

Antitrust Enforcement and Product Market Dynamics: Evidence from U.S. Government Procurement

Jonathan Brogaard Decio Coviello

Yunying Huang Stefano Manfredonia

Abstract

We examine how antitrust enforcement shapes product market dynamics using U.S. government procurement as a laboratory. Leveraging large language models to systematically identify DOJ procurement cases, we document a trade-off between broader market participation and procurement performance. Non-defendant contractors, particularly small, women-, and minority-owned businesses, gain market share as antitrust enforcement dismantles the structural barriers erected by anti-competitive conduct. However, enforcement also impairs procurement performance in affected markets, an effect concentrated among new entrants and complex projects, but largely absent in simpler procurements and simple goods delivery. Our findings provide new insights into how antitrust enforcement influences stakeholders.

Keywords: antitrust lawsuits; government procurement; market competition; firm performance; large language models (LLMs)

JEL Codes: G38, L41, H57

Jonathan Brogaard is associated with the University of Utah. E-mail: brogaardj@eccles.utah.edu. Decio Coviello is associated with HEC Montréal. E-mail: decio.coviello@hec.ca. Yunying Huang is associated with Fordham University. E-mail: yhuang374@fordham.edu. Stefano Manfredonia is affiliated with Fordham University. E-mail: smanfredonia1@fordham.edu. This paper is undertaken, in part, thanks to funding from the Canada Research Chairs program. The authors thank Logan Emery, Mara Faccio, Ron Masulis, Tommaso Valletti, Ekaterina Volkova, and seminar participants at the 10th SDU Finance Workshop and the 5th Global PhD Student Colloquium.

1 Introduction

A large body of work in finance has documented that U.S. industries have become substantially more concentrated over the past two decades (Grullon et al., 2019; Autor et al., 2020; Barkai, 2020). This increase in concentration has been accompanied by higher corporate profit margins and returns to shareholders, but also by growing concerns about the costs that market power can impose on employees, consumers, and the broader economy (Gutiérrez & Philippon, 2017; Van Reenen, 2018). These dynamics have reignited interest in antitrust enforcement among policymakers, researchers, and media, as more effective antitrust measures are considered potential solutions to limit corporate market power, protect competition, and promote market fairness (Bessen, 2016; Furman, 2016; Baker, 2019; Grullon et al., 2019; Philippon, 2019; Shapiro, 2019; Faccio & Zingales, 2022), with the potential to improve economic efficiency and societal welfare.¹

Antitrust enforcement is the primary tool government authorities use to address market failures stemming from anti-competitive conduct (Tirole, 1988). While successful lawsuits are expected to lower entry barriers, increase participation, and boost output, less is known about how enforcement affects competitive interactions and how resulting shifts in market power affect firm- and product-level outcomes. Existing studies primarily focus on the valuation effects of enforcement on defendant firms (Bittlingmayer, 1993; Bizjak & Coles, 1995; Aguzzoni et al., 2013), or on aggregate industry-market dynamics (e.g., Reed et al., 2022; Babina et al., 2023), leaving open the question of how competition evolves *within* product markets following enforcement and how these changes affect different stakeholder groups.

From a theoretical perspective, anti-competitive conduct distorts the allocation of resources across firms by channeling resources toward entrenched incumbents, excluding po-

¹While one strand of the literature argues that corporate market power can negatively impact employees, consumers, and productivity (Gutiérrez & Philippon, 2017; Autor et al., 2020; Barkai, 2020), another suggests that firms operating in less competitive markets may benefit from significant economies of scale. This allows a smaller number of large competitors to offer superior quality at lower prices, encourages innovation, and ultimately drives productivity growth (Syverson, 2004; Ganapati, 2018; Van Reenen, 2018; Bessen, 2020; Clark et al., 2024; Kang, 2025).

tentially more efficient competitors, and creating artificial barriers to entry. When enforcement removes these barriers, it is expected to expand market access, particularly for firms that were previously excluded and lack the connections or scale necessary to participate in collusive arrangements (Zeume, 2017; Giannetti et al., 2021). However, enforcement may also adversely affect corporations by reducing profit margins (Cestone et al., 2021) and by creating uncertainty about the future competitive and regulatory environment, which can increase the cost of capital and depress investment in affected industries (Bittlingmayer, 1993; Bittlingmayer & Hazlett, 2000). In addition, when enforcement displaces incumbents, it can disrupt the accumulated expertise, established supply relationships, and organizational capacity that underpin efficient production (Baker et al., 2002), thereby reducing efficiency in the product market. We investigate these dynamics using the U.S. government procurement as a laboratory experiment.

Government procurement is one of the largest markets in the U.S. economy, accounting for 10-15% of GDP, with hundreds of billions of dollars in contracts awarded annually across sectors ranging from construction and defense to information technology (Brogaard et al., 2021; Kang & Miller, 2022). These markets are not only critical for the efficient delivery of public goods, but also equally important for corporations. Government contractors that identify the federal government as a major customer account for approximately 10% of publicly listed firms, with these sales representing an average of almost a quarter of government contractors' total sales (Hasan et al., 2026). However, these markets are highly concentrated and characterized by substantial entry barriers and structural frictions, including specialized knowledge requirements, significant capital investments, and complex administrative procedures that limit contestability. These features make them particularly vulnerable to supplier collusion (Fazio & Zaldokas, 2024), and approximately 35% of all DOJ antitrust lawsuits involve government procurement.²

²Further evidence of the importance of preserving competition in procurement markets is the creation in 2019 of the Procurement Collusion Strike Force (PCSF), an inter-agency initiative led by the U.S. Department of Justice's Antitrust Division. The program has trained more than 38,000 procurement officials and investigators, opened over 140 investigations, and recovered more than \$65 million in fines and restitution.

To advance our research and extract information from lawsuit text data, our methodology integrates the analytical capabilities of large language models (LLMs) with human verification to ensure complete coverage and accuracy. By doing so, we identify lawsuits related to government procurement activities and extract data on the associated product markets and defendants. This approach also allows us to gather extensive information on the types of misconduct and other characteristics of antitrust lawsuits. We match this novel data set with information on federal government procurement contracts and alternative metrics of government contractors' performance.

We focus our analysis on the period 2001 to 2021, during which all our alternative sources of information are available. We observe significant variation in antitrust lawsuits both over time and across different product markets, which provides the basis for our empirical analysis. Using granular establishment-level data on government contractors and federal procurement contracts, we follow the standard approach in this literature and exploit the timing of antitrust lawsuit filings in a difference-in-differences research design (e.g., Sproul, 1993; Kang, 2025).

We document an increase in government contracts awarded to non-defendant government contractors operating in treated product markets, relative to those in control product markets. This effect is economically meaningful; our estimation results imply a 16% increase in government contracts awarded to an establishment in our sample following the antitrust investigation.

Consistent with enforcement leveling the playing field (Goldman & Zeume, 2023), we also document that these gains are not evenly distributed across firms; the benefits of enforcement are particularly large for establishments that historically faced greater barriers to entering procurement markets. Small establishments, women owned firms, and minority owned firms experience significantly larger increases in government contracts than their counterparts. In addition, in line with broader market participation, we also document substantial structural changes at the market level. Defendant establishments lose approximately 57% of their fed-

eral contract obligations following enforcement, the number of firms participating in affected markets increases by about 8%, and market concentration declines significantly.

Our identification strategy relies on the timing of antitrust lawsuits, which is significantly influenced by DOJ leadership priorities and political cycles, introducing uncertainty and variability into enforcement. We confirm that the timing of these lawsuits is unanticipated by financial markets: a day-by-day analysis of stock returns around lawsuit filings reveals no pre-filing abnormal returns, with the market pricing in a significant decline in expected cash flows only upon the public announcement of the enforcement action. This event study evidence serves a dual purpose in our analysis. First, it validates the parallel trends assumption underlying our difference-in-differences design by confirming that enforcement represents an unanticipated shock. Second, it provides direct evidence that financial markets recognize antitrust enforcement as a value-relevant event, consistent with the view that enforcement affects the distribution of future cash flows across firms in the affected market.

We further reinforce the credibility of our empirical analysis through several complementary exercises. We examine the dynamic effects of the main outcomes to assess both the validity of the parallel trends assumption and the timing and persistence of the estimated impacts. We also implement additional identification strategies and robustness checks, including controls for aggregate industry and product-market trends, propensity score matching, placebo tests, and a triple-difference design comparing government and non-government contractors within the same market and county, as well as an alternative treatment variable based on the filing court's location to capture local exposure to antitrust enforcement. Collectively, these analyses confirm the validity of our identification strategy and the robustness of our findings.

Using procurement data at the contract level, we next examine the implications of these competition dynamics for product market outcomes. To explore this point, we use alternative proxies for procurement performance, including the number of modifications, the dollar amount of renegotiations, the cost overrun ratio, and delivery delays (see, for instance,

Emery and Faccio (2020) and Spenkuch et al. (2023)). Although we do not find differences in average winning bids or other contract characteristics, we document that procurement performance deteriorates following antitrust lawsuits. Importantly, these negative effects are driven by new entrants and highly complex projects, diminish with contractors' experience, and are absent for simple contracts and the standard delivery of goods.

Together, our findings reveal how antitrust enforcement reshapes product market dynamics. By dismantling the structural barriers created by anti-competitive conduct, enforcement benefits non-defendant corporations and durably expands market access. Additional results suggest that the deterioration in procurement performance we document is not driven by changes in contract design, but rather by a compositional shift in the supplier pool: enforcement displaces entrenched incumbents and opens the markets to newer or smaller entrants who, while now able to access the market, may lack the capacity and expertise required for complex projects. Other interpretations, such as financial distress stemming from reduced profit margins or reduced investments driven by political and regulatory uncertainty, are not supported by our analysis.

Related literature and contribution Our paper contributes to the literature investigating how competition policies affect economic outcomes. Within this literature, some papers investigate the effects of antitrust enforcement on the equity prices of publicly traded firms under investigation, as well as other firms in the same industry, immediately following the release of indictment news. These studies find adverse stock market reactions (Bittlingmayer, 1993; Bizjak & Coles, 1995; Bittlingmayer & Hazlett, 2000), and their findings have been widely cited to support the argument that antitrust lawsuits can create inefficiency and uncertainty, potentially harming corporations, consumers, and the economy as a whole.

Closely related are studies that examine the impact of antitrust lawsuits across multiple industries and events, reporting mixed results on firm outcomes and real economic activity. For example, Babina et al. (2023) examine DOJ antitrust lawsuits and find that antitrust

enforcement actions in the U.S. permanently increase employment by 5.4% and business formation by 4.1% in non-tradable industries. Also investigating DOJ antitrust lawsuits, Kang (2025) examines how price-fixing cartels affect defendants' innovation, showing that collusion significantly enhances patent filings, R&D investment, and innovation breadth. Cestone et al. (2021), using the cases opened by the European Commission between 1991 and 2019, find that cartel investigations temporarily reduce profits for all firms in the affected industry and increase profits for their customers. In response to the negative shock to their profitability, firms engage in intense restructuring: they undertake mass layoffs and reduce employment; to a lesser extent, they increase leverage, cut investment, and sell assets.

Our analysis advances this literature, which has yielded contrasting conclusions on the impact of antitrust enforcement on corporate outcomes, along three dimensions. First, while existing studies measure enforcement's effects on aggregate outcomes, they cannot identify who gains *within* affected markets. We fill this gap by documenting that enforcement particularly benefits non-defendant firms, with gains disproportionately concentrated among small, women-, and minority-owned establishments that historically faced the greatest barriers to market access. Second, we show that this broader participation is not costless: procurement performance deteriorates in affected markets, and we document that this deterioration operates through a compositional shift in the supplier pool rather than through changes in contract structure or awarding procedure. Third, we demonstrate that these efficiency costs are driven by new contractors and concentrated in complex projects that require specialized expertise and organizational capacity, with no effects for simpler projects, revealing a trade-off between market access and product-market efficiency that aggregate analyses cannot detect.

Our paper is related to other research in the finance literature debating the use and effectiveness of antitrust enforcement. Azar et al. (2018) and Azar et al. (2022) demonstrate that institutional investors holding significant shares in competing firms can lead to higher consumer prices and reduced competition, advocating for enhanced antitrust scrutiny. On

the other hand, Koch et al. (2021) and Dennis et al. (2022) find no robust evidence that common institutional ownership reduces product market competition and conclude that antitrust restrictions targeting such ownership are currently unwarranted. More related to the conclusions of our findings, Donelson et al. (2025) document that antitrust enforcement against interlocking directorates reduces board industry experience by prompting the departure of experienced directors, thereby weakening corporate governance and firm performance despite no clear evidence of anti-competitive coordination.

2 Data

Information on antitrust lawsuits. We draw information on antitrust lawsuits from two complementary sources: Wolters Kluwer’s VitalLaw platform (which incorporates the Commerce Clearing House Trade Regulation Reporter, previously used by Babina et al. (2023) and Kang (2025)) and the official DOJ Antitrust Case Filings database. This dual-source approach enables extensive cross-validation and ensures complete coverage of enforcement actions.³

Although these alternative sources contain valuable information, extracting key metrics from this text data can be extremely complex. These documents often use specialized terminology and draw upon dense statutory and economic frameworks, including detailed discussions of relevant markets, industry practices, and case law precedents. Such language frequently requires substantial legal and economic expertise to interpret reliably. While prior studies have tried human assistants to identify and code these data, this manual process is prone to inconsistencies, subjective biases, and substantial labor demands, factors that can compromise both the accuracy and scalability of traditional approaches.

For this reason, we deploy OpenAI’s GPT-4 Omni (gpt-4o) via a systematic API im-

³VitalLaw serves as our primary source, providing detailed case summaries authored by antitrust law experts. As argued and demonstrated by Babina et al. (2023), these summaries serve as an authoritative source. Unlike the DOJ’s official website, which primarily contains recent cases, this database maintains comprehensive coverage across all periods. In addition, the summaries provide standardized information in a consistent format, facilitating systematic data collection.

plementation built on a structured JSON schema and supported by robust error-handling protocols. The process begins with a primary classification phase that identifies procurement-related cases within our corpus. Following this initial classification, we use LLMs to extract detailed information about violation types, defendant identities, and product market identifiers. This extraction follows predefined classification schemas and passes through multiple validation checkpoints to ensure accuracy.

Our approach is motivated by LLMs’ advanced capability of detecting subtle linguistic cues in complex legal documents. LLMs identifies specialized legal phrasing, contextual references, and descriptive language that signal anti-competitive behaviors such as collusion or monopolistic practices, nuances that traditionally require extensive manual review by legal experts. Recognizing the limitations inherent in LLMs, such as “hallucinations”, we implemented a comprehensive human review protocol where we manually examine all outputs, assuring that our final dataset consistently reflects the actual information content of antitrust enforcement documents. By integrating advanced LLMs with human verification, our methodology provides granular insights into procurement-related antitrust dynamics that would be difficult to capture with conventional manual coding or basic keyword searches.

