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

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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 due to reduced entry barriers and the temporary exclusion of defendants. However, enforcement also impairs procurement performance in affected markets, with adverse effects concentrated in complex projects. Our findings provide new insights into how antitrust actions influence market structure and performance outcomes.

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

JEL Codes: G38, L41, H57

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1 Introduction

An increase in corporate market concentration has been documented in the economics and finance literature (e.g., Van Reenen, 2018; Autor et al., 2020; Barkai, 2020). 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, reduce prices, and boost output, less is known about how enforcement reshapes product-market share and how resulting shifts in market power affect firm- and product-level outcomes. Existing studies focus primarily on aggregate product-market dynamics (e.g., Reed et al., 2022; Babina et al., 2023), leaving open the question of how competition evolves after enforcement and how these changes affect corporate performance, market participants, and efficiency.

To systematically evaluate how antitrust enforcement affects competition dynamics, we examine Department of Justice (DOJ) antitrust cases involving government procurement, a setting that is both empirically rich and policy-relevant. Government procurement accounts for 10–15% of GDP, but operates in a distinctly low-competitive environment. These markets face high entry barriers, such as specialized knowledge requirements, large capital needs, and complex bureaucratic processes, all of which limit contestability. As a result, procurement markets are especially prone to supplier collusion (Gallo et al., 1994; Goldman & Zeume, 2023; Fazio & Zaldokas, 2024), with roughly 35% of DOJ antitrust lawsuits involving government procurement.[2] More importantly, the availability of high-quality data on government procurement activities allows us to precisely identify these markets and their participants, and to evaluate the quality of procurement outcomes.

[1]While 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).

[2]Further evidence of the importance for the government of ensuring fair competition in procurement markets is the creation in 2019 of the Procurement Collusion Strike Force (PCSF), an inter-agency partnership led by the U.S. Department of Justice's Antitrust Division. The agency aims to protect taxpayer dollars from being misused due to collusion between companies seeking government contracts. To date, this agency has trained over 38,000 agents and procurement officials, opened more than 140 investigations, and recovered more than $65 million in fines and restitution.

To 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 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 exploit the timing of the filing of antitrust lawsuits and apply 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 10% increase in government contracts awarded to an establishment in our sample following the antitrust investigation. We also examine the impact of antitrust lawsuits on traditional measures of business outcomes. We find that these contractors experience a substantial increase in the number of employees and total sales. The evidence suggests that establishments expand their operations following antitrust enforcement actions, hiring additional workers, and generating higher revenue streams. Importantly, in contrast to Cestone et al. (2021), we do not find evidence that antitrust enforcement adversely affects their financial health.

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. A day-by-day analysis of stock returns confirms that the timing of these lawsuits is unanticipated, as the market does not appear to price in the associated decline in expected cash flows. We further reinforce the credibility of our empirical analysis through several complementary exercises. First, 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. Second, we further strengthen identification by implementing additional 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 by constructing 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.

Unlike previous studies focusing on aggregate industry outcomes, our establishment-level analysis allows us to investigate how enforcement actions affect corporations. In contrast to the hypothesis that antitrust enforcement can negatively impact corporations due to reduced profit margins, increased regulatory uncertainty, and reputational concerns (e.g., Bittlingmayer, 1993; Bittlingmayer & Hazlett, 2000; Besley et al., 2021; Cestone et al., 2021), our results demonstrate, from multiple perspectives and using alternative identification strategies, that non-defendant government contractors in affected product markets significantly expand and benefit from antitrust lawsuits.

The gains we document for non-defendant contractors are closely linked to competition dynamics and the institutional features of federal procurement markets. In particular, antitrust lawsuits often lead to defendants being temporarily barred from bidding on or receiving government contracts (Karpoff et al., 1999). Consistent with this mechanism, we provide evidence that defendant exclusion is a channel through which enforcement operates. However, our findings indicate that its consequences extend well beyond the short-term debarment effect. Removing entrenched incumbents effectively lowers entry barriers, enabling new participants, and prompting a structural reallocation of market opportunities. These dynamics help explain why the positive effects for non-defendants persist even after the typical three-year debarment period.

Additional results provide further insights into the heterogeneous effects of antitrust lawsuits on corporations. More specifically, we show that antitrust lawsuits particularly benefit non-defendant contractors in large procurement markets, where market-share reallocation is expected to be more significant. We also find that this effect is especially strong for small, women-, and minority-owned businesses, consistent with antitrust enforcement lowering structural entry barriers and expanding participation for firms that have historically faced limited financial resources, weaker reputational capital, and restricted access to procurement networks.

Using procurement data, we examine the implications of antitrust lawsuits for government procurement performance. From a conceptual perspective, enforcement can affect economies of scale and disrupt existing efficiencies in product markets by removing dominant incumbents, while new firms may lack the capacity, fixed investments, and specialized expertise required for complex projects. The lawsuits can also impose compliance burdens and destabilize long-standing supply relationships, both of which are critical to procurement outcomes (Baker et al., 2002; Kang & Miller, 2022). Furthermore, the relation between competition and procurement performance is itself highly nuanced: while the entrance of new firms can reduce managerial slack and, as a consequence, procurement costs (Copeland, 1934; Abbott, 1955; Matsa, 2011; Coviello & Mariniello, 2014), in more competitive settings, low-quality providers may underbid high-quality firms, potentially crowding out superior suppliers (Akerlof, 1970; Laffont & Tirole, 1993).

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)). While we do not find differences in the characteristics of awarded contracts, we do find that procurement performance deteriorates following antitrust lawsuits. We also document that these negative effects are driven by highly complex projects or those where quality assessment is difficult, while they are not present for simple contracts and standard delivery of goods. This evidence suggests the possibility of a trade-off. Although antitrust enforcement increases market participation and significantly benefits non-defendant corporations, the decline in procurement performance we document indicates that the benefits of these legal actions may not be reflected in better products, especially in complex product markets.

2 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. These effects, primarily driven by financial gains and managerial expectations, vary across industries, and dissipate after collusion ends. 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 complements these investigations, which have yielded contrasting conclusions on the impact of antitrust enforcement on corporate outcomes, by systematically examining how antitrust enforcement shapes product market dynamics. To do so, we focus on DOJ antitrust lawsuits related to government procurement activities in the U.S. As we show in the paper, a significant fraction of antitrust lawsuits (around 35%) are related to government procurement. More importantly, from an empirical perspective, this setting offers the advantage that both the affected markets and their participants can be clearly identified. More specifically, the use of granular-level data allows us to provide novel insights into the heterogeneous effects of these legal actions across corporations and product markets.

Our research shows that businesses in affected product markets significantly benefit from antitrust lawsuits, particularly small, women-, and minority-owned firms. These results are explained by increased market entry and the temporary removal of defendant businesses from competition. However, we document a deterioration in procurement performance for complex contracts following antitrust lawsuits. This suggests that in sectors with substantial knowledge and capital barriers, such lawsuits may lead to inefficiencies by disrupting incumbent supplier relationships, complicating contract management, and reducing operational efficiency.

Other papers in the finance literature debate the use and effectiveness of antitrust enforcement. For example, 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. In a similar vein, 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.

Finally, other studies investigate the impact of competition policies on corporate and industry performance by leveraging regulation changes across different countries. Dasgupta and Žaldokas (2019) use country-level variation across amnesty programs to measure the effects of equilibrium changes in antitrust policy on investment and financing decisions. They find that firms step up investment and increase equity issuance as the equilibrium switches from collusion to oligopolistic competition. As a result, debt ratios fall. Similarly, Faccio and Zingales (2022) provides evidence that rules promoting competition are associated with a lower concentration and lower prices. They find no evidence that pro-competition rules are associated with worse quality, lower investments, less employment, or lower wages. Besley et al. (2021) use a country-level antitrust enforcement index to show that firms operate with lower profit margins in countries with solid antitrust policies. Levine et al. (2021) document that stronger antitrust laws increase corporate valuations, especially for firms with greater agency problems, by mitigating managerial opportunism and enhancing governance.

3 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.

VitalLaw serves as our primary source, providing detailed case summaries authored by legal experts specializing in antitrust law. 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.

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 a substantial degree of 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.

Identification of government procurement cases. We deploy OpenAI's GPT-4 Omni (gpt-4o) via a systematic API implementation 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 through conventional manual coding or basic keyword-search approaches.

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.[3] 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 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 lawsuits. Following Reed et al. (2022), we define specific product markets using the corresponding 6-digit North American Industry Classification System (NAICS)

[3]Figure 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.

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,[4] 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 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 collusive behavior among suppliers. 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]

We show in Figure 4 the share of different settlement types and appeal statuses in our final sample. The majority (92.04%) of cases were resolved through plea agreements, followed by consent decrees (6.57%). Dismissed cases represent a smaller proportion (1.38%). Regarding appeal cases, most cases (95.56%) were not appealed, while 4.44% were appealed. These proportions are consistent with the numbers we observe for the whole sample of DOJ antitrust lawsuits.

[4] More information at the following website: https://www.justice.gov/atr/antitrust-case-filings-alpha

[**Insert Figure 4 about here**]

In Online Appendices A and B, we provide detailed information on the LLM-based approach and present five examples of antitrust lawsuits related to government procurement activities.

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 the sales and employees. We also use the PAYDEX score, a business credit score issued by Dun & Bradstreet, to evaluate the impact of antitrust lawsuits on establishments' financial health and riskiness. This measure objectively assesses financial health by reflecting their 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 have been able to classify 174 establishments as defendants in our final sample based on the extracted information from our antitrust lawsuit data.[5] 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).

[5] This 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.

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 collapse this information at the establishment and year level to obtain the total dollar amount of government contracts awarded to an establishment.[6] 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 (135,348 establishments), as indicated in the NETS database. After merging antitrust lawsuit data with establishment information and excluding establishments belonging to defendant firms, our final dataset consists of 1,678,543 establishment-year observations spanning the period from 2001 to 2021. We report the summary statistics for this sample in Table 1. Government contractors in our sample have an average of approximately 72 employees and report average annual revenues of $16 million. On average, they receive $820 thousand in government obligations. Their average PAYDEX score is 72, out of a maximum possible value of 100, and 13% of the establishments operate in product markets affected by antitrust lawsuits.

[**Insert Table 1 about here**]

4 Antitrust lawsuits and corporations

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:

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

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 char-

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

acteristics that we investigate. More specifically, we consider product market size, concentration (using the HHI index), and the number of participants.[7] 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 a more significant number of competitors and greater overall size. More specifically, a 1% increase in HHI reduces the likelihood of being treated by 0.005 percentage points, a 1% increase in the number of firms increases it by 0.003 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.[8] 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 related to government activities affect non-defendant government contractors operating in the product market impacted by the lawsuits. In particular, antitrust lawsuits are expected to reduce barriers to market entry in affected product markets and lead to a reallocation of sales across establishments.

To explore this hypothesis in greater depth, we collect information on government procurement contracts. We aggregate the dollar amount of each transaction at the establishment-year level and merge this information with the NETS database using the DUNS number identifier available in 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):

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

[7] We 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.

[8] While it is well known that illicit conducts are significantly more frequent in environments with weak institutions, the theoretical effect of firm competition on the benefits that firms derive from illegal conducts 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.[9] 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 10% in federal obligations after an antitrust lawsuit concerning the average transformed outcome variable. Given that the average non-transformed government obligation is 820.66 thousand dollars, this translates to an increase of approximately 215–250 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 how these events affect the 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 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 3.9% in employment after an antitrust lawsuit concerning the average transformed outcome variable. On the other hand, considering the results reported in Column (2), we find an increase of 0.6% in sales after an antitrust lawsuit concerning the average transformed outcome variable. These results provide evidence that antitrust lawsuits benefit both corporations and employees in the affected product markets. More specifically, employment increases by

[9] We 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.

5.8 additional employees per establishment, while sales increase by \$1.75 million per establishment.[10] 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 Payday score.

[Insert Table 4 about here]

The parallel trends assumption. 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 is used to indicate 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 regarding observable characteristics. However, as already shown in our previous results reported in 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

[10] Note that the higher magnitudes in terms of establishment sales, compared to revenues attributed solely from government contracts, can be explained by the fact that: i. a large proportion of sales in the NETS database tends to be estimated (Barnatchez et al., 2017); and ii. government contracts include only federal government contracts, and the data before 2006 is incomplete, leading to measurement errors that can potentially bias the coefficients toward zero. However, it is also possible that higher government contracts allow establishments to expand into other product markets as well.

