } H \text{ or clearing is appropriate given } H, \\ 0 & \text{otherwise.} \end{cases}$$

This definition specifies which enforcement actions would be correct for each level of true harm. Although Congress does not observe H, the appropriateness mapping $\theta(a, H)$ represents the normative benchmark—what the correct action would be if H were known. Congress observes only the evaluative signal p and thus forms a noisy belief about whether the regulator's action matches this benchmark.

Evaluative signal and visibility. Congress observes an evaluative signal p generated according to an information structure $\mathcal{I}(V)$:

$$p \sim \varphi(\cdot \mid \theta(a, H), V),$$

[11] Congress exercises control over the antitrust agencies through budget appropriations and public oversight hearings (see Weingast and Moran (1983)). Members of Congress are electorally accountable to voters and therefore have incentives to respond to salient regulatory actions, especially when interest groups or constituents mobilize attention. The audience in our model should be interpreted as Congress operating within this fire-alarm oversight structure (see McCubbins and Schwartz (1984)).

where $V \geq 0$ captures the acquirer's *public visibility*. In the model, V summarizes how prominent the acquiring firm is in the public domain and thus how confidently members of Congress can form a judgment about whether the regulator's action appears appropriate.

Assumption 3 (Visibility increases evaluative informativeness)**.** *For each $V \geq 0$, the evaluative signal p is drawn from a likelihood function $\varphi(p \mid \theta, V)$ satisfying the monotone likelihood ratio property in θ. Moreover, for any $V' > V$, the induced posterior belief $\mu_{V'}(\theta \mid p)$ is a mean-preserving spread of $\mu_V(\theta \mid p)$.*

Intuitively, the MLRP condition means that for any fixed level of visibility V, larger realizations of the evaluative signal p move Congress's belief in a consistent direction about which evaluative state θ is correct (whether toward appropriateness or inappropriateness). The second part of the assumption—that $\mu_{V'}(\theta \mid p)$ is a mean-preserving spread of $\mu_V(\theta \mid p)$—ensures that higher visibility V' yields a more informative evaluative signal: posteriors place more probability mass near $\{0, 1\}$ and less near 0.5, without requiring that these judgments be correct or unbiased.[12]

5.2 Congress's Evaluation and the Regulator's Objective

Congress observes the enforcement action a and the evaluative signal p, and forms a belief about whether the action was normatively appropriate. Recall that $\theta(a, H) \in \{0, 1\}$ denotes the appropriateness of action a in state H, where $\theta = 1$ indicates that the action is appropriate and $\theta = 0$ that it is inappropriate. Congress does not observe H or the regulator's private signal s; instead, it uses Bayes' rule to infer the likelihood that $\theta(a, H) = 1$ from the signal p and the visibility level V.

[12] Visibility affects the *confidence* with which Congress evaluates the regulator's action, not the underlying competitive harm H or the regulator's private information s. Evaluations may be unbiased or systematically distorted (for example, a "populist tilt" that treats highly visible firms as more problematic). Our results do not require any particular bias; the mechanism operates through increased evaluative informativeness as V rises.

Congress's evaluation rule. Let

$$\mu_V(\theta = 1 \mid p, a) \;\equiv\; \Pr\big(\theta(a, H) = 1 \mid p, a, V\big)$$

denote Congress's posterior belief that the regulator's action was appropriate, given the evaluative signal p and visibility V. Congress chooses an evaluation $e \in \{1, 0\}$, where $e = 1$ denotes approval and $e = 0$ denotes disapproval. Congress prefers to match the underlying appropriateness state $\theta(a, H)$, so that the expected payoff from choosing evaluation e is

$$U_C(e \mid p, a, V) \;=\; \Pr\big(\theta(a, H) = e \mid p, a, V\big) \;=\; \begin{cases} \mu_V(\theta = 1 \mid p, a) & \text{if } e = 1, \\ 1 - \mu_V(\theta = 1 \mid p, a) & \text{if } e = 0. \end{cases}$$

Thus Congress approves if and only if the posterior probability of appropriateness is at least one half:

$$e^*(p; V, a) \;=\; \begin{cases} 1 & \text{if } \mu_V(\theta = 1 \mid p, a) \;\geq\; \tfrac{1}{2}, \\ 0 & \text{otherwise.} \end{cases}$$

Visibility affects the evaluation only through the information structure $\mathcal{I}(V)$, which governs the distribution of p conditional on $\theta(a, H)$. Assumption 3 implies that higher V makes the posterior $\mu_V(\theta = 1 \mid p, a)$ more extreme, so realizations of p are more likely to induce strong approval or strong disapproval rather than agnostic beliefs near 1/2.

Regulator's reputational payoff. Congress's evaluation affects the regulator's future budgetary negotiations, oversight intensity, and political standing. Let the reputational value of receiving approval be r_G and the value of receiving disapproval be r_B, with $r_G > r_B$. The regulator's expected reputational payoff from taking action a at visibility V is

$$R(a, V) \;=\; r_G \Pr\big(e^* = 1 \mid a, V\big) \;+\; r_B \Pr\big(e^* = 0 \mid a, V\big),$$

where the probabilities are taken over the distribution of p induced by $\mathcal{I}(V)$ and the induced belief $\mu_V(\theta = 1 \mid p, a)$.

The relevant object for enforcement incentives is the *reputational differential* between challenging and clearing:

$$\Delta R(V) \;\equiv\; R(1, V) - R(0, V).$$

This term captures how much reputationally better (or worse) it is, in expectation, to challenge rather than clear a merger at visibility level V. By construction, $\Delta R(V)$ depends only on the visibility environment and the evaluation rule, not on the regulator's private signal s.

Assumption 4 (Directional reputational asymmetry)**.** *There exists $\sigma \in \{-1, 0, 1\}$ such that for all economically relevant visibility levels V,*

$$\operatorname{sign}\big(\Delta R(V)\big) = \sigma.$$

Moreover, when $\sigma = +1$, $\Delta R(V)$ is strictly increasing in V on the economically relevant range; when $\sigma = -1$, $\Delta R(V)$ is strictly decreasing in V on that range; and when $\sigma = 0$, $\Delta R(V)$ is identically zero.

When $\sigma = +1$, challenging is reputationally favored relative to clearing (under-enforcement is more costly than over-enforcement), and higher visibility amplifies this asymmetry. When $\sigma = -1$, clearing is reputationally favored (over-enforcement is more costly), and visibility amplifies this in the opposite direction. When $\sigma = 0$, reputational incentives are directionally neutral and visibility has no effect through this channel.

Regulator's objective. Let $\lambda \geq 0$ denote the weight that the regulator places on reputational considerations relative to policy accuracy. Given private signal s and visibility V, the regulator chooses $a \in \{0, 1\}$ to maximize

$$U(a, s, V) \;=\; m(s) \cdot \mathbf{1}\{a = 1\} \;-\; C(a) \;+\; \lambda\, \Delta R(V),$$

where $m(s) = \mathbb{E}[\Delta W(H) \mid s]$ is strictly increasing in s by Assumptions 1–2. The regulator trades off expected social efficiency against the reputational benefit or cost associated with the enforcement action.

5.3 Optimal Challenge Rule and Testable Predictions

Given the regulator's objective, the enforcement decision reduces to comparing the expected payoff from challenging versus clearing. For any private signal s and visibility level V, define

$$\Delta U(s, V) \;=\; U(1, s, V) - U(0, s, V) \;=\; m(s) - C(1) \;+\; \lambda\, \Delta R(V).$$

Because $\Delta R(V)$ does not depend on s, and $m(s)$ is strictly increasing, the regulator's choice follows a unique cutoff rule.

Lemma 1 (Threshold structure)**.** *For each visibility level V and accountability weight λ, there exists a unique cutoff $\tau(V)$ such that the optimal enforcement rule is*

$$a^*(s, V) = 1 \quad \Longleftrightarrow \quad s \geq \tau(V),$$

where $\tau(V)$ is implicitly defined by

$$m(\tau(V)) - C(1) + \lambda\, \Delta R(V) = 0.$$

Sketch of proof. $m(s)$ is strictly increasing by Assumptions 1–2, while the reputational term $\lambda \Delta R(V)$ is constant in s. Thus $\Delta U(s, V)$ is strictly increasing in s and crosses zero once, implying a unique cutoff rule. See Appendix B for the full proof. □

We now characterize how this cutoff responds to visibility. The sign is governed by the directional reputational asymmetry (Assumption 4).

Proposition 1 (Visibility and enforcement)**.** *If under-enforcement is reputationally more costly (i.e.* $\sigma = +1$ *so that* $\Delta R(V) \geq 0$ *and* $\Delta R'(V) > 0$*), then* $\tau'(V) < 0$ *for all* V*: higher visibility increases the likelihood of a challenge. If* $\sigma = -1$*, the relationship reverses; if* $\sigma = 0$*, visibility has no effect on enforcement.*

Sketch of proof. Implicit differentiation of the cutoff condition yields

$$\tau'(V) = -\frac{\lambda \, \Delta R'(V)}{m'(\tau(V))}.$$

When $\sigma = +1$, $\Delta R'(V) > 0$ and $m'(\tau(V)) > 0$, implying $\tau'(V) < 0$. See Appendix B for the full proof. □

This proposition clarifies how visibility shapes enforcement. The empirical results documented in Section 4 consistently point to the case $\sigma = +1$: regulators behave as if under-enforcement is reputationally more costly than over-enforcement. This interpretation aligns with institutional practice and anecdotal evidence. As a former DOJ official noted in discussing high-profile mergers such as CVS–Aetna, "you think twice before clearing a big, well-known acquirer, because everyone will be watching if it goes wrong." Congressional oversight hearings reinforce this asymmetry: criticism for failing to challenge a salient deal is more visible and politically damaging than criticism for pursuing a challenge too aggressively. These features make $\Delta R'(V) > 0$ the relevant case, implying $\tau'(V) < 0$ and generating the visibility gradient observed in the data. For the remainder of the analysis, we therefore focus on the empirically and institutionally relevant environment in which higher visibility increases the likelihood of challenge through the accountability channel.

