Antitrust Enforcement Increases Economic Activity*†

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Abstract

We hand-collect information describing all 3,055 antitrust lawsuits brought by the Department of Justice (DOJ) between 1971 and 2018. Using confidential U.S. Census microdata, we show that DOJ lawsuits targeting past anticompetitive conduct in local industries cause a persistent 5.4% increase in employment and 4.1% increase in business formation compared to the same industries in other states. We further find (1) a sharp increase in payroll exceeding the increase in employment, (2) an economically and statistically insignificant increase in sales, and (3) a precise increase in the labor share. Our results show that government antitrust enforcement increases economic activity.

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1 Introduction

A recent literature documents rising market power in the U.S. and its negative effects on aggregate wages, investment, and productivity (Gutiérrez and Philippon, 2017; Barkai, 2020; Autor et al., 2020). These patterns have sparked a renewed interest by policymakers, researchers, and the media in competition policy and antitrust enforcement.[1] This wave of interest in antitrust enforcement echoes similar cycles of attention to anticompetitive behavior that led to the passage of the four key federal laws regulating anticompetitive behavior.[2] But despite over a century of antitrust enforcement, there is little systematic empirical work measuring the effects of antitrust enforcement on economic outcomes.[3] A key challenge to empirical research in the area of antitrust enforcement is the absence of standardized data on antitrust enforcement actions.

In this paper, we hand-collect Department of Justice (DOJ) antitrust lawsuits covering the years 1971–2018 and study the real effects of these lawsuits on economic activity. We collect data on the legal characteristics of each case as well as the markets affected by anticompetitive behavior, namely the state and industry of each antitrust violation. We merge our hand-collected data on DOJ antitrust enforcement, aggregated to the level of an industry-state-year, with confidential establishment-level microdata from the U.S. Census aggregated to the same level of observation. In our analysis of enforcement effects, we focus on *conduct* cases, defined as cases that allege past anticompetitive behavior. Conduct cases consist of Horizontal Violations (e.g. bid rigging, price fixing, market allocation),

[1] See, for example, Booker (2019), Klobuchar (2019), and Warren (2019) for policy remedies proposed by politicians; and see, for example, Khan (2016), Posner, Scott Morton and Weyl (2017), Marinescu and Posner (2018), Naidu, Posner and Weyl (2018), Marinescu and Hovenkamp (2019), and Federico, Morton and Shapiro (2020) for policy remedies proposed by researchers. Other book-length discussions of antitrust and competition policy can be found in Baker (2019), Philippon (2019), Stoller (2019), Posner (2021), Eeckhout (2021), and Klobuchar (2022).

[2] These are the Sherman Act (1890), the Clayton Act (1914), the Federal Trade Commission Act (1914), and the Hart-Scott-Rodino Antitrust Improvements Act (1976).

[3] Notable exceptions, using data from other countries or country-level variation, include Dasgupta and Žaldokas (2019), Buccirossi et al. (2013), Besley, Fontana and Limodio (2021), and Gutiérrez and Philippon (2023), who use cross-country variation to evaluate effects of antitrust enforcement, and Reed et al. (2022), who use data from Mexico to evaluate antitrust enforcement in the context of a middle-income country.

Exclusionary Practices (e.g. IP misuse, bundling), and Vertical Violations (e.g. vertical price fixing).[4] Using the linked case-Census data, our paper provides the first systematic evidence of the real effects of DOJ antitrust enforcement. We find that DOJ enforcement targeting past anticompetitive conduct increases the level of economic activity (measured as employment), business formation, average wages, and the labor share in affected industries.

Our main source of information on antitrust enforcement is legal summaries of DOJ antitrust lawsuits provided by the Commerce Clearing House (CCH) Trade Regulation Reporter, a private information aggregator that publishes these summaries for lawyers and legal scholars. Building on the work of Posner (1970) and Gallo et al. (2000),[5] we manually review these summaries and collect a large number of standard variables such as the alleged violations, the name of the district court, and the case filing date. In addition to these standard variables, we collect detailed information on the geography and industry of alleged anticompetitive behavior. Specifically, we collect information that describes the location of the seller and the geographic scope of the alleged violation (ranging from city to international) and we manually match each case to an industry code, as classified by the North American Industry Classification System (NAICS). These additional market variables make it possible to isolate variation in economic activity targeted by antitrust lawsuits within an industry and across states.

[4]These three categories of anticompetitive behavior share high-level similarities in the ways they undermine competition: for the most part they either restrict output, which drives up prices, or they fix prices, which demands lower output Baker (2002). Therefore, to the extent that DOJ antitrust enforcement is effective at catching and correcting these behaviors, we could expect to see an increase in the level of economic activity and a decrease in prices in the years following the enforcement action. Merger violations have a different theory of harm: instead of addressing past anticompetitive behavior, merger prosecutions allege potential *future* anticompetitive concerns, should the merger be allowed to proceed. Therefore, effective DOJ antitrust enforcement of merger violations would not necessarily lead to increased economic activity and lower prices in the years following enforcement relative to the years preceding enforcement. For this reason, we exclude merger cases from our analysis in Section 5. We provide more detail on the breakdown of alleged violations in Table 1.

[5]The pioneering study by Posner (1970) was the first effort to systematically collect and characterize U.S. government lawsuits filed over 1890–1969, and Gallo et al. (2000) extend the original Posner data through 1997. Our main contribution to their data is extending the data through the modern period (through 2018) and adding information on industry, geography, and geographic scope of alleged anticompetitive behavior. Another significant data collection effort is by Connor (2014), who collected information on international cartels detected since 1990. We complement this effort by collecting lawsuits on all types of violations and by documenting trends in the DOJ enforcement over the past five decades.

We combine our hand-collected data on DOJ antitrust enforcement with confidential establishment-level microdata from the Longitudinal Business Database (LBD) and the Economic Censuses, aggregated to the level of an industry-state-year. The use of confidential microdata allows us (1) to measure a range of economic outcomes at the level of an industry-state-year, most of which are not consistently measured in publicly available data sources, (2) to estimate aggregate effects across both private and public firms, and (3) to construct a definition of each industry that is consistent over time, resulting in stable units of observation. Using the combined data, we measure the effect of antitrust enforcement on economic activity, as measured by employment, business formation, average wages, and the labor share.

Existing theory is clear on the predicted effect for some of these outcomes and ambiguous on the predicted effect for others. Regarding employment, standard economic models predict that it should rise as enforcement lifts constraints on output and thus raises demand for production inputs.[6] Some models also use labor market frictions to predict an adverse effect of anticompetitive conduct on employment and wages (Jacquemin and Slade, 1989), which combined with higher prices implies a lower labor share. Nonetheless, Cestone, Li and Volpin (2021) argue that antitrust enforcement triggers cost-cutting and restructuring, resulting in at least temporary declines in industry employment.

When it comes to firm entry, models are ambiguous about the effects of anticompetitive behavior (Asker and Nocke, 2021). By keeping prices high, collusion may induce firm entry (Fershtman and Pakes, 2000); at the same time, incumbents have strong incentives to deter entry and do so in a variety of ways (Levenstein and Suslow, 2006; Scott Morton, 1997; Cunningham, Ederer and Ma, 2021). For example, a 1996 case alleging collusion (via market allocation) by two waste management companies points out that the contracts the defendants entered into with their customers "significantly rais[ed] the cost and time required by a new entrant or small incumbent firm to build its customer base and obtain efficient scale and

[6]See for example Bork (1978); Whinston (2008); Asker and Nocke (2021).

route density" (United States of America, 1996).

We study the lawsuits that the DOJ Antitrust Division chose to bring to court, which means that treatment is not randomly distributed across industries. The DOJ's choice to sue is based on their internal assessment of the severity of the anticompetitive behavior and the likelihood that they will win in court.[7] Our results thus speak to the policy-relevant effect of chosen antitrust actions. To the extent the DOJ prioritizes the most important enforcement actions, increasing the quantity of enforcement actions would not necessarily yield economic effects of the same magnitude. Moreover, we do not study the effects of DOJ enforcement that are designed to prevent or limit a merger or acquisition (M&A) as these lawsuits do not claim past anticompetitive behavior but instead aim to prevent future harm.