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 5 provides evidence that our identified effects are long-lasting. There is a clear increase in employment, sales, and the amount of federal obligations awarded after the event; on the other hand, we do not find any effect on the PAYDEX score, which is consistent with our baseline results. It is also important to note that the coefficients before the event are close to zero. However, the F-test on the joint statistical significance of the coefficients before the event rejects the hypothesis that the coefficients are jointly equal to zero, except for the coefficients related to the PAYDEX score.

[**Insert Figure 5 about here**]

Roth (2022) argues that coefficients close to zero but statistically significant might suggest that, while the difference in trends is small, the model is detecting consistent, systematic deviations from parallel trends due to a large sample size or low variability. In particular, if the pre-treatment coefficients are statistically significant but their magnitude is close to zero, it could reflect a practical, negligible deviation from parallel trends, particularly if the effect size is economically or substantively insignificant. Therefore, the significance of the coefficients alone cannot definitively confirm or refute the validity of the assumption of parallel trends. Instead, it should be evaluated in combination with additional sensitivity analyses.

To further investigate this potential issue, we implement the sensitivity test proposed by Rambachan and Roth (2023) to evaluate the robustness of our results to potential violations of the parallel trends assumption. This approach provides formal bounds on treatment effects under progressively relaxed constraints on the counterfactual trend's behavior. More specifically, our analysis follows two complementary approaches: the relative magnitudes and the average effects methods.[11] Using these two alternative approaches, we calculate the 95% confidence intervals for our main estimators 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

[11] The "relative magnitudes" method examines the sensitivity of period-specific point estimates to violations of parallel trends, testing how robust individual treatment effects are across different time periods. The "average effects" method examines weighted averages of treatment effects throughout all post-treatment periods, which provides a more holistic assessment of overall impact while potentially improving precision.

violations of parallel trends. Larger values of M allow for more significant deviations from linearity.

As shown in Figure A3, our treatment effect estimates for employment and sales remain statistically significant across increasingly relaxed constraints on trend violations in both approaches. The confidence intervals widen as M increases, but the estimated effects remain positive and significant even at M equal to 0.5. For government awards, while the confidence intervals are relatively wider, particularly in the period-specific analysis, the point estimates consistently remain positive across specifications, and the average effect shows greater stability. The stronger robustness in the average effects analysis suggests that our estimates of aggregate impact are less sensitive to potential non-linearities than period-specific estimates. The consistency of these results, even under cautious assumptions about trend breaches, offers strong proof that our conclusions represent real treatment effects of antitrust enforcement rather than byproducts of parallel trend violations.

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 6, 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.

[**Insert Figure 6 about here**]

4.1 Additional results and robustness checks

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

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.[12] 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.

[12] We 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.

Spillover effects. Antitrust enforcement in one product market might affect related markets through competitive spillovers or resource reallocation. To investigate this possibility, we test whether establishments in adjacent markets, defined as operating in the same broader 4-digit NAICS sector but not in the specific 6-digit NAICS code exposed to antitrust lawsuits, experience indirect effects. We create an indicator variable, *Antitrust Lawsuit Spillover*, equal to one for establishments in these adjacent markets following an antitrust lawsuit (and zero otherwise). As shown in Table A6, we do not find any statistically significant spillover effects, suggesting that the benefits of antitrust enforcement are concentrated within directly affected product markets rather than spread more broadly. This finding strengthens our identification strategy by confirming the specificity of the treatment effect to exposed markets.

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 market as government contractors as a placebo group. Using this substantially larger sample of 185,463,304 establishment-year observations, we estimate our baseline specification with employment, sales, and financial health as outcomes.[13] As reported in Table A7, 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:

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

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 A8 presents these results. The coefficients on the triple interaction are positive and highly significant across all outcomes. Employment increases by 18.1% and sales by 30.7%

[13] It 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.

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 1.598-point improvement (a 2.2% increase relative to the mean) following an antitrust lawsuit.

Federal versus no-federal antitrust lawsuits. We use LLMs to extract information on whether antitrust lawsuits are related to federal or non-federal procurement activities. As reported in Panel (a) of Figure A4, we find that nearly 70% of our antitrust cases involve federal procurement. This is ideal because (1) consistent with prior literature, we rely on data from *USAspending*, which provides comprehensive information on federal procurement contracts, and (2) by focusing on federal procurement, we avoid the need to identify the geographic location of the misconduct, which can be difficult to determine, while federal procurement naturally constitutes national markets.[14]

In our baseline specification, we include all the cases for several reasons: (1) DOJ prosecutions create reputational effects that influence all government contracting opportunities; (2) the legal and compliance costs stemming from federal violations affect a firm's overall competitive position; and (3) although formal federal debarment rules do not automatically apply to state and local contracts, procurement officials at all levels often avoid contractors with recent federal violations. As a robustness check, we distinguish between antitrust lawsuits related to federal versus non-federal procurement. As we show in Panel (b) of Figure A4, we find that our main results are mainly driven by federal procurement antirust lawsuits.

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

[14] Unlike private markets, where geographic boundaries critically define competitive scope, federal agencies source from qualified suppliers nationwide through standardized procurement vehicles and regulations. The federal government acts as a unified buyer, using consistent specifications (FAR requirements, security clearances, technical standards) that create natural market boundaries independent of geography.

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 A9. The coefficient on antitrust lawsuits for government contracts is 0.246 and statistically significant, representing an 11.7% increase relative to the baseline average value of 2.406. 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.

5 Market-share redistribution

Our first set of results suggests that antitrust lawsuits halt anti-competitive practices and facilitate the redistribution of sales across establishments within the exposed product market. This section provides more direct evidence of these mechanisms.

Fraud, corruption, and debarment. To better understand how antitrust lawsuits affect corporations and product markets, we exploit the heterogeneity in their characteristics. More specifically, we identify antitrust lawsuits that encompass bribery, government fraud, mail fraud, wire fraud, tax evasion, money laundering, and obstruction of justice. We then estimate Equation (2) separately for this group of antitrust lawsuits and for the other group that is not linked to any of these misconducts. We report the two coefficients of interest in Figure 7.

We find that our results are driven by antitrust lawsuits involving fraud, corruption, or bribery, forms of misconduct that typically lead to mandatory exclusion from government contracting under federal procurement rules (Karpoff et al., 1999). While the FAR grants agencies discretion, such offenses are widely recognized as grounds for debarment and are treated as de facto mandatory in practice. In contrast, other anticompetitive practices, such as bid rigging or price fixing, while serious, often result in fines, settlements, or corrective measures without necessarily leading to exclusion.

[Insert Figure 7 about here]

The exclusion of defendants. To provide more direct evidence that defendant firms are excluded from the markets, we identified 174 unique establishments of defendant firms. 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:

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

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 5. More specifically, we find a statistically significant decrease of 35.34% 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 supporting evidence that the exclusion of defendants facilitates the redistribution of sales across other establishments within the product market.

[Insert Table 5 about here]

We also report the dynamic effects in Figure A5. Although the small number of defendant observations limits statistical power, the dynamics remain informative. A sharp decline in federal contract awards emerges starting in the second year after the lawsuit is filed and persists for approximately three years, a duration that aligns closely with the typical debarment period under federal procurement law (Karpoff et al., 1999). In contrast, the positive effects for non-defendants persist well beyond this period, suggesting that enforcement triggers longer-lasting reallocation dynamics rather than merely redistributing contracts during the exclusion window.

We also report the results from Equation (4), using the natural logarithm of employment and sales as well as the PAYDEX score as outcome variables. The results are presented in Table A10. We find a negative and statistically significant effect on employment, consistent with the exclusion of defendants from procurement markets. The coefficients for sales and financial health are negative and economically meaningful, but not statistically

[15]Notably, 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.

significant at conventional levels. This pattern indicates that while defendants suffer substantial losses in government procurement, many reallocate their activity to other segments. Taken together, these results show that antitrust enforcement has concentrated but temporary effects on defendants, whereas the benefits for non-defendants appear persistent and structurally transformative.

Product market size effects. The benefits of antitrust lawsuits are expected to be larger when the affected product market is more substantial. To test this hypothesis, we divide the sample based on the median value of the size of the procurement market, proxied by the total spending, and re-estimate Equation (2).

We report the results in Table 6. According to our hypothesis, we find that the coefficient is positive and statistically significant only for the sample of establishments operating in larger, exposed product markets. Given the substantial costs of antitrust enforcement for the government, our findings suggest that focusing investigations on firms operating in large product markets may be a more effective strategy to stimulate the economy.[16]

[Insert Table 6 about here]

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 7. 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 20% more than non-women-owned establishments ($160,000 additional per establishment),[17] while minority-owned businesses receive 117% more in government contracts compared to non-minority establishments, translating to approximately $953,000 in additional procurement per establishment beyond the baseline effect. Small

[16] Baker (2002) estimate that costs of antitrust enforcement, both for the government and private firms, are estimated at approximately $1–2 billion annually.

[17] Dollar magnitudes represent differential effects calculated from the interaction coefficients in Columns (5)-(8) of Table 7. Using our sample mean of $816,920 in annual government obligations per establishment, the women-owned interaction coefficient of 0.196 implies they receive approximately 20% more than non-women-owned establishments, translating to $816,920 $\times$ 0.196 = $160,000 in additional contracts beyond what non-women-owned establishments receive from antitrust enforcement.

establishments receive 42% more than large establishments ($345,000 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 7 about here]

6 Market participation and procurement performance

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).

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

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.[18] *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 8. 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 7% 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 A6 confirms these patterns dynamically. We observe significant increases in market participation and decreases in concentration following enforcement, with pre-treatment coefficients close to zero.

[18] We 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.

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 0.121 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 30.1% reduction relative to the sample average protest rate of 0.402%. However, despite this economically meaningful effect, it is not statistically significant at the conventional 90% level.

[**Insert Table 8 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 22,421,702 contracts. Using this database, we estimate the following Equation:

$$Procurement\ Performance_{c,i,j,t} = \eta_s + \theta_{tc} + \beta Antitrust\ Lawsuit_{j,t} + \epsilon_{c,i,j,t} \tag{6}$$

Procurement Performance are alternative proxies for procurement performance of a contract c awarded to an establishment i that win award the contract 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 after the first antitrust lawsuit in a specific product market. η_s and θ_{tc} are product market and county-year fixed effects. Finally, we weigh each contract by (the natural logarithm of its) respective contract value.

We report the results in Table 9. Interestingly, we find that procurement performance deteriorates following antitrust lawsuits. More specifically, following an antitrust lawsuit, the number of modifications increases by approximately 0.093 per contract, and the IHS-transformed renegotiation amount increases by 0.238.[19] Additionally, the cost overrun ratio increases by 1.6 percentage points (69.6% relative to the baseline of 0.023), and the probability of a delivery delay rises by 2.6 percentage points (48.1% relative to the baseline of 0.054).

[**Insert Table 9 about here**]

Figure 8 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, concentration, and competitive structure. As shown in Table A11, both one-to-one (Panel A) and one-to-five (Panel B) matching specifications confirm our main results. The number of modifications increases by 0.090-0.099 per contract, cost overrun ratios rise by 1.6-3.1 percentage points, and delivery delays increase by 2.2-2.5 percentage points. Second, using our alternative treatment definition based on court filing location and product market, Table A12 shows even stronger effects: modifications increase by 0.133 per contract, cost overruns rise by 3.1 percentage points, and delivery delays increase by 2.7 percentage points.

These consistent results across different specifications and identification strategies confirm

[19] For the IHS transformation, this coefficient represents approximately a 26.9% increase (exp(0.238) - 1) for contracts with positive baseline renegotiations. The effect varies by baseline level due to the nonlinear nature of the IHS transformation, which handles both zeros and positive values.

that the deterioration in procurement performance following antitrust enforcement is robust, potentially causal, and not driven by product market boundaries 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 10. The coefficient on Antitrust Lawsuits 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 improves the performance, reducing modifications by 0.223 per contract (18.8% decrease) and delivery delays by 5.3 percentage points (98.1% decrease relative to the baseline rate). However, procurement performance significantly worsens for longer-duration contracts, where sustained relationships and accumulated project-specific expertise are most critical. Each additional year of contract duration amplifies the negative effects by 0.54 additional modifications per contract, substantial increases in renegotiated amounts, 8.9 additional percentage points in cost overruns, and 10.6 additional percentage points in delivery delays.[20] This pattern 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.