The framework also delivers a deeper and more diagnostic prediction. Because visibility enters the regulator's objective only through the reputational term, any effect of visibility on enforcement should be mediated by the strength of accountability incentives. This observation is important for distinguishing the accountability channel from alternative explanations, such as omitted structural screens, or time-varying political preferences, which could gener-

ate a positive visibility gradient but would not imply that the magnitude of that gradient should vary systematically with the intensity of congressional oversight. The next result formalizes this comparative-static relationship and provides a mechanism-based prediction that is uniquely informative about the underlying source of the visibility effect.

Proposition 2 (Visibility and accountability incentives)**.** *Suppose* $\sigma = +1$*. Then the marginal effect of visibility on enforcement is strictly increasing in the accountability weight* λ*:*

$$\frac{\partial}{\partial \lambda}\big(-\tau'(V)\big) > 0.$$

Sketch of proof. From the expression

$$-\tau'(V) = \frac{\lambda\, \Delta R'(V)}{m'(\tau(V))},$$

the right-hand side is linear and strictly increasing in λ when $\Delta R'(V) > 0$. See Appendix B for the full proof. □

5.4 Accountability Cycles and the Visibility Gradient

Proposition 2 implies that the marginal effect of acquirer visibility on enforcement should vary systematically with the intensity of accountability. Accountability pressures are especially high during the appropriations hearings, when agency leadership testifies before the relevant committees. These hearings directly influence the agencies' future budgets and their standing with Congress, so decisions that attract negative attention from legislators carry greater reputational consequences for regulators. In contrast, accountability pressures weaken sharply during strict lame-duck periods preceding a party-changing transition and the appointment of a new FTC Chair, when incumbent officials face little prospect of future oversight. If visibility affects enforcement through reputational incentives, the visibility gradient should be strongest when oversight is consequential and should substantially diminish

when it is not.

Appropriations cycle. We first use the federal appropriations calendar, which divides the fiscal year into six windows: Off-season, Budget Preparation, Budget Release, Congressional Hearings, Markups, and Final Appropriations. The hearings window is the period in which FTC and DOJ leadership appear before Congress, and thus when the reputational cost of clearing a salient merger is likely to be highest.

To allow the visibility gradient to vary across these windows, we estimate:

$$\text{I(Deal Flagged)}_d = \alpha + \sum_w \beta_w \big(\text{Visibility}_d \times \mathbf{1}\{\text{Window}_d = w\}\big) + \gamma' Z_d + \lambda_{j,p} + \delta_k + \varepsilon_d, \quad (5)$$

where $\mathbf{1}\{\text{Window}_d = w\}$ indicates the appropriations window in which deal d is announced. Figure 4 reports the resulting window-specific marginal effects of visibility on the probability that a deal is challenged.

The estimated effects display a clear accountability cycle. The hearings window exhibits the largest and most precisely estimated marginal effect of visibility, with a positive and statistically significant effect on the probability of challenge. In contrast, for most other windows the marginal effects are smaller in magnitude and generally not statistically different from zero. These patterns are consistent with the prediction that visibility matters most when congressional scrutiny—and hence the reputational return to appearing vigilant—is strongest.

Strict lame-duck periods. We next study periods in which accountability incentives weaken sharply. Following institutional convention, we define a strict lame-duck period as the final three months before the appointment of a new FTC Chair *following a presidential party change*. During these months, the outgoing agency leadership faces minimal future oversight risk.

To compare the visibility gradient in normal versus strict lame-duck months, we estimate:

$$\text{I(Deal Flagged)}_d = \alpha + \sum_{c \in \{\text{Normal, LameDuck}\}} \beta_c \big(\text{Visibility}_d \times \mathbf{1}\{\text{Status}_d = c\}\big) + \gamma' Z_d + \lambda_{j,p} + \delta_k + \varepsilon_d, \tag{6}$$

where $\mathbf{1}\{\text{Status}_d = c\}$ identifies deals announced during normal months versus strict lame-duck periods. The corresponding status-specific marginal effects of visibility are plotted in Figure 5.

The contrast is obvious. During normal months, visibility has a strong and statistically significant effect on the likelihood of challenge. During strict lame-duck periods, the marginal effect is statistically indistinguishable from zero and opposite in sign. In other words, the visibility gradient effectively disappears precisely when future accountability pressures for incumbent leadership are weakest.

5.5 Consumer Salience and Cross-Sectional Variation in the Visibility Gradient

Proposition 2 also implies that the marginal effect of acquirer visibility on enforcement should be stronger in environments where accountability pressures are intrinsically higher. While the previous subsection exploited time-series variation in the strength of congressional oversight, the same logic applies to cross-sectional differences across industries. Mergers involving consumer-facing acquirers—such as retail, telecommunications, airlines, or healthcare services—tend to generate greater public and electoral attention than transactions in business-to-business sectors. Because legislators are more likely to face constituent pressure when a highly visible consumer brand engages in a merger, the reputational cost of underenforcement should be larger for consumer-facing firms. In the framework, this corresponds to a higher accountability weight λ, which Proposition 2 predicts will amplify the visibility gradient.

To test this implication, we classify each acquirer into a consumer-facing or non-consumer-facing industry following Delgado and Mills (2020). An industry is coded as consumer-facing if more than 35% of its output is sold directly for personal consumption expenditure. We then estimate:

$$\text{I(Deal Flagged)}_d = \alpha + \sum_{c \in \{0,1\}} \beta_c \left(\text{Visibility}_d \times \mathbf{1}\{\text{ConsumerFacing}_d = c\} \right) + \gamma' Z_d + \lambda_{j,p} + \delta_k + \varepsilon_d, \tag{7}$$

Figure 6 plots the implied marginal effects for consumer-facing and non-consumer-facing industries. The contrast is distinct. For acquirers in non-consumer-facing industries, the marginal effect of visibility is close to zero and statistically insignificant. In consumer-facing industries, by contrast, the marginal effect is economically significant and positive, approximately 0.52, with a 95% confidence interval spanning roughly 0.22 to 0.81. This divergence indicates that visibility is consequential only when the acquiring firm operates in an industry in which consumers (and, by extension, voters and legislators) are directly attentive.

Taken together, both the time-series appropriation cycle test and the cross-sectional industry evidence provide direct support for Proposition 2. Visibility matters for enforcement precisely when accountability pressures are operative: the visibility gradient strengthens during appropriations hearings, disappears in strict lame-duck periods, and is substantially larger for consumer-facing acquirers whose mergers attract greater external attention. These patterns are difficult to reconcile with seasonality, cyclical deal composition, or unobserved structural screens, yet they arise naturally from the model's accountability mechanism. When the political cost of under-enforcement is higher, regulators respond more strongly to public visibility.

6 Real Consequences of Regulatory Spotlight Chasing

The preceding section shows that acquirer visibility systematically affects the probability of antitrust challenge in a manner consistent with the accountability mechanism formalized in Section 5. If visibility shifts the set of mergers that receive regulatory scrutiny, an immediate question is whether such visibility-driven enforcement has real economic consequences. In particular, when two otherwise comparable transactions receive different enforcement treatment solely because one acquirer is more visible, the difference in post-merger outcomes isolates the effects of being flagged versus being overlooked. This section examines those downstream consequences by studying whether challenged and non-challenged mergers exhibit systematically different trajectories in firm behavior and performance after deal completion.

To evaluate whether visibility-driven enforcement patterns have substantive economic consequences, we examine two complementary margins. First, we study "false negative" transactions—mergers that were not challenged but appear similar, based on observable deal fundamentals, to those that typically are. This comparison allows us to assess whether low-visibility acquirers that avoided scrutiny exhibit different post-merger expansion than observationally similar unchallenged acquirers. Second, we analyze mergers that were challenged but ultimately completed and examine whether the experience of being flagged moderates the acquiring firm's subsequent trajectory. Across both exercises, we track post-merger outcomes related to competitive impact—including growth in sales, profits, industry share, and investment. Together, these analyses quantify the downstream effects of visibility-sensitive enforcement and assess whether the accountability mechanism alters post-merger competitive dynamics.

6.1 Effects of Overlooked Deals

In this section, we study the firm outcomes for acquirers whose deals should have been flagged but were overlooked. We construct a sample of firms that should be flagged, but not via propensity score matching. First, we compute propensity scores by running logit regressions year by year:

$$\text{P(Flag=1)}_{i,t} = \Phi(\alpha_t + \gamma_t' Z_{i,t} + \varepsilon_{i,t}) \tag{8}$$

We estimate logistic regressions on a firm-year panel, using an indicator for being flagged as a treatment among the sample of all deals with non-missing *announced* in year t. We estimate the logit model using relevant deal characteristics based on the FTC merger guidelines, including deal size (in millions USD), an indicator for horizontal mergers, the industry Herfindahl index, and the acquirer's net sales, industry sales shares, and recent deal activity. We use the predicted values from the logit regressions as deal-level propensity scores for being flagged. We then run a propensity score matching to obtain five nearest neighbors in the control deals for each treatment deal. These deals have a high propensity to be flagged under the FTC's merger guidelines, meaning that if regulators followed them closely, these deals should have been flagged for inspection. However, due to distortions, including regulator spotlight-chasing, these deals went overlooked.

We study the real consequences of these overlooked deals by comparing them with other unchallenged deals. We restrict the sample to HSR-mandatory deals to ensure that all treatment and control groups enter the FTC filing process. We run deal-level regressions of future firm growth and investment after the deal is *completed*.

$$Y_{i,t+\tau} - Y_{i,t} = \alpha_{FE} + \beta \text{Should be Flagged}_{i,t} + \gamma' W_{i,t} + \varepsilon_{i,t} \tag{9}$$

In this regression, we only use the firms that are not flagged by the FTC or DOJ. The

treatment variable equals 1 for acquirers of deals with a high propensity to be flagged under FTC guidelines, and equals 0 for other control firms. We control for ex-ante characteristics of the firm in year $t-1$ at the completion date to satisfy the conditional exogeneity condition: net leverage, log capital, log employment, log output, log profit, age, and book-to-market ratio. We also control for acquirer-industry interactions with POTUS-term fixed effects, as well as target-industry fixed effects.