To construct a counterfactual for how an affected industry would evolve in the absence of an antitrust lawsuit, we focus our analysis on nontradable industries as defined in Barkai and Karger (2020). Specifically, we compare outcomes in industry-states subject to a DOJ antitrust lawsuit (e.g., Waste Management in Louisiana) to outcomes of the same industry in other states not subject to the lawsuit (Waste Management in other states). By doing these comparisons within each industry, using industry-year fixed effects, we account for common changes in technology or demand that impact the level of economic activity, patterns of business formation, and other economic outcomes in the absence of an antitrust lawsuit.[8] Similarly, using state-year fixed effects, our regressions account for changes in a state that are common to all industries, such as population growth.

In our first set of results, we find that DOJ antitrust enforcement induces a lasting increase in economic activity, measured as employment. We present year-by-year estimates of the

[7]For conduct violations, the DOJ receives a complaint from either the public or a government agency. They then produce an internal report within the Antitrust Division examining the need for an inquiry or investigation. The decision to pursue the case in court in turn depends on the results of this investigation.

[8]We focus on nontradable industries to minimize the potential for spillover effects. For example, Waste Management providers in Louisiana operate in a different market than Waste Management providers in Michigan, and as such, we expect Waste Management providers in Michigan (and other states) to provide a good counterfactual trend for Waste Management providers in Louisiana. To the extent that a Waste Management lawsuit in Louisiana induces extra deterrence effects for this industry in other states, this will bias estimates towards zero and we may underestimate the true effect.

effect of antitrust enforcement on log employment, measured in event time (±8 years around the filing of the DOJ antitrust lawsuit). The results show a clear increase in employment that starts when the antitrust lawsuit is filed and persists in all subsequent years. We then repeat the analysis in a difference-in-differences setting and find a long-run increase in employment of 5.4%. The estimate of the difference-in-differences analysis is similar in magnitude to the estimates in the later years of the year-by-year analysis, which implies that there is no later reversion or decline in employment, even though the average post-period length is 25 years. We use a series of robustness checks with different weighting strategies to confirm that these results are not driven by small industries. We also confirm using public data (with higher levels of measurement error) that these results are not driven by the most common violation type (bid-rigging), or by cases brought after the 1993 introduction of the DOJ's leniency program for cartel informants.

In our second set of results, we find that DOJ antitrust enforcement also induces a lasting increase in business formation. Year-by-year estimates show a clear and gradual increase in the number of establishments in affected industry-states starting in the year of the lawsuit and stabilizing at an increase of nearly 3%. Difference-in-differences analysis shows a long-run increase in the number of establishments of 2.9%. Like the employment results described above, our estimate from the difference-in-differences analysis is similar in magnitude to the estimates in the later years of the year-by-year analysis, which implies that there is no later reversion or decline in the number of establishments, even though the average post-period length is 25 years. We further find a long-run increase in the number of firms as well as *new* establishments and *new* firms operating in the affected industry-state following the antitrust enforcement action, indicating increased business dynamism within the industry and not just reorganization.

The increase in the number of new establishments and firms is not solely due to entry in the immediate aftermath of the DOJ antitrust lawsuit. Instead, the results tell us that each year, including many years later, more new firms and establishments are entering the industry-state affected by the lawsuit. This implies a robust effect of antitrust enforcement on business dynamism, in line with arguments from Levenstein and Suslow (2006) and Baker, Sallet and Scott Morton (2017) that new firms will not always enter to capture elevated noncompetitive profits; incumbents can build barriers to entry and antitrust enforcement may be necessary to remove these barriers.

In our last set of results, we use data from the Economic Census to study the effects of DOJ antitrust enforcement on payroll, sales, and the labor share, defined as the ratio of payroll to sales. We find an increase in payroll that exceeds the increase in employment, implying an increase in average wages. In addition, we find an economically smaller increase in sales (relative to employment) that is statistically insignificant. While we do not have separate measures of the quantity and price of output, the increase in production inputs (employment), together with a proportionally smaller and statistically insignificant increase in sales, strongly suggests an increase in the quantity of output and, at the same time, a decrease in the price of output. Last, we find a 3.5% increase in the labor share, suggesting that antitrust enforcement has implications for the distribution of income.

In summary, we construct the first comprehensive database of DOJ antitrust cases since 1971 and provide the first systematic evidence that typical DOJ enforcement actions targeting past anticompetitive conduct lead to a long-run increase in the level of economic activity, business formation, average wages, and the labor share. Our estimates at the level of industry-states cover the economic activity of all firms, private as well as public. Magnitudes are meaningful: employment increases 5% and new firm entry 4%, with results persisting over the long run. These effects start rapidly but do not just reflect a one-time adjustment in industry structure. Together these results indicate that DOJ antitrust enforcement actions are effective at bringing about lasting improvements in competition and increased economic activity. The database of DOJ antitrust cases will be made available to other researchers upon publication of this paper.

There are three potential limitations to our research. First, our analysis is not able to capture the effects of general deterrence. Large efforts to detect and prosecute economic crimes are likely to reduce anticompetitive misconduct by firms. Second, our analysis does not speak to the effect of antitrust enforcement on nationally dominant firms due to the challenges of constructing a credible control group for these firms. To the extent that these cases provide unique economic benefits,[9] they are not captured in our results. Last, it is possible that spillovers bias our estimates toward zero. Once the DOJ Antitrust Division brings a case against a particular industry, there may be non-targeted firms in the same industry in different states that had been engaged in anticompetitive behavior but stopped after they learned of the lawsuit. This could lead to increased competition in the control group, thereby biasing our estimates toward zero and leading us to understate the true effects of antitrust enforcement.

1.1 Related Literature

Our data collection and empirical analysis contribute to a growing literature on the economic effects of antitrust enforcement. Historically, much of this work focuses on prices, using both the stock prices of firms (Burns, 1977; Binder, 1988; Bittlingmayer, 1992; Bosch and Eckard Jr, 1991; Aguzzoni, Langus and Motta, 2013) and retail prices faced by consumers (e.g. Sproul 1993) to evaluate the effects of antitrust prosecution. More recently, several studies investigate the effect of enforcement actions on innovation (Watzinger et al., 2020; Watzinger and Schnitzer, 2022; Kang, 2023). We build on this prior work, focusing on measures of economic activity in the form of employment, business formation, payroll, and labor share.

Taking a macroeconomic perspective, a recent set of papers draws on country-level analysis to evaluate the effects of stronger antitrust enforcement. 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. Using country-level in-

[9]For example, Watzinger et al. (2020) find that the 1956 antitrust Consent Decree that forced Bell Labs to license all its existing patents royalty-free led to lasting increases in innovation.

dexes of competition policy, Buccirossi et al. (2013) show that countries with stronger competition policies have greater productivity growth and Besley, Fontana and Limodio (2021) show that in countries with strong antitrust policies, firms operate with lower profit margins. Reed et al. (2022) show that Mexican antitrust sanctions have had positive effects on that country's growth. Gutiérrez and Philippon (2023) develop a model of political support and consumer welfare that is supported by a comparative analysis of anticompetitive enforcement and changing market power in U.S. and E.U. markets.

Our economic analysis examines the aftermath of conduct cases alleging past anticompetitive behavior. Many of these cases involve collusive behavior, so our work also ties into the literature more specifically focused on modeling the effects of cartels (and their demise) in specific industries (e.g. retail gasoline (González and Moral, 2019; Byrne and De Roos, 2019); the generic drug market (Cuddy, 2020; Starc and Wollmann, 2022); the market for vitamins (Igami and Sugaya, 2022); the market for insulin (Barkley, 2023)).[10] We complement this research with a broader perspective, showing that as a whole, DOJ cases against anticompetitive conduct have systematically led to increased economic activity.[11] Moreover, a recent paper by Cestone, Li and Volpin (2021) shows that E.U. cartel investigations led to industry-wide cost-cutting, including mass layoffs. In their setting, antitrust enforcement depresses industry employment in the short term, though this later mean-reverts. Beside covering a distinct agency and union, an important difference is that their study focuses on listed firms, whereas ours covers economic activity across all firms.

In addition to the large literature on antitrust policy, a related set of papers focuses on other forms of competitive policy more broadly, including strict or lenient responses to mergers. For example, Wollmann (2019) shows that weakening M&A guidelines has led to a

[10] Levenstein and Suslow (2006) and Miller (2009) provide excellent overviews of this literature.