We also consider an indicator variable equal to one for contracts awarded by the Department of Defense (DoD). These projects typically involve highly specialized technical requirements and extensive regulatory compliance. As shown in Panel B of Table 10, DoD contracts experience substantially amplified negative effects. The interaction effects alone represent increases of 33.3% in modifications, 348% in cost overruns, and 162% in delivery delays relative to baseline performance levels, with total effects reaching 40.3% increases in modifications and 404% increases in cost overruns for DoD contracts, further supporting the role of complexity in driving performance deterioration following antitrust enforcement.

Next, we perform a more granular analysis examining how procurement performance effects vary across Product Service Code (PSC) categories, which represent the federal government's standardized classification system for categorizing purchases by their fundamental nature and complexity. More specifically, as an alternative measure of contract complexity

[20]The marginal effect "per additional year" effect is calculated as $\beta_3 \times \ln(365) \approx \beta_3 \times 5.9$.

and difficulty in assessing quality, we identify Information Technology (IT) services.

Panel C of Table 10 provides evidence that antitrust enforcement significantly affects IT services contracts, which experience 27.7% increases in modifications, substantial increases in renegotiated amounts, 370% increases in cost overruns, and 107% increases in delivery delays, compared to 7.4%, moderate increases, 61%, and 46% respectively for non-IT contracts, with the interaction effects contributing an additional 20.3%, 309%, and 61% beyond the standard antitrust impact. The results imply the inherent difficulty in assessing quality and performance in highly technical, rapidly evolving IT services where specialized expertise and long-term vendor relationships are critical for successful project delivery.

Along the same line, Panel D shows that general services contracts exhibit moderate deterioration with 14.5% increases in modifications compared to 4.8% for non-services contracts. However, cost overrun and delay effects remain insignificant at the 90% conventional level, suggesting that professional and administrative services present intermediate complexity and quality assessment challenges.

Our results suggest that the performance of simple contracts actually benefits from antitrust lawsuits. Consistent with this hypothesis, standardized products with observable quality characteristics are expected to benefit from increased participation rather than suffer from relationship-specific disruptions. Panel E confirms this conjecture: antitrust enforcement improves performance for goods procurement, leading to 0.8% fewer modifications and 39% lower cost overruns, compared to 24.5% more modifications and 274% higher cost overruns for non-goods contracts. This evidence supports the view that standardized products with observable quality characteristics benefit from increased participation rather than suffer from relationship-specific disruptions.

[Insert Table 10 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, the contract duration, whether the contract is cost-plus, whether it was awarded through a competitive process, and the number of bids.

Table A13 shows that enforcement does not significantly affect initial contract values, the prevalence of cost-plus versus fixed-price contracts, or the level of competition in contract awards. The null results in Columns (1)-(3) show that the documented decline in procurement performance cannot be attributed to changes in contract structure, pricing, or competitive procedures. Interestingly, we also do not find in Column (4) 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 A13 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.

In Online Appendix C, we formalize these findings through a simple stylized conceptual framework in which projects vary in complexity, and firms differ in capacity. In our setting, low-complexity projects can be delivered by any firm once a collusive incumbent is removed. However, in high-complexity procurement, only large or experienced firms can match prior quality standards. When antitrust enforcement removes such firms, reallocation to lower-capacity entrants or overburdened incumbents leads to performance deterioration. The model predicts, and our data confirm, that enforcement improves market participation but not necessarily outcomes, especially in markets requiring significant know-how, capital, and organizational depth.

7 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. Defendant contractors, by contrast, experience sharp declines in government awards and market presence. At the product-market level, we observe an increase in the number of participants and a decline in concentration. However, on the other hand, we also document that procurement performance deteriorates, especially in contracts that require significant technical expertise or long-term project-specific capabilities.

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.

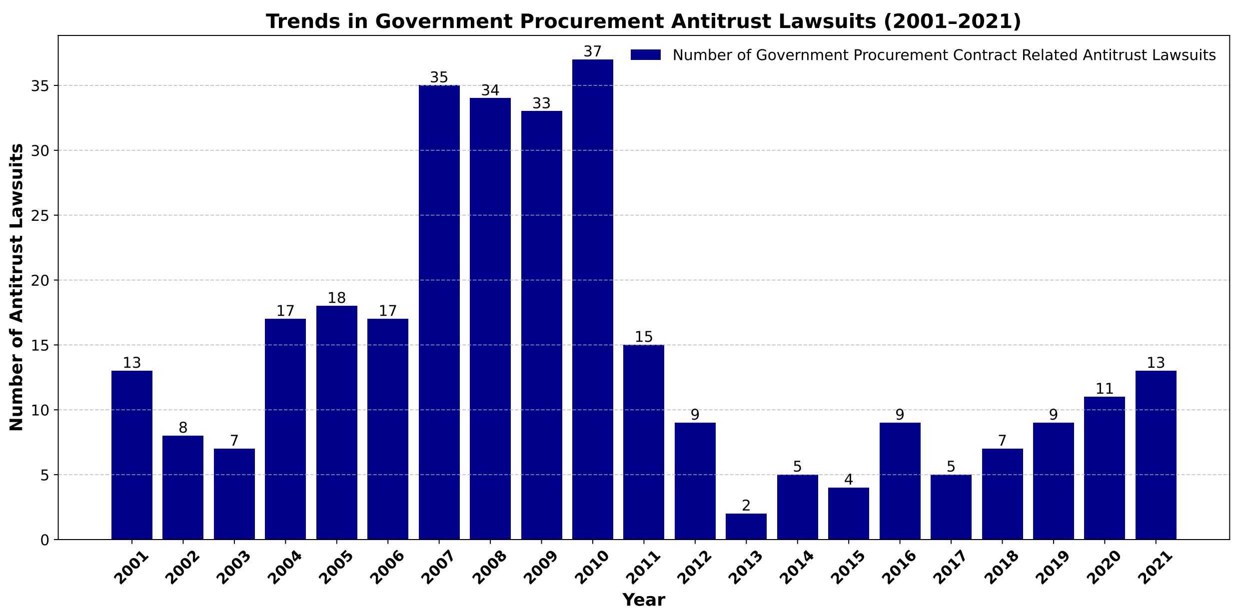


Figure 2: **Antitrust Lawsuits Related to Government Procurement Activities Across Sectors**

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.

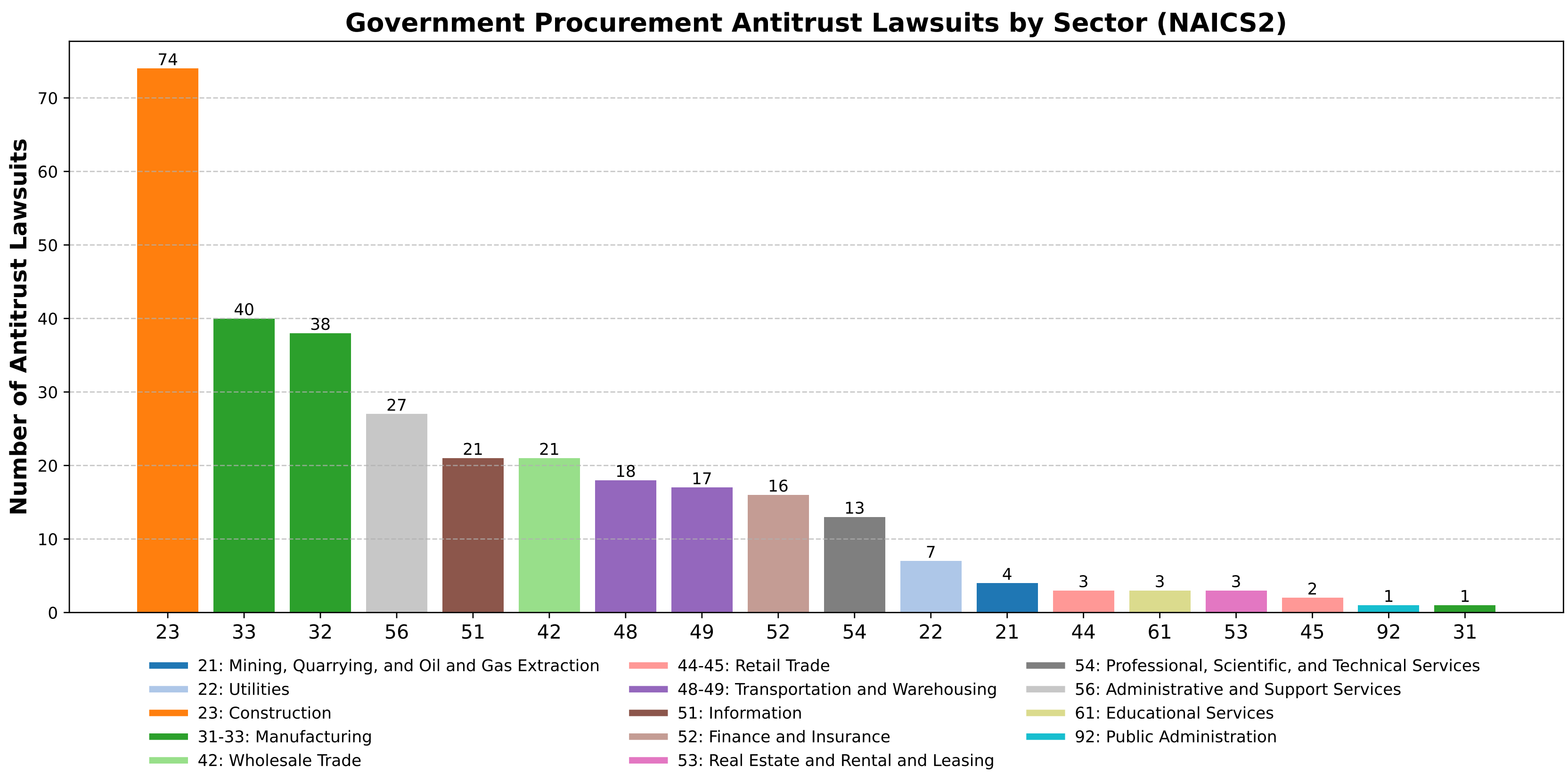


Figure 3: **Types of Antitrust Lawsuits Violation**

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.