In Table 6, we report results of interesting dependent variables: future profit growth $\log(P_{i,t+\tau}) - \log(P_{i,t})$, future sales $\log(Sale_{i,t+\tau}) - \log(Sale_{i,t})$, future industry sales shares $\%\text{Sales}_{i,t+\tau} - \%\text{Sales}_{i,t}$, as well as market power related variables, including future gross profit margins $\text{Gross Margin}_{i,t+\tau} - \text{Gross Margin}_{i,t}$, and future markups $\text{Markup}_{i,t+\tau} - \text{Markup}_{i,t}$, as defined in De Loecker et al. (2020). We find that acquirers of overlooked deals grow in profits and sales and take up larger industry sales shares. They can do this without incurring higher marginal costs, thereby increasing gross margin. These results indicate that acquirers of overlooked deals exert a higher market power and introduce a less competitive environment. We provide additional supporting evidence using the markup measure constructed by De Loecker et al. (2020), and confirm that acquirers of overlooked deals increase their markups after deal completion, compared with other unflagged control deals.

6.2 Effects of Getting Flagged and Over-Enforcement

In this section, we first study the regulatory treatment effect: the effects of being flagged by the FTC or DOJ on acquirers' future growth, performance, and investment, compared with similar deals based on observables. We discuss the effectiveness and shortcomings of getting flagged on the acquirer and argue that although the regulatory scrutiny seems effective on the focal deal, the effects on consumer welfare are ambiguous. Finally, we discuss the potential over-enforcement costs for deals flagged due to heightened visibility.

We first show that flag decisions by FTC or DOJ are effective in reducing market power

on the focal firm, but with side effects of inefficient investments and financing risk. To test the regulatory treatment effect, we match flagged acquirers to ex-ante observationally similar control firms. In Appendix C, we discuss how exactly we use a propensity score reweighting regression to estimate the treatment effects. We run regressions of future firm growth, performance, and investment on the indicator of being flagged, using inverse propensity scores as regression weights. Table C.1 presents the results. Being flagged by the FTC or the DOJ and going through the regulatory process indeed leads the focal firm to have lower market power, resulting in lower profit margins and lower markups, which aligns with the FTC's objectives. However, regulatory scrutiny also has side effects. The flagged firms invest heavily in capital and labor, but without good investment opportunities, the increased capital and labor are spent inefficiently. As a result, firms pile on debt and increase leverage, thereby increasing financing risk and potentially creating a debt overhang problem.

Moreover, even though a decrease in the focal firm's market power is indicative of the effectiveness of antitrust regulatory inspections, its effect on consumer welfare remains ambiguous. For example, if a competitor in the same market takes advantage of the regulatory inspection of the focal firm and gains market power, the increase in its market power may exceed the focal firm's decrease in market power under our regulatory treatment. In that case, market competition may worsen, and consumer welfare may still be harmed. We plan to further analyze whether consumer welfare improves after the FTC/DOJ inspection in a consumer-market-level analysis.

The regulatory treatment effects also raise concerns about over-enforcement, due to spotlight chasing. There can be deals that should not be flagged based on hard information in the FTC guidelines, but that eventually get flagged *solely* because their acquirers have high public visibility. If we extrapolate our estimated regulatory treatment effect for being flagged in Appendix C to these false-positive deals, we get costly over-enforcement. Firms that should not be flagged but are flagged suffer reduced profitability and investment efficiency, worsening their performance and financial condition. It is, however, important to note that we

estimate treatment effects by comparing flagged firms to similar firms in terms of visibility, size, industry, and deal characteristics; the comparison will differ for these false-positive firms. To estimate the regulatory cost of false positives, we plan to design an empirical strategy to compare these firms with similar firms that differ only in terms of firm and deal characteristics and are not flagged solely due to low visibility.

7 Conclusion

In this paper, we document a systematic pattern in U.S. antitrust enforcement: acquirers with greater public visibility face a significantly higher likelihood of being flagged for in-depth review under the Hart–Scott–Rodino Act. We refer to this pattern as *spotlight chasing*—the tendency of regulators to respond more strongly to mergers involving highly visible firms. Using firm-level media coverage unrelated to the transaction, we show that this visibility gradient emerges in industry-year specifications, within-acquirer comparisons, and an instrumental-variables design based on proximity to Dow Jones news bureaus. Market-implied approval probabilities further indicate that investors do not fully internalize this enforcement margin, highlighting a visibility channel not captured by standard assessments of competitive harm.

Motivated by these empirical findings, we develop a simple accountability framework in the spirit of Maskin and Tirole (2004). In the model, public visibility increases the assertiveness with which congressional overseers can form an opinion on whether a challenge or clearance appears appropriate. Because agency leadership is evaluated through appropriations and oversight hearings, visibility raises the reputational stakes associated with appearing overly lenient on salient mergers. The framework microfounds regulators' spotlight-chasing behavior and predicts that the visibility gradient should vary with the strength of congressional oversight. Consistent with this prediction, the effect of visibility is most potent during appropriations hearings—when agency leaders testify before Congress and future bud-

gets are negotiated—and effectively disappears during strict lame-duck periods preceding a party-changing transition. This mechanism also yields a cross-sectional prediction: visibility should matter more in industries with higher levels of legislative scrutiny. Supporting this, the visibility gradient is substantially steeper for consumer-facing acquirers than for firms operating in business-to-business sectors.

We then examine whether visibility-driven enforcement has real economic consequences. Studying overlooked low-visibility mergers that resemble challenged deals on structural criteria, and comparing flagged-but-completed mergers to observably similar controls, we find systematic differences in acquirers' profits, sales, market shares, and investment behavior. Overlooked deals accumulate market power, and flagged deals experience inefficient expansion and increased financial burdens, despite an ambiguous effect of scrutiny on the market-level competitive environment. These patterns indicate that spotlight chasing influences not only which mergers receive scrutiny but also the post-merger evolution of firms, with implications for competitive dynamics and resource allocation.

Taken together, our findings highlight an accountability mechanism through which public visibility shapes merger enforcement under the HSR framework. This mechanism generates variation in scrutiny that market participants do not fully anticipate and has material consequences for market structure and firm behavior. The results point to several policy implications. First, they underscore the importance of institutional arrangements that preserve the independence of federal antitrust agencies, particularly during periods of heightened political attention. Second, they suggest that reforms to the early-stage merger review—such as clearer standards, more transparent disclosure, and greater predictability in preliminary assessments—could reduce visibility-driven distortions. Third, they infer a complementary role for private antitrust enforcement: empowering well-designed private actions may help offset the selective pressures inherent in politically exposed public enforcement. More broadly, by showing how political accountability distorts the allocation of regulatory scrutiny toward highly visible firms, the paper underscores a systemic risk for antitrust enforcement: when

attention rather than competitive harm shapes intervention, merger policy may fail to protect consumers and instead allow durable market power to accumulate in ways that existing institutional designs do not fully anticipate.

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Figure 1: Schematic Overview of the HSR Review Process

This flowchart illustrates the screening process established under the Hart–Scott–Rodino (HSR) Act. Proposed mergers are first evaluated based on deal size. Transactions below the $50 million threshold are exempt from review. Transactions between $50 million and $200 million are subject to the size-of-person test, which requires one party to have assets or annual sales of at least $100 million and the other at least $10 million. If the test is met, an HSR filing is required. Transactions above the $200 million threshold are subject to mandatory review regardless of party size. All reportable deals are subject to a 30-day waiting period, during which the FTC or DOJ may request additional information (a Second Request). At the end of this process, the transaction may proceed without intervention or be flagged for enforcement action. Threshold values shown reflect the standards set in 2001 under the HSR Modernization Act; actual thresholds are adjusted annually based on changes in nominal GDP, as mandated by statute.

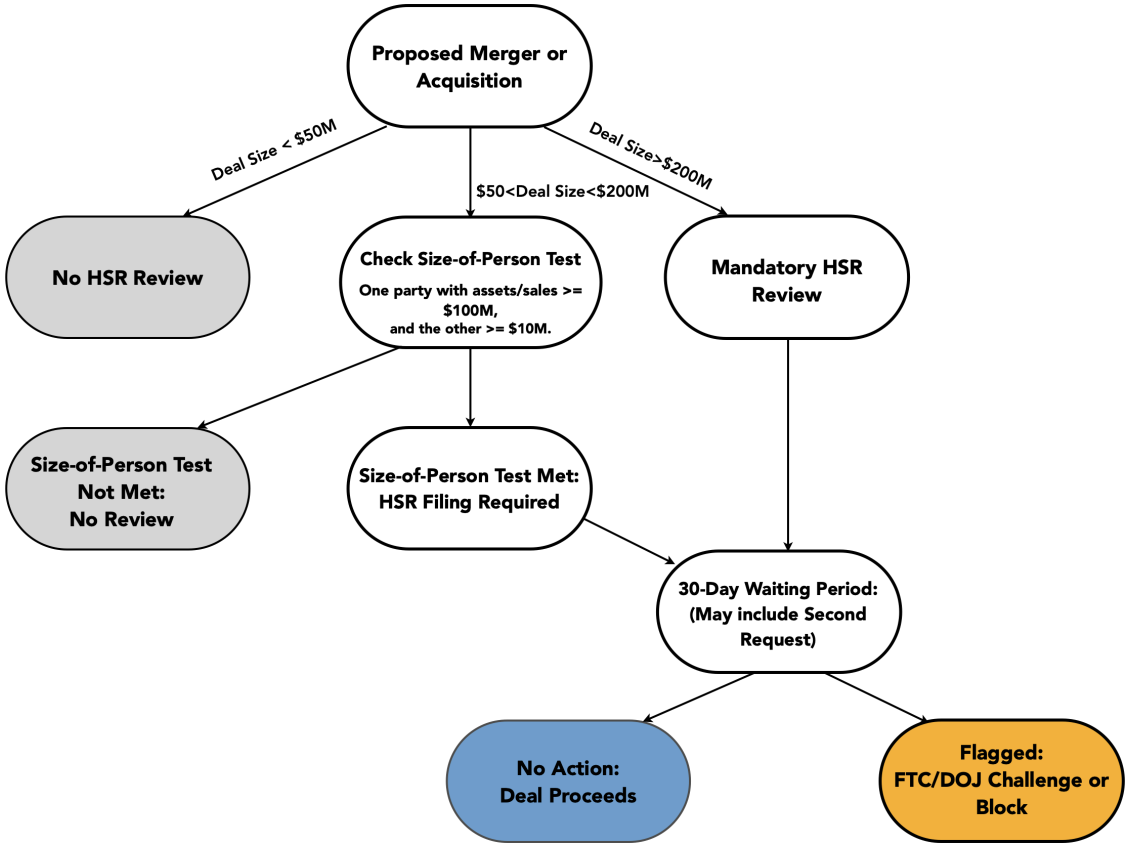


Figure 2: Number of Deals above HSR Transaction Threshold

This figure compares the monthly number of transactions above HSR-related thresholds using different sources. The gray dashed line represents the official number of premerger notification filings reported by the FTC. The blue solid line shows the number of deals in the SDC database that exceed the inflation-adjusted HSR size-of-transaction threshold based on reported deal values. The red solid line plots the subset of transactions in SDC that are subject to mandatory HSR review – i.e., deals exceeding the upper threshold for which the size-of-person test does not apply. This final series defines the main empirical sample used in our analysis.