[11] This is not necessarily obvious: in a well-cited paper, Crandall and Winston (2003) conclude that "Until economists have hard evidence that the current antitrust statutes and the institutions that administer them are generating social benefits, the Federal Trade Commission and the Department of Justice should focus on the most significant and egregious violations." They also point out that "The Department of Justice and the FTC could help advance our knowledge of the effects of antitrust policy by making more data generated by cases available to researchers." The dataset we construct and will make available for this paper are in the spirit of advancing this goal.

large increase in the volume of M&A transactions just under the legal size thresholds; Affeldt et al. (2021) find that strict past merger enforcement negatively correlates with product market concentration; and Cunningham, Ederer and Ma (2021) find that "killer" acquisitions in which firms acquire and shut down competing products occur disproportionately just below these review thresholds. Watzinger and Schnitzer (2022) show that following the breakup of the Bell System, the scale and diversity of telecommunications innovation increased.

We contribute to prior work in three ways. First, by hand-collecting and standardizing a complete history of DOJ antitrust enforcement over the period 1971–2018, we provide an opportunity for others to build on our analysis of federal antitrust enforcement actions using high-quality data. Second, by merging our hand-collected data with the Census microdata, our paper provides the first systematic evidence that antitrust enforcement increases employment, payroll, the labor share, and business formation across all firms in an affected industry-state. Third, while much of the prior literature focuses on macroeconomic trends in competitive policy or on a small subset of antitrust enforcement actions in a handful of industries, we measure the effect of all DOJ conduct cases since 1971 on industry-level economic activity in nontradable industries. This provides an answer to the policy-relevant question of how the typical antitrust enforcement action targeting anticompetitive behavior affects industry-level outcomes.

In addition to our contribution to the literature on antitrust enforcement and competition policy, our empirical results, particularly our results on the labor share and business formation, are consistent with and contribute to a recent literature that attributes the decline in the labor share to a decline in competition (Autor et al., 2020; Barkai, 2020; De Loecker, Eeckhout and Unger, 2020; Gutiérrez, Jones and Philippon, 2021). Our results are further consistent with a new literature that jointly attributes the decline in the labor share and the decline in business dynamism (e.g. Decker et al. 2016) to declining competition (Barkai and Panageas, 2021; De Loecker, Eeckhout and Mongey, 2021; Ederer and Pellegrino, 2023).

2 Collection of Antitrust Enforcement Data

In this section, we describe our manual collection and classification of DOJ antitrust lawsuits covering the time period 1971–2018. Our focus is on lawsuits filed by the DOJ Antitrust Division. This division is, along with the FTC, one of two major enforcers of federal antitrust laws. There are two key differences between the DOJ Antitrust Division and the FTC. First, while both the FTC and the DOJ Antitrust Division prosecute violations of federal antitrust laws, the FTC has a broader mandate that includes a focus on non-antitrust topics such as consumer fraud, deception, and unfair business practices.[12] Second, while the DOJ Antitrust Division can prosecute both civil and criminal violations of antitrust law, the FTC is limited to civil violations. Perhaps for these two reasons, the FTC files relatively few antitrust conduct enforcement actions. For example, in the 23 years between 1996 and 2018, the FTC reports filing a total of 162 conduct enforcement actions (7 per year), with over half of those actions occurring in the healthcare industry.[13] Over the same time period, the DOJ Antitrust Division filed nearly five times as many conduct enforcement actions as the FTC.

2.1 Data Collection

Our underlying sources of information are legal summaries of DOJ antitrust lawsuits provided by the Commerce Clearing House (CCH) Trade Regulation Reporter. A typical case summary is two to three paragraphs long and describes the initiation and resolution of an antitrust lawsuit filed by the DOJ Antitrust Division in federal court. From each case we collect the detailed information described below. We rely on independent double entry: each case is read and coded independently by two individuals, and we then compare the entered values and reconcile disagreements. A more complete description of our data collection, including a comparison to data available from the DOJ website, is provided in our Antitrust Data Appendix A.

[12] For more details, see https://www.ftc.gov/enforcement.

[13] For more details, see https://www.ftc.gov/policy-notices/open-government/data-sets.

First, we collect identifying information about the case. This includes the date on which the case was brought to court, the name of the case (e.g., United States V. Tidewater Crushed Stone and Asphalt Co.), the court in which the case was brought (e.g., District Court in Alexandria, Virginia), whether the case was brought under criminal or civil law, and the case docket number. We also collect the set of named parties, when that information is available.

Second, we collect and classify the alleged legal violations. This includes the law the defendant is alleged to have violated (e.g., Section 1 of the Sherman Act) and the specific alleged violation(s) (e.g., price discrimination). A case can contain multiple alleged violations and we record all listed alleged violations. We record the date or dates of the alleged violations, when that information is available.

We classify each alleged violation into the following categories. *Horizontal Violation* includes allegations such as price fixing, bid rigging, and market allocation. *Exclusionary Practice* includes allegations such as predatory pricing, price discrimination, and exclusive dealings. *Vertical Violation* includes allegations such as price fixing in vertical markets and resale price maintenance. *Merger* includes DOJ suits to block or partially block mergers and violations of Hart-Scott-Rodino premerger notification requirements. Last, *Other* includes allegations of violations of consent decrees into which the party had entered at an earlier date, alleged violations that are not directly related to antitrust law such as false statements, and alleged violations that we were unable to classify.

Third, we collect geographic information about the alleged anticompetitive behavior in each lawsuit. We separately record the location where the seller operates and the location of the product market of the alleged violation. When multiple parties or product markets are involved, we record all of the locations. For example, if firm A is located in New York and firm B is located in New Jersey and the two firms are being sued for alleged bid-rigging in Pennsylvania, then we record the locations of the firms as New York and New Jersey and we record the location of the violation as Pennsylvania. In addition to the location of the violations, we collect the geographic scope of the affected market, which can range from a city to international.

Fourth, we collect the outcomes of the case. We record the legal outcome of the court case (e.g., found guilty) and all available information on penalties (e.g., fines and prison sentences). We further collect information on all appeals of a case to an appellate court or to the Supreme Court.

Fifth, we match each case to an industry code, as classified by the North American Industry Classification System (NAICS). Each case provides a product category such as *Limestone*, or *Metal Building Installation*. The listed product category is often insufficient on its own for us to determine the industry. For example, the product category *Milk* can match many industries, including Milk Production, Dairy Cattle (NAICS code 112120), Pasteurizing Milk (NAICS code 311511), or Raw Milk Merchant Wholesalers (NAICS code 424430). We therefore match each case to an industry by manually reading the full description of the case and searching through the NAICS industry classification manual to find the closest match.[14] If a case contains multiple product categories, we map each to an industry code.

2.2 Trends in Antitrust Enforcement 1971–2018

We identify 3,055 antitrust lawsuits brought by the DOJ antitrust enforcement division against firms and individuals between 1971 and 2018. In Figure 3, we plot the annual count of cases from 1971–2018. Panel A shows that the number of cases increased from the early 1970s to the 1980s before subsequently declining to lower levels today, reaching an average rate of around 40–50 cases per year in the 2010s. Nonetheless, the decline is not linear, and we observe an upswing in antitrust litigation centered around 2010, coming back down to a low of around 30 cases each in 2017 and 2018. Panel B shows that the vast majority of DOJ antitrust cases are conduct cases.

[14] For recent cases, the DOJ website provides both a product market description and an industry description. See the Appendix for a detailed comparison of our data to the DOJ website. Similar to the DOJ website, the European Commission publishes the product market description and an industry description for reviewed mergers. Affeldt et al. (2021) use the European Commission data to construct a mapping between product markets and industries.

Table 1 reports case counts of the different types of antitrust violations. One case may cover multiple violations, so we report frequency as both the percent of cases affected (summing to more than 100%) and the percent of violations (summing to exactly 100%). DOJ antitrust lawsuits focus mainly on bid-rigging (42% of cases, 29% of violations), price fixing (27% of cases, 19% of violations), and other market allocation-related violations (16% of cases, 11% of violations). Lawsuits to block a merger (partially or completely) constitute a smaller but meaningful block of cases (14% of cases, 10% of violations). Other violations prosecuted by the DOJ Antitrust Division include false statements, wire fraud, and bribery. These violations often overlap with other violations, so we do not drop them from analysis. Merger violations are excluded from our economic analysis in Section 5, because they do not concern past anticompetitive activity rectified by the DOJ.

Table 2 shows that many of the lawsuits are local in scope, focusing on violations by firms operating in a city (20% of cases), state (27%), or several states (10%), as opposed to firms operating nationally (21%) or worldwide (12%). An additional 9% of cases do not have an easily classifiable geographic scope.