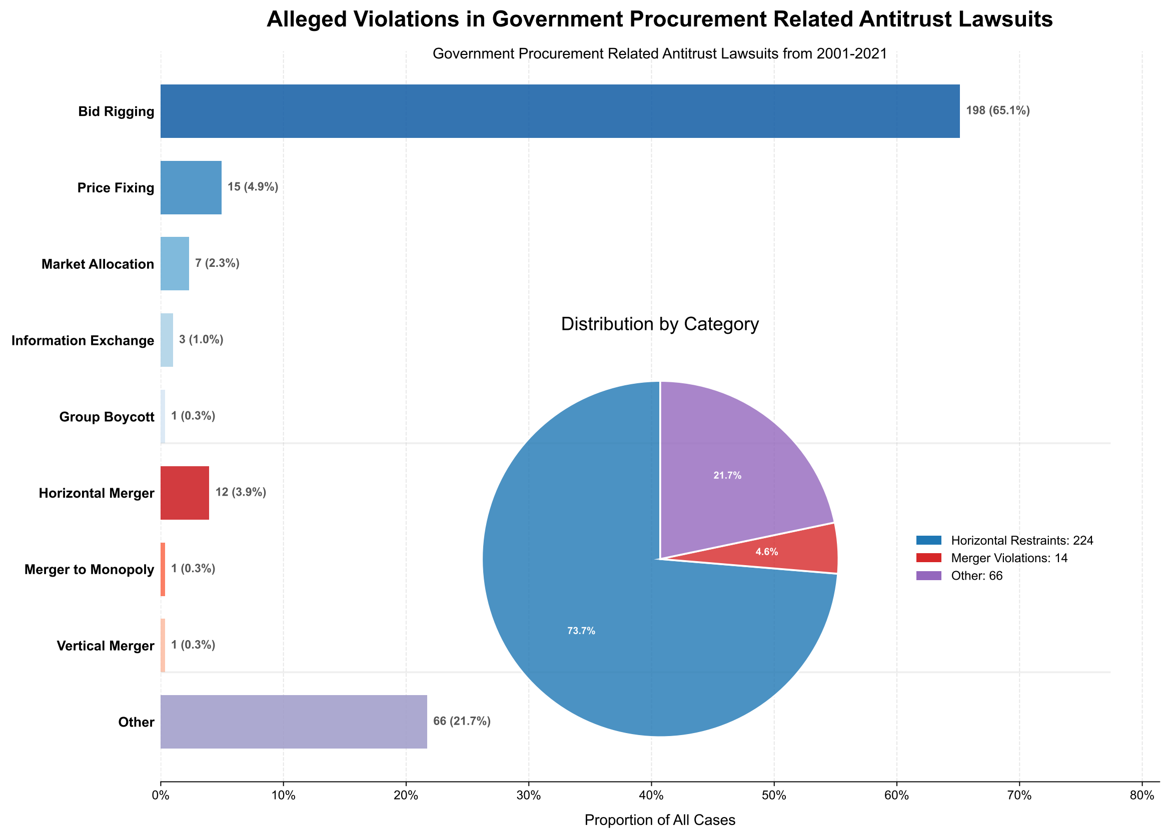
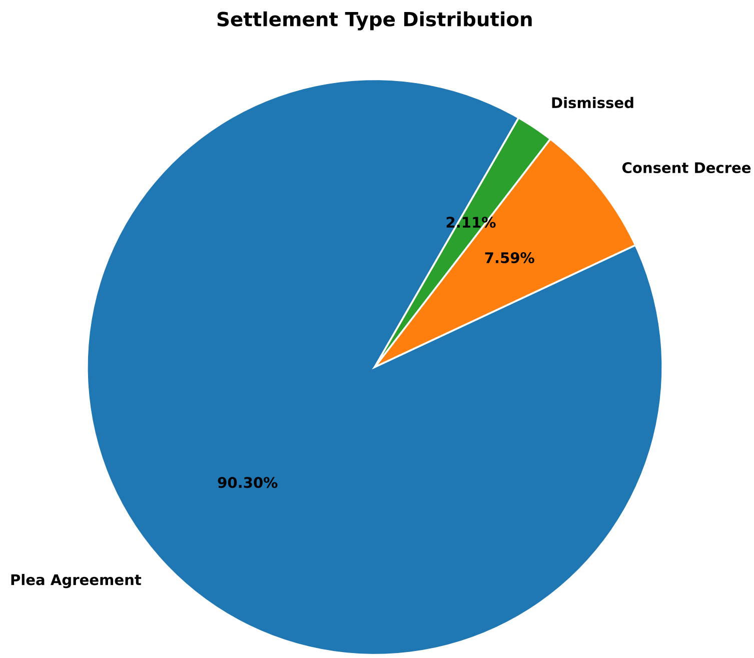
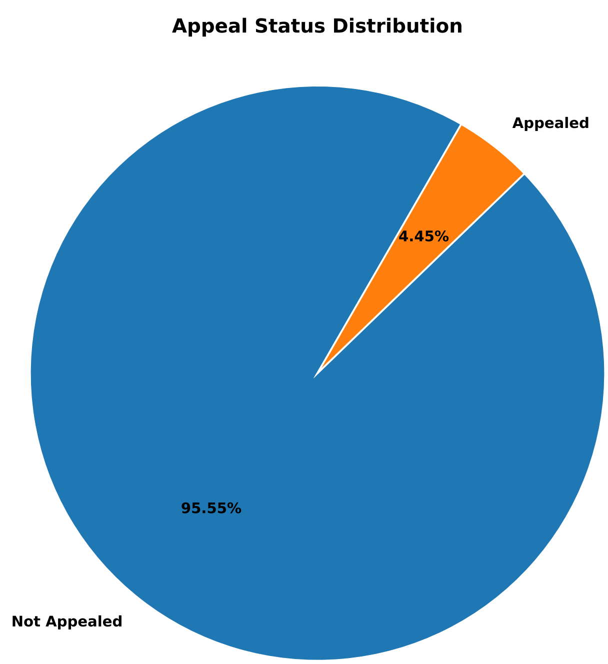


Figure 4: **Antitrust Lawsuits Legal Outcomes**

Figure 4 reports information on the relative share of different settlement types and appeal statuses in our final sample. VitalLaw case summaries serve as our source of information. We use LLMs to extract this information.



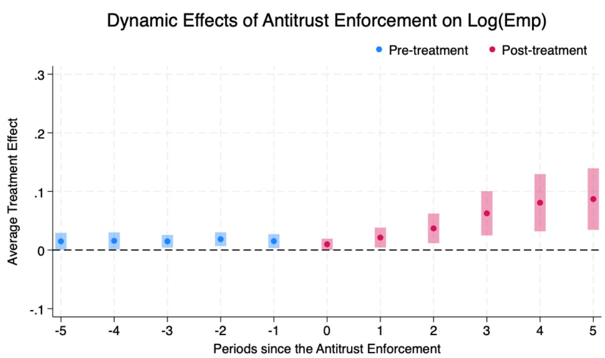
(a) Antitrust Lawsuits Settlement Type



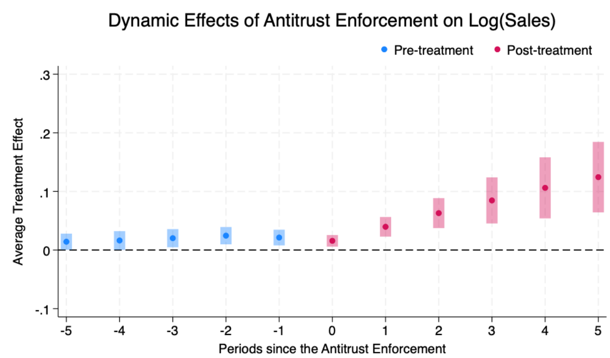
(b) Antitrust Lawsuits Appeal Status

Figure 5: **Antitrust Lawsuits and Non-Defendants Outcomes: Dynamics Effects**

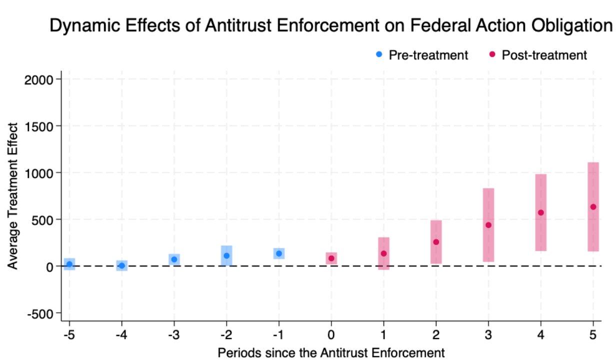
Figure 5 displays the dynamic effects of antitrust lawsuits on non-defendant establishments' outcomes to test the validity of our difference-in-differences identification strategy. Using the estimator proposed by Callaway and Sant'Anna (2021), we address potential endogeneity concerns in staggered treatment 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.



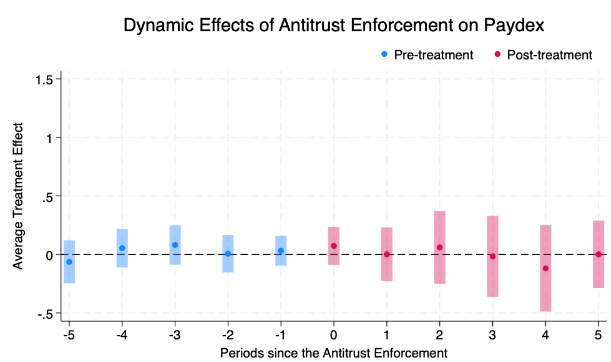
(a) Log(Employment)



(b) Log(Sales)



(c) Federal Government Awards



(d) PAYDEX

Figure 6: **Stock Market Reactions to Antitrust Lawsuits**

Figure 6 presents an event study analysis examining stock market reactions around antitrust lawsuit filing dates. We analyze a sample of 196 publicly traded firms that operate within 6-digit NAICS codes affected by antitrust enforcement but were not defendants themselves. For each firm, we calculate buy-and-hold abnormal returns (BHAR) over a 21-day window surrounding the filing date (10 days before to 10 days after). The BHAR methodology accounts for the difference between a firm's actual return and its expected return based on a market model estimated during a pre-event period, capturing the abnormal performance attributable to the antitrust event. The x-axis represents trading days relative to the filing date (day 0), while 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.

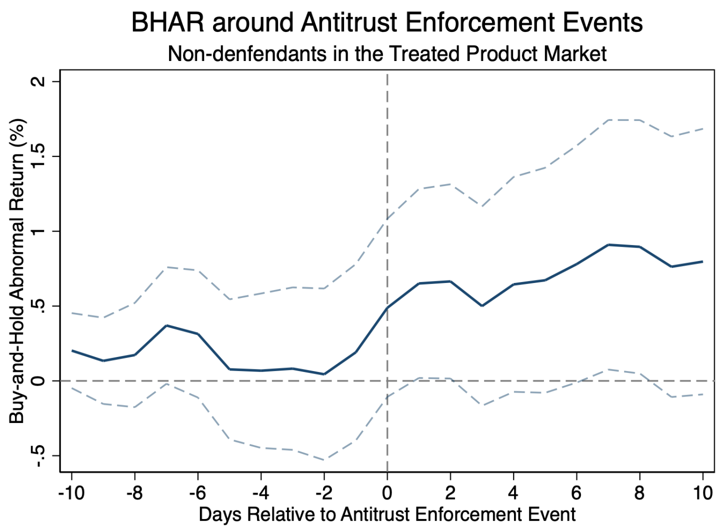
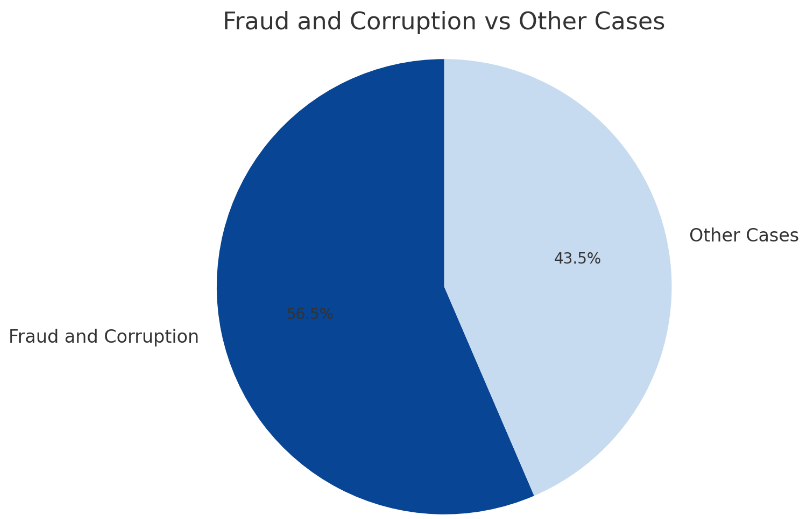
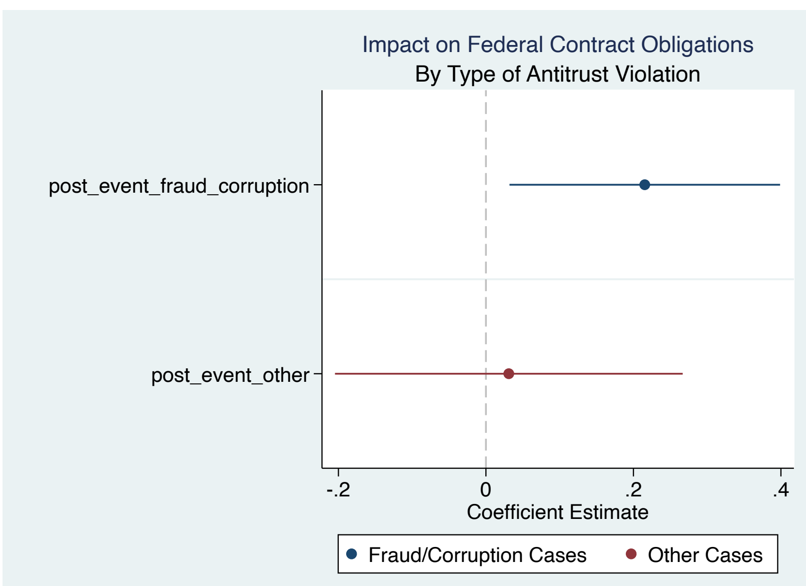


Figure 7: **Fraud and Corruption and Debarment**

Figure 7 examines how different categories of antitrust violations affect market access outcomes. We expect businesses in exposed product markets to benefit from antitrust lawsuits primarily when defendant businesses lose market share. Panel (a) categorizes violations into two broad areas: fraud/corruption (encompassing bribery, government fraud, mail fraud, wire fraud, tax evasion, money laundering, and obstruction of justice), and (b) other cases. Panel (b) presents coefficient estimates from Equation (2) for two categories of violations: *post_event_fraud_corruption* is a dummy variable equal to one for the period after an antitrust lawsuit involving fraud or corruption violations, and *post_event_other* is a dummy variable equal to one for the period after antitrust lawsuits involving other types of violations (primarily anti-competitive practices without fraud elements).