Antitrust lawsuits related to government procurement. We collect information on 3,438 antitrust cases from 1971 onward and identify those associated with government procurement activities.⁴ We find that a sizable share of antitrust lawsuits (35.46% of the sample, or 1,203 cases) relates to government procurement. We then restrict our analysis to the period 2001–2021, which aligns with the coverage of our alternative data sources. Within this window, we identify 308 procurement-related cases. The frequency of these cases is reported

⁴Figure A1 presents two complementary text visualizations: a frequency-weighted word cloud displaying key terms across all procurement-related antitrust cases, and a specialized visualization highlighting procurement-specific terminology identified via LLMs. The first visualization excludes common legal terminology (e.g., “defendant,” “plaintiff,” “court”), procedural language, temporal references, numerical expressions, generic business terms, common verbs/adjectives, and geographic designations to emphasize case-specific content. The second uses multi-role LLMs to extract procurement-specific terms (e.g., “bid rigging,” “contract awards,” “federal acquisition”) that distinguish procurement-related violations from general antitrust cases.

in Figure 1. The timing of these lawsuits displays substantial heterogeneity, which we exploit in our empirical analysis.

[Insert Figure 1 about here]

An essential aspect of our analysis is the identification of the procurement market affected by the antitrust lawsuit. Following Reed et al. (2022), we define specific product markets using the corresponding 6-digit North American Industry Classification System (NAICS) code, the most granular level of aggregation available. Our data collection follows a strict hierarchy: when cases are available on the DOJ’s Antitrust Case Filings website,⁵ we directly use the NAICS classification officially assigned by the DOJ. For cases not available on the website, we use LLMs to extract this information from VitalLaw summaries, followed by manual verification to ensure maximum fidelity to official designations while providing comprehensive coverage across our sample.

Our product market definitions align precisely with how government contracting officers organize competition, using the same NAICS classifications that define real competitive sets in government procurement. This approach captures actual competitive dynamics as they occur in practice, rather than imposing theoretical market boundaries that may not reflect genuine substitutability patterns (Hoberg & Phillips, 2025).

We present the distribution of antitrust lawsuits across industries (2-digit NAICS code) in our final sample in Figure 2. Most events are concentrated in the construction, manufacturing, and other service markets, which are the largest procurement sectors. Figure A2 provides a more detailed breakdown at the product market level.

[Insert Figure 2 about here]

Another notable aspect of our approach is its ability to extract detailed information about the various facets of antitrust lawsuits, including the type of misconduct. Figure 3 illustrates that antitrust enforcement in government procurement is overwhelmingly driven

⁵More information at the following website: <https://www.justice.gov/atr/antitrust-case-filings-alpha>

by horizontal restraints, with bid rigging comprising more than 65% of all cases between 2001 and 2021. This concentration reflects the particular vulnerability of procurement markets to supplier collusion. While other forms of coordination, such as price fixing, market allocation, and information exchange, are present, they are far less prevalent. Merger-related violations are rare, underscoring that concerns in this domain are less about structural consolidation and more about strategic coordination.

[Insert Figure 3 about here]

Information on government contractors and procurement activities. After collecting and classifying information on antitrust lawsuits, we gathered additional data on government contractors. In addition, we collect information on government procurement contracts to build our final database and advance our empirical analysis.

We use data from the 2022 version of the National Establishment Time Series (NETS) database, which includes information on the universe of establishments in the U.S., spanning both private and public firms. Using this database, we identify establishments that are listed as government contractors. Whenever possible, our analysis always focuses on establishments rather than firms, as many firms operate multiple establishments, not all of which are government contractors or active in the same product markets.

This source provides information on establishments' sales and employees. We also use the PAYDEX score, a business credit score issued by Dun & Bradstreet; this measure objectively assesses financial health by reflecting establishment payment behavior, and it is extensively used by creditors, suppliers, and financial institutions to evaluate credit risk. Furthermore, this score predicts future short-term default risk and business failure (e.g., Chava et al., 2023). Finally, the NETS database also provides information on the location and industry of the establishments.

After identifying government contractors, we identify 542 establishments as defendants

in our final sample using information extracted from our antitrust lawsuit data.⁶ Additionally, we perform fuzzy matching to align the headquarters' names of these establishments with corporate balance sheet data from Compustat and stock price information from CRSP. Through this process, we identified 196 public companies that are government contractors operating in product markets exposed to antitrust lawsuits.

We gather comprehensive information on federal government contracts from *USAspending*. Detailed data on these contracts have been available since the Federal Funding Accountability and Transparency Act (FFATA) was enacted in 2006. This law was designed to improve transparency in government spending, and the database includes records dating back to 2001 (e.g., Brogaard et al., 2021).

It is important to note that a single award may consist of multiple transactions, as some awards involve several payments or modifications over time. We aggregate this information at the establishment and year levels to obtain the total dollar amount of government contracts awarded to each establishment.⁷ Additionally, this database provides detailed information on the characteristics of government procurement contracts, the relative procurement processes, and the winners. This enables us to conduct an empirical analysis to examine changes in market competitiveness, participant dynamics, and efficiency at the contract and product market levels.

Summary statistics. In our main analysis, we focus on establishments classified as government contractors (186,586 establishments), as indicated in the NETS database. After merging antitrust lawsuit data with establishment information and excluding establishments owned by defendant firms, our final dataset consists of 2,260,640 establishment-year obser-

⁶This analysis is limited by the fact that, in criminal antitrust cases, the DOJ Antitrust Division often prosecutes individuals rather than companies. Additionally, when confidentiality concerns are paramount, the DOJ may list individuals' names instead of companies' in its lawsuits.

⁷We merge this information with the NETS database, as both databases use the same establishment identifier, the DUNS number. Notably, the identifier used to report establishment-level information in the procurement database changed after 2021, limiting our ability to extend the analysis beyond that period. Furthermore, the most recent and final release of the NETS database includes information up to the year 2022.

vations spanning 2001 to 2021. We report the summary statistics for this sample in Table 1. Government contractors in our sample have an average of approximately 62 employees and report average annual revenues of \$14 million. On average, they receive \$811 thousand in government obligations. Their average PAYDEX score is 73, out of a maximum possible value of 100, and 17% of the establishments operate in product markets affected by antitrust lawsuits.

[Insert Table 1 about here]

3 Antitrust lawsuits and corporate performance

The determinants of antitrust lawsuits. Figure 1 shows the yearly, significant variation in antitrust lawsuit intensity over time. To better understand the dynamics of antitrust lawsuit activities, we begin our empirical analysis by examining their determinants. To do so, we estimate the following Equation:

$$Antitrust\ Lawsuit\ Event_{j,t} = \alpha_j + \gamma_t + \beta Industry\ Characteristics_{j,t} + \epsilon_{j,t} \quad (1)$$

Antitrust Lawsuit Event is an indicator variable equal to one if an antitrust lawsuit takes place in a product market j at time t . *Industry Characteristics* are alternative industry characteristics that we investigate. More specifically, we consider product market size, concentration (using the HHI index), and the number of participants.⁸ α_j and θ_t are, respectively, product market and year fixed effects, that we use in an alternative specification.

We report estimation results in Table 2. Contrary to our expectations, we find that antitrust lawsuits are more common in less concentrated product markets, as well as in markets with more competitors and greater overall size. More specifically, a 1% increase in

⁸We obtain this information by collapsing information from *USAspending* at the product-market and year level. We present the average values of these variables in the table containing the estimation results. A detailed description of each variable is available in Table A1.

HHI reduces the likelihood of being treated by 0.006 percentage points, a 1% increase in the number of firms increases it by 0.004 percentage points, and a 1% increase in government spending increases it by 0.002 percentage points. These findings align with the idea that anti-competitive and illegal behaviors are more likely where the benefits of such actions are higher.⁹ However, when we control for industry and year-fixed effects, the coefficients are not statistically significant anymore, suggesting that the exact timing of antitrust lawsuits is unexpected.

[Insert Table 2 about here]

Access to the product market. We aim to investigate how antitrust lawsuits involving government activities affect non-defendant government contractors operating in the product market affected by the lawsuits. In particular, antitrust lawsuits are expected to reduce barriers to market entry in affected product markets and expand market access.

To explore this hypothesis in greater depth, we collect information on government procurement contracts. We aggregate the dollar amounts of each transaction at the establishment-year level and merge this information with the NETS database using the DUNS number identifier common to both databases. Following previous literature (Sproul, 1993; Babina et al., 2023; Kang, 2025), we employ a difference-in-differences approach to compare affected government contractors with establishments operating in different, unaffected product markets before and after an antitrust lawsuit. In doing so, we exclude the establishments of identified defendant firms from the sample. More specifically, we estimate Equation (2):

$$IHS\ Government\ Contracts_{i,j,t} = \alpha_i + \gamma_t + \delta_{c,t} + \beta Antitrust\ Lawsuit_{i,j,t} + \epsilon_{i,j,t} \quad (2)$$

⁹While it is well known that illicit conduct is significantly more frequent in environments with weak institutions, the theoretical effect of firm competition on the benefits that firms derive from illegal conduct is ambiguous (Svensson, 2005; Cheung et al., 2021).

The outcome variable is the inverse hyperbolic sine transformation (IHS) of total government awards in an establishment i at time t . This transformation is appropriate because of the presence of many zeros and some negative values (e.g., adjustments from previous government-award contracts). In addition, it allows a similar interpretation of the regression results as the logarithmic transformation (e.g., Carroll et al., 2003). In this specification, *Antitrust Lawsuit* is an indicator that takes a value equal to one after an establishment i that is operating in a product market j at time t is exposed to an antitrust lawsuit for the first time.¹⁰ α_i and γ_t are respectively establishment and year fixed effects. Furthermore, in an additional specification, we control for county times year fixed effects ($\delta_{c,t}$), which enables us to account for time-invariant geographic characteristics.

We report the results in Table 3. We find a positive effect of antitrust lawsuits on the dollar amount of government contracts awarded to non-defendant establishments. This effect is also economically meaningful; considering the results reported in Column (2), the coefficient implies an increase of 16% in federal obligations after an antitrust lawsuit concerning the average transformed outcome variable. Given that the average non-transformed government obligation is 811 thousand dollars, this translates to an increase of approximately 130 thousand dollars in government obligations per establishment. This confirms that the estimated effect is both statistically and economically significant.

[Insert Table 3 about here]

Establishments' performance, employment, and financial health. To get a broader picture of how antitrust lawsuits affect corporations, we aim to estimate their impact on the overall performance of these establishments. To do so, we estimate Equation (2) and use the natural logarithm transformations of employment and sales as alternative outcome variables. In addition, we also use the PAYDEX score, which measures the financial health

¹⁰We identify the product market in which an establishment operates by using the NAICS-6 code that is reported when the establishment first appears in the NETS database.

of establishments.

We report estimation results in Table 4. We find a positive impact on sales and employees. More specifically, considering the results reported in Column (1), we find an increase of 6.1% in employment after an antitrust lawsuit. On the other hand, considering the results reported in Column (2), we find an increase of 9.2% in sales after an antitrust lawsuit. These results provide evidence that antitrust lawsuits benefit both corporations and employees in the affected product markets. These results suggest that corporations not only benefit from increased access to specific exposed product markets, but also experience overall growth.

Within this framework, we also consider previous literature suggesting that incumbent firms in affected product markets experience a decrease in profit margins, which could undermine their financial stability (Aguzzoni et al., 2013; Besley et al., 2021; Cestone et al., 2021). Accordingly, in Column (3), we examine the impact of antitrust lawsuits on the financial health of the establishments. However, we find a negative but statistically insignificant effect on the PAYDEX score.

[Insert Table 4 about here]

About the validity of our identification strategy. Our empirical strategy assumes the timing of the antitrust lawsuits in a product market is unexpected. This is supported by several features of antitrust enforcement: the DOJ typically conducts early investigations covertly to prevent evidence destruction and gather third-party information, creating uncertainty about if and when a lawsuit will occur. Even if firms suspect or know of an investigation, the likelihood and timing of litigation remain unclear.

To empirically investigate the validity of our identification strategy, we test whether there is any fundamental difference between our treatment and control groups. Table A2 shows the normalized differences for both industry and establishment characteristics. Panel A reports characteristics at the industry level, while Panel B reports characteristics at the establishment level. In the two panels, ND indicates normalized differences.

As reported in Imbens and Wooldridge (2009), two variables are considered similar if the normalized differences fall within the threshold of ± 0.25 . Based on this criterion, we conclude that establishments in exposed and non-exposed product markets are identical with respect to observable characteristics. However, as shown in our previous results (Table 2), we find that affected product markets are more competitive. Nonetheless, these differences should be accounted for by including time-invariant fixed effects.

To more explicitly test the validity of the parallel trend assumption, we consider the dynamic effects of our outcome variables. To do so, we use the estimator proposed by Callaway and Sant’Anna (2021). This approach also addresses the potential endogeneity of the difference-in-differences estimator when the treatment variable is staggered (De Chaisemartin & d’Haultfoeuille, 2020; Baker et al., 2022). Unlike traditional difference-in-differences methods that rely on a fixed reference year, this estimator compares treatment effects dynamically over multiple periods by leveraging units with the same initial treatment level as controls, allowing for non-binary and non-absorbing treatment processes.

Figure 4 provides evidence that our identified effects are long-lasting. There is a clear increase in employment, sales, and federal obligations awarded after the event; however, we do not find any effect on the PAYDEX score, consistent with our baseline results. It is also important to note that the coefficients before the event are close to zero.¹¹

[Insert Figure 4 about here]

¹¹It is important to note that the F-test on the joint statistical significance of the pre-treatment coefficients rejects the null that they are jointly equal to zero for all outcomes except the PAYDEX score. As argued by Roth (2022), statistically significant but near-zero pre-treatment coefficients may reflect negligible deviations from parallel trends detected by large sample sizes rather than economically meaningful pre-trends. To formally assess robustness to potential parallel trends violations, we implement the sensitivity test proposed by Rambachan and Roth (2023), which bounds treatment effects under progressively relaxed constraints on the counterfactual trend. Specifically, we adopt the average effects approach, which examines weighted averages of treatment effects across all post-treatment periods, providing a more holistic assessment of overall impact with greater precision than period-specific estimates. As shown in Figure A3, our treatment effect estimates for employment, sales, and government awards remain positive and statistically significant even at $M=0.3$, suggesting that our main conclusions are unlikely to be driven solely by violations of the parallel trends assumption.

Heterogeneous effects across establishments. We investigate whether the benefits of antitrust enforcement vary across establishment characteristics. To test this hypothesis, we interact the treatment variable in Equation (2) with indicator variables equal to one if the establishment belongs to a public corporation, is women- or minority-owned, or is small (fewer than five employees).