Our case-level data show clear regimes of antitrust enforcement at a granular industry level. In the top panel of Figure 4 we show counts of antitrust cases brought by the DOJ, broken down by sector. The bottom panel shows the fraction of cases each year within each sector. Figure 4 highlights three striking patterns: federal antitrust enforcement in the 1980s prioritized the construction sector, with a peak of over 75% of DOJ-initiated antitrust lawsuits brought against firms and individuals in the construction sector in 1982.[15] In the 1990s, the DOJ shifted focus to a more diverse set of sectors, including manufacturing and wholesale trade, more closely matching patterns from the 1970s (in case distribution), albeit at higher levels of case activity. The distribution of cases across sectors then followed no noticeable patterns until the Great Recession, after which the DOJ turned its focus to firms

[15] This is consistent with a historical retrospective of DOJ antitrust enforcement actions in the 1980s, as reported by the U.S. General Accounting Office, which noted "over half of the criminal cases filed between fiscal years 1982 and 1988 involved either price fixing or bid rigging in road construction or government procurement." Report accessed via https://www.gao.gov/assets/ggd-91-2.pdf.

operating in the finance, insurance, and real estate sectors, with those cases exceeding a third of all filings in several years.

3 Industry Data and Analysis Samples

To study the effect of DOJ antitrust litigation on economic activity, we combine our antitrust enforcement data with three additional data sources: the U.S. Census Bureau's Longitudinal Business Database (LBD), its Economic Census data, and non-tradable industry classification from Barkai and Karger (2020). The combination of these data sources allows us to measure a range of economic outcomes at the level of an industry-state, most of which are not available through publicly available data sources.

3.1 Data on Industry Outcomes

Our first data source is the LBD. The LBD provides annual information on employment, geographic location, and industry for all private non-farm establishments located in the United States, covering the time period 1976–2015. The data further provide firm identifiers that link all establishments owned by a single firm. We use the most recent version of the LBD, as constructed by and described in Chow et al. (2021). Important for our purposes, the most recent construction of the LBD improves upon (1) longitudinal linking, thereby providing data that are as consistent as possible over the entire time series, (2) identification of new business formation, and (3) time-consistent industry classification of all establishments, based on a single classification vintage that builds upon the work of Fort and Klimek (2018).

The U.S. Census also publicly provides aggregated, noise-infused employment data via the County Business Patterns (CBP) series. The CBP does not allow for time-consistent industry classification (an issue solved by our use of the LBD for main results). Also, data-masking through noise infusion and cell suppression make the CBP suboptimal for analysis. Nonetheless, we rerun our results in this public dataset and find effects in-line with our results using restricted data. Furthermore, we use the CBP to address some subsample questions (in the appendix).[16]

Our second data source is the U.S. Census Bureau's Economic Census. The Economic Census provides information on employment, payroll, sales, geographic location, industry, and firm identifiers for establishments located in the United States. Unlike the LBD, which provides annual data for all private non-farm establishments, our sample from the Economic Census covers five major sectors (Wholesale Trade, Retail Trade, Transportation and Warehousing, Finance and Insurance, and Services), consists of data collected only in Economic Census years (every five years, in years ending in 2 and 7), and covers the time period 1977–2012 (though not all sectors have data available in all of the Economic Census years). We use the longitudinal links to the LBD to import into the Economic Census the time-consistent industry classification of all establishments.[17]

Our third data source, used to determine the set of non-tradable industries, is the classification of industries provided by Barkai and Karger (2020). In the most general terms, those industries that have establishments located in close proximity to a large fraction of the population are classified as non-tradable. To illustrate, Figure 5, reproduced from Barkai and Karger (2020), presents the geographic locations of establishments in the industries Convenience Stores and Automobile Manufacturing. Panels A and B show the locations of establishments in the industries, where location is defined as a five-digit Zip Code Tabulation Area (ZCTA). Panels C and D shows the locations (ZCTAs) that are within 50 miles of an establishment in the industries. In line with these examples, Barkai and Karger (2020) classify a six-digit NAICS industry as non-tradable if the data show that a large fraction of the population live in close proximity to an establishment in the industry. Since our

[16]The Census Bureau limits the number of coefficients that may be disclosed for a given project, complicating subsample analysis. So at this stage, we use the public (CBP) data to show that employment results are not driven by bid-rigging or by post-1993 leniency cases. The noisiness of CBP precludes us from using it for narrower subsample analysis.)

[17]For any establishment in the Economic Census without a longitudinal link to the LBD, we fill in this time-consistent industry classification using the most common time-consistent industry in the same year for other establishments in the same time-inconsistent industry.

analysis in this paper is carried out at the level of a four-digit NAICS industry, we classify a four-digit NAICS industry as non-tradable if the majority of employment in the four-digit NAICS occurs in non-tradable six-digit NAICS industries.[18]

3.2 Analysis Samples

For our empirical analysis, we focus on a subsample of cases that meet several criteria. First, we focus on cases with sufficient information available both before and after lawsuits. This refines our timeframe to the years 1977 to 2014, reducing the number of lawsuits to 2,515. We exclude cases from the District of Columbia, Puerto Rico, and Guam, as well as any foreign cases, which brings the count to 2,270. Next, our identification strategy requires keeping only cases with information on the seller's state (1,914 lawsuits), cases unrelated to mergers (1,581 lawsuits), and cases in nontradable industries (1,178 lawsuits). Additional filtering to include only local cases yields a total of 1,002 lawsuits. The exclusion of national cases is a necessary restriction because our identification strategy relies on variation across states within the target industry. Finally, focusing exclusively on cases from the first year that an industry faced an antitrust lawsuit narrows the set to 470 lawsuits. Initial DOJ lawsuits often lead to follow-up lawsuits, and our focus on the first lawsuit targeting a sued industry in each state means that our treatment effects are measuring the combined effect of initial lawsuits and any follow-up lawsuits that are part of the typical DOJ enforcement process. We provide descriptive statistics for these cases in Tables A.1 and A.2.

Within Census data, we construct two analysis samples, one using the LBD and one using the Economic Census. Our LBD analysis sample consists of annual LBD data for all non-tradable industries that are affected by a DOJ antitrust lawsuit at some time during the time period 1976–2015, across all 50 states. Before aggregating the data, we remove establishments with missing or zero employment. The outcomes measured in the data are employment, the

[18] In line with Barkai and Karger (2020), aggregation is based on national employment in the year 2010. For the purpose of this aggregation, we use publicly available data on employment in each six-digit NAICS industry as provided by the County Business Patterns. In almost all four-digit NAICS industries, all of the nested six-digit NAICS industries are either entirely tradable or entirely non-tradable.

number of establishments, the number of firms, the number of new establishments, and the number of new firms. To limit the reliance on outliers and potential data errors, after aggregating the data we remove industry-state-year observations with a year-on-year change in log employment that in absolute value exceeds 0.25.

Our second analysis sample combines our antitrust enforcement data with the Economic Census, covering five major sectors of the economy: Wholesale Trade, Retail Trade, Transportation and Warehousing, Finance and Insurance, and Services. This analysis sample consists of data measured every five years for non-tradable industries in the covered sectors that are targeted by a DOJ antitrust lawsuit, across all 50 states, covering the time period 1977–2012. Before aggregating the data, we remove establishments with missing or zero employment or payroll. The outcomes measured in the data are employment, payroll, sales, and the labor share, measured as the ratio of payroll to sales.

The unit of observation in both samples is an industry-state-year, where the industry is defined as a time-consistent four-digit NAICS industry (building on work from Fort and Klimek (2018)). Market definitions for alleged violations are typically narrower than four-digit NAICS, but we err on the side of overinclusion to allow for spillovers to adjacent markets Cestone, Li and Volpin (2021). If anything, an overly broad market definition should attenuate the effects we measure.

4 Empirical Design

The key challenge for empirical analysis is constructing a counterfactual for how a targeted industry would have evolved in the absence of enforcement. Our main concerns when constructing a counterfactual are accounting for (1) improvements in industry technology and (2) increases in demand that could each lead to increases in economic activity in the absence of DOJ antitrust enforcement actions.

In order to construct a counterfactual for how a targeted industry would have evolved in the absence of an antitrust lawsuit, we focus our analysis on non-tradable industries. Specifically, we compare outcomes in industry-states targeted by a DOJ antitrust lawsuit (e.g., Waste Management in Massachusetts) to outcomes of the same industry in other states not targeted by the lawsuit (Waste Management in other states). This comparison accounts for common changes in both the production technology of and demand for the products of the targeted industry. In the example of Waste Management, this comparison can account for improvements in waste management technology as well as variation in demand that is common across geographic locations.[19] To further account for variation that is common to all industries in a state, we include state-year fixed effects. This can account, for example, for an increase in population that leads to an overall increase in the demand for goods and services.