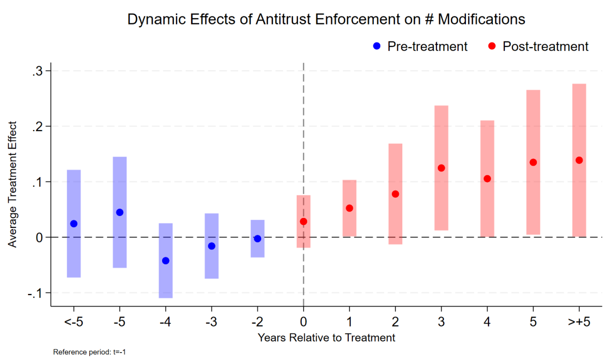
(a) Share of type of violations



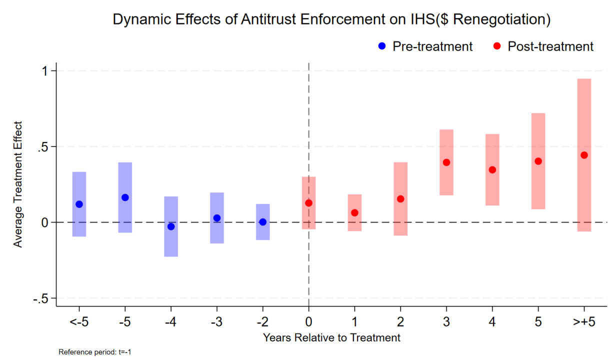
(b) Heterogeneous impact

Figure 8: **Antitrust Lawsuits and Procurement Performance: Dynamic Effects**

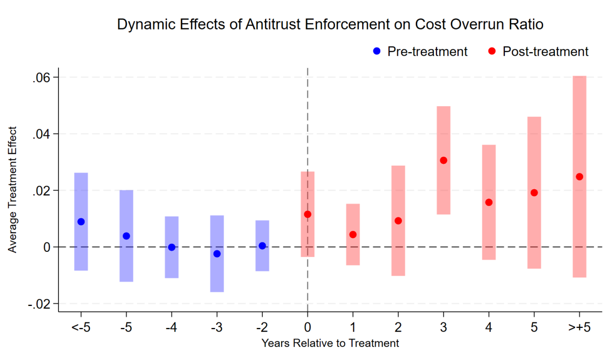
Figure 8 displays coefficient estimates from a dynamic difference-in-differences specification with two-way fixed effects (product market and year). We estimate the effects of antitrust enforcement on four key procurement performance metrics: the number of contract modifications, renegotiated amounts (inverse hyperbolic sine transformation), cost overrun ratio, and delivery delays. 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.



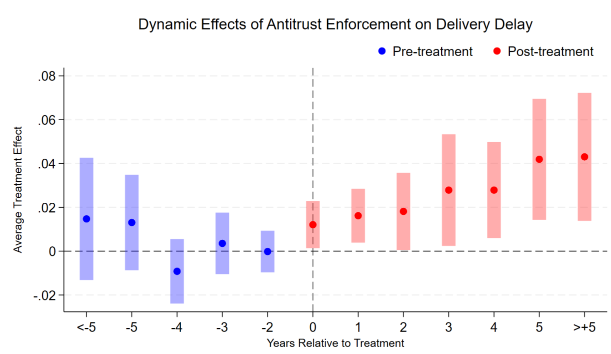
(a) # Modifications



(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 ($thousand)	1,678,543	866.221	4,000.002	0.000	67.325
IHS Government Obligation	1,678,543	2.422	3.277	0.000	4.903
Antitrust Lawsuit	1,678,543	0.136	0.343	0.000	0.000
Employment	1,678,543	70.888	372.100	3.000	40.000
Log(Employment)	1,678,543	2.477	1.714	1.099	3.689
Sales ($thousand)	1,678,543	15,812.024	135,943.546	262.500	6,000.020
Log(Sales)	1,678,543	7.215	2.111	5.570	8.700
PAYDEX	1,197,437	72.572	8.931	69.500	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***	-0.001
					(0.000)	(0.000)
Product Market FE	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes
Observations	23,636	23,618	23,636	23,618	23,636	23,618
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	0.260**	0.227**
	(0.109)	(0.092)
Establishment FE	Yes	Yes
Year FE	Yes	No
County-Year FE	No	Yes
Observations	1,678,543	1,678,543
R-squared	0.547	0.561

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	0.075***	0.103***	-0.090
	(0.024)	(0.024)	(0.142)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	1,678,543	1,678,543	1,189,104
R-squared	0.935	0.943	0.525
Average Outcome	2.477	7.215	72.57

Table 5: **Market Share Loss for Defendant Establishments**

Table 5 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	-0.949***	-0.964***
	(0.280)	(0.303)
Establishment FE	Yes	Yes
Year FE	Yes	No
County-Year FE	No	Yes
Observations	1,339,546	1,339,546
R-squared	0.535	0.551
Average Outcome	2.301	2.301

Table 6: **Antitrust Lawsuits Effects by Product Market Size**

Table 6 examines whether the effects of antitrust lawsuits vary with the size of the affected product market. The benefits of antitrust enforcement may be more significant when the exposed market is larger. We test this hypothesis by dividing our sample based on the median size of the procurement market, proxied by total government spending. Column (1) reports results for establishments in larger markets (above median size), while column (2) shows results for those in smaller markets (below median size). 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 (Size Above the Median)	IHS Government Contracts (Size Below the Median)
Antitrust lawsuit	0.313***	-0.016
	(0.103)	(0.069)
Establishment FE	Yes	Yes
County-Year FE	Yes	Yes
Observations	716,893	716,000
R-squared	0.582	0.588

Table 7: **Heterogeneous Effects of Antitrust Lawsuits by Establishment Characteristics**

Table 7 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	IHS Government Contracts							
Antitrust Lawsuit	0.286**	0.235**	0.088	0.117	0.257***	0.203**	0.071	0.089
	(0.112)	(0.106)	(0.094)	(0.104)	(0.095)	(0.090)	(0.084)	(0.088)
Antitrust Lawsuit × Public	-0.385**				-0.440***			
	(0.164)				(0.159)			
Antitrust Lawsuit × Women-Owned		0.193**				0.196***		
		(0.076)				(0.071)		
Antitrust Lawsuit × Minority-Owned			0.843***				0.773***	
			(0.149)				(0.119)	
Antitrust Lawsuit × Small				0.360***				0.352***
				(0.099)				(0.090)
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	1,678,543	1,678,543	1,678,543	1,678,543	1,678,543	1,678,543	1,678,543	1,678,543
Adjusted R-squared	0.513	0.513	0.513	0.513	0.513	0.513	0.513	0.513

Table 8: **Industry-Level Effects of Antitrust Enforcement**

Table 8 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***	-0.148**	-0.121
	(0.074)	(0.070)	(0.155)
Product market FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	23,610	23,610	14,078
R-squared	0.874	0.642	0.432
Average Outcome	3.816	7.616	0.402

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

Table 9 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. 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	0.093**	0.238**	0.016**	0.026**
	(0.042)	(0.104)	(0.008)	(0.010)
Product Market FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Observations	22,421,702	22,421,702	22,350,004	18,274,233
R-squared	0.329	0.110	0.153	0.193
Average Outcome	1.186	0.177	0.023	0.054

Table 10: **Effects of Antitrust Enforcement on Procurement Performance: Heterogeneous Effects**

Table 10 examines how antitrust enforcement affects procurement performance across different contract characteristics. Panel A analyzes heterogeneity by contract length. Panel B examines differential effects for Department of Defense contracts. Panels C-E investigate variations across Product Service Code (PSC) categories: Information Technology services, General Services, and Goods procurement. 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. 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
Panel A: Contract Length				
Antitrust Lawsuits	-0.223*	-0.585*	-0.038	-0.053*
	(0.116)	(0.333)	(0.025)	(0.027)
Log(Contract Length)	0.106***	0.136***	0.016***	0.015***
	(0.014)	(0.022)	(0.002)	(0.002)
Antitrust Lawsuits × Log(Contract Length)	0.091***	0.229***	0.015**	0.018***
	(0.027)	(0.088)	(0.007)	(0.006)
Observations	22,421,616	22,421,616	22,349,929	18,274,232
R-squared	0.355	0.115	0.162	0.199
Panel B: Department of Defense (DoD) Contract				
Antitrust Lawsuits	0.083**	0.084**	0.013*	0.023**
	(0.039)	(0.043)	(0.007)	(0.010)
DoD Contract	0.122	-0.034	-0.012	0.027
	(0.117)	(0.097)	(0.018)	(0.022)
Antitrust Lawsuits × DoD Contract	0.395***	0.404***	0.080***	0.088***
	(0.142)	(0.116)	(0.028)	(0.026)
Observations	22,421,702	22,421,702	22,350,004	18,274,233
R-squared	0.330	0.155	0.153	0.194
Panel C: Information Technology Services				
Antitrust Lawsuit	0.088**	0.087**	0.014*	0.025**
	(0.040)	(0.043)	(0.007)	(0.010)
IT Services Contract	0.406***	0.444***	0.112***	0.072***
	(0.056)	(0.054)	(0.015)	(0.015)
Antitrust Lawsuit × IT Services Contract	0.241***	0.371***	0.071***	0.033*
	(0.069)	(0.073)	(0.019)	(0.018)
Observations	22,421,702	22,421,702	22,350,004	18,274,233
R-squared	0.332	0.157	0.156	0.194
Panel D: General Services				
Antitrust Lawsuit	0.057	0.059	0.011	0.019*
	(0.044)	(0.045)	(0.009)	(0.011)
Services Contract	0.457***	0.337***	0.088***	0.097***
	(0.046)	(0.044)	(0.009)	(0.009)
Antitrust Lawsuit × Services Contract	0.115*	0.116**	0.014	0.019
	(0.062)	(0.052)	(0.013)	(0.011)
Observations	22,421,702	22,421,702	22,350,004	18,274,233
R-squared	0.336	0.157	0.156	0.197
Panel E: Goods Procurement				
Antitrust Lawsuit	0.290***	0.346***	0.063***	0.053***
	(0.080)	(0.087)	(0.016)	(0.017)
Goods Contract	-0.610***	-0.508***	-0.127***	-0.125***
	(0.044)	(0.040)	(0.008)	(0.008)
Antitrust Lawsuit × Goods Contract	-0.300***	-0.382***	-0.072***	-0.045***
	(0.069)	(0.081)	(0.016)	(0.013)
Observations	22,421,702	22,421,702	22,350,004	18,274,233
R-squared	0.345	0.163	0.163	0.201
Product Market FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Average Outcome	1.186	0.177	0.023	0.054

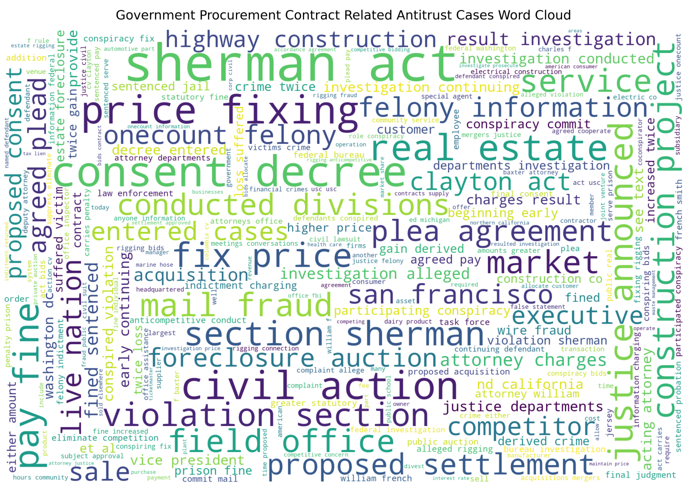
Online Appendix

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. OnlineAppendix 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). Online Appendix B presents selected case summaries that illustrate typical antitrust actions in our sample. Online Appendix C introduces a stylized theoretical framework that captures the trade-off between expanded participation and procurement performance following enforcement.