We report the results in Table 5. Following antitrust enforcement, procurement contracts flow disproportionately to traditionally disadvantaged contractors rather than to establishments with operational capabilities similar to those of the sanctioned defendants. Private establishments benefit substantially more than public corporations from antitrust enforcement. Women-owned businesses gain approximately 36% more than non-women-owned establishments (\$292,000 additional per establishment),¹² while minority-owned businesses receive approximately 64% more in government contracts compared to non-minority establishments, translating to roughly \$521,000 in additional procurement per establishment beyond the baseline effect. Small establishments receive approximately 27% more than large establishments (\$222,000 additional per establishment). In general, these results suggest that antitrust enforcement disproportionately benefits establishments that have traditionally faced greater barriers to market access, such as limited financial resources, weaker reputational capital, and exclusion from established procurement networks.

[Insert Table 5 about here]

3.1 Additional results and robustness checks

We provide additional results to demonstrate the robustness of our findings and the validity of our identification strategy.

¹²Dollar magnitudes represent differential effects calculated from the interaction coefficients in Columns (5)-(8) of Table 5. Using our sample mean of \$811,000 in annual government obligations per establishment, the women-owned interaction coefficient of 0.360 implies they receive approximately 36% more than non-women-owned establishments, translating to $\$811,000 \times 0.360 \approx \$292,000$ in additional contracts beyond what non-women-owned establishments receive from antitrust enforcement.

Stock market reactions to antitrust lawsuits. To further validate our empirical approach and test whether antitrust lawsuits are unexpected, we evaluate stock market returns for non-defendant firms around the filing dates. To do so, we identify affected firms by antitrust lawsuits as those operating within 6-digit NAICS codes subject to antitrust enforcement (excluding identified defendants).

As reported in Figure A4, we observe positive stock market reactions for this sample of 196 firms. More specifically, the figure presents the buy-and-hold abnormal return (BHAR) for these firms surrounding the days of antitrust enforcement filings. Before the enforcement event, BHAR remains relatively stable, fluctuating around zero. This further validates our empirical approach showing that antitrust lawsuits are unexpected. Following the event, we observe positive abnormal returns, reaching approximately 1% by day 10. This translates to an increase in shareholder wealth of \$8.45 million for the average firm in our sample (market capitalization of \$845 million), with effects ranging from \$1.96 million at the 25th percentile to \$34.4 million at the 75th percentile of firm size. For our sample of 196 non-defendant firms, this represents a total wealth creation of approximately \$1.66 billion.

These results are consistent with the idea that antitrust enforcement disrupts anti-competitive practices, effectively leveling the playing field for non-defendant firms. As market power shifts, these firms may gain access to previously restricted opportunities, secure more favorable contract terms, and increase sales. Investors, anticipating these benefits, revise their earnings expectations upward, resulting in positive stock price reactions.

Alternative transformations of the outcome variable. We consider alternative ways to measure government contracts. In our main results, we use the inverse hyperbolic sine (IHS) transformation of total government awards as the outcome variable, as this approach is suitable for handling the presence of many zeros and some negative values. We show that our findings remain robust when we use the level of the outcome variable or when we consider the average award values over two or three years, as well as the relative IHS transformation.

In fact, since government contracts often span multiple years, using the average award value provides a more stable measure that smooths out year-to-year fluctuations. We report the estimation results in Table [A3](#).

Propensity score matching. A potential concern with our baseline difference-in-differences approach is that product markets exposed to antitrust lawsuits may differ systematically from unexposed markets. To address this selection concern, we combine our difference-in-differences approach with a propensity score matching based on product market characteristics. Specifically, we match treated and untreated markets using one-to-one and one-to-five nearest-neighbor matching algorithms based on market size, concentration, and competitive structure. After matching, (unreported) normalized differences for our alternative samples confirm that both industries and establishments are similar in terms of observable characteristics. Table [A4](#) shows the estimation results using these matched samples. The coefficients remain positive and statistically significant, though slightly smaller in magnitude than our baseline estimates, falling within one standard error of the original results. This confirms that our findings are not driven by selection based on observable market characteristics.

Industry confounding trends. We further test whether other product market trends explain our findings. Specifically, we first explicitly control for time-variant product market characteristics.¹³ Next, we include industry (2-digit NAICS code)-by-year fixed effects to control for broader industry trends. Finally, we account for linear and quadratic time trends in the product market. We report consistent results in Table [A5](#). These results further indicate that broader industry or product market trends do not explain our findings.

Placebo test and triple difference-in-differences. We use establishments that are not government contractors but are located in the same county and operate in the same product

¹³We control for the share of fixed-price contracts, the share of contracts awarded through competitive auctions, the average number of contract renegotiations, and the HHI index at the product procurement market level.

market as government contractors as a placebo group. Using this substantially larger sample of 250,828,465 establishment-year observations, we estimate our baseline specification with employment, sales, and financial health as outcomes.¹⁴ As reported in Table A6, we do not find any effect of antitrust lawsuits on the performance of this group of establishments.

As an additional robustness check, we implement a triple difference-in-differences framework using non-government contractors as a control group. The identification assumption for this estimator is that no other events in the product market are generating differential trends between government and non-government contractors that could affect their relative outcomes (Gruber, 1994). If this assumption holds, we can isolate the causal effect of government spending from other confounding factors, thereby clarifying its true impact on corporate outcomes. More specifically, we estimate:

$$Y_{i,j,t} = \alpha_i + \delta_{c,t} + \gamma_{j,t} + \beta(GovtContractor_i \times Antitrust Lawsuit_{j,t}) + \epsilon_{i,j,t} \quad (3)$$

where $Y_{i,t}$ is the outcome for establishment i at time t . α_i captures establishment fixed effects, $\delta_{c,t}$ controls for county-by-year effects, and $\gamma_{j,t}$ represents product market-by-year fixed effects. The main effects of $GovtContractor_i$ and $Antitrust Lawsuit_{j,t}$ are absorbed by the establishment and product market-by-year fixed effects, respectively. Our coefficient of interest, β , isolates the differential effect of antitrust lawsuits on government contractors.

Table A7 presents these results. The coefficients on the triple interaction are positive and highly significant across all outcomes. Employment increases by 18.8% and sales by 30.9% for government contractors relative to non-contractors following antitrust enforcement. In addition, and in contrast to our baseline findings, the positive interaction coefficient we uncover when using the PAYDEX score as the outcome variable suggests that contractors experience a 1.537-point improvement (a 2.1% increase relative to the mean) following an antitrust lawsuit.

¹⁴It should be noted that government awards cannot be used as an outcome variable in this setting, as non-government firms do not obtain government contracts.

Alternative product market boundary definition. As explained above, by focusing on federal procurement, we avoid the need to pinpoint the exact geographic location of the misconduct, which is often difficult to determine, while federal procurement contracts typically constitute national markets. Nevertheless, geographic exposure may still matter, particularly for sectors such as construction and maintenance services, where production and contract execution occur on-site and can involve local contractor networks. In these cases, the economic effects of antitrust enforcement may be more concentrated in the areas surrounding the enforcement action.

To examine this possibility, we re-define treatment at the industry–state level, and incorporate information on the location of the federal district court where the DOJ filed the case. The filing court provides a meaningful proxy for the geography of exposure, as DOJ venue rules require cases to be brought where the defendants transact business or where the alleged collusive conduct occurred. Accordingly, proximity to the filing court captures local information diffusion, oversight intensity, and reallocation channels.

We estimate Equation (2) using this alternative treatment definition and report the results in Table A8. The coefficient on antitrust lawsuits for government contracts is 0.273 and statistically significant, representing an 11.3% increase relative to the baseline average value of 2.409. The consistency of these results across both our baseline market definition and this state-level geographic specification demonstrates that antitrust enforcement generates substantial benefits for non-defendant contractors through market reallocation, with court filing locations effectively capturing the geographic concentration of enforcement effects.

4 Market participation and procurement performance

Enforcement and the erosion of incumbent barriers. To provide evidence that antitrust enforcement dismantles the structural barriers erected by anti-competitive conduct, we identify 542 unique establishments belonging to defendant firms and examine how their

market presence evolves following enforcement. We use the same control group and compare their government award dynamics with those of establishments operating in product markets that have never been affected by antitrust lawsuits.

We next estimate the following Equation:

$$IHS\ Government\ Contracts_{i,t} = \alpha_i + \theta_{t,c} + \beta Antitrust\ Lawsuit_{i,t} + \epsilon_{i,t} \quad (4)$$

In this setting, *Antitrust Lawsuit* is an indicator variable equal to 1 after an antitrust lawsuit that affects the establishments of an affected firm. On the other hand, α_i and $\theta_{t,c}$ are establishment and county and year fixed effects, respectively.

We find a negative effect on the dollar amount of government contracts for these establishments, as reported in Table 6. More specifically, we find a statistically significant decrease of 57.40% in federal obligations for defendant establishments after an antitrust lawsuit concerning the average outcome variable. Thus, after an antitrust lawsuit, defendant firms lose an average of \$460–\$470 thousand dollars in government contracts.¹⁵ Overall, these results provide direct evidence that antitrust enforcement dismantles the anti-competitive barriers that incumbent defendants had erected.

[Insert Table 6 about here]

Increased market participation. To further investigate whether antitrust lawsuits halt anti-competitive practices, we conduct an analysis at the product market level. Specifically, we examine whether the number of participants in exposed product markets increases following an antitrust lawsuit. To do so, we estimate the following Equation (5).

$$Outcome_{j,t} = \alpha_s + \gamma_t + \beta Antitrust\ Lawsuit_{j,t} + \epsilon_{j,t} \quad (5)$$

¹⁵Notably, Heese and Pérez-Cavazos (2019) show that regulatory scrutiny does not always lead to contract reductions; they show that federal agencies can substitute cost-plus contracts with fixed-price contracts when firms face fraud allegations, though reductions occur after settlements.

In this setting, *Outcome* refers to alternative measures, including the number of participants in a product market, market concentration, the HHI index, and the bid protest rate.¹⁶ *Antitrust lawsuits* is an indicator variable equal to 1 after the first antitrust lawsuits in a product market, and 0 otherwise. α_s and γ_t are product market and year-fixed effects, respectively.

The results are presented in Table 7. We find that antitrust lawsuits result in an increased number of market participants and a reduction in market concentration. Specifically, the number of participants increases by 8% relative to the average transformed value. This suggests that an antitrust lawsuit leads to an increase of approximately 16 additional establishments per product market, representing a substantial rise in market participation. At the same time, we document a significant 2% decrease in the natural logarithm of the HHI index relative to its average transformed value. This coefficient implies that an antitrust lawsuit reduces the HHI index from approximately 2,027 to 1,750, indicating a meaningful decline in market concentration. Figure A5 confirms these patterns dynamically. We observe significant increases in market participation and decreases in concentration following enforcement, with pre-treatment coefficients close to zero.

Finally, we use data on bid protests from Canayaz et al. (2025) to investigate whether antitrust lawsuits effectively impact market fairness, or at least how market fairness is perceived by participants. This information is available for the spanning period from 2005 to 2016. Using the number of bid protests adjusted for the number of unique firms participating in a product market as an outcome variable, we identify a negative relationship between antitrust enforcement and the protest rate. More specifically, the coefficient indicates a 0.051 percentage point decrease in the protest rate following antitrust lawsuits, implying that enforcement actions may improve market transparency and reduce the need for firms to challenge procurement decisions. This effect represents a substantial 86.4% reduction

¹⁶We constructed these variables using data from *USASpending*, except for the protest rate, which is derived from bid protest data from Canayaz et al. (2025). The estimation results table presents the average values of these variables, and a detailed description of each variable is provided in Table A1.

relative to the sample average protest rate of 0.059. However, despite this economically meaningful effect, it is not statistically significant at the conventional 90% level.

[Insert Table 7 about here]

Procurement performance. After providing evidence of increased market participation and decreased market concentration, we examine procurement performance. While firms with significant market power may underprovide quality to reduce costs and maximize profits (Copeland, 1934; Abbott, 1955; Matsa, 2011), in our context, the increase in the number of market participants following antitrust lawsuits may come at the expense of efficiency. Removing dominant incumbents can undermine previous efficiency and scale advantages, while new entrants may lack the capacity, fixed investments, and specialized expertise required for complex projects. Moreover, such disruptions can impose compliance burdens on suppliers and destabilize long-standing supply relationships. In addition, in competitive procurement markets, low-quality firms may underbid and displace higher-quality providers, especially when quality is hard to observe or contract on (Akerlof, 1970; Klein & Leffler, 1981; Laffont & Tirole, 1993; Manelli & Vincent, 1995; Albano et al., 2017).

To investigate this point, we merge our establishment-level database with government contracts. After identifying the main contract based on the earliest transaction and removing contracts with a value of zero or negative and Indefinite Vehicle Contracts (IDV), our final database consists of 35,575,254 contracts. Using this database, we estimate the following Equation:

$$Procurement\ Performance_{c,i,j,t} = \eta_s + \theta_{t,c} + \lambda_{a,t} + \beta Antitrust\ Lawsuit_{j,t} + \epsilon_{c,i,j,t} \quad (6)$$

Procurement Performance are alternative proxies for procurement performance of a contract c awarded to an establishment i in a product market s at time t . More specifically, we

follow the previous literature and use alternative proxies for procurement performance. We consider the number of modifications, the (IHS transformation of the) total amount of renegotiations, the cost overrun ratio (measured as the difference between the final and initial contract costs divided by the initial contract value), and delivery delays (an indicator equal to one if the actual delivery date differs from the initially scheduled date). *Antitrust Lawsuit* is an indicator variable that takes a value equal to one after the first antitrust lawsuit in a specific product market. η_s , θ_{tc} , and λ_{at} are product market, county-by-year, and funding agency-by-year fixed effects, respectively. The funding agency-by-year fixed effects absorb time-varying demand-side shocks at the agency level, ensuring that our estimates are not driven by changes in agency-specific procurement patterns. Finally, we weigh each contract by (the natural logarithm of its) respective contract value.

We report the results in Table 8. Interestingly, we find that procurement performance deteriorates following antitrust lawsuits. More specifically, following an antitrust lawsuit, the number of modifications increases by approximately 0.097 per contract, and the IHS-transformed renegotiation amount increases by 0.193.¹⁷ Additionally, the cost overrun ratio increases by 1.7 percentage points, and the probability of a delivery delay rises by 2.6 percentage points.

[Insert Table 8 about here]

Figure 5 presents the dynamic effects of antitrust enforcement on procurement performance. The estimates of the event study reveal two key patterns. First, the pre-treatment coefficients are small and statistically insignificant, supporting the parallel trends assumption underlying our identification strategy. Second, in the post-period, the coefficients turn positive and statistically significant. In addition, we conduct two robustness exercises to validate these findings. First, we use propensity score matching to address potential selection concerns, matching treated and untreated product markets based on market size,

¹⁷For the IHS transformation, this coefficient represents approximately a 21.3% increase for contracts with positive baseline renegotiations.

concentration, and competitive structure. As shown in Table A9, both one-to-one (Panel A) and one-to-three (Panel B) matching specifications broadly confirm our main results. The number of modifications increases by 0.088–0.107 per contract, and delivery delays increase by 1.7–2.4 percentage points, with cost overrun ratios rising by 1.1–1.6 percentage points, though not all coefficients reach conventional significance thresholds across both specifications. Second, using our alternative treatment definition based on court filing location and product market, Table A10 shows consistent effects: modifications increase by 0.112 per contract, cost overruns rise by 1.9 percentage points, and delivery delays increase by 2.8 percentage points.