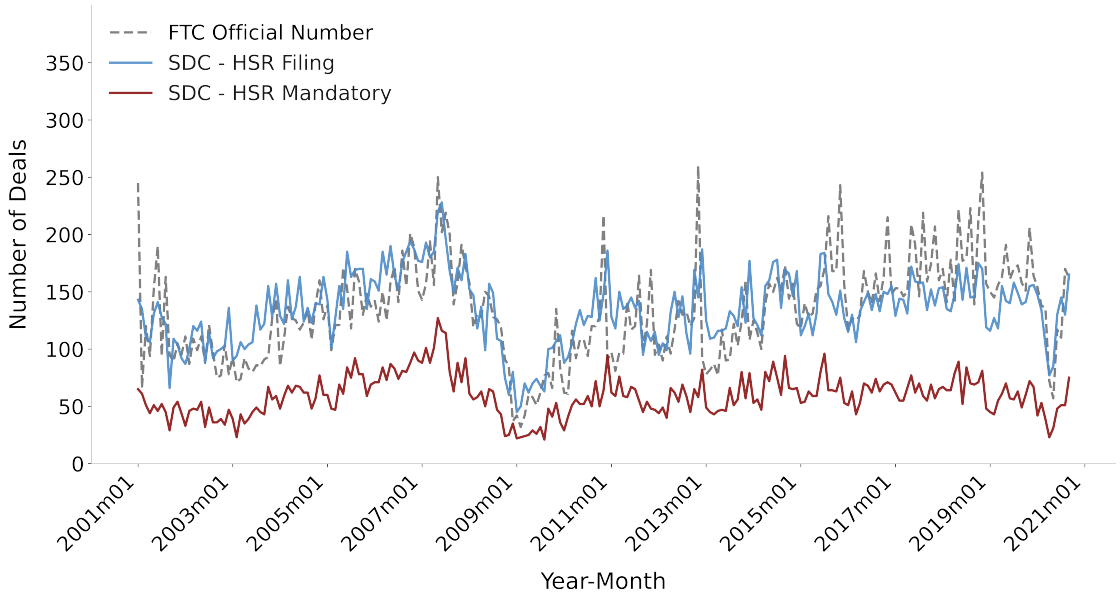
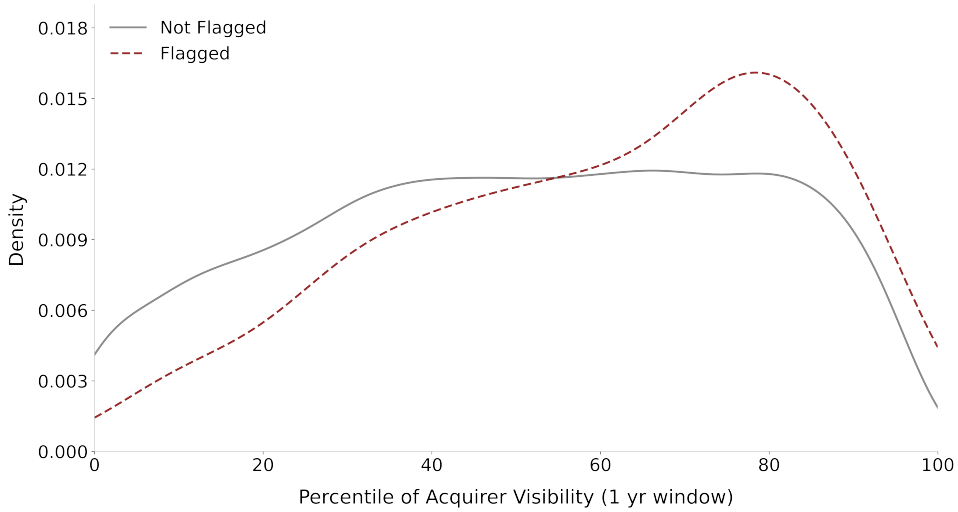


Figure 3: Visibility Distributions for Flagged vs. Non-Flagged Mergers

This figure plots the kernel density of acquirer visibility, measured as the firm's share of non-M&A and non-regulatory media coverage within its 4-digit NAICS industry during the one-year period prior to deal announcement. Panel A includes all transactions that exceed the HSR reporting thresholds and involve public acquirers between 2000 and 2020. Panel B restricts the sample to transactions subject to mandatory HSR review, i.e., those exceeding the upper transaction threshold where the size-of-person test no longer applies. Visibility is measured using RavenPack Dow Jones data. Flagged deals are those formally challenged by the FTC or DOJ.

(a) **Panel A:** All Transactions exceed the HSR reporting thresholds



(b) **Panel B:** Transactions subject to mandatory HSR review

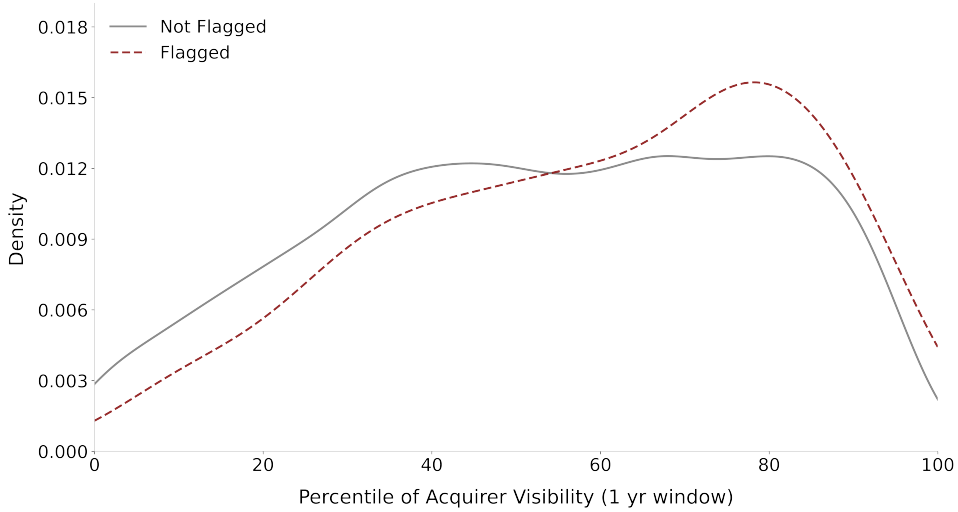


Figure 4: Marginal Effect of Visibility Across Appropriations Windows

This figure plots the estimated marginal effect of acquirer visibility on the probability that a merger is challenged, by the phase of the federal appropriations cycle. We estimate an interacted version of the baseline linear probability model,

$$\text{I(Deal Flagged)}_d = \alpha + \sum_w \beta_w \left(\text{Visibility}_d \times \mathbf{1}\{\text{Window}_d = w\}\right) + \gamma' Z_d + \lambda_{j,p} + \delta_k + \varepsilon_d,$$

where $\mathbf{1}\{\text{Window}_d = w\}$ indicates that deal d is announced in appropriations window w (Off-Season, Budget Preparation, Budget Release, Congressional Hearings, Markups, Final Appropriations). For each window w, the bar reports the marginal effect of a one-unit increase in Visibility_d on the probability that a deal is flagged, as implied by this specification, and the gray vertical line shows the associated 95% confidence interval. All regressions include the benchmark deal-level controls Z_d, acquirer industry-by-year fixed effects $\lambda_{j,p}$, and target 6-digit NAICS industry fixed effects δ_k. Standard errors are two-way clustered at the acquirer and year–month level.

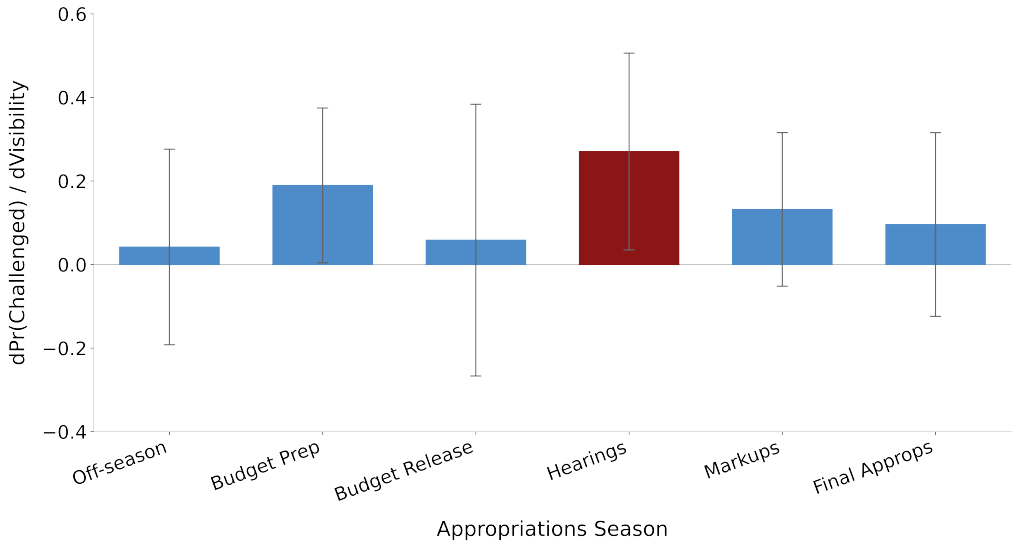


Figure 5: Marginal Effect of Visibility During Normal Months vs. Lame-Duck Periods

This figure plots the estimated marginal effect of acquirer visibility on the probability that a merger is challenged during normal months and during lame-duck periods. Lame-duck periods are defined as the final three months before a new FTC Chair is appointed following a change in the president's party. We estimate an interacted version of the baseline linear probability model,

$$\text{I(Deal Flagged)}_d = \alpha + \sum_{c \in \{\text{Normal, LameDuck}\}} \beta_c \left(\text{Visibility}_d \times \mathbf{1}\{\text{ChairStatus}_d = c\}\right) + \gamma' Z_d + \lambda_{j,p} + \delta_k + \varepsilon_d,$$

The two bars report, respectively, for normal months and lame-duck periods, the marginal effect of a one-unit increase in Visibility_d on the probability that a deal is flagged, as implied by this specification. The gray vertical lines show the associated 95% confidence intervals. All regressions include the benchmark deal-level controls Z_d, acquirer industry-by-year fixed effects $\lambda_{j,p}$, and target 6-digit NAICS industry fixed effects δ_k. Standard errors are two-way clustered at the acquirer and year–month level.