Online Appendix Figures

Figure A1: **Word Cloud of Government Procurement-Related Antitrust Cases**

Figure A1 presents textual analysis of government procurement-related antitrust lawsuits from 2001-2021, highlighting the frequency and prominence of specific terms in antitrust litigation. Term size corresponds to relative frequency in the corpus. Panel (a) displays significant terms from case documents after removing common legal terminology, stopwords, and generic business language. Panel (b) illustrates procurement-specific terminology identified through large language model analysis of the same corpus. These visualizations provide intuitive insights into the textual dimensions of our dataset and reveal prominent themes across government procurement-related antitrust cases during the study period.



(a) Word Clouds of Antitrust Cases

(b) Procurement-Specific Words

Figure A2: **Distribution of Antitrust Violations by Product Market**

Figure A2 shows the distribution of antitrust lawsuits related to government procurement activities across different product markets from 2001-2021. We define specific product markets using the 6-digit NAICS code classification system. When information about the product market is unavailable from the Antitrust Case Filings database, we utilize large language models (LLMs) to extract this information from case summaries. The figure displays both the broad industry sectors (2-digit NAICS codes) and the specific product markets (6-digit NAICS codes) within each sector affected by antitrust enforcement, revealing concentration patterns across the U.S. economy. VitalLaw case summaries serve as our primary source of information for identifying and classifying these violations.

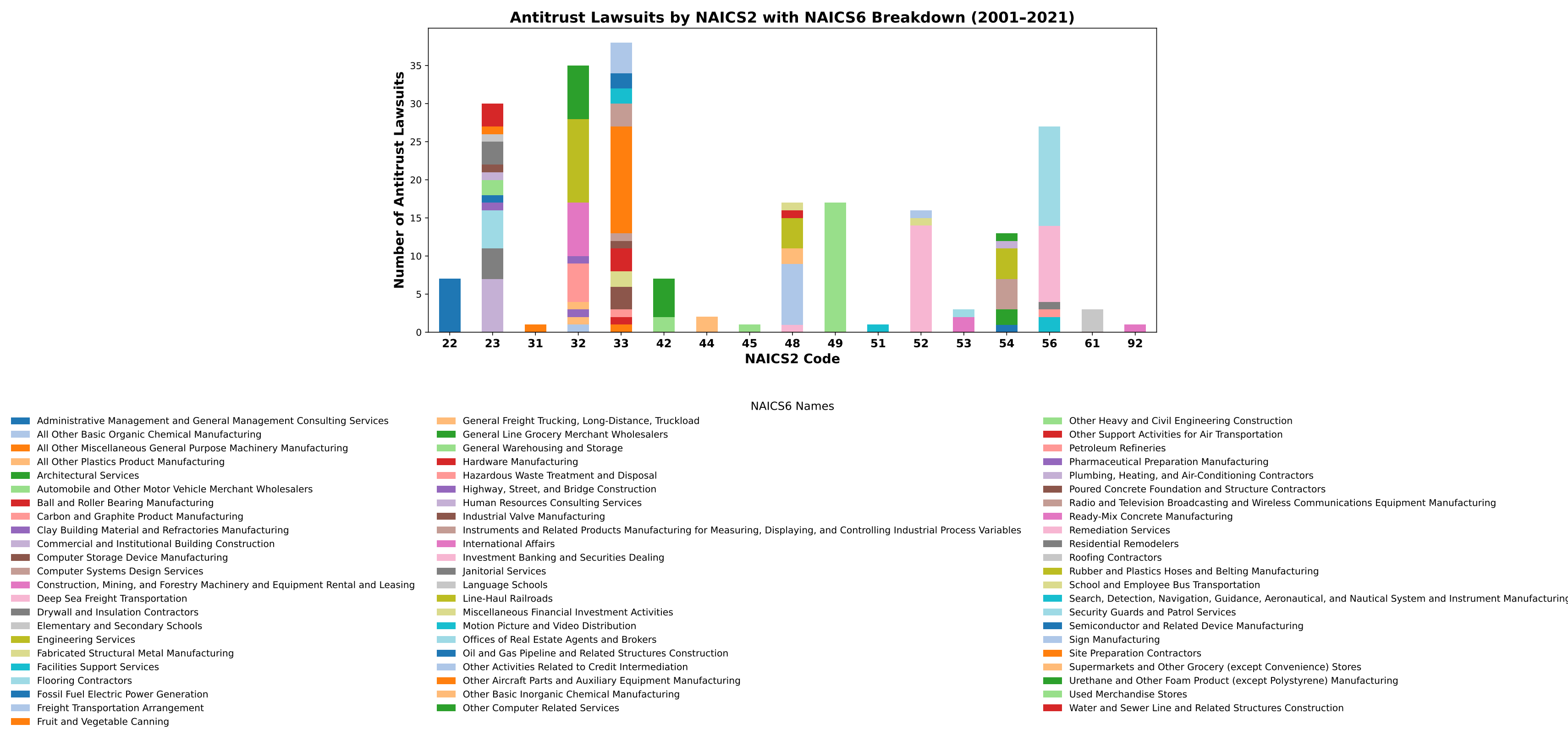
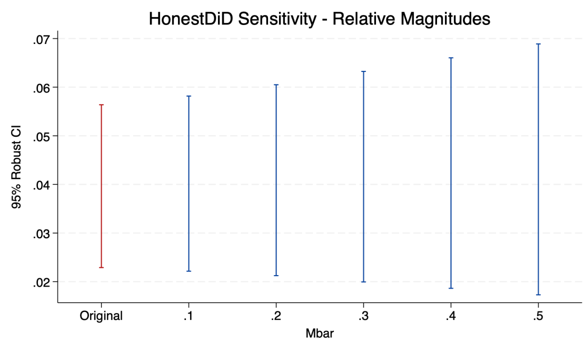
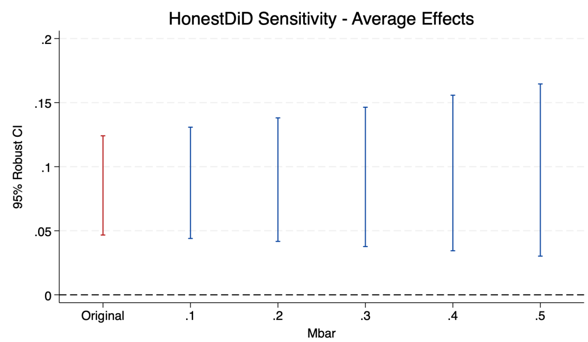


Figure A3: **Parallel Trend Test**

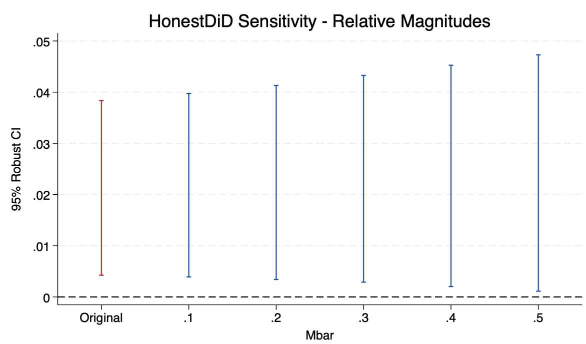
Figure A3 reports the results of the sensitivity test proposed by Rambachan and Roth (2023) to test whether the parallel trend assumption holds more formally. This procedure examines the extent to which our main results remain robust against potential nonlinearities with varying magnitudes in the counterfactual trend. We calculate 95% confidence intervals for our main estimators 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. Left panels (a,c,e) show period-specific effects, while right panels (b,d,f) display average treatment effects across post-treatment periods, providing a more comprehensive assessment of the parallel trends assumption by focusing on aggregate impacts rather than individual time points.



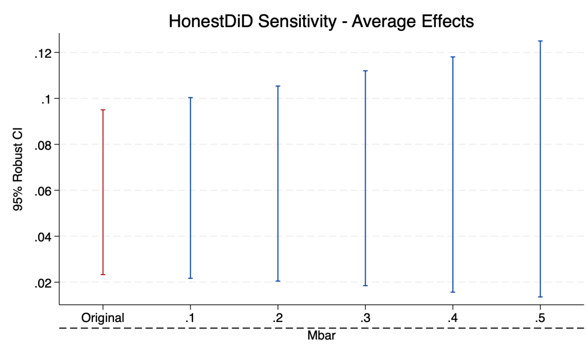
(a) Log(Sales)



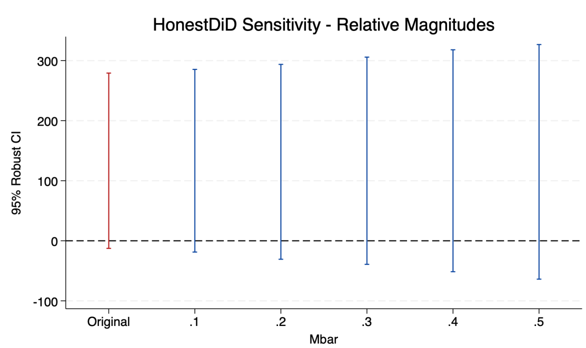
(b) Log(Sales) Average Effects



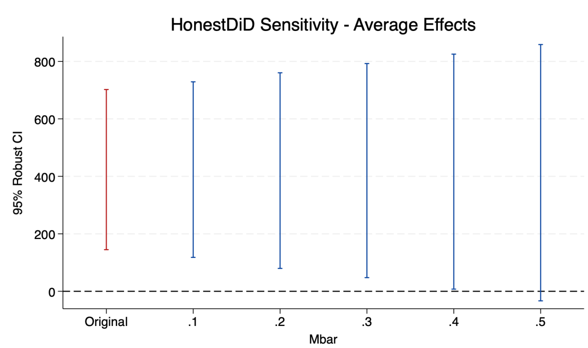
(c) Log(Emp)



(d) Log(Emp) Average Effects



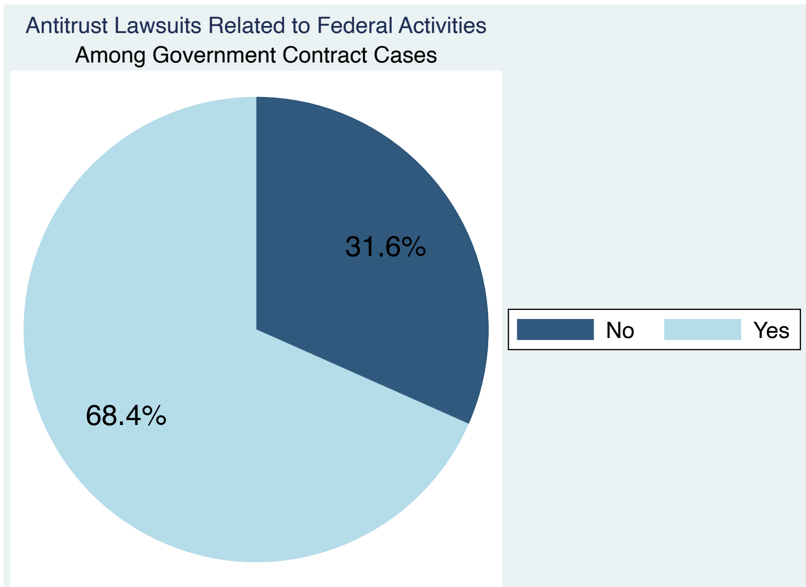
(e) Federal Obligation



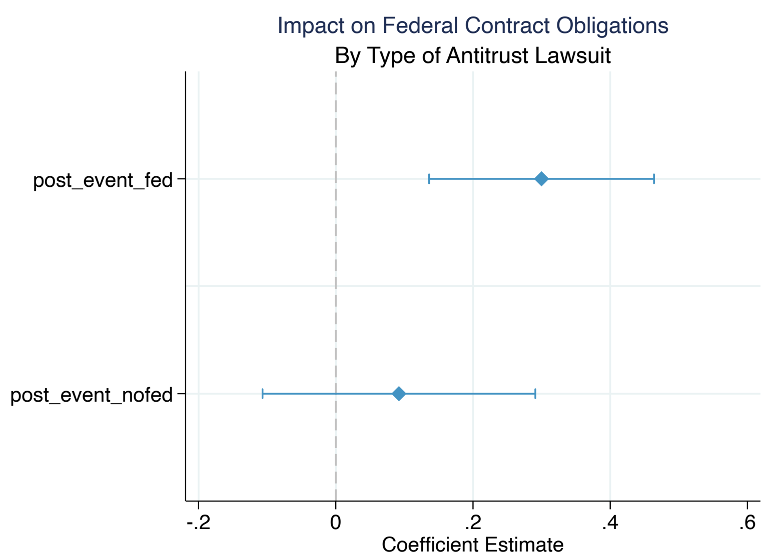
(f) Federal Obligation Average Effects

Figure A4: **Federal vs Non-Federal Antitrust Lawsuits**

Figure A4 examines differences between federal and non-federal procurement activities. Panel (a) shows the share of antitrust lawsuits related to federal versus non-federal procurement. Panel (b) presents coefficient estimates from Equation (2) for two categories of violations: $post\_event\_fed$ is a dummy variable equal to one in the period following an antitrust lawsuit involving federal procurement, and $post\_event\_nofed$ is a dummy variable equal to one in the period following an antitrust lawsuit involving non-federal procurement.