These consistent results across different specifications and identification strategies confirm that the deterioration in procurement performance following antitrust enforcement is robust, potentially causal, and not driven by product market boundary definition choices.

Heterogeneous effects across contracts. To test which set of contracts drives our findings and better understand why procurement quality deteriorates after antitrust lawsuits, we fully interact the treatment variable with alternative proxies for contract complexity and difficulty in assessing quality. First, we consider the duration of the contract, measured at the award date. More specifically, we use the natural logarithm of one plus the initial length of the contract in days.

The results are reported in Panel A of Table 9. The coefficient of interest is negative and statistically significant across the different outcome variables, indicating that when the contract length is hypothetically zero, antitrust lawsuits are associated with improved procurement performance. For simple contracts (same-day completion), antitrust enforcement reduces modifications by 0.187 per contract and delivery delays by 5.0 percentage points. However, procurement performance significantly worsens for longer-duration contracts, where sustained relationships and accumulated project-specific expertise are most critical. For example, for one-year contracts, enforcement increases modifications by 0.309

per contract, cost overruns by 5.5 percentage points, and delivery delays by 5.0 percentage points. This results demonstrates that longer-duration contracts, where sustained relationships and accumulated project-specific expertise are most critical, experience the most severe performance deterioration following antitrust enforcement.

Panel B examines heterogeneous effects by contract type, distinguishing standard good delivery from other contracts. For non-standard good delivery contracts, antitrust enforcement leads to substantially worse outcomes: modifications increase by 0.221 per contract, cost overrun ratios rise by 4.8 percentage points, and delivery delays increase by 4.2 percentage points. By contrast, goods contracts experience considerably more moderate deterioration. The interaction term implies that goods contracts experience 0.172 fewer modifications per contract and a 4.4 percentage point lower cost overrun ratio relative to non-goods contracts. The interaction effect on delivery delays is negative but does not reach conventional significance levels.

Panel C tests whether the performance deterioration is concentrated among inexperienced contractors, defined as firms executing one of their first three government contracts. For experienced contractors, antitrust enforcement increases modifications by 0.092 per contract, cost overrun ratios by 1.5 percentage points, and delivery delays by 2.5 percentage points. These effects are substantially amplified for inexperienced contractors: the interaction term alone implies 0.200 additional modifications per contract and 3.5 additional percentage points in delivery delays on top of the baseline antitrust effect. The interaction term on cost overruns is positive but does not reach conventional significance thresholds. These results are consistent with the compositional mechanism: enforcement opens procurement markets to new entrants who, while now able to access contracts, may lack the accumulated experience required for successful delivery.

Finally, Panel D explores the role of within-firm learning by interacting the treatment variable with the log of each contractor's cumulative contract count. For contractors executing their first government contract, antitrust enforcement is associated with an increase

of 0.292 modifications per contract, a 6.4 percentage point increase in cost overruns, and a 6.0 percentage point increase in delivery delays. Importantly, the interaction coefficients are negative and statistically significant across all outcomes, indicating that the adverse performance effects diminish as contractors accumulate experience and move up the learning curve. This pattern further supports the view that inexperienced entrants, who become more prevalent in the post-enforcement supplier pool, are the primary driver of the observed procurement performance deterioration.

[Insert Table 9 about here]

The mechanisms behind worse procurement performance. To better understand the mechanisms behind our findings, we examine the characteristics of the awarded contracts. Specifically, we re-estimate Equation (6) using alternative outcome variables. In particular, we consider the initial contract size, whether the contract is awarded on a cost-plus basis, whether it was awarded through a competitive process, whether the contract is fixed-price, and the number of bids.

Table A11 shows that enforcement does not significantly affect initial contract values, the level of competition in contract awards, or the prevalence of fixed-price contracts. We document a marginally significant decrease in the use of cost-plus contracts (Column 2), though this effect is small in magnitude and unlikely to account for the deterioration in performance we document. The null results in Columns (1), (3), and (4) show that the documented decline in procurement performance cannot be attributed to changes in contract structure or competitive procedures. Interestingly, we also do not find in Column (5) any difference in the number of bids. However, this conclusion should be interpreted with caution because the bids variable is missing for a substantial share of contracts and varies systematically with the procurement method, making it an imperfect proxy for competitive intensity.

[Insert Table A11 about here]

Together, these results suggest that the deterioration in procurement performance operates through changes in the composition of suppliers rather than through changes in how contracts are designed or competed. This pattern is consistent with a selection mechanism: enforcement removes entrenched defendants and reallocates awards to new or smaller suppliers who, while now able to access the market, may lack the capacity, experience, or specialized expertise required for complex projects.

5 Conclusion

A growing literature in economics and finance debates whether governments should intervene to foster competition in product markets and whether antitrust actions ultimately improve economic outcomes. Our analysis offers new evidence on the effectiveness of these interventions.

Using government procurement as a laboratory, we study how antitrust lawsuits affect product market dynamics. We find that enforcement substantially improves access to affected markets for non-defendant firms, particularly small, women-, and minority-owned businesses. At the product-market level, we observe an increase in the number of participants and a decline in concentration. However, we also document that procurement performance deteriorates. This effect is driven by contracts that require significant technical expertise or long-term project-specific capabilities, diminishes with contractors' experience, and is muted, or even reversed, for simpler contracts.

Taken together, these results suggest that antitrust enforcement can meaningfully expand opportunities for non-defendant businesses and their employees, but that these gains may come with efficiency costs. In markets with high capital requirements and significant knowledge barriers, antitrust actions may disrupt established capabilities in ways that ultimately diminish the quality of product market outcomes.

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Figures

Figure 1: **Antitrust Lawsuits Related to Government Activities**

Figure 1 shows the number of antitrust lawsuits related to government procurement activities over time, specifically for the period 2001–2021. VitalLaw case summaries serve as our source of information. We use LLMs to identify government-procurement-related antitrust cases.

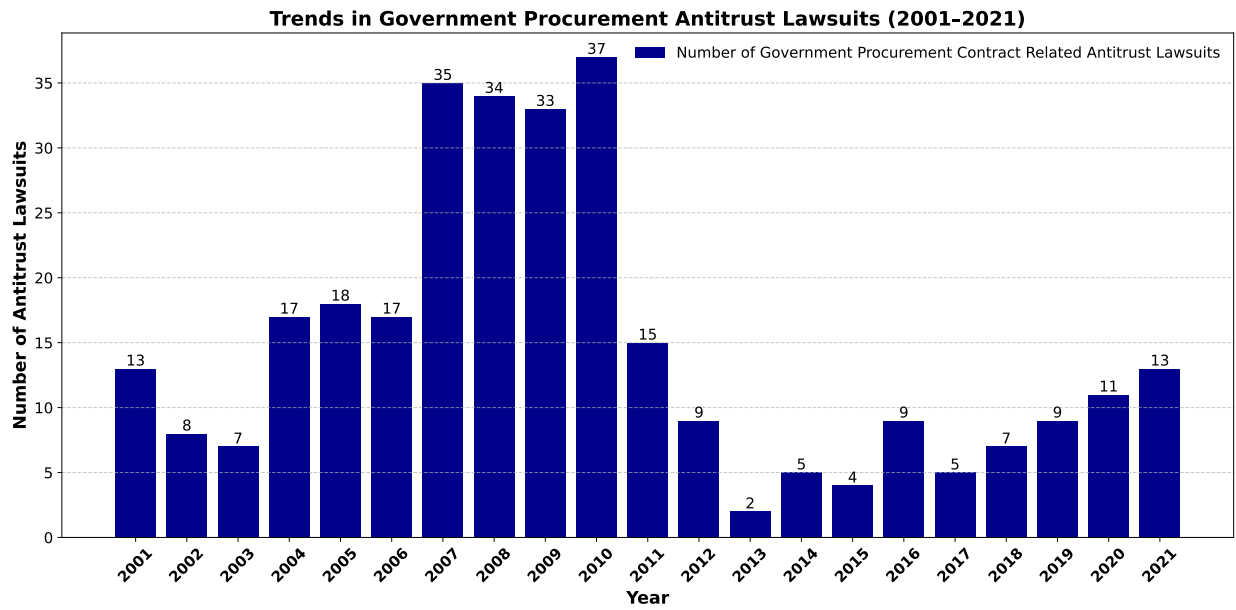


Figure 2: Antitrust Lawsuits Related to Government Procurement Activities Across Sectors

Figure 2 illustrates the frequency of antitrust lawsuits related to government procurement activities across product markets during the period 2001–2021. When information about the product market is unavailable from the Antitrust Case Filings database, we use LLMs to extract this information from VitalLaw case summaries.

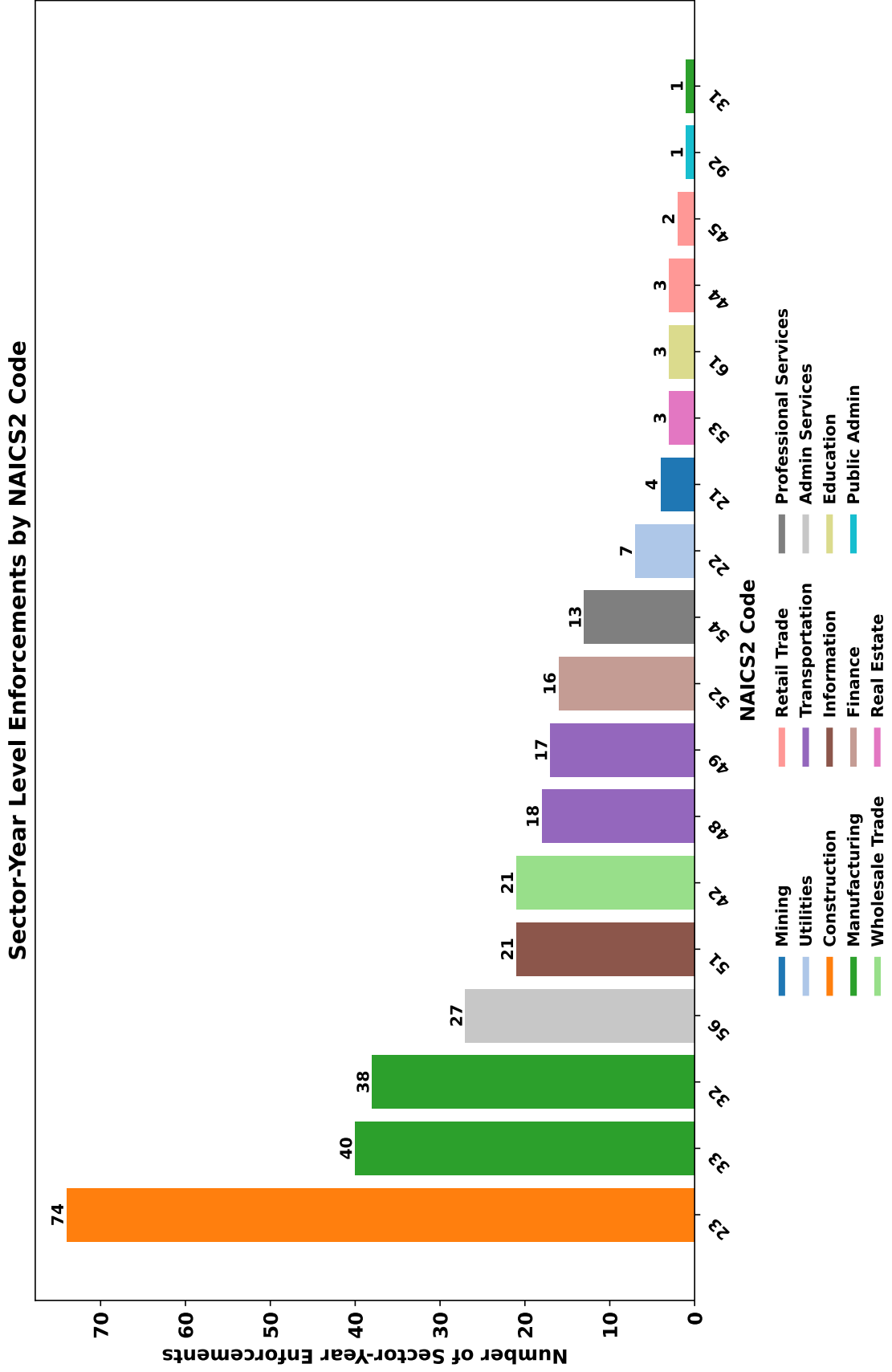


Figure 3: Types of Antitrust Lawsuits Violation

Figure 3 displays the frequency distribution of different violation types in antitrust lawsuits related to government procurement activities from 2001-2021. The horizontal bars show the count of each violation type, arranged in descending order of frequency. This analysis is based on DOJ antitrust case data from VitalLaw summaries, with violations identified and classified using LLMs.