The DOJ decides which antitrust cases to bring to court based on the availability of evidence of anticompetitive behavior. This is of course not a random process. Our approach is not to try to find quasi-random variation across all industries in DOJ antitrust enforcement. Even if such a situation could be found, it is unclear what lessons could be drawn from randomizing antitrust enforcement rather than pursuing those cases with the most apparent merit. Instead, our approach is to construct a credible counterfactual trend for the industry-states subject to DOJ antitrust litigation.[20] The availability of data on real outcomes in a large time window around the enforcement actions allows us to assess pre-trends and gives us confidence in our estimation strategy. In addition, the very high R-squared values, which exceed 95% in every regression, give us some confidence that our control group provides a good counterfactual for how a targeted industry would have evolved in the absence of DOJ antitrust enforcement.

The unit of observation in our analysis is an industry-state-year. Our analysis sample

[19] Restricting our sample to nontradable industries minimizes the potential for our estimates to be biased by spillover effects across industries that compete nationally.

[20] Using data from Mexico, Reed et al. (2022) show that effects are larger when the control group consists of cases that were investigated but not sanctioned, relative to a within-sector approach. If this pattern holds in the US, we may underestimate the true effect.

comprises all non-tradable industries (NAICS4) targeted at least once by a DOJ antitrust lawsuit during our sample period (1971–2018). We exclude from our analysis M&A antitrust lawsuits, since these are designed to prevent future harm rather than correct past harm due to anticompetitive practices. For the same reason, we also exclude cases that do not allege any form of anticompetitive behavior (such as cases where the only allegation is wire fraud). This ensures that our estimates measure the correction of past harm due to anticompetitive practices that are targeted by the DOJ antitrust enforcement. Last, to ensure that we have a control group, we require that the industry not be targeted nationally in the antitrust case.[21]

Indexing industries by j, states by s, and time by t, we estimate the following linear equations:

$$\text{Outcome}_{jst} = \sum_r \beta_r \text{Antitrust Enforcement}_{j,s,t-r} + \phi_{j,s} + \gamma_{j,t} + \pi_{s,t} + \epsilon_{jst} \tag{1}$$

$$\text{Outcome}_{jst} = \beta \times \text{Post Antitrust Enforcement}_{j,s,t} + \phi_{j,s} + \gamma_{j,t} + \pi_{s,t} + \epsilon_{jst} \tag{2}$$

where, in both equations, $\phi_{j,s}$ is an industry-state fixed effect, $\gamma_{j,t}$ is an industry-year fixed effect, and $\pi_{s,t}$ is a state-year fixed effect. These fixed effects account for changes to an industry that are common to all geographic locations (e.g., technology or demand), and changes to a state that are common to all industries (e.g., population growth). In all specifications, standard errors are clustered by industry-state.

Equation 1 provides year-by-year estimates of the effect of antitrust enforcement (measured in event time). The variable Antitrust Enforcement$_{j,s,t-r}$ is an indicator variable equal to one in the first year that an industry-state is targeted by a DOJ antitrust case. The

[21] There are non-tradable industries in which firms operate nationally. In such cases, a DOJ antitrust lawsuit can allege that such a firm has engaged in illegal anticompetitive behavior throughout the country. We do not include such cases in our analysis due to the lack of a control group. In addition, there are lawsuits that target several states and these are included in our analysis. For example, if a lawsuit targets waste management providers operating in the Midwest region, then we assign waste management providers in all Midwest states to the treated group and we assign all waste management providers in all states outside the Midwest to the control group.

coefficients of interest in this equation are $\{\beta_r\}$, which are the year-by-year estimates of the effect of antitrust enforcement. The estimates capture the aggregate effects of antitrust enforcement on industry-state outcomes, both the direct effect on prosecuted firms and indirect effects on other existing and potential firms in the same industry-state, relative to controls.

Equation 2 provides an overall measure of the effect of antitrust enforcement. The variable Post Antitrust Enforcement$_{j,s,t}$ is an indicator variable equal to one from the first year that an industry-state is targeted by a DOJ antitrust case and remains equal to one in all subsequent years. The coefficient of interest in this equation is β, which measures the overall effect of antitrust enforcement.

A recent literature highlights potential problems with estimating difference-in-differences regressions when units are treated at different points in time.[22] The econometric problems that arise are due to the use of treated units in the estimation of control coefficients (in our case, fixed effects). To overcome this problem, we estimate our equations using the two-stage estimation approach of Gardner (2020). By this two-stage estimation procedure, we estimate the control coefficients (in our case, fixed effects) using only industry-state-year observations that have not yet been treated (which includes the industry-states that are never treated).[23]

5 Antitrust Enforcement and Economic Activity

In this section, we present the results of our empirical analysis of the effects of DOJ antitrust enforcement on the level of economic activity (measured as employment), business formation, payroll, sales, and the labor share.

In our first set of results, we provide evidence that antitrust enforcement induces a long-run increase in the level of employment, which is the most widely available measure of economic activity. Figure 1 presents year-by-year estimates of the effect of antitrust enforcement on log employment, measured in event time (± 8 years around the filing of the

[22] See, for example, De Chaisemartin and d'Haultfoeuille (2020) and Goodman-Bacon (2021).

[23] Harmon (2022) reviews different estimators for addressing staggered difference-in-differences. Gardner (2020) falls in the category of Regression Imputation (RI) estimators.

DOJ antitrust lawsuit), as presented in Equation 1. The figure shows a clear and immediate increase in log employment in the industry-state targeted by the DOJ antitrust lawsuit after the lawsuit is filed. In the year of the DOJ antitrust lawsuit, employment increases by around 3%. Over the eight years following the DOJ enforcement action employment stabilizes at an increase of around 5%.[24]

Table 3 presents our estimates of the effect of antitrust enforcement on log employment, as presented in Equation 2. Our main specification, presented in Column 2, shows a long-run increase in employment of 5.4%. The estimate of the difference-in-differences analysis presented in Table 3 is similar in magnitude to the estimates in the later years of the year-by-year analysis presented in Figure 1. This comparison implies that there is no later reversion or decline in employment, even though the average post-period length is 25 years.

In addition to the main employment results, Column 1 shows that this effect is relatively unchanged when we estimate Equation 2 using Ordinary Least Squares (point estimate 4.7%) instead of the Gardner (2020) estimation approach. This is the only result of the paper that is estimated using Ordinary Least Squares. In Columns 3 and 4, we repeat the analysis after weighing each industry-state-year cell by log employment in 1985 (Column 3) and by the level of employment in 1985 (Column 4), yielding respective estimates of 5.3% and 7.5% increases in employment following a DOJ antitrust lawsuit. By weighting by 1985 employment, we reduce our reliance on small industries that may have noisier year-to-year changes in employment. These estimates also show that our results are not driven by the DOJ's ability to affect small but economically unimportant industries. If anything, our results suggest that the employment effects are larger in larger industries targeted by the DOJ. In Appendix Table B.1, we show that results hold when estimated in the publicly-available data from CBP, though noise infusion and cell suppression in CBP mean the estimates do not match exactly. Using the CBP data, we also show that results

[24]The figure shows a small increase in log employment in the year prior to the DOJ antitrust lawsuit. This may be a response on the part of firms to the DOJ investigation that ultimately led to the filing of the antitrust lawsuit.

are robust to dropping bid-rigging cases, which represent the most common violation, and to limiting to cases prior to the introduction of the 1993 cartel leniency policy.

In our second set of results, we provide evidence that antitrust enforcement induces a long-run increase in business formation. This supports the Baker, Sallet and Scott Morton (2017) view that dominant incumbents can find ways to suppress entry to sustain elevated profits due to anticompetitive behavior and that antitrust enforcement is needed to break these barriers to entry. Figure 2 presents year-by-year estimates of the effect of antitrust enforcement on the log of the number of establishments, measured in event time (± 8 years around the filing of the DOJ antitrust lawsuit), as presented in Equation 1. The figure shows a clear and gradual increase in the number of establishments in the industry-state targeted by the DOJ antitrust lawsuit starting in the year that the lawsuit is filed and stabilizing eight years later at an increase of nearly 3%.

Table 4 presents our estimates of the effect of antitrust enforcement on different measures of business formation. Column 1 presents results for the log number of establishments and finds a 2.9% increase in the number of establishments. This estimate of the increase is similar in magnitude to the estimates in the later years of the year-by-year analysis presented in Figure 2. This comparison implies that there is no later reversion or decline in the number of establishments, even though the average post-period length is 25 years.