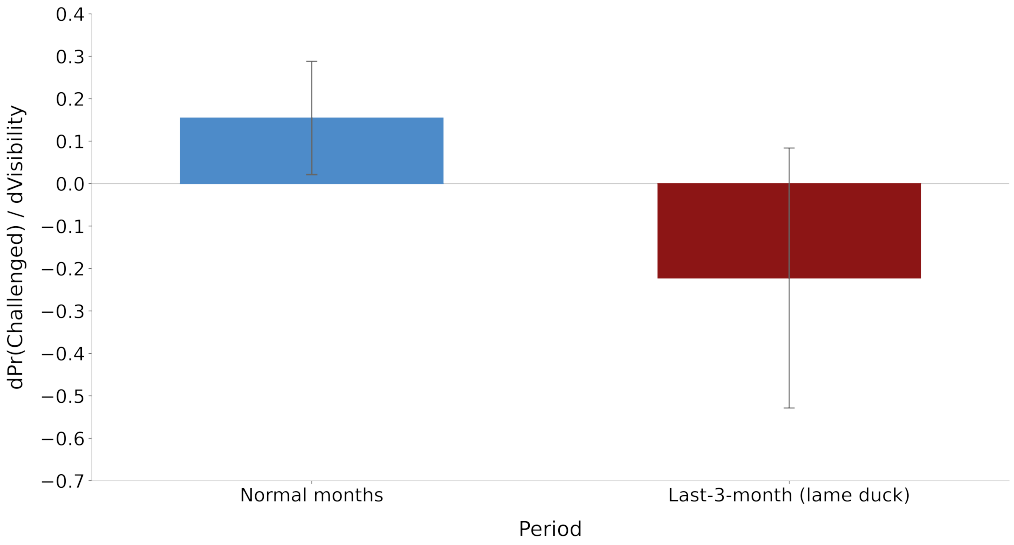


Figure 6: Marginal Effect of Visibility in Consumer-Facing vs. Other Industries

This figure plots the estimated marginal effect of acquirer visibility on the probability that a merger is challenged, separately for consumer-facing and non–consumer-facing industries. Industries are classified using the Delgado and Mills (2020) definition: an industry is consumer-facing if more than 35% of its output is sold to personal consumption expenditure. We estimate an interacted version of the baseline linear probability model,

$$\text{I(Deal Flagged)}_d = \alpha + \sum_{c\in\{0,1\}} \beta_c \left(\text{Visibility}_d \times \mathbf{1}\{\text{ConsumerFacing}_d = c\}\right) + \gamma' Z_d + \lambda_{j,p} + \delta_k + \varepsilon_d,$$

The two bars report the marginal effect of a one-unit increase in Visibility_d on the probability that a deal is flagged for consumer-facing and non–consumer-facing acquirers, as implied by this specification. The gray vertical lines show the associated 95% confidence intervals. All regressions include the benchmark deal-level controls Z_d, acquirer industry-by-year fixed effects $\lambda_{j,p}$, and target 6-digit NAICS industry fixed effects δ_k. Standard errors are two-way clustered at the acquirer and year–month level.

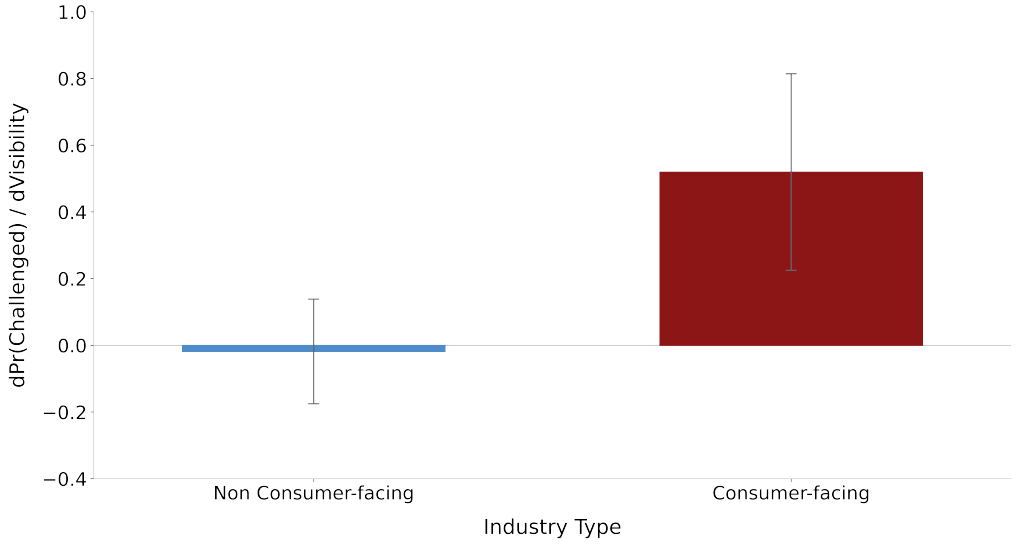


Table 1: Deal-Level Summary Statistics

This table reports summary statistics for the sample of completed M&A transactions involving public acquirers between 2000 and 2020. Panel A includes all deals meeting the HSR size-of-transaction threshold, while Panel B restricts the sample to the subset of deals that were subject to mandatory HSR review. "Horizontal Ind." is an indicator for within-industry acquisitions based on 6-digit NAICS concordance. Spotlight measures are based on the acquirer's share of non-M&A, non-regulatory media coverage in the Dow Jones edition of RavenPack over various pre-merger windows. "Shares of Sales" measures the acquirer's share of total industry sales, and "Industry HHI" reports the Herfindahl–Hirschman Index based on BEA industry output adjusted for private firm coverage.

Panel A: All Deals Meeting HSR Size-of-Transaction Threshold								
	mean	count	sd	p5	p25	p50	p75	p95
Flagged Deal Ind.	0.022	12785	0.148	0.000	0.000	0.000	0.000	0.000
Horizontal Ind.	0.310	12785	0.463	0.000	0.000	0.000	1.000	1.000
Deal Size (in million dollars)	791.943	12777	1809.263	65.000	116.000	234.567	596.000	3421.684
% of 2-yr Spotlight	0.098	11752	0.151	0.003	0.015	0.039	0.124	0.358
% of 1-yr Spotlight	0.098	12422	0.151	0.003	0.015	0.039	0.123	0.363
% of 9-mons Spotlight	0.098	12422	0.151	0.003	0.015	0.039	0.122	0.361
% of 6-mons Spotlight	0.097	12423	0.151	0.003	0.015	0.039	0.121	0.365
% of 3-mons Spotlight	0.096	12422	0.151	0.003	0.014	0.038	0.117	0.362
% of 2-mons Spotlight	0.095	12419	0.153	0.002	0.013	0.036	0.113	0.367
% of 1-mon Spotlight	0.093	12398	0.157	0.000	0.010	0.032	0.106	0.377
% of Donut Spotlight	0.098	12051	0.154	0.001	0.014	0.038	0.122	0.369
Shares of Sales	0.062	12311	0.122	0.001	0.004	0.015	0.058	0.276
Industry HHI	0.253	11634	0.249	0.038	0.064	0.166	0.337	0.803
Panel B: Deals Subject to Mandatory HSR Review								
	mean	count	sd	p5	p25	p50	p75	p95
Flagged Deal Ind.	0.039	6067	0.194	0.000	0.000	0.000	0.000	0.000
Horizontal Ind.	0.333	6067	0.471	0.000	0.000	0.000	1.000	1.000
Deal Size (in million dollars)	1520.229	6067	2424.897	250.000	377.163	632.000	1444.614	6309.848
% of 2-yr Spotlight	0.104	5613	0.151	0.004	0.017	0.043	0.138	0.365
% of 1-yr Spotlight	0.103	5920	0.149	0.004	0.017	0.043	0.134	0.366
% of 9-mons Spotlight	0.102	5920	0.149	0.004	0.017	0.044	0.134	0.359
% of 6-mons Spotlight	0.102	5921	0.149	0.004	0.016	0.043	0.133	0.360
% of 3-mons Spotlight	0.100	5922	0.150	0.004	0.016	0.041	0.129	0.357
% of 2-mons Spotlight	0.099	5920	0.151	0.003	0.015	0.039	0.123	0.364
% of 1-mon Spotlight	0.098	5916	0.158	0.000	0.012	0.035	0.118	0.394
% of Donut Spotlight	0.104	5758	0.153	0.002	0.016	0.042	0.137	0.369
Shares of Sales	0.077	5833	0.134	0.001	0.006	0.022	0.086	0.355
Industry HHI	0.231	5518	0.230	0.038	0.064	0.144	0.291	0.781

Table 2: Public Visibility and the Probability of Antitrust Flagging

This table reports estimates from linear probability models of whether a merger is formally challenged by the FTC or DOJ. The main independent variable is *Visibility*, measured as the acquirer's share of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the indicated window preceding deal announcement. All specifications control for standardized deal size, a horizontal merger indicator, acquirer net sales, industry concentration (HHI), and recent acquisition activity. Acquirer industry × presidential term fixed effects and target 6-digit NAICS industry fixed effects are included throughout. Standard errors are clustered by acquirer and announcement year.