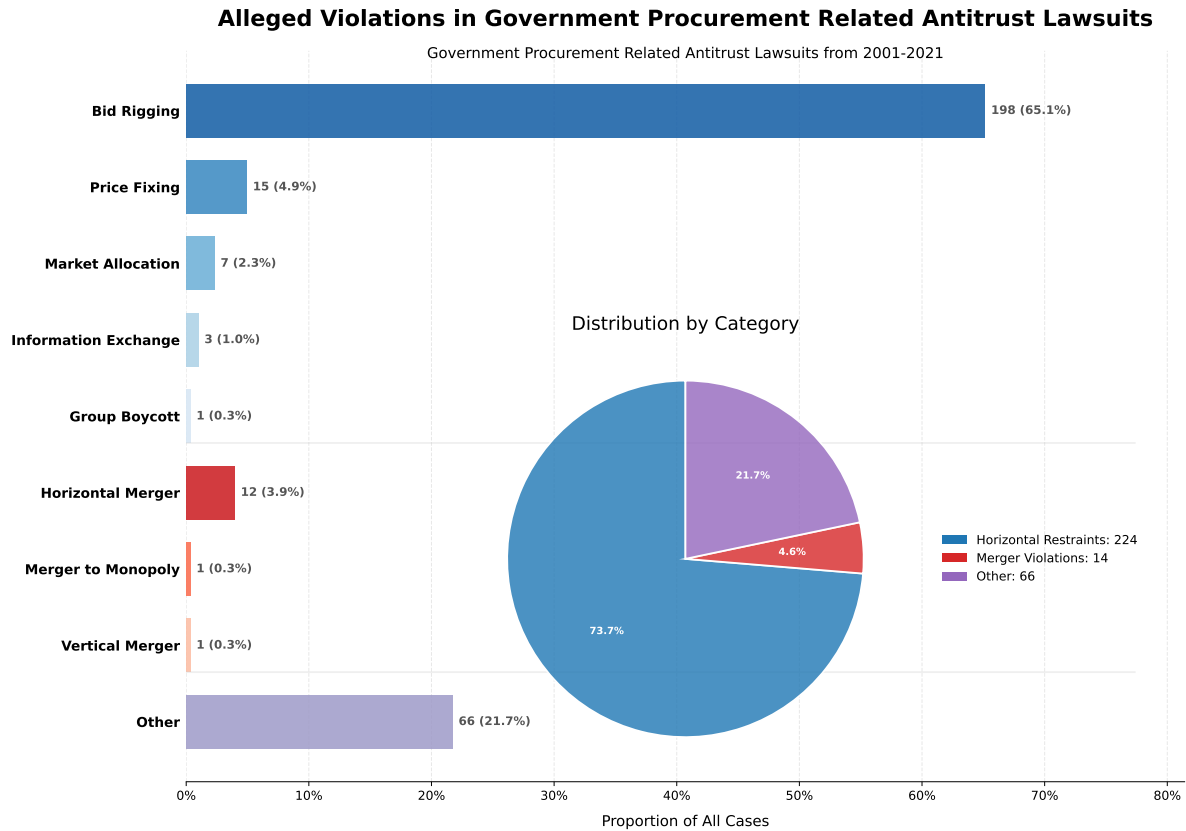
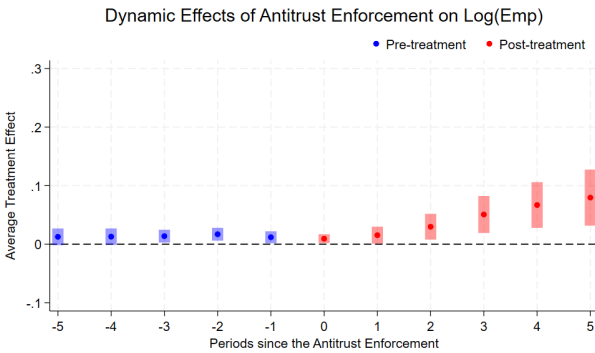
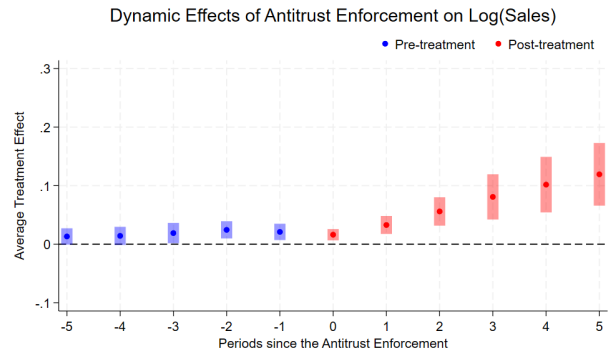


Figure 4: Antitrust Lawsuits and Non-Defendants Outcomes: Dynamics Effects

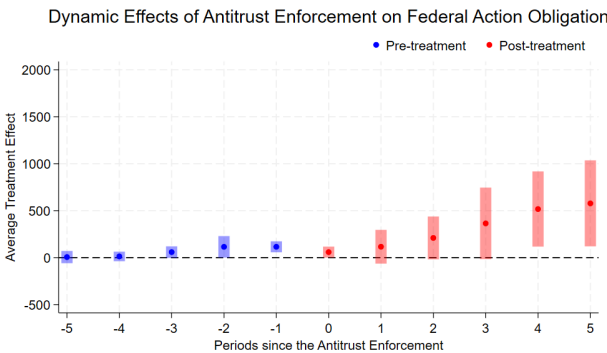
Figure 4 displays the dynamic effects of antitrust lawsuits on non-defendant establishments' outcomes to test the validity of our difference-in-differences identification strategy. Using the estimator proposed by Callaway and Sant'Anna (2021), we address potential endogeneity concerns in staggered treatment settings that traditional difference-in-differences methods may not fully account for (Baker et al., 2022). We examine four key outcome variables: employment, sales, federal government contract awards, and financial health (PAYDEX score). The x-axis represents time periods relative to the filing date of antitrust lawsuits (event time), with negative values indicating pre-treatment periods and positive values indicating post-treatment periods. The y-axis shows the average treatment effect on the treated (ATT) for each outcome variable. The shaded areas represent 95% confidence intervals.



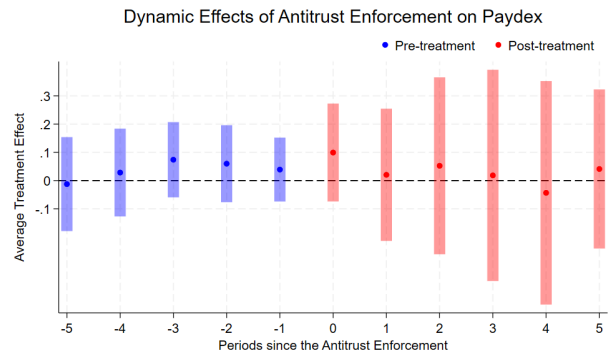
(a) Log(Employment)



(b) Log(Sales)



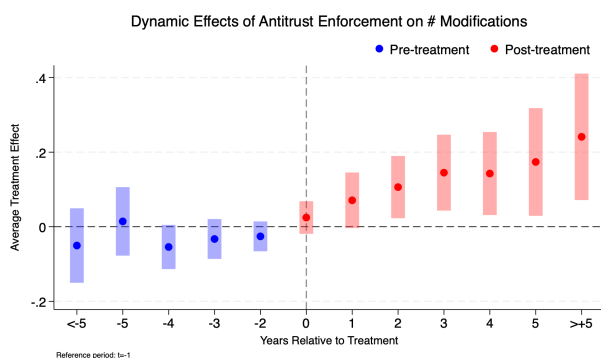
(c) Federal Government Awards



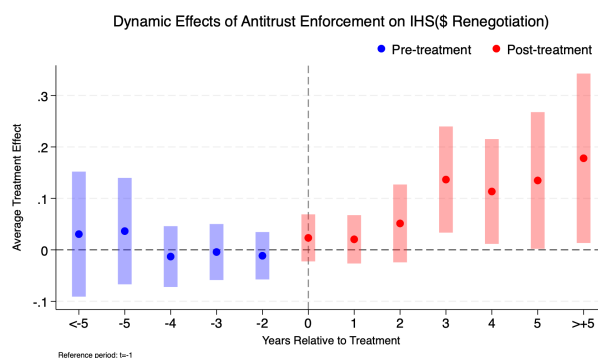
(d) PAYDEX

Figure 5: **Antitrust Lawsuits and Procurement Performance: Dynamic Effects**

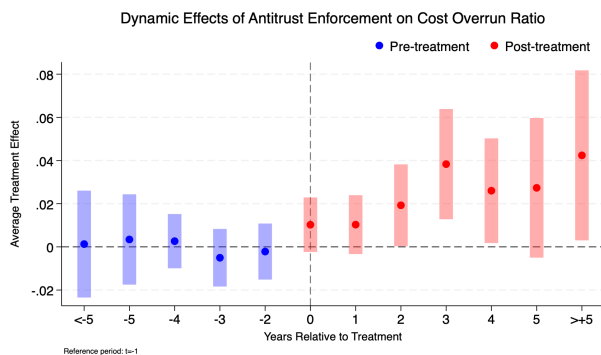
Figure 5 displays coefficient estimates from a dynamic difference-in-differences specification. We estimate the effects of antitrust enforcement on four key procurement performance metrics: the number of contract modifications, renegotiated amounts (inverse hyperbolic sine transformation), cost overrun ratio, and delivery delays. All specifications include product market, funding agency-by-year, and county-by-year fixed effects. The funding agency-by-year fixed effects absorb time-varying demand-side shocks at the agency level, ensuring that estimates are not driven by changes in agency-specific procurement patterns. The x-axis represents years relative to the treatment (antitrust lawsuit filing), with negative values indicating pre-treatment periods and positive values indicating post-treatment periods. The y-axis shows the estimated treatment effects, with the reference period normalized to $t=-1$. Shaded areas represent 95% confidence intervals based on standard errors clustered at the product market level. Each regression is weighted by the natural logarithm of contract value to reflect economic significance.



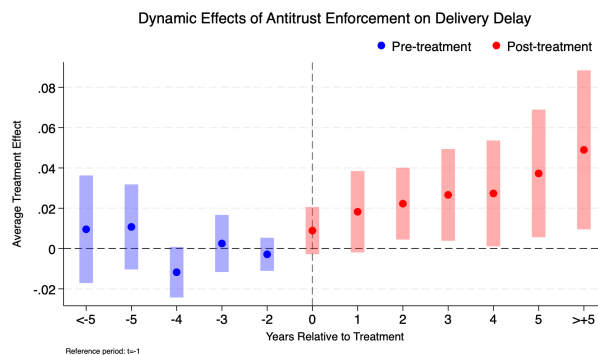
(a) # Modifications



(b) IHS(\$ Renegotiation)



(c) Cost Overrun Ratio



(d) Delivery Delay

Table 1: **Summary Statistics: Establishment Level Database**

Table 1 provides information on the main sample we use in our empirical analysis. It reports the summary statistics of the variables in our establishment-level database. The period of analysis goes from 2001 to 2021. Definitions of the variables are provided in Table A1 in the Appendix. All continuous variables are winsorized by year at the 1st and 99th percentiles.

Variables	Count	Mean	SD	p25	p75
Government Obligation (\$thousand)	2,260,640	811.408	3,697.976	0.000	65.000
IHS Government Obligation	2,260,640	2.409	3.259	0.000	4.868
Antitrust Lawsuit	2,260,640	0.165	0.371	0.000	0.000
Employment	2,260,640	62.295	340.849	3.000	35.000
Log(Employment)	2,260,640	2.357	1.685	1.099	3.555
Sales (\$thousand)	2,260,640	13,998.217	133,471.792	228.000	5,043.584
Log(Sales)	2,260,640	7.052	2.103	5.429	8.526
PAYDEX	1,575,009	72.505	9.211	69.000	79.500

Table 2: **The Determinants of Antitrust Lawsuits**

Table 2 shows regression results from Equation (1). We use an indicator variable equal to one if the product market has been exposed to an antitrust lawsuit as an outcome variable. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Treated	Treated	Treated	Treated	Treated	Treated
Log(HHI)	-0.006*** (0.001)	-0.001 (0.001)				
Log(Number of Firms)			0.004*** (0.001)	-0.001 (0.001)		
Log(Government Spending)					0.002*** (0.000)	-0.001 (0.000)
Product Market FE	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes
Observations	23,633	23,615	23,633	23,615	23,633	23,615
R-squared	0.005	0.172	0.006	0.172	0.007	0.172

Table 3: **Antitrust Lawsuits and Access to the Product Market for Non-Defendant**

Table 3 shows regression results from Equation (2). We use the inverse hyperbolic sine transformation (IHS) of total government awards as an outcome variable. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)
Variables	IHS Government Contracts	IHS Government Contracts
Antitrust lawsuit	0.193** (0.093)	0.160** (0.076)
Establishment FE	Yes	Yes
Year FE	Yes	No
County-Year FE	No	Yes
Observations	2,260,640	2,260,640
R-squared	0.542	0.554

Table 4: **Antitrust Lawsuits and Non-Defendants Outcomes**

Table 4 shows regression results from Equation (2), applying the same model to three different outcome variables: Log(Employment), Log(Sales), and PAYDEX. These variables measure establishment size, performance, and financial health, respectively. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)	(3)
Variables	Log(Employment)	Log(Sales)	PAYDEX
Antitrust Lawsuit	0.061*** (0.021)	0.092*** (0.023)	-0.048 (0.127)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	2,260,640	2,260,640	1,565,481
R-squared	0.932	0.942	0.521
Average Outcome	2.357	7.052	72.51

Table 5: **Heterogeneous Effects of Antitrust Lawsuits by Establishment Characteristics**

Table 5 examines whether the effects of antitrust lawsuits on non-defendant establishments vary by establishment characteristics. We interact the post-event indicator with indicator variables for public corporations, women-owned businesses, minority-owned businesses, and small establishments. Columns (1) and (5) show effects for public corporations, columns (2) and (6) for women-owned businesses, columns (3) and (7) for minority-owned businesses, and columns (4) and (8) for small establishments. Columns (1)-(4) include establishment and year fixed effects, while columns (5)-(8) include establishment and county-year fixed effects. The outcome variable is the inverse hyperbolic sine transformation (IHS) of total government awards. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IHS Government Contracts							
Antitrust Lawsuit	0.214** (0.097)	0.100 (0.083)	0.046 (0.082)	0.043 (0.094)	0.185** (0.080)	0.072 (0.070)	0.029 (0.072)	0.014 (0.081)
Antitrust Lawsuit \times Public	-0.327** (0.157)				-0.385** (0.155)			
Antitrust Lawsuit \times Women-Owned		0.378*** (0.088)				0.360*** (0.079)		
Antitrust Lawsuit \times Minority-Owned			0.715*** (0.141)				0.642*** (0.113)	
Antitrust Lawsuit \times Small				0.281*** (0.090)				0.274*** (0.081)
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	No	No	No	No
County-Year FE	No	No	No	No	Yes	Yes	Yes	Yes
Observations	2,260,640	2,260,640	2,260,640	2,260,640	2,260,640	2,260,640	2,260,640	2,260,640
Adjusted R-squared	0.505	0.505	0.505	0.505	0.505	0.505	0.505	0.505

Table 6: **Market Share Loss for Defendant Establishments**

Table 6 examines whether defendant establishments experience market exclusion following antitrust enforcement. To provide direct evidence of market exclusion mechanisms, we identify 174 unique establishments that are government contractors and part of defendant firms. We compare their government contract dynamics with those of establishments operating in product markets that have never been affected by antitrust lawsuits using Equation (4). The Defendant Establishment variable captures the effect of antitrust lawsuits on establishments belonging to firms found to have violated antitrust laws. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)
Variables	IHS Government Contracts	IHS Government Contracts
Defendant Establishment	-1.234*** (0.255)	-1.296*** (0.272)
Establishment FE	Yes	Yes
Year FE	Yes	No
County-Year FE	No	Yes
Observations	1,752,092	1,752,092
R-squared	0.531	0.545
Average Outcome	2.258	2.258

Table 7: **Industry-Level Effects of Antitrust Enforcement**

Table 7 presents product market-level analysis examining how antitrust lawsuits affect market structure, concentration, and perceived fairness. We estimate Equation (5) using three different outcome variables: the number of market participants (logarithm of unique firms), market concentration (logarithm of HHI index), and perceived market fairness (bid protest rate). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)	(3)
Variables	Log(# of Unique Firms)	Log(HHI)	Protest Rate (%)
Antitrust lawsuit	0.303*** (0.074)	-0.148** (0.070)	-0.051 (0.038)
Product market FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	23,615	23,615	14,080
R-squared	0.873	0.642	0.148
Average Outcome	3.816	7.616	0.059