(a) Antitrust lawsuits in federal procurement activities



(b) Heterogeneous impact

Figure A5: **Dynamic Effects on Defendant Establishments' Procurement**

Figure A5 displays the temporal pattern of how antitrust enforcement affects government contract awards to defendant establishments over time. Using the Callaway and Sant'Anna (2021) estimator, we track the evolution of federal contract obligations (measured using inverse hyperbolic sine transformation) for establishments belonging to defendant firms relative to the filing date of antitrust lawsuits. The x-axis represents event time in periods relative to the enforcement action, with negative values indicating pre-treatment periods and positive values representing post-treatment periods. The y-axis shows the average treatment effect on the treated (ATT). The shaded areas represent 95% confidence intervals.

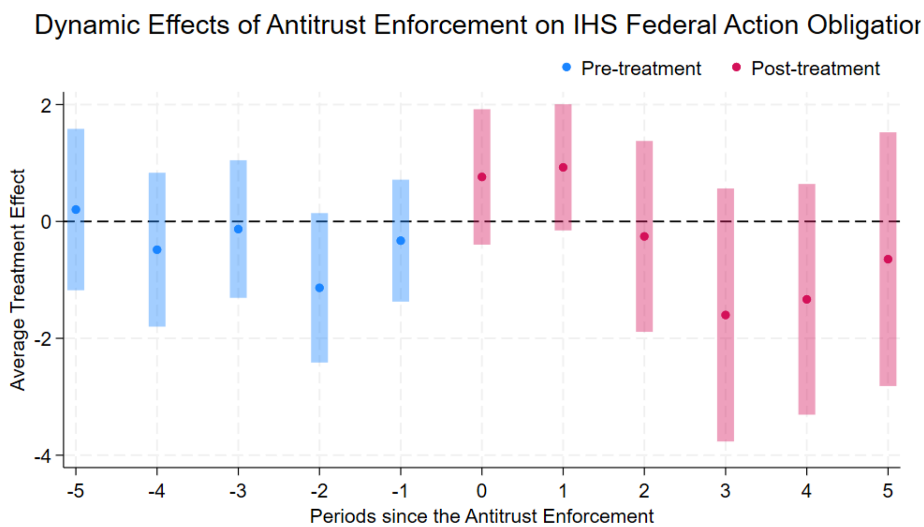
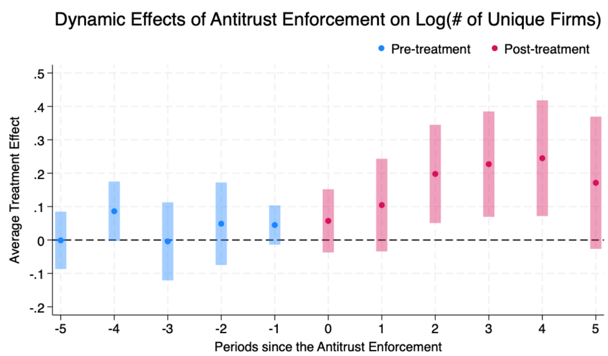
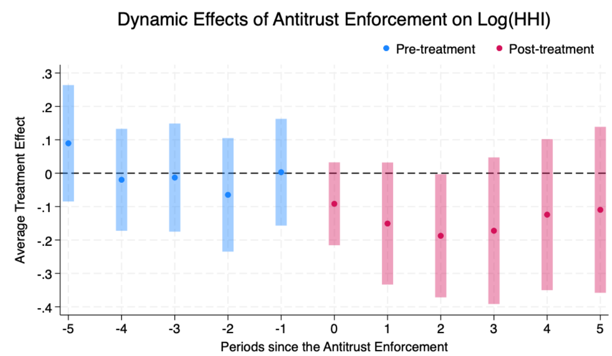


Figure A6: **Antitrust Lawsuits and Industry-Level Outcomes: Dynamic Effects**

Figure A6 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 after the first antitrust lawsuits 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 belong to a public corporation.	NETS
Age	The difference between the year and the first year establishment was active in the database.	NETS
Small	An indicator variable equal to one if the establishment has less than 5 employees.	NETS
Minority	An indicator variable equal to one if the establishment is minority owned.	NETS
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

Table A1: **Continued**

Variable	Definition	Source
# 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		
# Modifications	The total number of contract modifications for each contract, based on the number of transactions beyond the initial award.	USASpending
Renegotiated Amount	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
IHS($ Renegotiation)	The inverse hyperbolic sine transformation of the total dollar amount of renegotiated amount.	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
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
DoD Contract	An indicator variable equal to one if the contract was awarded by the Department of Defense, identified through agency codes and contract award unique key patterns.	USASpending
PSC Category	Product Service Code classification indicating the fundamental nature of the procurement: Information Technology Services, General Services, or Goods/Supplies. Based on standardized federal procurement classification codes.	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		
	Mean	SD	Mean	SD	ND
Log(HHI)	7.44	1.16	8.04	0.95	-0.40
Log(# firms)	3.54	1.72	2.48	1.36	0.48
Average Renegotiation	1.38	0.53	1.29	0.71	0.11
Competition	0.72	0.26	0.70	0.30	0.04
Average Contract Value	445,148.70	976,219.44	260,241.87	587,804.05	0.16
Share Fixed Price	0.84	0.26	0.90	0.20	-0.18

Panel B: Establishment characteristics

	Treated		Untreated		
	Mean	SD	Mean	SD	ND
Age	9.12	4.17	9.61	3.95	-0.09
Women Owned	0.21	0.40	0.17	0.38	0.06
Minority	0.16	0.37	0.09	0.29	0.15
Dummy Women	0.11	0.32	0.13	0.34	-0.04
Log(Employment)	2.69	1.59	2.77	1.71	-0.03
Log(Sales)	7.41	1.87	7.52	1.96	-0.04
Log(Productivity)	4.73	0.84	4.75	0.86	-0.02
PAYDEX	71.42	8.37	71.23	8.25	0.02
Federal obligation	161.66	720.83	83.00	502.21	0.09

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	0.227**	452.990***	442.044***
	(0.092)	(118.916)	(125.548)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	1,678,543	1,678,543	1,422,945
R-squared	0.561	0.640	0.729
Average Outcome	2.422	866.2	986.9

	(4)	(5)	(6)
Variables	IHS (3 years average)	Level (2 years average)	IHS (2 years average)
Antitrust Lawsuit	0.191**	435.012***	0.210**
	(0.088)	(120.344)	(0.087)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	1,422,945	1,549,440	1,549,440
R-squared	0.679	0.691	0.626
Average Outcome	3.142	938.3	2.843

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-three (column 1) and one-to-five (column 2) nearest-neighbor matching algorithms. 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 (1-to-3 Matching)	IHS Government Contracts (1-to-5 Matching)
Antitrust Lawsuits	0.179*	0.182*
	(0.104)	(0.102)
Establishment FE	Yes	Yes
County-Year FE	Yes	Yes
Observations	543,990	633,166
R-squared	0.575	0.576

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	0.213**	0.192**	0.240***	0.240***
	(0.086)	(0.077)	(0.089)	(0.089)
Product Market Controls	Yes			
Industry × Year		Yes		
Product Market Linear Trend			Yes	Yes
Product Market Quadratic Trend				Yes
Observations	1,456,591	1,678,543	1,678,543	1,678,543
R-squared	0.563	0.565	0.561	0.561
Average Outcome	2.484	2.422	2.422	2.422

Table A6: **Spillover Effects of Antitrust Lawsuits**

Table A6 tests whether antitrust enforcement in one product market affects related markets through competitive spillovers or resource reallocation. The variable *Antitrust Lawsuit Spillover* is a binary indicator equal to one if the establishment operates in a broader 4-digit NAICS sector containing a product market exposed to an antitrust lawsuit (but not in the specific 6-digit NAICS code directly affected). 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 Lawsuit	0.250**	0.229***
	(0.108)	(0.094)
Antitrust Lawsuit Spillover	-0.023	0.004
	(0.070)	(0.063)
Establishment FE	Yes	Yes
Year FE	Yes	Subsumed
County-Year FE	No	Yes
Observations	1,678,543	1,678,543
R-squared	0.547	0.561
Average Outcome	2.422	2.422

Table A7: **Placebo Test: Non-Government Contractors**

Table A7 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 using this substantially larger sample of 185,158,511 establishment-year observations. 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	0.012	0.025	-0.277
	(0.012)	(0.029)	(0.360)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	185,463,304	185,158,511	39,273,724
R-squared	0.910	0.918	0.587

Table A8: **Triple Difference-in-Differences Analysis**

Table A8 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.

	(1)	(2)	(3)
Variables	Log(Employment)	Log(Sales)	PAYDEX
Antitrust lawsuit × Government Contractor	0.181***	0.307***	1.598***
	(0.028)	(0.037)	(0.155)
Establishment FE	Yes	Yes	Yes
Product Market × Year FE	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes
Observations	187,162,559	186,845,138	40,483,362
R-squared	0.927	0.935	0.650
Average Outcome	0.890	12.023	71.895

Table A9: **Alternative Treatment Definition: Court Location**

Table A9 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.

	(1)	(2)
Variables	IHS Government Contracts	IHS Government Contracts
Antitrust Lawsuit	0.249**	0.246**
	(0.111)	(0.097)
Establishment FE	Yes	Yes
Year FE	Yes	No
County × Year FE	No	Yes
Observations	1,678,543	1,678,543
R-squared	0.549	0.563
Average Outcome	2.406	2.406

Table A10: **Antitrust Lawsuits and Defendant Establishment Outcomes**

Table A10 examines how antitrust enforcement affects the performance of establishments belonging to defendant firms. We estimate Equation (4) using three different outcome variables measuring establishment size (Log(Employment)), performance (Log(Sales)), and financial health (PAYDEX). 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	-0.233***	-0.036	-0.530
	(0.073)	(0.090)	(0.538)
Establishment FE	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes
Observations	1,339,546	1,339,546	934,124
R-squared	0.941	0.948	0.535
Average Outcome	2.444	7.165	72.51

Table A11: **Propensity Score Matching: Contract-Level Analysis**

Table A11 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-five 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. 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	0.090**	0.105	0.031*	0.025***
	(0.043)	(0.113)	(0.018)	(0.009)
Product Market FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Observations	4,616,327	4,616,327	4,586,073	3,760,338
R-squared	0.320	0.125	0.166	0.188
Average Outcome	1.415	0.523	0.0558	0.118

Panel B: One-to-Five Matching

	(5)	(6)	(7)	(8)
Variables	# Modifications	IHS($ Renegotiation)	Cost Overrun Ratio	Delivery Delay
Antitrust Lawsuit	0.099**	0.212**	0.016**	0.022**
	(0.042)	(0.104)	(0.008)	(0.010)
Product Market FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Observations	10,211,382	10,211,382	10,166,862	7,789,079
R-squared	0.324	0.113	0.158	0.183
Average Outcome	1.271	0.296	0.0352	0.0843

Table A12: **Alternative Treatment Definition: Contract-Level Analysis**

Table A12 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. 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	0.133**	0.247	0.031*	0.027***
	(0.067)	(0.206)	(0.018)	(0.010)
Product Market FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Observations	22,421,710	22,421,710	22,350,402	18,274,234
R-squared	0.329	0.110	0.153	0.193
Average Outcome	1.186	0.176	0.0232	0.0542

Table A13: **Effects of Antitrust Enforcement on Contract Characteristics**

Table A13 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. 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)	(5)
Variables	IHS(Initial Value)	Cost-Plus	Competition	Fixed-Price	# Offers
Antitrust lawsuit	0.055	-0.010	-0.019	0.006	-1.525
	(0.104)	(0.007)	(0.030)	(0.014)	(1.115)
Product market FE	Yes	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	22,421,702	22,421,702	22,421,702	22,421,702	13,398,535
R-squared	0.477	0.296	0.366	0.395	0.740
Average Outcome	8.122	0.007	0.860	0.973	0.956

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 Law[21] (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.[22]

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 cases[23], 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.[24] 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.