	Deal Being Flagged							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-yr Spotlight	0.067**	0.146**						
	(2.27)	(2.49)						
9-mons Spotlight			0.069**	0.147**				
			(2.35)	(2.55)				
6-mons Spotlight					0.065**	0.131**		
					(2.32)	(2.34)		
3-mons Spotlight							0.061**	0.135***
							(2.32)	(2.58)
(Standardized) Deal Size	0.035***	0.034***	0.035***	0.034***	0.035***	0.034***	0.035***	0.034***
	(9.09)	(8.30)	(9.10)	(8.31)	(9.09)	(8.31)	(9.09)	(8.33)
Horizontal M&A	0.008**	0.010	0.008**	0.010	0.008**	0.010	0.008**	0.010
	(2.00)	(1.23)	(2.00)	(1.22)	(1.99)	(1.21)	(1.99)	(1.20)
Acq. Industry X POTUS Term FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-square	0.251	0.308	0.251	0.308	0.251	0.308	0.251	0.308
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	11081	5094	11081	5094	11083	5094	11084	5095
Sample	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Filing

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Public Visibility and Merger Enforcement: Within-Firm Specification

This table reports estimates from linear probability models of whether a merger is flagged for formal investigation by the FTC or DOJ. The key explanatory variable is *Visibility*, measured as the acquirer's share of non-M&A, non-regulatory Dow Jones media coverage in its 4-digit NAICS industry over the indicated pre-announcement window. All specifications include controls for standardized deal size, a horizontal merger indicator, acquirer net sales, industry concentration (HHI), and recent acquisition activity. Fixed effects for acquirer × presidential term and target 6-digit NAICS industry are included. Standard errors are clustered by acquirer and announcement year.

	Deal Being Flagged			
	(1)	(2)	(3)	(4)
1-yr Spotlight	0.316**			
	(2.23)			
9-mons Spotlight		0.252**		
		(2.03)		
6-mons Spotlight			0.212*	
			(1.93)	
3-mons Spotlight				0.180**
				(2.14)
(Standardized) Deal Size	0.032***	0.032***	0.032***	0.033***
	(6.67)	(6.71)	(6.67)	(6.70)
Horizontal M&A	0.002	0.002	0.002	0.002
	(0.21)	(0.21)	(0.20)	(0.20)
Acquire X POTUS Term FE	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes
R-square	0.492	0.491	0.491	0.491
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020
Observations	3777	3777	3777	3778
Sample	HSR Mondatory	HSR Mondatory	HSR Mondatory	HSR Mandatory

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Instrumental Variables Estimates Using Proximity to News Bureaus

This table reports two-stage least squares (2SLS) estimates of the effect of acquirer visibility on the likelihood of antitrust enforcement. The main endogenous regressor is *Visibility*, measured as the acquirer's share of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the indicated pre-announcement window. The instrument is the acquirer's geodesic distance to the nearest Dow Jones news bureau, constructed as an interaction between a 300-mile indicator and the standardized distance to that office. First-stage regressions (columns 2, 4, 6, 8) show the effect of this instrument on media visibility. All specifications control for standardized deal size, horizontal merger indicator, acquirer net sales, industry HHI, and recent acquisition activity. Fixed effects for acquirer 4-digit NAICS × announcement year and target 6-digit NAICS industry are included throughout. Standard errors are clustered by acquirer and announcement year.

	Deal Being Flagged							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1-yr	FS	9-mons	FS	6-mons	Visibility	3-mons	FS
Visibility	1.8862*		1.8436*		1.9954*		1.8466*	
	(1.80)		(1.79)		(1.80)		(1.86)	
Closeness to DJ Office		0.0168***		0.0173***		0.0160***		0.0172***
		(2.90)		(2.89)		(2.64)		(2.93)
Acq. Industry X Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stats		36.127		37.672		30.268		31.078
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	4277	4277	4277	4277	4277	4277	4277	4277

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: FTC/DOJ Spotlight Chasing: Controlling for Market-Implied Approval Probability

This table examines the relationship between the acquirer's share of news exposure within the industry and the indicator of the deal getting flagged, conditional on the market-implied success probability. The main independent variable is *Visibility*, measured as the acquirer's share of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the indicated window preceding deal announcement. We calculate the market implied probability using pre-period and announcement day stock prices, and the deal bid price. We control for deal size, an indicator of horizontal integration, and share of sales. We also control for acquirer industry, announcement year, and target industry fixed effects. Standard errors are clustered at the acquirer level.

	Deal Being Flagged							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-yr Spotlight	0.336**	0.741**						
	(2.00)	(2.34)						
9-mons Spotlight			0.310*	0.671**				
			(1.84)	(2.19)				
6-mons Spotlight					0.228	0.514*		
					(1.40)	(1.74)		
3-mons Spotlight							0.228	0.491
							(1.33)	(1.65)
Mkt-Implied Success Prob	-0.048	-0.069	-0.048	-0.069	-0.047	-0.067	-0.047	-0.066
	(-1.44)	(-1.22)	(-1.45)	(-1.22)	(-1.43)	(-1.19)	(-1.41)	(-1.17)
(Standardized) Deal Size	0.017*	0.014	0.016*	0.014	0.016*	0.014	0.016*	0.014
	(1.84)	(1.30)	(1.82)	(1.28)	(1.82)	(1.27)	(1.81)	(1.27)
Horizontal M&A	0.023	0.009	0.022	0.010	0.022	0.009	0.022	0.009
	(0.74)	(0.21)	(0.73)	(0.22)	(0.71)	(0.19)	(0.71)	(0.20)
Acq. Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-square	0.432	0.449	0.431	0.448	0.430	0.445	0.430	0.445
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	569	384	569	384	569	384	569	384
Sample	HSR Filing	HSR Mandatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mandatory

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Future Outcomes of Acquirers that Should be Flagged

This table examines the future growth and market power of acquirers of deals with a high propensity to be flagged under FTC guidelines after deal completion. We compare these firms that should be flagged with other control firms. We estimate propensity scores for each deal year by year using a logit model with standardized deal size, a horizontal merger indicator, acquirer net sales, industry concentration (HHI), and recent acquisition activity as independent variables, which are of primary interest according to FTC guidelines. For each treated deal, we find five control deals with the closest propensity scores and categorize them as deals that should be flagged. We run the following regressions:

$$Y_{i,t+\tau} - Y_{i,t} = \alpha_{FE} + \beta \text{Should be Flagged}_{i,t} + \gamma' W_{i,t} + \varepsilon_{i,t}$$

We control for the ex-ante characteristics of the firm in year $t-1$ of the execution date: net leverage, log capital, log employment, log output, log profit, age, and book-to-market ratio. We also control for acquirer industry times POTUS term fixed effects, and target industry fixed effects. Standard errors are clustered at the acquirer and year level.

	Year 1	Year 2	Year 3	Year 4	Year 5
Δ Profits	0.038**	0.037	0.056**	0.053*	0.028
	(2.30)	(1.65)	(2.75)	(1.80)	(0.77)
Δ COGS	0.021	0.010	0.019	0.003	0.038
	(1.44)	(0.50)	(0.99)	(0.11)	(0.93)
Δ Sales	0.025*	0.030*	0.043***	0.029	0.038
	(2.00)	(1.89)	(3.38)	(1.60)	(1.15)
Δ Gross Margin	0.001	0.018*	0.019***	0.006	0.005
	(0.06)	(1.86)	(3.08)	(0.43)	(0.54)
Δ Sale Share in Industry	0.001**	0.001**	0.002**	0.002	0.002*
	(2.44)	(2.61)	(2.34)	(1.61)	(1.78)
Δ Markup	0.113*	0.162*	0.203*	0.159	0.134
	(1.84)	(1.88)	(1.91)	(1.38)	(1.51)
Acquirer Industry	Yes	Yes	Yes	Yes	Yes
X President Term. FE					
Target Industry FE	Yes	Yes	Yes	Yes	Yes
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2019

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A Additional Figures and Tables

Table A.1: Visibility Measures over Alternative Pre-Merger Time Windows

This table reports estimates from linear probability models of whether a merger is formally challenged by the FTC or DOJ. The key independent variable is *Visibility*, defined as the acquirer's share of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the indicated pre-merger window (2-year, 13–18 month "donut" window, and 2-month). All specifications include controls for standardized deal size, a horizontal merger indicator, log acquirer net sales, industry concentration (HHI), and recent acquisition activity. Fixed effects for acquirer industry × presidential term and target 6-digit NAICS industry are included in all regressions. Standard errors are clustered by acquirer and announcement year.

	Deal Being Flagged					
	(1)	(2)	(3)	(4)	(5)	(6)
2-yr Spotlight	0.056*	0.126*				
	(1.77)	(1.82)				
"Donut" Spotlight			0.045*	0.123**		
			(1.84)	(2.39)		
2-mons Spotlight					0.045*	0.120**
					(1.82)	(2.44)
(Standardized) Deal Size	0.034***	0.033***	0.035***	0.034***	0.035***	0.034***
	(8.52)	(7.74)	(8.93)	(8.16)	(9.10)	(8.33)
Horizontal M&A	0.008**	0.012	0.007*	0.009	0.008**	0.009
	(2.06)	(1.41)	(1.78)	(1.14)	(1.97)	(1.18)
Acq. Industry X POTUS Term FE	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
R-square	0.255	0.318	0.252	0.309	0.250	0.308
Sample Period	2002-2020	2002-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	10483	4802	10748	4936	11082	5094
Sample	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Excluding Top-Decile Acquirers by Industry Visibility

This table reports estimates from linear probability models of whether a merger is formally challenged by the FTC or DOJ, omitting deals involving acquirers in the top 10% of the visibility distribution. *Visibility* is defined as the acquirer's share of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the indicated window preceding deal announcement. All specifications control for standardized deal size, a horizontal merger indicator, log acquirer net sales, industry concentration (HHI), and recent acquisition activity. Acquirer industry × presidential term fixed effects and target 6-digit NAICS industry fixed effects are included throughout. Standard errors are clustered by acquirer and announcement year.