Table 8: **Effects of Antitrust Enforcement on Procurement Performance**

Table 8 examines whether antitrust enforcement affects government procurement performance. Column (1) shows the number of contract renegotiations, Column (2) presents the inverse hyperbolic sine transformation of dollar amount of contract renegotiations, Column (3) reports the cost overrun ratio, and Column (4) examines delivery delays. Each observation is weighted by contract value to reflect economic significance. All specifications include product market, funding agency-by-year, and county-by-year fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)	(3)	(4)
Variables	# Modifications	IHS(\$ Renegotiation)	Cost Overrun Ratio	Delivery Delay
Antitrust Lawsuit	0.097** (0.039)	0.193** (0.092)	0.017** (0.008)	0.026*** (0.010)
Product Market FE	Yes	Yes	Yes	Yes
Funding Agency \times Year FE	Yes	Yes	Yes	Yes
County \times Year FE	Yes	Yes	Yes	Yes
Observations	35,575,254	35,575,254	35,453,772	30,151,310
R-squared	0.340	0.114	0.161	0.200
Average Outcome	1.189	0.200	0.029	0.052

Table 9: **Heterogeneous Effects of Antitrust Enforcement on Procurement Performance**

Table 9 examines how antitrust enforcement affects procurement performance across different contract and contractor characteristics. Panel A analyzes heterogeneity by contract length. Panel B examines differential effects for goods procurement. Panel C tests whether contracts awarded to inexperienced contractors (firms executing one of their first three government contracts) exhibit differential performance. Panel D examines within-firm learning by interacting treatment with the log of each contractor’s cumulative contract count. The dependent variables measure contract modifications frequency, renegotiated amounts (inverse hyperbolic sine transformation), cost overrun ratios, and delivery delays. Each observation is weighted by contract value to reflect economic significance. All specifications include product market, funding agency-by-year, and county-by-year fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

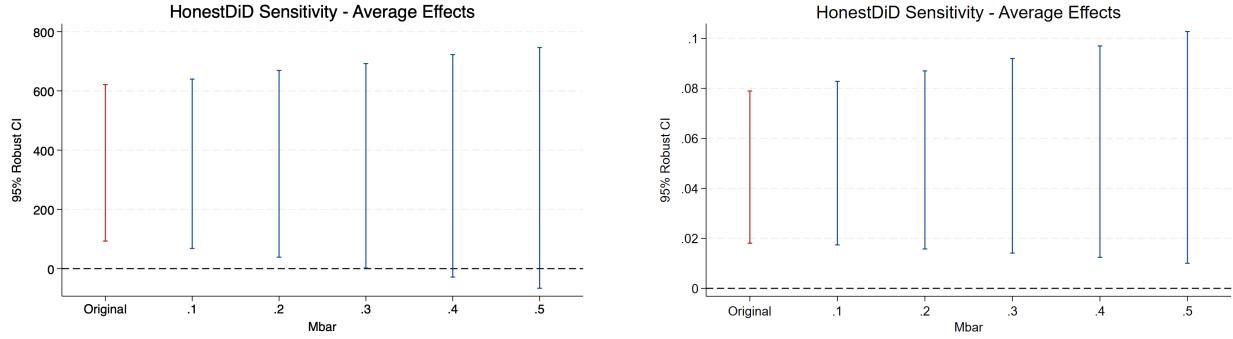
Variables	(1)	(2)	(3)	(4)
	# Modifications	IHS(\$ Renegotiation)	Cost Overrun Ratio	Delivery Delay
Panel A: Contract Length				
Antitrust Lawsuits	-0.187*	-0.477	-0.033	-0.050**
	(0.113)	(0.323)	(0.029)	(0.025)
Log(Contract Length)	0.111***	0.175***	0.022***	0.018***
	(0.017)	(0.032)	(0.004)	(0.003)
Antitrust Lawsuits × Log(Contract Length)	0.084***	0.196**	0.015*	0.017***
	(0.027)	(0.085)	(0.008)	(0.006)
Observations	35,557,013	35,557,013	35,437,948	30,151,308
R-squared	0.365	0.120	0.171	0.207
Panel B: Goods Procurement				
Antitrust Lawsuits	0.221***	0.562***	0.048**	0.042**
	(0.084)	(0.181)	(0.021)	(0.017)
Goods Contract	-0.684***	-1.299***	-0.179***	-0.136***
	(0.062)	(0.140)	(0.020)	(0.013)
Antitrust Lawsuits × Goods Contract	-0.172**	-0.518***	-0.044*	-0.024
	(0.085)	(0.198)	(0.024)	(0.016)
Observations	35,575,254	35,575,254	35,453,772	30,151,310
R-squared	0.357	0.120	0.173	0.209
Panel C: Inexperienced Contractors				
Antitrust Lawsuits	0.092**	0.179*	0.015*	0.025**
	(0.039)	(0.093)	(0.008)	(0.010)
Inexperienced Contractor	0.156***	0.577***	0.059***	0.033***
	(0.021)	(0.072)	(0.008)	(0.004)
Antitrust Lawsuits × Inexperienced Contractor	0.200**	0.486**	0.048	0.035**
	(0.079)	(0.192)	(0.029)	(0.016)
Observations	35,575,254	35,575,254	35,453,772	30,151,310
R-squared	0.340	0.115	0.162	0.201
Panel D: Within-Firm Learning Curve				
Antitrust Lawsuits	0.292***	0.654***	0.064**	0.060***
	(0.088)	(0.242)	(0.027)	(0.021)
Log(Contract Sequence)	-0.032***	-0.071***	-0.006***	-0.008***
	(0.003)	(0.010)	(0.001)	(0.001)
Antitrust Lawsuits × Log(Contract Sequence)	-0.028**	-0.065**	-0.007*	-0.005**
	(0.011)	(0.029)	(0.004)	(0.002)
Observations	35,575,254	35,575,254	35,453,772	30,151,310
R-squared	0.343	0.116	0.163	0.203
Product Market FE	Yes	Yes	Yes	Yes
Funding Agency × Year FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Average Outcome	1.189	0.200	0.029	0.052

Online Appendix

This Online Appendix provides supplementary material supporting the main findings of the paper "Antitrust Enforcement and Product Market Dynamics: Evidence from U.S. Government Procurement". It includes five components. Online Appendix Figures presents additional visual analyses, robustness checks, and graphical diagnostics referenced in the main text. Online Appendix Tables provides supplementary empirical results, variable definitions, robustness tests, and validation exercises. Online Appendix A details our methodology for constructing the antitrust enforcement dataset using large language models (LLMs).

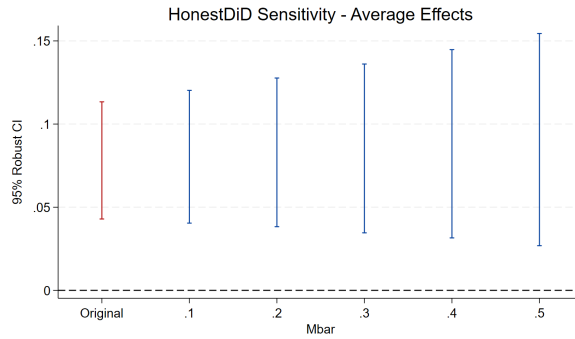
Figure A3: Parallel Trend Test

Figure A3 reports the results of the sensitivity test proposed by Rambachan and Roth (2023) to assess the robustness of our main results to potential violations of the parallel trends assumption. We adopt the average effects approach, which examines weighted averages of treatment effects across all post-treatment periods, providing a comprehensive assessment of aggregate impact with greater precision than period-specific estimates. We calculate 95% confidence intervals under different assumptions of the value M , the upper limit for the change between two consecutive periods in the slope of the underlying linear trend. A value of M equal to 0 on the x-axis corresponds with allowing for linear violations of parallel trends, while larger values of M allow for more significant deviations from linearity.



(a) Federal Obligation

(b) Log(Employment)



(c) Log(Sales)

Figure A4: Stock Market Reactions to Antitrust Lawsuits

Figure A4 presents an event study analysis examining stock market reactions around antitrust lawsuit filing dates. We analyze publicly traded firms operating under 6-digit NAICS codes affected by antitrust enforcement that were not defendants themselves. For each firm, we calculate buy-and-hold abnormal returns (BHAR) over a 21-day window surrounding the filing date, from 10 days before to 10 days after. Expected returns are estimated using the Fama-French four-factor plus momentum model over a 252-trading-day pre-event window. The BHAR captures the cumulative difference between realized and expected returns attributable to the antitrust event. The x-axis represents trading days relative to the filing date, where day 0 is the filing date itself. The y-axis shows cumulative BHAR in percentage terms. The solid line represents the average BHAR across all firms, with dashed lines indicating 90% confidence intervals.

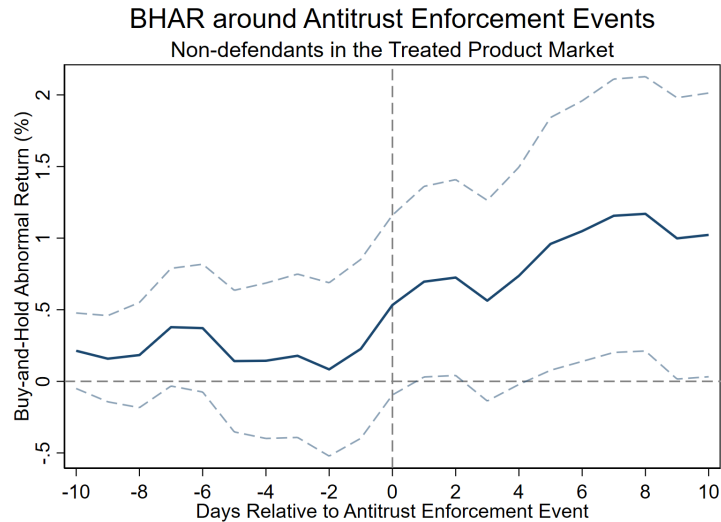
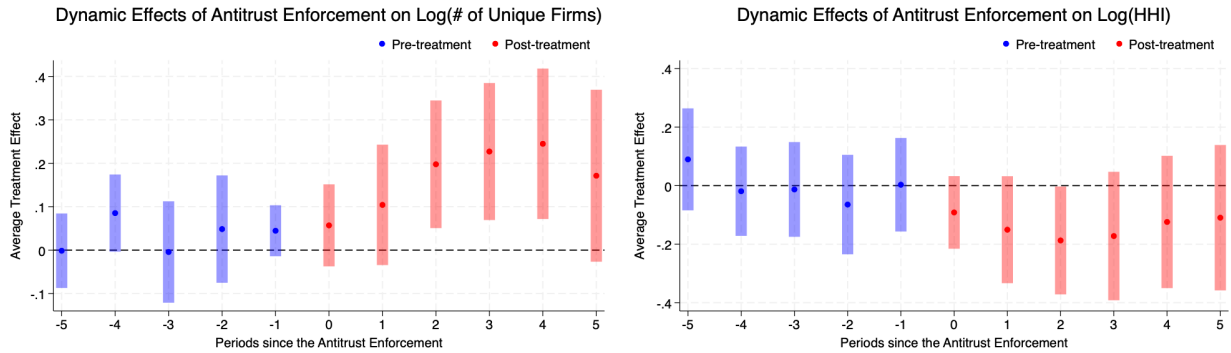


Figure A5: Antitrust Lawsuits and Industry-Level Outcomes: Dynamic Effects

Figure A5 displays the dynamic effects of antitrust lawsuits on industry-level outcomes to further test the validity of our difference-in-differences identification strategy. Using the estimator proposed by Callaway and Sant'Anna (2021), we address potential endogeneity concerns in staggered treatment settings that traditional difference-in-differences methods may not fully account for (Baker et al., 2022). We examine two key industry-level variables: the number of unique firms participating in the market and market concentration (HHI index). The x-axis represents time periods relative to the filing date of antitrust lawsuits (event time), with negative values indicating pre-treatment periods and positive values indicating post-treatment periods. The y-axis shows the average treatment effect on the treated (ATT) for each outcome variable. The shaded areas represent 95% confidence intervals.



(a) Log(# of Unique Firms)

(b) Log(HHI)

Online Appendix Tables

Table A1: **Variable Definition**

We report in this Appendix a detailed definition and description of all the variables that we use in our empirical analysis.

Variable	Definition	Source
Establishment level variables		
Antitrust Lawsuit	An indicator equal to one after the first antitrust lawsuit in a product market.	Wolters Kluwer's VitalLaw & the Department of Justice (DOJ) Antitrust Division
Government Contracts	The dollar amount (\$ thousand) of federal obligation awarded to an establishment in a given year.	USASpending
IHS Government Contracts	The inverse hyperbolic sine transformation of government contracts (federal obligations).	USASpending
Log(Employment)	The natural logarithm of the number of employees in a given establishment and year.	NETS
Log(Sales)	The natural logarithm of the dollar amount (\$ thousand) of sales in a given establishment and year.	NETS
PAYDEX	A measure of the financial health of the establishment. It reflects the establishment's payment behavior, typically based on its history of paying invoices on time. It is a numerical value that ranges between 0 and 100. A higher score indicates timely or early payments, while lower scores suggest delays, signaling potential financial distress or poor credit management.	NETS
Public	An indicator variable equal to one if the establishment belongs to a public corporation.	NETS
Small	An indicator variable equal to one if the establishment's initial (pre-treatment) employment is 10 or fewer, measured at the first observed year in the panel.	NETS
Women-Owned	An indicator variable equal to one if the establishment is women-owned.	NETS
Minority	An indicator variable equal to one if the establishment is minority owned.	NETS
Defendant Establishment	An indicator variable equal to one if the establishment belongs to a firm identified as a defendant in an antitrust lawsuit.	Wolters Kluwer's Vital Law & DOJ Antitrust Division

Table A1: **Continued**

Variable	Definition	Source
Industry level variables		
HHI	HHI index in a product market (6-digit NAICS code) and year. It measures the market concentration of government contractors in a product market. When multiple transactions are associated with a contract, we identify the main transaction as the first one in time.	USASpending
# Unique Firms	The number of government contractors that won a procurement contract in a product market (6-digit NAICS code) and year. When multiple transactions are associated with a contract, we identify the main transaction as the first one in time.	USASpending
Protest Rate	The total number of bid protests in a product market (6-digit NAICS code) and year, divided by the total number of government contractors. This information is available only for the period 2005 to 2016.	Canayaz et al. (2025)
Contract level variables		
IHS(Initial Value)	The inverse hyperbolic sine transformation of the initial contract value (federal action obligation) at the time of award.	USASpending
# Modifications	The total number of contract modifications for each contract, based on the number of transactions beyond the initial award.	USASpending
IHS(\$ Renegotiation)	The inverse hyperbolic sine transformation of the total dollar amount of contract modifications in thousands of dollars. When multiple modifications occur, this represents the sum of all modification values (both positive and negative).	USASpending
Cost Overrun Ratio	The proportional difference between final contract value and initial contract value, calculated as (final contract value – initial award value) / initial award value.	USASpending
Delivery Delay	An indicator variable equal to one if the actual contract completion date exceeds the originally scheduled completion date from the initial contract award.	USASpending
Competition	An indicator variable equal to one if the contract was awarded under a competitive bidding process. Based on competition classifications including full and open competition, competitive delivery orders, and follow-on competed actions.	USASpending
Fixed-Price	An indicator variable equal to one if the contract uses fixed-price pricing mechanisms. Includes contract pricing types A, B, J, K, L, and M.	USASpending

Table A1: **Continued**

Variable	Definition	Source
Cost-Plus	An indicator variable equal to one if the contract uses cost-plus pricing mechanisms. Includes contract pricing types R, S, T, U, and V.	USASpending
# Offers	The number of offers received for a given contract award.	USASpending
Contract Length (Days)	The planned duration of the contract in days, calculated as the difference between the planned completion date and the performance start date from the initial contract award.	USASpending
Goods Contract	An indicator variable equal to one if the contract is classified as a goods or supplies procurement, based on the Product Service Code (PSC) classification system.	USASpending
Inexperienced Contractor	An indicator variable equal to one if the contractor is executing one of its first three government contracts at the time of award.	USASpending
Contract Sequence	The cumulative number of government contracts awarded to a given contractor up to and including the current contract, used to measure within-firm learning and experience accumulation.	USASpending

Table A2: **Normalized Differences Between Treatment and Control Groups**

Table A2 compares observable characteristics between treatment and control groups at the beginning of the analyzed period to assess the validity of our identification strategy. Panel A reports industry-level characteristics, while Panel B reports establishment-level characteristics. Normalized differences (ND) are calculated following Imbens and Wooldridge (2009), with values within ± 0.25 indicating similar distributions between groups. As shown in Panel B, establishments in exposed and non-exposed product markets appear similar in observable characteristics, though the markets themselves (Panel A) show some differences in concentration and size. These market-level differences are accounted for by including fixed effects in our empirical specifications.