[21] Wolters 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.

[22] The 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.

[23] As 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.

[24] Our 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:

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

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

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

Our extraction task is then formulated as selecting:

$$Y^* = \arg\max_{Y} P(Y \mid X),$$

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 $y_i^{(j,k)} : j \in \{1, 2, 3\}$, $k \in \{1, \ldots, R\}$. 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_cout_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_cout_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.[25] 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"]
  }
}
```

[25] This 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.

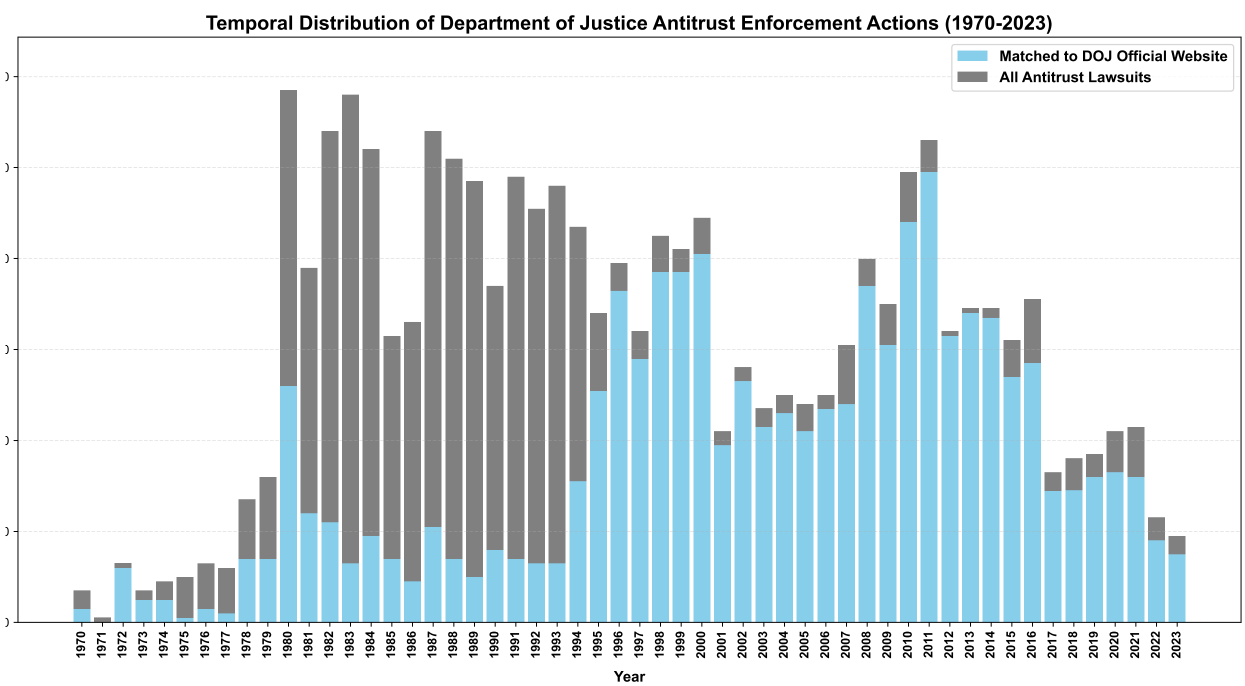
Horizontal restraints involve agreements between competitors at the same level of production that directly restrict competition, typically prosecuted as per se violations under Section 1 of the Sherman Act and representing the DOJ's highest criminal enforcement priority. Monopolization captures conduct 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.[26] 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.[27] Furthermore, we classify the collusion mechanisms, capturing the tools used to facilitate anticompetitive conduct.

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

[27]This 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.



Online Appendix B: Examples

Procurement Related Antitrust Lawsuits Example

Example 1: United States v. MCC Construction Corp.

Case Summary: In January 2016, MCC Construction Company, a construction management and general contractor, was charged with one count of conspiracy to commit major fraud against the United States (Case No. 1:16-cr-00004). The U.S. Department of Justice (DOJ) alleged that MCC conspired with two companies eligible for federal contracts set aside for small, disadvantaged businesses, with MCC performing the work while the eligible companies submitted the bids. MCC waived indictment, agreed to the filing of the information, and accepted responsibility for its actions.

Field	Information
Case Filed	January 5, 2016
Defendants	MCC Construction Company
Industry (NAICS6)	Commercial and Institutional Building Construction (236220)
Court	United States District Court for the District of Columbia (Case No. 1:16-cr-00004)
Legal Basis	18 U.S.C. § 371 (conspiracy to defraud the United States) and 18 U.S.C. § 1031 (major fraud against the United States)
Key Events	- January 2016: Criminal information filed by the DOJ.
	- February 2, 2016: U.S. District Judge Ketanji B. Jackson accepts MCC's guilty plea.
	- March 15, 2016: Sentencing hearing; judgment entered by Judge Jackson.
Outcome	MCC pleaded guilty and agreed to pay $1,769,294 in criminal penalties and forfeiture.

Example 2: United States v. SG Interests I, Ltd., SG Interests VII, Ltd. and Gunnison Energy Corporation

Case Summary: SG Interests I, Ltd., SG Interests VII, Ltd. (collectively "SGI"), and Gunnison Energy Corporation (GEC) were charged with violating Section 1 of the Sherman Act for their agreement not to compete in bidding for natural gas leases auctioned by the Bureau of Land Management (BLM). On February 8, 2005, just days before a BLM auction, the companies executed a Memorandum of Understanding (MOU) agreeing that only SGI would bid as nominee for both parties at the February and May 2005 auctions, and if successful, would assign a 50% interest in the acquired leases to GEC at cost. As a result of this agreement, the United States received less revenue than it would have had the companies competed. The Department of Justice filed the civil antitrust complaint on February 15, 2012, marking the first time the DOJ challenged an anticompetitive bidding agreement for mineral rights leases.

Field	Information
Case Filed	February 15, 2012
Defendants	SG Interests I, Ltd., SG Interests VII, Ltd., and Gunnison Energy Corporation
Industry (NAICS6)	Crude Petroleum and Natural Gas Extraction (211111)
Court	United States District Court for the District of Colorado (Civil Action No. 12-cv-00395-RPM)
Legal Basis	Section 1 of the Sherman Act (15 U.S.C. § 1) and False Claims Act
Key Events	- February 8, 2005: SGI and GEC executed a MOU - February and May 2005: BLM auctions with collusive bidding - October 2009: Former GEC VP filed qui tam whistleblower complaint - February 15, 2012: DOJ filed civil antitrust complaint - December 12, 2012: Judge rejected initial settlement - April 22, 2013: Final judgment entered
Outcome	Companies paid $550,000 to settle antitrust and False Claims Act violations, with required advance notice of future joint bidding practices for five years.

Example 3: United States v. Scott "Max" Anthony Walker and Ryan Scott McMonigle

Case Summary: In 2009, Scott "Max" Anthony Walker and Ryan Scott McMonigle were charged with conspiring to violate the Anti-Kickback Act of 1986 by soliciting kickbacks from security vendors in connection with a subcontract under a $1.4 billion USAID contract for the Afghanistan Infrastructure Rehabilitation Project (AIRP). The scheme aimed to influence the bidding process for security services, with kickbacks initially set at $250,000 and later negotiated to 1.8% of the contract value. Both individuals pleaded guilty, with Walker entering a plea agreement in November 2009 and McMonigle in January 2010.

Field	Information
Case Filed	August 4, 2009
Defendants	Scott "Max" Anthony Walker, Ryan Scott McMonigle
Industry (NAICS Code)	Security Guards and Patrol Services (561612)
Court	United States District Court for the Eastern District of Virginia, Alexandria Division (Case No. 1:09-CR-478)
Legal Basis	18 U.S.C. § 371 (conspiracy) and 41 U.S.C. § 53 (Anti-Kickback Act)
Key Events	- August 4, 2009: Information filed under seal - August 26, 2009: Seal lifted - November 16, 2009: Plea agreement for Scott Anthony Walker - January 26, 2010: Guilty plea for Ryan Scott McMonigle
Outcome	Both defendants pleaded guilty to the conspiracy charges.

Example 4: United States v. Peter W. Schmidt

Case Summary: In 2001, Peter W. Schmidt, president of Schmidt Construction Company, was charged with conspiracy to defraud the United States by rigging bids for federally funded construction projects in Alabama. He was convicted and sentenced to 21 months in prison.

Field	Information
Case Filed	July 25, 2001
Defendants	Peter W. Schmidt
Industry (NAICS6)	Water, Sewer, and Pipeline Construction (237110)
Court	United States District Court for the Northern District of Alabama
Legal Basis	18 U.S.C. § 371 (conspiracy to defraud the United States)
Outcome	Convicted and sentenced to 21 months in prison.

Example 5: United States v. Woodson & Associates Inc.

Case Summary: On September 29, 2005, Woodson & Associates Inc. agreed to plead guilty to bid rigging on electrical construction contracts at Cape Canaveral Air Force Station, violating Section 1 of the Sherman Act. The conspiracy, spanning from March 1998 to June 2002, involved projects for the Evolved Expendable Launch Vehicle program. Woodson was fined $175,000, with the plea subject to court approval. The investigation was conducted by the Antitrust Division, NASA, and the Air Force.

Field	Information
Case Filed	September 29, 2005
Defendants	Woodson & Associates Inc.
Industry (NAICS6)	Electrical Contractors and Other Wiring Installation Contractors (238210)
Court	U.S. District Court for the Middle District of Florida
Legal Basis	Section 1 of the Sherman Act (15 U.S.C. § 1)
Key Events	- March 1998 to June 2002: Conspiracy to rig bids on electrical construction contracts at CCAFS
	- September 29, 2005: Woodson & Associates Inc. agrees to plead guilty and pay a $175,000 fine
Outcome	Woodson & Associates Inc. pleaded guilty to bid rigging and was fined $175,000

Online Appendix C: Procurement Performance

Consider a procurement market with two project types: *low complexity* ($\theta = L$) and *high complexity* ($\theta = H$), with $H > L$. Let each firm i be characterized by its capacity level $c_i \in [0, 1]$, where higher values reflect greater operational, financial, and organizational ability. Firms face a fixed cost of entry F required to participate in procurement.

Let $\bar{c}_\theta$ denote the minimum capacity threshold needed to execute a project of complexity θ. We assume:

$$\bar{c}_L = 0, \quad \bar{c}_H > 0$$

so that all firms can handle low-complexity projects, but only sufficiently capable firms can deliver high-complexity ones.

Suppose the government awards a project of type θ to firm i. The match quality (i.e., procurement performance) is given by:

$$q(c_i, \theta) = \begin{cases} \bar{q}, & \text{if } \theta = L \\ \phi(c_i), & \text{if } \theta = H, \text{ with } \phi' > 0, \ \phi'' < 0, \text{ and } c_i \geq \bar{c}_H \\ 0, & \text{if } c_i < \bar{c}_\theta \end{cases}$$

Thus, performance is uniform across firms in simple projects, but strictly increasing in capacity for complex ones, and zero if the firm lacks minimum capability.

Now consider that antitrust enforcement removes a subset of high-capacity incumbents (e.g., colluding firms with $c_i \geq \bar{c}_H$). This generates two possible effects:

1. In low-complexity markets, new low-capacity entrants (with $c_i \approx 0$) can enter (paying F) and fully maintain procurement performance, since $q(c_i, L) = \bar{q}$ regardless of c_i.

2. In high-complexity markets, performance deteriorates:
   - Either because no firm with $c_i \geq \bar{c}_H$ remains,
   - Or because low-capacity firms bid and win, but $q(c_i, H) \approx 0$,
   - Or because remaining incumbents stretch capacity, increasing cost overruns or delays.

In this environment, enforcement generates a clear trade-off:

- It increases market participation (more firms enter, and HHI falls),
- But may reduce efficiency in complex procurement where capability is critical.

This stylized model rationalizes our core empirical findings: increased market participation and reallocation post-enforcement, disproportionately captured by high-capacity incumbents, coupled with declines in performance indicators for high-θ projects.