	Deal Being Flagged							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-year Spotlight	0.086*	0.189**						
	(1.83)	(1.99)						
9-month Spotlight			0.094**	0.194**				
			(1.99)	(2.12)				
6-month Spotlight					0.075	0.164*		
					(1.55)	(1.69)		
3-month Spotlight							0.106**	0.189**
							(2.36)	(2.17)
(Standardized) Deal Size	0.035***	0.034***	0.035***	0.035***	0.035***	0.034***	0.035***	0.035***
	(8.44)	(7.75)	(8.51)	(7.81)	(8.33)	(7.72)	(8.78)	(8.00)
Horizontal M&A	0.006	0.002	0.006	0.001	0.005	0.001	0.006	0.001
	(1.45)	(0.26)	(1.52)	(0.16)	(1.40)	(0.09)	(1.46)	(0.15)
Acq. Industry X POTUS Term FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-square	0.237	0.290	0.232	0.288	0.230	0.281	0.244	0.303
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	9927	4538	9936	4550	9938	4563	9948	4566
Sample	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Filing

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Excluding Earnings-Related News from Visibility Measure

This table reports estimates from linear probability models of whether a merger is formally challenged by the FTC or DOJ, using a refined visibility measure that excludes articles related to firm earnings announcements or financial reporting. *Visibility* is defined as the acquirer's share of non-M&A, non-regulatory, and non-earnings media coverage within its 4-digit NAICS industry over the indicated window prior to deal announcement. All specifications control for standardized deal size, a horizontal merger indicator, log acquirer net sales, industry concentration (HHI), and recent acquisition activity. Fixed effects include acquirer industry × presidential term, FTC chair term, and target 6-digit NAICS industry. Standard errors are clustered at the acquirer and announcement year levels.

	Deal Being Flagged					
	(1)	(2)	(3)	(4)	(5)	(6)
2-yr Spotlight	0.125**					
	(2.68)					
1-yr Spotlight		0.155**				
		(2.85)				
9-mons Spotlight			0.155**			
			(2.78)			
6-mons Spotlight				0.139**		
				(2.49)		
3-mons Spotlight					0.158***	
					(3.21)	
2-mons Spotlight						0.138***
						(3.53)
(Standardized) Deal Size	0.033***	0.033***	0.033***	0.034***	0.034***	0.034***
	(8.99)	(9.48)	(9.54)	(9.56)	(9.51)	(9.50)
Horizontal M&A	0.011	0.009	0.009	0.009	0.009	0.009
	(1.64)	(1.47)	(1.44)	(1.44)	(1.44)	(1.42)
Acq. Industry X POTUS Term FE	Yes	Yes	Yes	Yes	Yes	Yes
FTC Chair Term FE	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
R-square	0.320	0.312	0.312	0.312	0.312	0.312
Sample Period	2002-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	4802	4961	4961	4961	4962	4960

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Flagged Merger Probability Among Deals Meeting Size-of-Person Threshold

This table reports estimates from linear probability models of whether a merger is formally challenged by the FTC or DOJ, using a sample of transactions that meet both the size-of-transaction and size-of-person thresholds under the HSR Act. Because party-level financials are not always available—particularly for private targets—this sample includes only deals for which sufficient data are observed to verify eligibility. The visibility measure is defined as the acquirer's share of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the indicated window preceding deal announcement. All regressions include controls for standardized deal size, a horizontal merger indicator, acquirer net sales, industry concentration (HHI), and recent acquisition activity. Fixed effects for acquirer industry × presidential term and for target 6-digit NAICS industry are included throughout. Standard errors are clustered at the acquirer and announcement year levels.

	Deal Being Flagged			
	(1)	(2)	(3)	(4)
1-yr Spotlight	0.093*			
	(1.65)			
9-mons Spotlight		0.097*		
		(1.75)		
6-mons Spotlight			0.094*	
			(1.75)	
3-mons Spotlight				0.081*
				(1.66)
(Standardized) Deal Size	0.033***	0.033***	0.033***	0.033***
	(8.21)	(8.22)	(8.22)	(8.23)
Horizontal M&A	0.012*	0.012*	0.012*	0.012*
	(1.82)	(1.81)	(1.80)	(1.80)
Acq. Industry X POTUS Term FE	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes
R-square	0.301	0.301	0.301	0.300
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020
Observations	6222	6222	6222	6223
Sample	Size of Person	Size of Person	Size of Person	Size of Person

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Public Visibility and Enforcement: Controlling for FTC Chair Fixed Effects

This table reports estimates from linear probability models of whether a merger is formally challenged by the FTC or DOJ. The main independent variable is *Visibility*, measured as the acquirer's share of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the indicated window preceding deal announcement. All specifications include controls for standardized deal size, a horizontal merger indicator, acquirer net sales, industry concentration (HHI), and recent acquisition activity. Fixed effects for acquirer industry × FTC Chair term and target 6-digit NAICS industry are included. Standard errors are clustered by acquirer and announcement year.

	Deal Being Flagged							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-yr Spotlight	0.082**	0.141**						
	(2.27)	(2.04)						
9-mons Spotlight			0.086**	0.147**				
			(2.28)	(2.03)				
6-mons Spotlight					0.080**	0.138**		
					(2.22)	(2.01)		
3-mons Spotlight							0.068**	0.122*
							(2.14)	(1.95)
(Standardized) Deal Size	0.035***	0.034***	0.035***	0.034***	0.035***	0.034***	0.035***	0.034***
	(8.75)	(7.83)	(8.76)	(7.84)	(8.76)	(7.83)	(8.76)	(7.84)
Horizontal M&A	0.007*	0.012	0.007*	0.011	0.007*	0.011	0.007*	0.011
	(1.69)	(1.32)	(1.68)	(1.31)	(1.68)	(1.31)	(1.67)	(1.30)
Acq. Industry X FTC Chair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-square	0.290	0.351	0.290	0.351	0.289	0.351	0.289	0.351
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	10609	4756	10609	4756	10611	4756	10612	4757
Sample	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mandatory

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Public Visibility and Enforcement: Congressional Control Fixed Effects

This table reports estimates from linear probability models of whether a merger is formally challenged by the FTC or DOJ. The main independent variable is *Visibility*, measured as the acquirer's share of non-M&A, non-regulatory media coverage within its 4-digit NAICS industry over the indicated window preceding deal announcement. All regressions include controls for standardized deal size, a horizontal merger indicator, acquirer net sales, industry concentration (HHI), and recent acquisition activity. Fixed effects for acquirer industry × Congressional control (Unified Democratic, Unified Republican, or Divided) and target 6-digit NAICS industry are included. Standard errors are clustered by acquirer and announcement year.

	Deal Being Flagged							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-yr Spotlight	0.045*	0.087*						
	(1.80)	(1.66)						
9-mons Spotlight			0.048*	0.090*				
			(1.91)	(1.68)				
6-mons Spotlight					0.045*	0.073		
					(1.87)	(1.41)		
3-mons Spotlight							0.045*	0.090*
							(1.91)	(1.76)
(Standardized) Deal Size	0.034***	0.034***	0.034***	0.034***	0.034***	0.034***	0.034***	0.034***
	(9.12)	(8.33)	(9.13)	(8.35)	(9.13)	(8.35)	(9.12)	(8.34)
Horizontal M&A	0.006*	0.009	0.006*	0.009	0.006*	0.009	0.006*	0.009
	(1.67)	(1.20)	(1.67)	(1.19)	(1.67)	(1.18)	(1.67)	(1.18)
Acq. Industry X Congress Control FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-square	0.245	0.287	0.245	0.287	0.245	0.287	0.245	0.287
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	11087	5091	11087	5091	11087	5091	11088	5092
Sample	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mandatory

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Market-Implied Success Probability and Public Visibility

This table examines the relationship between the acquirer's share of news exposure within the industry and the market-implied success probability. We calculate the market implied probability using pre-period and announcement day stock prices, and deal bid price. We control for deal size, an indicator of horizontal integration, and share of sales. All specifications control for standardized deal size, a horizontal merger indicator, acquirer net sales, industry concentration (HHI), and recent acquisition activity. We also control for acquirer industry × presidential term fixed effects and target 6-digit NAICS industry fixed effects. Standard errors are clustered at the acquirer level.

	Market-Implied Success Probability							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1-yr Spotlight	-0.486	-0.118						
	(-0.86)	(-0.17)						
9-mons Spotlight			-0.668	-0.244				
			(-1.20)	(-0.36)				
6-mons Spotlight					-0.700	-0.238		
					(-1.15)	(-0.33)		
3-mons Spotlight							-0.739	-0.357
							(-1.21)	(-0.55)
(Standardized) Deal Size	-0.016*	-0.010	-0.016*	-0.010	-0.016*	-0.010	-0.017*	-0.009
	(-1.72)	(-0.91)	(-1.73)	(-0.94)	(-1.71)	(-0.88)	(-1.76)	(-0.84)
Horizontal M&A	0.030	0.013	0.032	0.015	0.034	0.014	0.033	0.013
	(0.50)	(0.20)	(0.54)	(0.23)	(0.56)	(0.22)	(0.54)	(0.20)
Acq. Industry X POTUS Term FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-square	0.349	0.419	0.351	0.420	0.348	0.418	0.349	0.418
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020	2001-2020
Observations	478	313	478	313	478	313	478	313
Sample	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Mondatory	HSR Filing	HSR Filing

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Proofs

This appendix provides complete proofs for Lemma 1, Proposition 1, and Proposition 2. All results follow from the assumptions introduced in Sections 5.1–5.2: policy monotonicity (Assumption 1), monotone likelihood ratios (Assumption 2), visibility-based evaluative precision (Assumption 3), and directional reputational asymmetry (Assumption 4). The reputational differential $\Delta R(V)$ is as defined in Section 5.2.

B.1 Proof of Lemma 1

Proof. The regulator challenges when

$$\Delta U(s, V) = m(s) - C(1) + \lambda \, \Delta R(V) \geq 0.$$

Step 1: $\Delta U(s, V)$ is strictly increasing in s. By Assumption 1, $\Delta W(H)$ is strictly increasing in H. By Assumption 2, the posterior distribution of H shifts in likelihood-ratio order with s, implying

$$m'(s) = \frac{d}{ds} \mathbb{E}[\Delta W(H) \mid s] > 0.$$

Since $\Delta R(V)$ does not depend on s, $\Delta U(s, V)$ is strictly increasing in s.