Column 2 presents results for the log number of firms and finds a 4.1% increase in the number of firms. The finding that the increase in the number of firms is greater (in percentage terms) than the increase in the number of establishments is consistent with the entry of new firms that operate fewer than the average number of establishments (as is common for all new firms). Column 3 presents results for log of one plus the number of new establishments, and Column 4 presents results for log of one plus the number of new firms. These columns show even larger effects on the number of new establishments and new firms.

The estimated increases in the number of new establishments and new firms presented in Table 4 are not due to entry of new firms or the construction of new establishments in the immediate aftermath of the DOJ antitrust enforcement. Instead, the results tell us that in each year, including many years later, more new firms and establishments are entering the industry-state targeted by the DOJ antitrust lawsuit. In other words, antitrust enforcement leads to a lasting increase in business dynamism.

In our last set of results, we turn to the Economic Census analysis sample that allows us to measure a wider set of economic outcomes. We provide evidence that antitrust enforcement induces a long-run increase in payroll and the labor share. We further provide evidence that strongly suggests that antitrust enforcement reduces prices.

Table 5 presents our estimates of the effect of antitrust enforcement on log employment, log payroll, log sales, and log labor share, as presented in Equation 2. To allow for consistent comparisons of outcomes, we only compare results derived from the same empirical sample. In Column 1, we find a 4.1% increase in employment using the Economic Census sample, which is consistent with if slightly smaller than the 5.4% increase that we found using the annual LBD analysis sample.

Column 2 presents the results for payroll. The estimated increase in payroll (+5.9%) exceeds the estimated increase in employment (4.1%), suggesting that DOJ antitrust enforcement increases average wages. Increased wages are what we would expect if economic activity increases, driving up demand for workers. In addition to rising employment, an increase in payroll further supports the finding that DOJ antitrust lawsuits boost economic activity in the targeted industry-states.

In Column 3, we find an economically smaller increase in sales that is statistically insignificant (2.5%). While we do not have separate measures for the quantity and price of output, the increase in production inputs (employment), together with the economically smaller (and statistically insignificant) increase in sales, strongly suggests an increase in the quantity of output and, at the same time, a decrease in the price of output. To illustrate this logic, it helps to consider a simple case with constant returns to labor: if inputs into production increase by δ (measured in log points) then the quantity of output should increase by δ.

Therefore, if we find in the data that nominal sales only increased by $\epsilon < \delta$ then we would infer that the quantity of output increased by δ and the price of output declined by[25] $\delta - \epsilon$.

Last, we show that antitrust enforcement increases the labor share of sales: in Column 4, we find a 3.5% increase in the labor share after DOJ antitrust enforcement actions. This indicates that antitrust enforcement has implications for the distribution of income.

To summarise our results, we find clear evidence that DOJ antitrust enforcement induces long-run increases in the level of economic activity (measured as employment), business formation, payroll, and the labor share. We further find an estimated increase in payroll that exceeds the estimated increase in employment, implying that DOJ antitrust enforcement increases average wages. We also find an economically smaller increase in sales that is statistically insignificant. While we do not have separate measures for the quantity and price of output, the increase in production inputs (employment) together with a proportionally smaller (and statistically insignificant) increase in sales, strongly suggests an increase in the quantity of output and at the same time a decrease in the price of output.

6 Conclusion

Policymakers and researchers often debate the effectiveness of U.S. antitrust policy. But there is a lack of systematic evidence linking typical government-instigated antitrust lawsuits to real economic outcomes. A key challenge for empirical research in the area of antitrust enforcement has been the lack of detailed and standardized data on antitrust cases for use in empirical evaluation. To help fill this gap in our understanding of the empirical effects of antitrust enforcement, this paper accomplishes two goals.

[25]This indirect inference of quantity and prices requires two assumptions. First, it assumes that the production of the quantity of output displays constant return to scale. This assumption is commonly used in the literature and in production function estimation and is further supported by empirical evidence. See, for example, Basu and Fernald (1997) for evidence of constant returns to scale at the industry level. Second, it assumes that firms do not substitute labor for capital inputs as a result of the antitrust enforcement action. This assumption could be violated if the enforcement action leads to higher wages, thereby increasing the price of labor relative to capital. In this case, the indirect inference would understate the increase in the quantity of output and also understate the decline in the price of output.

First, we hand-collect and standardize a complete history of 3,055 DOJ antitrust lawsuits covering the time period 1971–2018. In addition to variables related to the legal aspects of each case, we record the location of each antitrust violation and we match each case to a standard industry code. The collection of these additional variables allows us to merge the antitrust enforcement data with data on industry-level outcomes derived from confidential firm- and establishment-level tax records.

Second, using our newly collected data, matched to industry-level outcomes, we provide clear empirical evidence that DOJ antitrust enforcement has a real impact on the economic outcomes of affected industries. We compare outcomes in industry-states (e.g., Waste Management in Massachusetts) targeted by a DOJ antitrust lawsuit to the same industry located in other states that are not targeted by a DOJ antitrust lawsuit (e.g., Waste Management in other states). Using annual data from the Longitudinal Business Database (LBD) and data collected every five years by the Economic Census, we find that DOJ antitrust enforcement induces long-run increases in employment, the labor share, average wages, and business formation, as well as evidence that strongly suggests an increase in the quantity of output and a simultaneous decrease in the price of output.

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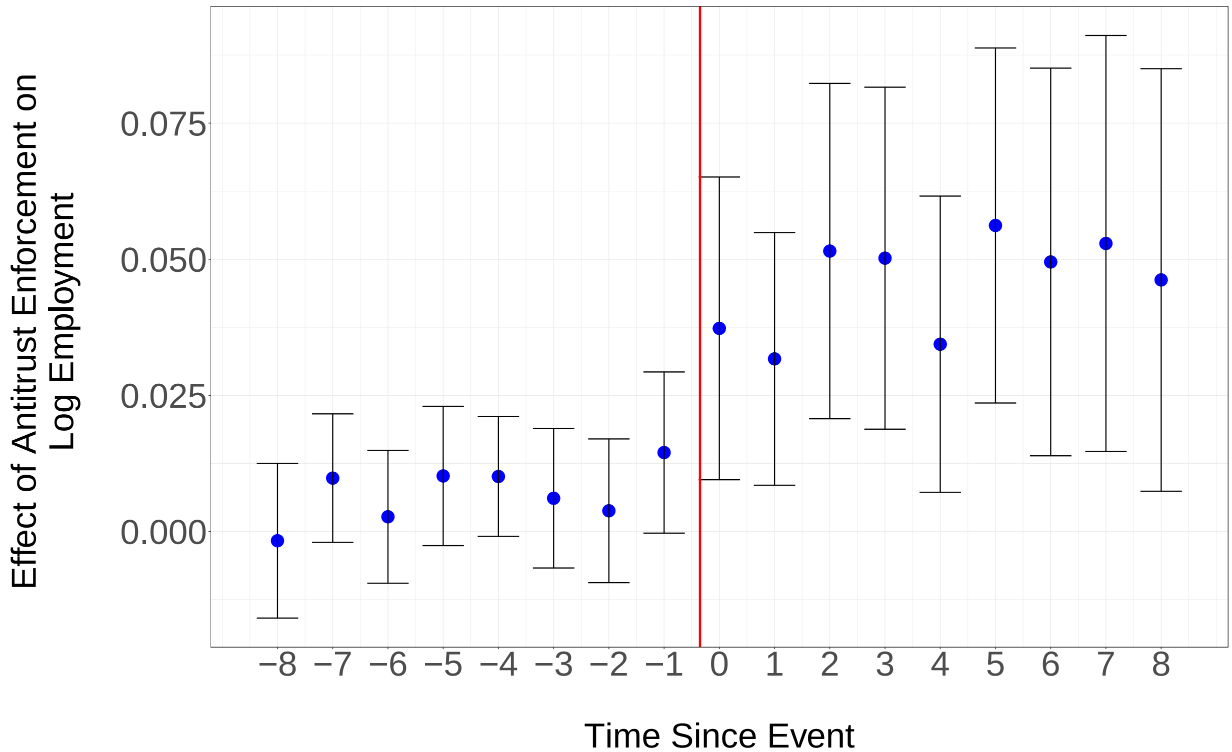


Figure 1: **Effects of Antitrust Enforcement on Industry-Level Employment**
This figure presents year-by-year estimates of the effect of antitrust enforcement on log employment, measured in event time, as presented in Equation 1. The analysis sample combines our hand-collected data on DOJ antitrust enforcement with the LBD and consists of annual data for all non-tradable industries that are targeted by a DOJ antitrust lawsuit at some time during the time period 1976–2015, across all 50 states. The unit of observation is an industry-state-year, where industry is defined as a time-consistent four-digit NAICS industry. The regression equation is estimated using the two-stage estimation procedure of Gardner (2020). Standard errors are clustered by industry-state. See Section 4 for further details on the empirical design, Section 3 for further details of our analysis sample, and Section 5 for further discussion of the results.