Panel A: Industry characteristics

	Treated		Untreated		ND
	Mean	SD	Mean	SD	
Log(HHI)	7.44	1.16	8.04	0.95	-0.57
Log(# firms)	3.54	1.72	2.48	1.37	0.68
Average Renegotiation	1.38	0.53	1.29	0.70	0.15
Competition	0.72	0.26	0.70	0.30	0.07
Average Contract Value	498,783.98	1,240,000	275,978.56	640,550.57	0.23
Share Fixed Price	0.83	0.26	0.90	0.20	-0.30

Panel B: Establishment characteristics

	Treated		Untreated		ND
	Mean	SD	Mean	SD	
Age	8.86	3.97	9.30	3.82	-0.11
Women Owned	0.21	0.41	0.18	0.39	0.08
Minority	0.17	0.38	0.10	0.30	0.21
Dummy Women	0.12	0.32	0.14	0.35	-0.06
Log(Employment)	2.59	1.56	2.64	1.68	-0.03
Log(Sales)	7.29	1.83	7.39	1.93	-0.05
Log(Productivity)	4.70	0.86	4.74	0.87	-0.05
PAYDEX	71.24	8.66	71.10	8.47	0.02
Federal obligation	149.20	652.97	73.70	445.76	0.14

Table A3: **Alternative Government Awards Measures**

Table A3 presents regression results using different specifications of the government contract award variable to test the robustness of our findings. We examine both level measures and inverse hyperbolic sine (IHS) transformations, as well as multi-year averaging approaches. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)	(3)
Variables	Baseline	Level	Level (3 years average)
Antitrust Lawsuit	0.160** (0.076)	390.514*** (107.910)	404.736*** (114.658)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	2,260,640	2,260,640	1,910,361
R-squared	0.554	0.639	0.731
Average Outcome	2.409	811.4	936.0

	(4)	(5)	(6)
Variables	IHS (3 years average)	Level (2 years average)	IHS (2 years average)
Antitrust Lawsuit	0.161** (0.073)	391.417*** (107.965)	0.171** (0.074)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	1,910,361	2,083,483	2,083,483
R-squared	0.675	0.692	0.620
Average Outcome	3.134	877.2	2.827

Table A4: **Propensity Score Matching**

Table A4 presents regression results using propensity score matching to address potential selection concerns in our difference-in-differences approach. We match treated and untreated product markets based on market size, concentration, and competitive structure using one-to-five nearest-neighbor matching. This approach ensures that both industries and establishments in our treatment and control groups are similar in terms of observable characteristics. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)
Variables	IHS Government Contracts	IHS Government Contracts
Antitrust Lawsuits	0.231** (0.099)	0.179** (0.078)
Establishment FE	Yes	Yes
Year FE	Yes	No
County-Year FE	No	Yes
Observations	773,210	763,309
R-squared	0.547	0.568

Table A5: **Controlling for Industry Trends**

Table A5 presents regression results with various controls for industry-specific and product market-specific trends to address potential confounding factors. We include time-variant product market characteristics, industry-by-year fixed effects, and product market-specific time trends. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)	(3)	(4)
Variables	IHS Government	IHS Government	IHS Government	IHS Government
Antitrust Lawsuit	0.149** (0.073)	0.118** (0.059)	0.170** (0.074)	0.170** (0.074)
Product Market Controls	Yes			
Industry \times Year		Yes		
Product Market Linear Trend			Yes	Yes
Product Market Quadratic Trend				Yes
Observations	1,967,417	2,260,640	2,260,640	2,260,640
R-squared	0.555	0.557	0.554	0.554
Average Outcome	2.470	2.409	2.409	2.409

Table A6: **Placebo Test: Non-Government Contractors**

Table A6 presents regression results from a placebo test using establishments that are not government contractors but are located in the same counties and operate in the same product markets as our treatment sample. We estimate our baseline specification with employment, sales, and financial health as outcomes. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)	(3)
Variables	Log(Employment)	Log(Sales)	PAYDEX
Antitrust Lawsuit	-0.009 (0.010)	0.005 (0.025)	-0.119 (0.356)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	250,828,465	250,828,465	58,486,588
R-squared	0.924	0.933	0.642

Table A7: **Triple Difference-in-Differences Analysis**

Table A7 presents results from a triple difference-in-differences framework that compares government contractors to non-government contractors operating in the same product markets and locations. This approach includes both product market-by-year and county-by-year fixed effects, allowing us to control for time-varying product market characteristics while isolating the differential effect of antitrust lawsuits on government contractors. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

Variables	(1) Log(Employment)	(2) Log(Sales)	(3) PAYDEX
Antitrust lawsuit \times Government Contractor	0.188*** (0.025)	0.309*** (0.033)	1.537*** (0.155)
Establishment FE	Yes	Yes	Yes
Product Market \times Year FE	Yes	Yes	Yes
County \times Year FE	Yes	Yes	Yes
Observations	253,096,045	253,096,045	60,061,701
R-squared	0.927	0.936	0.642
Average Outcome	0.890	12.023	71.895

Table A8: **Alternative Treatment Definition: Court Location**

Table A8 presents regression results using an alternative treatment definition based on the geographic location of antitrust enforcement. We define treatment at the product market-state-year level, where state corresponds to the location of the court filing. This approach captures local exposure to antitrust enforcement actions. Both columns show the inverse hyperbolic sine transformation of government contracts as the dependent variable. Column (1) includes establishment and year fixed effects, while Column (2) includes establishment and county-year fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

Variables	(1) IHS Government Contracts	(2) IHS Government Contracts
Antitrust Lawsuit	0.276*** (0.098)	0.273*** (0.089)
Establishment FE	Yes	Yes
Year FE	Yes	No
County \times Year FE	No	Yes
Observations	2,260,640	2,260,640
R-squared	0.542	0.554
Average Outcome	2.409	2.409

Table A9: **Propensity Score Matching: Contract-Level Analysis**

Table A9 presents contract-level regression results using propensity score matching to address potential selection concerns. We match treated and untreated product markets based on market size, concentration, and competitive structure using one-to-one in Panel A and one-to-three in Panel B nearest-neighbor matching algorithms. The dependent variables measure contract renegotiation frequency, renegotiated amounts using inverse hyperbolic sine transformation, cost overrun ratios, and delivery delays. Each observation is weighted by contract value. All specifications include product market, funding agency-by-year, and county-by-year fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: One-to-One Matching				
	(1)	(2)	(3)	(4)
Variables	# Modifications	IHS(\$ Renegotiation)	Cost Overrun Ratio	Delivery Delay
Antitrust Lawsuit	0.088* (0.046)	0.035 (0.045)	0.011 (0.010)	0.017* (0.010)
Product Market FE	Yes	Yes	Yes	Yes
Funding Agency \times Year FE	Yes	Yes	Yes	Yes
County \times Year FE	Yes	Yes	Yes	Yes
Observations	7,685,224	7,685,224	7,636,793	6,292,980
R-squared	0.313	0.166	0.154	0.183
Average Outcome	1.388	0.255	0.0617	0.107

Panel B: One-to-Three Matching				
	(5)	(6)	(7)	(8)
Variables	# Modifications	IHS(\$ Renegotiation)	Cost Overrun Ratio	Delivery Delay
Antitrust Lawsuit	0.107*** (0.041)	0.062 (0.042)	0.016* (0.009)	0.024** (0.009)
Product Market FE	Yes	Yes	Yes	Yes
Funding Agency \times Year FE	Yes	Yes	Yes	Yes
County \times Year FE	Yes	Yes	Yes	Yes
Observations	9,700,542	9,700,542	9,640,529	8,231,531
R-squared	0.298	0.157	0.147	0.172
Average Outcome	1.370	0.231	0.0577	0.0990

Table A10: **Alternative Treatment Definition: Contract-Level Analysis**

Table A10 presents contract-level regression results using an alternative treatment definition based on the geographic location of antitrust enforcement. We define treatment at the product market-state-year level, where state corresponds to the location of the court filing. Column (1) shows the number of contract renegotiations, Column (2) presents the inverse hyperbolic sine transformation of dollar amount of contract renegotiations, Column (3) reports the cost overrun ratio, and Column (4) examines delivery delays. Each observation is weighted by contract value to reflect economic significance. All specifications include product market, funding agency-by-year, and county-by-year fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

	(1)	(2)	(3)	(4)
Variables	# Modifications	IHS(\$ Renegotiation)	Cost Overrun Ratio	Delivery Delay
Antitrust Lawsuit	0.112*** (0.041)	0.088** (0.042)	0.019** (0.009)	0.028*** (0.011)
Product Market FE	Yes	Yes	Yes	Yes
Funding Agency \times Year FE	Yes	Yes	Yes	Yes
County \times Year FE	Yes	Yes	Yes	Yes
Observations	35,750,584	35,750,584	35,627,138	30,314,861
R-squared	0.327	0.159	0.154	0.194
Average Outcome	1.190	0.108	0.0296	0.0517

Table A11: **Effects of Antitrust Enforcement on Contract Characteristics**

Table A11 examines whether antitrust enforcement affects the characteristics of government procurement contracts. Column (1) shows the inverse hyperbolic sine transformation of the initial contract value (federal action obligation), Column (2) presents an indicator for cost-plus contracts (pricing codes R, S, T, U, V), Column (3) reports an indicator for competitive contracts (full and open competition, competitive delivery orders), Column (4) examines an indicator for fixed-price contracts (pricing codes A, B, J, K, L, M), and Column (5) shows the number of offers received. All specifications include product market, funding agency-by-year, and county-by-year fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the product market level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of the variables are provided in Table A1 in the Appendix.

Variables	(1) IHS(Initial Value)	(2) Cost-Plus	(3) Competition	(4) Fixed-Price	(5) # Offers
Antitrust lawsuit	0.038 (0.075)	-0.013* (0.007)	-0.029 (0.021)	-0.004 (0.013)	-2.105 (1.455)
Product market FE	Yes	Yes	Yes	Yes	Yes
Funding Agency \times Year FE	Yes	Yes	Yes	Yes	Yes
County \times Year FE	Yes	Yes	Yes	Yes	Yes
Observations	35,575,254	35,575,254	35,575,254	35,575,254	21,321,666
R-squared	0.504	0.334	0.422	0.376	0.713
Average Outcome	7.892	0.007	0.832	0.973	6.713

Online Appendix A: Antitrust Data and LLMs

Antitrust Data

In this appendix, we provide the details on the construction of our comprehensive database of Department of Justice antitrust lawsuits. Our data collection relies on case summaries provided by Wolters Kluwer’s Vital Law¹⁸ (formerly known as Commerce Clearing House (CCH) Trade Regulation Reporter), which serves as the authoritative source for legal professionals and scholars in antitrust law. Our data source is similar to that used in Babina et al. (2023), as we access the same CCH Trade Regulation Reporter database through Vital Law, which is Wolters Kluwer’s enhanced digital platform for delivering this content.¹⁹

The Department of Justice (DOJ) antitrust case summaries available through VitalLaw provide detailed information covering several key dimensions of each enforcement action: (1) legal identifiers, including case numbers, case names, filing dates, and docket information; (2) case details, describing alleged violations, legal proceedings, and final outcomes; (3) temporal information about when violations began and ended; and (4) market information identifying affected industries and firms. Unlike the DOJ’s official website, which primarily contains recent cases²⁰, VitalLaw maintains comprehensive coverage across all periods. Furthermore, the summaries provide standardized information in a consistent format, facilitating systematic data collection.

For collection of these case summaries, we develop and implement an automated data collection procedure using Python and Selenium WebDriver.²¹ To ensure data quality and consistency, our automated process implements multiple verification steps, including duplicate detection, automated cross-validation of key fields, and systematic logging of all extracted information.

¹⁸Wolters Kluwer rebranded its legal research platform, Cheetah, as VitalLaw on November 1, 2021. This transition expanded the platform’s content and introduced new features to enhance legal research capabilities, including improved search functionality and digital accessibility. The CCH Trade Regulation Reporter, a key resource for antitrust case data, was integrated into VitalLaw during this rebranding, ensuring continued access to its comprehensive content while adding modern digital research tools.

¹⁹The Commerce Clearing House (CCH) Trade Regulation Reporter used by Babina et al. (2023) continues to be maintained and updated as part of VitalLaw following Wolters Kluwer’s acquisition of CCH. The integration of the Trade Regulation Reporter into VitalLaw represents a technological advancement in content delivery while maintaining the underlying data source, enabling more efficient data collection while ensuring continuity and consistency with previous research using this database.

²⁰As we show in Figure OA1, replicating one of the results from (Babina et al., 2023), the DOJ website’s coverage is particularly limited before the mid-1990s, with significant gaps in historical case documentation.

²¹Our automated collection process ensures consistency and efficiency while maintaining data quality. The code systematically accesses and downloads case summaries from VitalLaw’s DOJ Antitrust Division Case Summaries database, with built-in verification steps and logging mechanisms to prevent duplication and ensure completeness.