Step 2: Boundary crossings. For sufficiently low s, $\Delta U(s, V) < 0$; for sufficiently high s, $\Delta U(s, V) > 0$.

Step 3: Unique cutoff. Since $\Delta U(s, V)$ is continuous, strictly increasing, and crosses zero once, there exists a unique $\tau(V)$ such that

$$a^*(s, V) = 1 \quad \Longleftrightarrow \quad s \geq \tau(V).$$

□

B.2 Proof of Proposition 1

Proof. The cutoff $\tau(V)$ satisfies the implicit equation

$$m(\tau(V)) - C(1) + \lambda \, \Delta R(V) = 0. \tag{B.1}$$

Define

$$F(s, V) \equiv m(s) - C(1) + \lambda \Delta R(V).$$

Then $F(\tau(V), V) = 0$.

Step 1: Apply the implicit function theorem.

$$\frac{\partial F}{\partial s}(\tau(V), V)\, \tau'(V) + \frac{\partial F}{\partial V}(\tau(V), V) = 0.$$

Step 2: Compute derivatives.

$$\frac{\partial F}{\partial s}(\tau(V), V) = m'(\tau(V)) > 0, \qquad \frac{\partial F}{\partial V}(\tau(V), V) = \lambda \Delta R'(V).$$

Hence,

$$\tau'(V) = -\frac{\lambda \Delta R'(V)}{m'(\tau(V))}. \tag{B.2}$$

Step 3: Use directional reputational asymmetry. From Assumption 4:

- If $\sigma = +1$ (under-enforcement more costly): $\Delta R'(V) > 0$.

- If $\sigma = -1$ (over-enforcement more costly): $\Delta R'(V) < 0$.

- If $\sigma = 0$ (symmetric): $\Delta R'(V) = 0$.

Substituting into (B.2):

- $\sigma = +1 \Rightarrow \tau'(V) < 0$. - $\sigma = -1 \Rightarrow \tau'(V) > 0$. - $\sigma = 0 \Rightarrow \tau'(V) = 0$.

This proves Proposition 1. □

B.3 Proof of Proposition 2

Proof. Under $\sigma = +1$, Proposition 1 implies $\Delta R'(V) > 0$. From equation (B.2),

$$-\tau'(V) = \frac{\lambda \Delta R'(V)}{m'(\tau(V))}. \tag{B.3}$$

All terms except λ are strictly positive. Differentiating the right-hand side with respect to λ yields

$$\frac{\partial}{\partial \lambda}\left[-\tau'(V)\right] = \frac{\Delta R'(V)}{m'(\tau(V))} > 0.$$

Thus the magnitude of the visibility gradient increases strictly with λ. □

C Effects of Flagged but not Blocked Deals

In this section, we study the effect on acquirers' future outcomes of being flagged by the FTC or DOJ. In our sample, these flagged deals are eventually completed, meaning they go through after a lengthy inspection process and a high regulatory burden. We study the future behavior of these approved firms, conditional on getting flagged. Does getting flagged and inspected alleviate the firm's anti-competitive threat? Does it lead to slower growth of the acquiring firm?

To study this regulatory treatment effect of getting flagged, we match flagged firms to control firms that are ex-ante similar. We apply propensity score reweighting to the regression of firm outcomes on the indicator of getting flagged. This allows us to account for the observable differences that lead firms to get flagged or not (Cunningham et al., 2021). In particular, we calculate the propensity score for each firm by running the following regression year by year.

$$\text{P(Flag=1)}_{i,t} = \Phi(\alpha_t + \beta_t \text{Visibility}_{i,t} + \gamma_t' Z_{i,t} + \varepsilon_{i,t}) \tag{10}$$

Every year, we estimate a logit model of being flagged based on deal characteristics, including deal size (in millions USD), an indicator for a horizontal merger, the industry Herfindahl index, and the acquirer's net sales, industry sales shares, and recent deal activity. Different from Section 6.1, we also include firms' news coverage in the model because here we want to match treated deals to control deals as close as possible, while in Section 6.1, we want deals with a high probability of getting flagged based only on FTC guidelines. Intuitively, we are comparing two deals that appear similar in terms of antitrust-related hard information and public visibility: one is flagged while the other is not. By matching the deals on both hard information and visibility, we aim to cleanly estimate the treatment effect of regulatory scrutiny by the FTC or DOJ.

With the deal-level propensity score, we run regressions of future firm growth, performance, and investment on the indicator of being flagged, using the inverse propensity weights as regression weights. More specifically, the regression weights for treated firms are $1/p$ and for control firms are $1/(1-p)$, so control firms with higher propensity scores (more similar to treatment) get higher weights in the regressions. Following Cunningham et al. (2021), we also restrict the sample to firms that fall within the common support (from the minimum propensity of treated firms to the maximum propensity of control firms), ensuring they are directly comparable. We restrict the sample to only HSR mandatory deals to provide a sharper comparison because we know for sure that every deal in that sample needs to file for

antitrust review. The regression specification is as follows:

$$Y_{i,t+\tau} = \alpha_{FE} + \beta \text{Flag}_{i,t} + \gamma' W_{i,t} + \varepsilon_{i,t}, \tag{11}$$

where $Y_{i,t+\tau}$ denotes future firm growth, performance or investment variables in τ years after the deal is completed. We control for the ex-ante characteristics of the firm in year $t-1$ of the execution date: net leverage, log capital, log employment, log output, log profit, age, and book-to-market ratio. We also control for acquirer-industry interactions with POTUS-term fixed effects, as well as target-industry fixed effects.

Table C.1 presents the results. In terms of future growth, we observe that flagged firms experience higher profit and sales growth in the five years following deal completion than non-flagged firms, after a lengthy investigation into anti-competitive behavior. However, we observe that the gross margin is lower than that of control firms, indicating *weaker* profitability after regulatory scrutiny. This is consistent with the finding that sales grow faster than profits, as production also becomes more costly, leading to lower profitability. Flagged acquirers also have a lower markup, as defined in De Loecker et al. (2020), indicating that regulatory scrutiny indeed decreases the market power of the focal firm.

We next investigate the future investments of the flagged acquirers. We observe that these firms grow faster in capital and labor stock five years after deal completion than control firms, indicating that they aggressively expand after the regulatory scrutiny. These investments, however, tend to be inefficient because these firms have a decreasing total Q than similar control firms that are not flagged, indicating worse investment opportunities. With lower profitability but investment expansions, the firms accumulate more leverage than control firms, with a 3% higher increase in leverage in flagged acquirers in years $t+4$ and $t+5$.

Combining these results, we see that being flagged by the FTC or the DOJ and going through the regulatory process indeed leads the focal firm to have lower market power, resulting in lower profit margins and lower markups, which aligns with the FTC's objectives. However, regulatory scrutiny also has side effects. The flagged firms try to get back on their feet by investing in capital and labor, but without good investment opportunities, the additional capital and labor are spent inefficiently. As a result, firms pile on debt and increase leverage, thereby increasing financing risk and potentially creating a debt overhang. Moreover, a decrease in the focal firm's market power does not necessarily improve the market's competitive environment. If a competitor in the same market takes advantage of the regulatory inspection of the focal firm and gains market power, market competition may worsen, and consumer welfare may still be harmed. Whether the consumer welfare improves after the FTC/DOJ inspection needs further analysis.

Table C.1: Future Outcomes of Flagged Deals

This table examines future performance of acquirers of flagged deals after deal completion. We compare these firms with ex ante similar control firms using a propensity-score reweighting regression. We estimate propensity score for each deal year by year using a logit model with public visibility, standardized deal size, a horizontal merger indicator, acquirer net sales, industry concentration (HHI), and recent acquisition activity as predictors, to ensure the closest match given observables. We run the following regressions of future firm growth or performance on indicator for being flagged, using inverse probability weighting as regression weights:

$$Y_{i,t+\tau} = \alpha_{FE} + \beta \text{Flag}_{i,t} + \gamma' W_{i,t} + \varepsilon_{i,t}$$

We control for the ex-ante characteristics of the firm in year $t-1$ of the execution date: net leverage, log capital, log employment, log output, log profit, age, and book-to-market ratio. We also control for acquirer industry $\times$ POTUS term fixed effects, and target industry fixed effects. Standard errors are clustered at the acquirer and year level.

	Year 1	Year 2	Year 3	Year 4	Year 5
Δ Profits of Flagged Acquirers	0.042**	0.026	0.071**	-0.016	0.003
	(2.44)	(1.07)	(2.32)	(-0.34)	(0.05)
Δ Sales of Flagged Acquirers	0.045***	0.048**	0.100***	0.059*	0.058*
	(2.93)	(2.82)	(5.59)	(2.02)	(1.89)
Gross Margin of Flagged Acquirers	-0.028**	-0.017	-0.020	-0.038**	-0.047***
	(-2.83)	(-1.37)	(-1.53)	(-2.68)	(-4.07)
Markups of Flagged Acquirers	-0.540***	-0.643**	-0.589	-0.321***	-0.307*
	(-3.07)	(-2.23)	(-1.76)	(-3.68)	(-1.83)
Δ Capital of Flagged Acquirers	0.026	0.033	0.088**	0.109***	0.092**
	(1.31)	(1.29)	(2.74)	(3.21)	(2.34)
Δ Employment of Flagged Acquirers	0.053***	0.076***	0.105***	0.121***	0.103***
	(4.47)	(3.70)	(3.77)	(3.96)	(3.16)
Δ Total Q of Flagged Acquirers	-0.137	0.042	-0.595**	-0.333**	-0.316*
	(-1.26)	(0.19)	(-2.42)	(-2.47)	(-2.03)
Δ Net Leverage of Flagged Acquirers	0.006	-0.004	-0.005	0.028**	0.023*
	(0.81)	(-0.48)	(-0.37)	(2.31)	(1.91)
Acquirer Industry	Yes	Yes	Yes	Yes	Yes
X President Term. FE					
Target Industry FE	Yes	Yes	Yes	Yes	Yes
Sample Period	2001-2020	2001-2020	2001-2020	2001-2020	2001-2019

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$