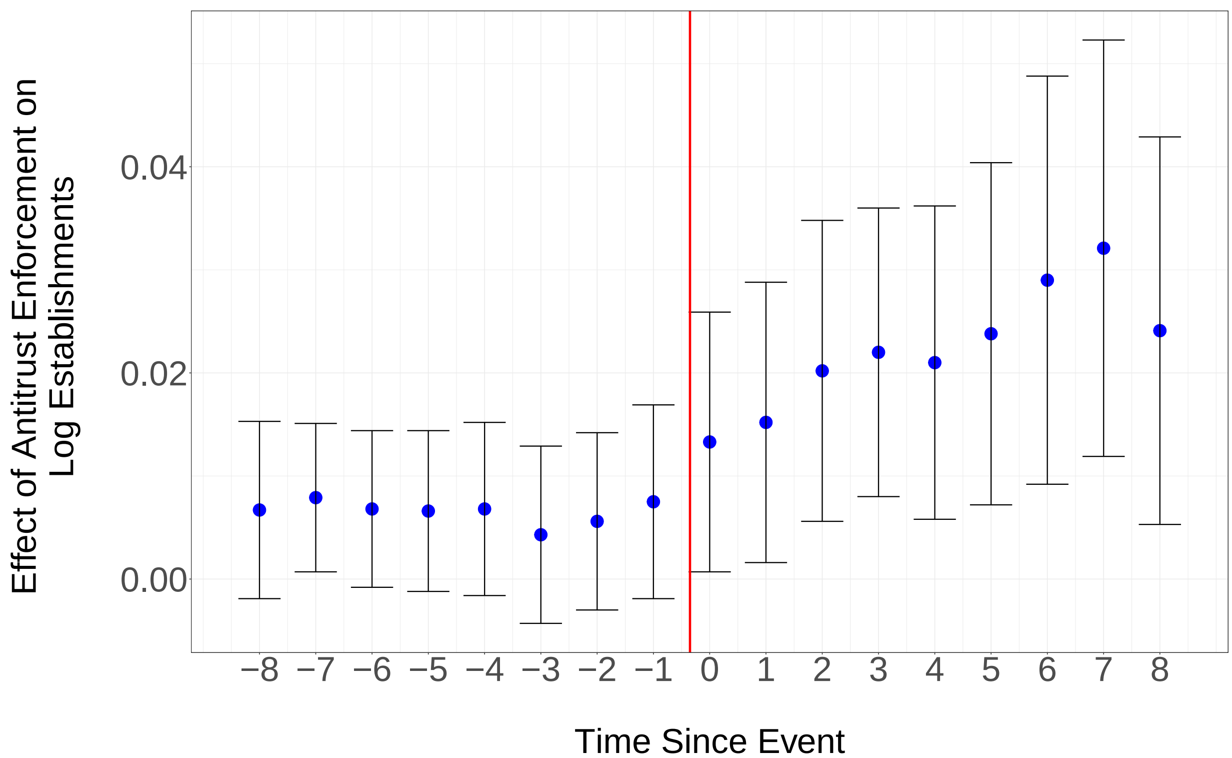
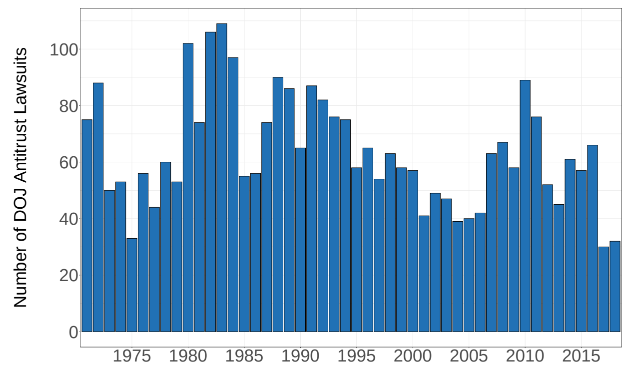
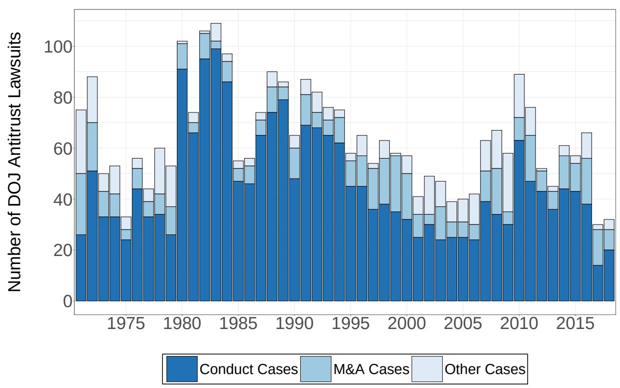


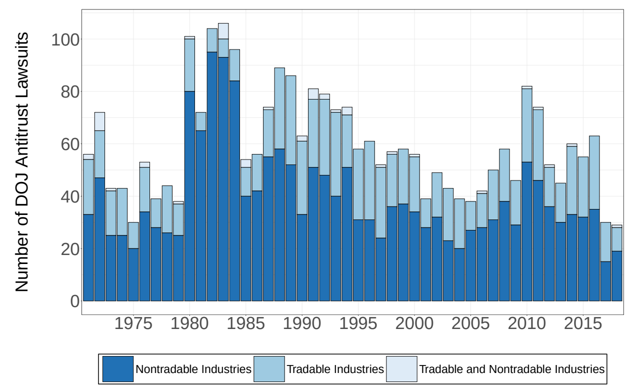
Figure 2: **Effects of Antitrust Enforcement on Industry-Level Business Formation** This figure presents year-by-year estimates of the effect of antitrust enforcement on log number of establishments, measured in event time, as presented in Equation 1. The analysis sample combines our hand-collected data on DOJ antitrust enforcement with the LBD and consists of annual data for all non-tradable industries that are targeted by a DOJ antitrust lawsuit at some time during the time period 1976–2015, across all 50 states. The unit of observation is an industry-state-year, where industry is defined as a time-consistent four-digit NAICS industry. The regression equation is estimated using the two-stage estimation procedure of Gardner (2020). Standard errors are clustered by industry-state. See Section 4 for further details on the empirical design, Section 3 for further details of our analysis sample, and Section 5 for further discussion of the results.