More specifically, we employ a two-stage approach combining structural extraction and machine learning techniques. In the first stage, we develop Python-based automated extraction procedures to systematically collect and clean key identifying information from the case summaries, including filing dates, case names, and citation numbers. Our data extraction algorithm incorporates specialized error correction for common typographical errors and standardizes date formats across all entries. Case name extraction employs a sophisticated regular expression system that handles multiple formats and sources, combining information from both file names and document headlines while accounting for various legal naming conventions and potential inconsistencies.

Additional Details of Using LLMs (ChatGPT)

Methodological Framework for Implementing LLMs

The extraction of detailed information from antitrust case summaries presents unique challenges that limit the effectiveness of traditional rule-based extraction methods or manual collection. While basic information like dates and case numbers can be reliably extracted using regular expressions and pattern matching, many crucial aspects of antitrust cases require sophisticated contextual understanding and legal expertise. Legal documents often contain complex syntax, specialized terminology, and implicit references that create significant cognitive burden even for trained researchers, leading to inconsistent interpretations and potential oversight of critical details during manual review. For instance, distinguishing between different types of government contracts and accurately classifying federal procurement activities requires understanding both explicit and implicit references in the legal text, as well as the ability to interpret the broader context of the case.

In the second stage of our data collection process, we employ a large language model (LLMs) approach using OpenAI’s GPT-4 Omni (gpt-4o) to extract detailed case information that requires deep contextual understanding and legal expertise. Given an input legal text X , our goal is to produce a structured output Y comprising 24 key fields—such as merger and acquisition indicators, government contract involvement, defendant identities, geographic scope, legal codes, outcomes, and various date fields related to case proceedings. Formally, we define:

$$Y = \{y_1, y_2, \dots, y_{24}\},$$

and model the conditional probability of these outputs given X as:

$$P(Y | X) = \prod_{i=1}^{24} P(y_i | X, y_1, \dots, y_{i-1}).$$

Our extraction task is then formulated as selecting:

$$Y^* = \arg \max_Y P(Y | X),$$

which represents our best estimate of the case’s structured information.

To enhance accuracy in legal texts, we implement a multi-role analysis system and setting temperature hyperparameter to 0 for all roles for deterministic outputs. For each role, the extraction process is executed R times per field (with $R = 3$ for most fields and $R = 4$ for key fields such as government procurement-related information), yielding a total of $N = 3R$ independent outputs for each field. Denote the outputs for field y_i as $y_i^{(j,k)} : j \in \{1, 2, 3\}, k \in \{1, \dots, R\}$. We then determine the final accepted value y_i^* using a consensus criterion:

$$y_i^* = \text{mode} \left(\{y_i^{(j,k)}\} \right) \quad \text{if} \quad \frac{f(\text{mode})}{N} \geq \frac{2}{3},$$

where $f(\text{mode})$ is the frequency of the most common output. If this condition is not met, the field is flagged for manual review.

This consensus-based methodology efficiently replics the work of 9 to 12 independent human research assistants—a technique theoretically anticipated to exceed the accuracy of individual human extractions, as shown in works including Babina et al. While useful for extracting simple data (e.g., dates and case numbers), traditional rule-based approaches usually fail to capture complicated contextual signals such implicit references to controlling interests, joint ventures, and corporate restructuring language (e.g., Hart-Scott-Rodino Act references), which are critical for identifying M&A-related cases.

Furthermore, we utilize a robust JSON schema to define our output structure, and we employ a JSON structural formatting prompt (as detailed in He et al. (2024)) to optimize LLMs performance. This multi-run, consensus-based approach ensures that our final dataset reliably reflects the true substance of the legal documents while mitigating the occasional ”hallucinations” or omissions inherent in LLMs outputs.

Prompt Design

Our prompt design is tailored to extract precise information from legal documents by leveraging role-based expertise. The prompts are structured to maximize the contextual understanding capabilities of the large language model while minimizing hallucinations or factual inconsistencies.

The core system prompt varies based on the expert role:

```
# Economic Professor Role
```

```
"You are an economic professor specialized in antitrust enforcement."  
# Data Scientist Role  
"You are a data scientist specialized in extracting structured data from case summaries."  
# Legal Specialist Role  
"You are a professional lawyer specialized in antitrust lawsuits."
```

For each case analysis, we provide a consistent user message structure:

```
# User message with case identification  
User: "Analyze the case '[CASE_NAME]' and provide structured information as outlined."  
# Case text follows  
User: "[FULL_CASE_TEXT]"
```

The model is configured to return responses in a JSON format that strictly adheres to our predefined schema. This ensures consistency and facilitates subsequent automated analysis.

Example Prompt Implementation

Below is a complete example of how a prompt is constructed and sent to the API for the economic professor role as an example. The prompt consists of a system message establishing expertise, a user message identifying the analysis task, and the case text from regulatory documents: (only showing the first few sentences of the full case document):

```
# System message  
System: "You are an economic professor specialized in antitrust enforcement."  
# User message with case identification  
User: "Analyze the case 'United States v. MCC Construction Corp.' and provide structured information as outlined."  
# Case text follows  
User: "MCC Construction Corporation, of Greenwood Village, Colorado, was charged on January 5, 2016, in a one- count information filed in the federal district court in Washington, D.C. with conspiring to defraud the U.S. government. According to the charge, MCC partnered with other companies to gain access to GOVERNMENT CONTRACTS that were awarded through the Small Business Administration's 8(a) program for businesses controlled by a socially or economically disadvantaged U.S. citizens, even though MCC was not eligible. The Justice Department announced [on February 2, 2016] that MCC Construction Company (MCC) has agreed to pay $1,769,294 in criminal penalties and forfeiture for conspiring to commit fraud on the United States by illegally obtaining government contracts that were intended for small, disadvantaged businesses. The court agreement was announced [on February 2, 2016] by Assistant Attorney General William J. Baer of the Justice Department's Antitrust Division, U.S. Attorney Channing D. Phillips of the District of Columbia, Assistant Director in Charge Paul M. Abbate of the FBI's Washington Field Office, Inspector General Peggy E. Gustafson of the Small Business Administration (SBA), Inspector General Carol Fortine Ochoa of the U.S. General Services Administration (GSA), Special Agent in Charge Brian J. Reihms of the Defense Criminal
```

Investigative Service's (DCIS) Central Field Office and Director Frank Robey of the U.S. Army Criminal Investigation Command's Major Procurement Fraud Unit (MPFU). [...]"

This classification task of government procurement related cases is crucial for our empirical analysis. Based on our structured schema requirements, the model returns a JSON response with the following key determinations:

```
{
  "Government_Contract_Indicator": "Yes",
  "Government_Contract_Keywords": "federal contracts, small, disadvantaged businesses, U.
    S. Department of Justice, [...]",
  "Federal_Procurement_Activities": "Yes",
  "Federal_Contract_Keywords": "federal contracts set aside for small, disadvantaged
    businesses, [...]",
  "Filing_Date": "2016-01-01"
}
```

We implement a comprehensive JSON schema that defines all relevant fields. This schema provides explicit guidance to the model regarding the expected output format and classification criteria. The schema definition below shows key fields related to government procurement identification, though our full implementation includes additional fields for comprehensive case analysis:

```
schema = {
  "name": "AntitrustCaseSummary",
  "schema": {
    "type": "object",
    "properties": {
      # Government Procurement Classification
      "Government_Contract_Indicator": {"type": "string", "enum": ["Yes", "No", "Unclear"]},
      "Government_Contract_Keywords": {"type": "string"},
      "Federal_Procurement_Activities": {"type": "string", "enum": ["Yes", "No", "Unclear"]},
      "Federal_Contract_Keywords": {"type": "string"},
      # Case Identification and Classification
      "Filing_Date": {"type": "string"},
      "M_A_Indicator": {"type": "string", "enum": ["Yes", "No", "Unsure"]},
      "Geographic_scope": {"type": "string", "enum": ["City", "State", "Several States", "National", "
        International", "No Information"]},
      # Industry Classification
      "NAICS4": {"type": "string"},
      "NAICS6": {"type": "string"},
      # Defendants Information
      "Defendants_Individual": {"type": "string"},
      "Defendants_Company": {"type": "string"},
      "Seller_state": {"type": "string"},
      "Product_state": {"type": "string"},
      # Legal Classification
      "Legal_code": {"type": "string", "enum": ["Sherman Act", "Clayton Act", "Robinson-Patman Act", "
        Hart-Scott-Rodino Act", "Other", "No Information"]},
```

```

"Legal_outcome": {"type": "string", "enum": ["Pleaded Guilty", "Nolo Contendere", "Dismissed", "Dropped", "Enjoined", "Plea Agreement", "Found Guilty", "Found Not Guilty", "Consent Decree", "Other", "No Information"]},
"Types_of_violations": {"type": "string"},
# Penalties and Appeals
"Fine_imposed": {"type": "string"},
"Jail_sentence_imposed": {"type": "string"},
"Probation_sentence_imposed": {"type": "string"},
"District_court_appeal": {"type": "string", "enum": ["Yes", "No"]},
"Appellate_court_appeal": {"type": "string", "enum": ["Yes", "No"]},
"Supreme_court_appeal": {"type": "string", "enum": ["Yes", "No"]},
# Timeline Information
>Date_of_Plea": {"type": "string"},
>Date_of_Sentencing": {"type": "string"},
>Date_of_beginning_of_conspiracy": {"type": "string"},
>Date_of_beginning_of_violation": {"type": "string"}
[Additional fields omitted for brevity] ]}]

```

This schema-guided approach ensures that the output is both comprehensive and consistent across all analyzed cases. For each antitrust lawsuit, we collect a complete structured output that contains all relevant fields. Below is an example of the full JSON output:

```

{
  "Government_Contract_Indicator": "Yes",
  "Government_Contract_Keywords": "federal contracts, Small Business Administration's 8(a) program, government contracts, disadvantaged businesses",
  "Federal_Procurement_Activities": "Yes",
  "Federal_Contract_Keywords": "federal contracts set aside for small, disadvantaged businesses",
  "Filing_Date": "2016-01-05",
  "Seller_state": "CO",
  "Product_state": "DC",
  "Defendant_Location": "CO",
  "NAICS4": "2362",
  "NAICS6": "236220",
  "M_A_Indicator": "No",
  "Defendants_Individual": "No Information",
  "Defendants_Company": "MCC Construction Corporation",
  "Geographic_scope": "National",
  "Legal_code": "Other",
  "Legal_outcome": "Plea Agreement",
  "Types_of_violations": "Government Fraud;Bid Rigging",
  "Fine_imposed": "$500,000;$1,269,294",
  "Jail_sentence_imposed": "No Information",
  "Probation_sentence_imposed": "No Information",
  "District_court_appeal": "No",
  "Appellate_court_appeal": "No",
  "Supreme_court_appeal": "No",
  "Date_of_Plea": "2016-02-02",
  "Date_of_Sentencing": "2016-03-15",
  "Date_of_beginning_of_conspiracy": "2008-01-01",
  "Date_of_beginning_of_violation": "2008-01-01",
  [Additional fields omitted for brevity]
}

```

This methodological rigor is especially important for accurately identifying the subset of antitrust cases that involve government procurement activities—a critical distinction for our research focus. Additionally, to ensure maximum accuracy, all LLMs outputs were independently validated by two human research assistants with professional expertise in antitrust law, using primary sources including Wolters Kluwer’s Vital Law database, the Department of Justice’s official website, and contemporaneous news reports. This dual validation process provides further confidence in the reliability of our final dataset.

Antitrust Violation Types

Additionally, we classify the antitrust violations into the following types by utilizing information from the DOJ Antitrust Division Manual and Vital Law’s legal summaries. Our classification system reflects the established taxonomic framework employed by the Department of Justice, capturing both statutory authority and enforcement priorities.²² For each antitrust violation classification, we restricted our large language model to select only from this predefined dictionary of violation types, applying a consensus-based methodology requiring agreement across multiple runs. All classifications were subsequently verified by human researchers with expertise in antitrust law, providing an additional validation layer to ensure classification accuracy.

```

schema = {
  "name": "Antitrust_Violation_Classification",
  "schema": {
    "properties": {
      "Broad_Category": {
        "enum": ["Horizontal Restraints", "Monopolization", "Vertical Restraints", "Merger Violations", "Unknown"]
      },
      "Specific_Violation": {
        "enum": ["Price Fixing", "Bid Rigging", "Market Allocation", "Group Boycott", "Information Exchange", "Monopolization", "Attempted Monopolization", "Predatory Pricing", "Exclusive Dealing", "Refusal to Deal", "Tying Arrangement", "Resale Price Maintenance", "Territorial Restriction", "Merger to Monopoly", "Horizontal Merger", "Vertical Merger", "Conglomerate Merger", "HSR Violation", "Other", "Unknown"]
      },
      "Collusion_Tools": {
        "enum": ["Bribery", "Wire Fraud", "Mail Fraud", "Government Fraud", "Tax Evasion", "Money Laundering", "Obstruction of Justice", "None", "Unknown"]
      }
    },
    "required": ["Broad_Category", "Specific_Violation", "Collusion_Tools"]
  }
}

```

²²This classification follows the DOJ Antitrust Division Manual (5th edition), which structures enforcement actions along these primary categories. See U.S. Department of Justice, Antitrust Division Manual (5th ed.), available at <https://www.justice.gov/atr/division-manual>.

Horizontal restraints involve agreements between competitors at the same level of production that directly restrict competition, typically prosecuted as per se violations under Section 1 of the Sherman Act and representing the DOJ's highest criminal enforcement priority. Monopolization captures conduct by a dominant firm that maintains or acquires monopoly power through exclusionary practices, prosecuted under Section 2 of the Sherman Act and requiring both market power and anticompetitive conduct elements. Vertical Restraints encompass restrictions imposed by firms at different levels of the supply chain that may unreasonably restrain trade, typically evaluated under the rule of reason and balancing procompetitive efficiencies against anticompetitive effects.²³ Merger Violations include both substantive challenges to transactions that may substantially lessen competition and procedural violations of Hart-Scott-Rodino Act premerger notification requirements, with the latter constituting a distinct category of enforcement activity.²⁴ Furthermore, we classify the collusion mechanisms, capturing the tools used to facilitate anticompetitive conduct.

²³This categorization follows both DOJ enforcement practice and Supreme Court precedent, which has increasingly recognized the potential pro-competitive benefits of vertical arrangements.

²⁴This distinction between substantive and procedural merger violations reflects the DOJ's operational approach, where HSR violations are pursued independently of competitive effect determinations.

Figure OA1: Temporal Distribution of DOJ Antitrust Lawsuits

Figure OA1 shows the yearly count of Department of Justice antitrust enforcement actions from 1970 to 2023. The stacked bar chart displays two categories: cases matched to the DOJ's official website (light blue) and the total number of antitrust lawsuits (full bar height, including gray portions). This visualization illustrates the historical variation in enforcement activity across different presidential administrations and economic periods. Data was collected from both the DOJ Antitrust Case Filings database and VitalLaw case summaries, with case matching performed between these two sources to identify overlapping records.