(a) Number of DOJ Lawsuits over Time

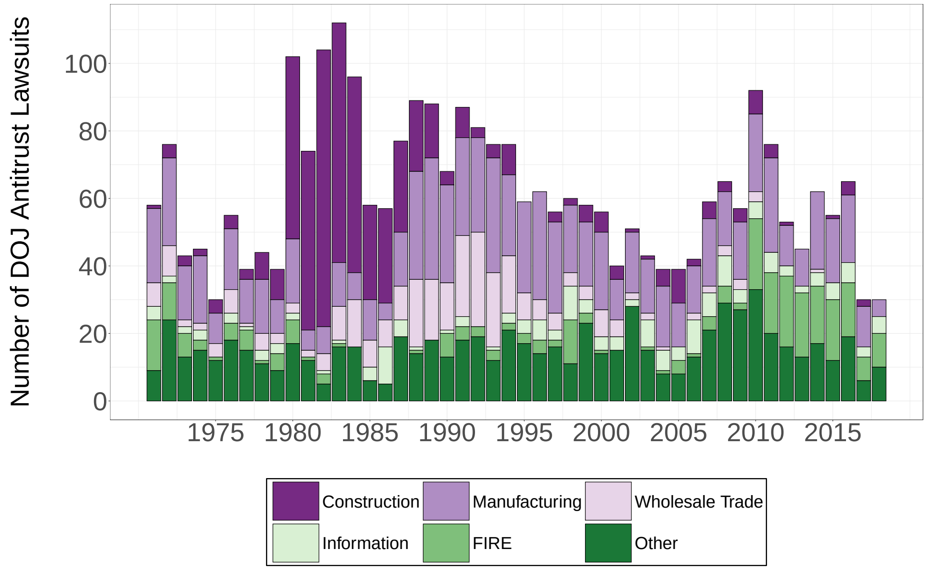


(b) Number of DOJ Lawsuits by Case Type

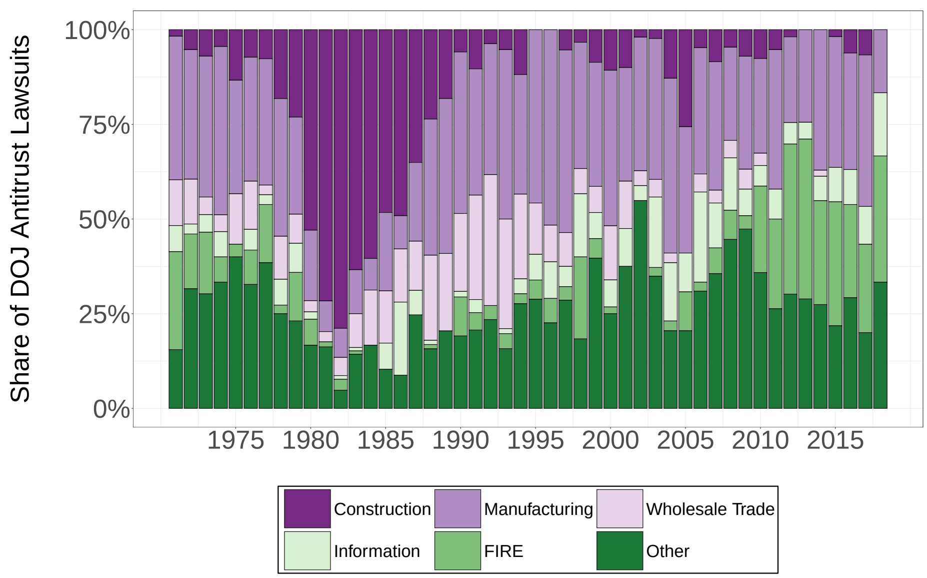


(c) Number of DOJ Lawsuits by Industry Type

Figure 3: **Trends in DOJ Antitrust Lawsuits over Time**
Panel A shows the number of DOJ antitrust lawsuits for each year of the period 1971–2018. The total number of lawsuits over the sample period is 3,055. Panel B provides a breakdown of all DOJ antitrust lawsuits into three mutually exclusive categories: M&A cases, conduct cases, and other cases as defined in Section 2. Panel C provides a breakdown of all DOJ antitrust lawsuits for which we were able to determine an industry code, dividing cases into three mutually exclusive categories: cases that target non-tradable industries, cases that target tradable industries, and cases that target both tradable and non-tradable industries. The number of DOJ antitrust lawsuits for which we were able to determine an industry code is 2,862. The classification of industries into tradable and non-tradable is taken from Barkai and Karger (2020). See Section 2 for further details.

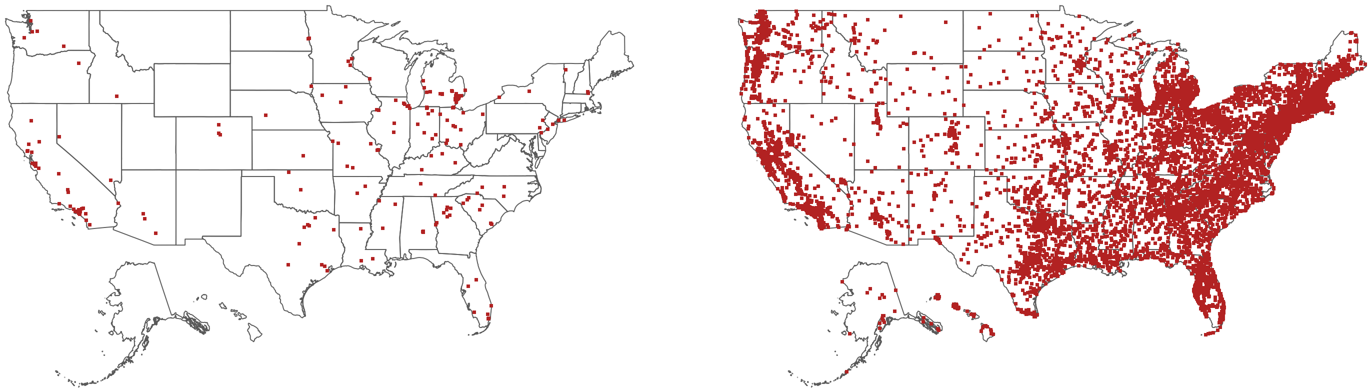


(a) Number of DOJ Lawsuits over Time



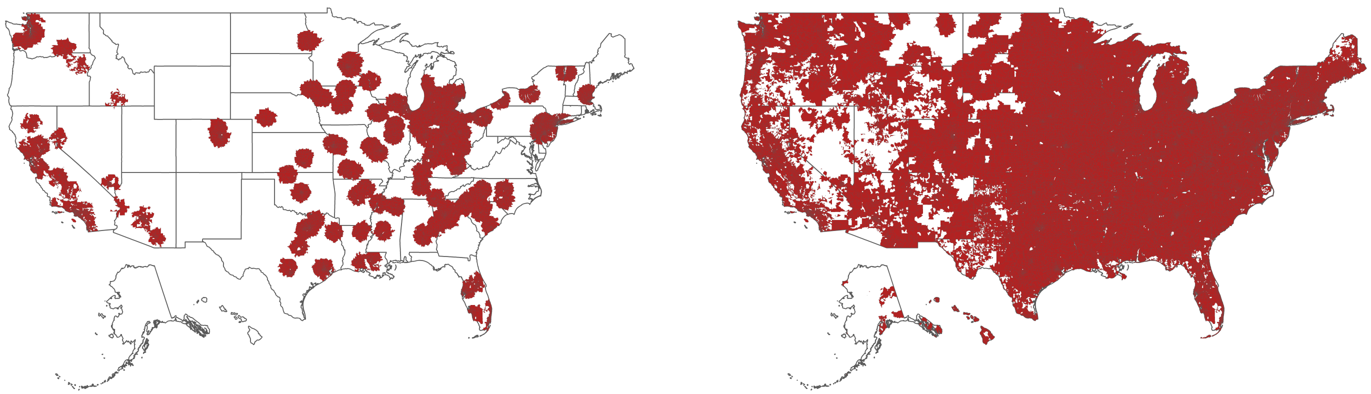
(b) Share of DOJ Lawsuits over Time

Figure 4: **DOJ Antitrust Lawsuits by Sector**
Panel A shows the number of DOJ antitrust lawsuits in each sector for each year of the period 1971–2018. Panel B shows the percentage of DOJ antitrust lawsuits in each sector for each year of the period 1971–2018. The data cover the 2,899 cases for which we could determine a four-digit NAICS industry code. The sectors correspond to NAICS codes beginning with the following two digits (in parentheses): Construction (23), Manufacturing (31, 32, 33), Wholesale Trade (42), Information (51), Finance, Insurance, and Real Estate (52, 53), and Other (all other). See Section 2 for further details.



(a) Automobile Manufacturing (NAICS 336111)

(b) Convenience Stores (NAICS 445120)



(c) Automobile Manufacturing, Nearby Population

(d) Convenience Stores, Nearby Population

Figure 5: **Geographic Footprint of Convenience Stores and Automobile Manufacturing**
Panels A and B show the locations of establishments in the industries Convenience Stores (NAICS 445120) and Automobile Manufacturing (NAICS 336111). Panels C and D shows the locations that are within 50 miles of an establishment in the two industries. Location is defined as a five-digit ZIP Code Tabulation Area (ZCTA). See Section 3 and Barkai and Karger (2020) for further details.

Table 1

Counts and Frequencies of Alleged Violations

This table presents counts and frequencies of DOJ antitrust lawsuits. A single case can allege multiple violations so we report frequencies both as a fraction of cases affected (summing up to more than 100%) and as a fraction of violations (summing up to 100%). See Section 2 for further details.

Violation Category	Violation	N	% cases	% violations
Horizontal Violation	Bid rigging	1,288	42.2%	29.0%
	Price fixing	826	27.0%	18.6%
	Market allocation	477	15.6%	10.8%
	Reciprocity	34	1.1%	0.8%
	Boycott and exclusive dealings	29	0.9%	0.7%
	Other horizontal violation	72	2.4%	1.6%
Exclusionary Practices	Patent or other IP misuse	9	0.3%	0.2%
	Tying and bundling	8	0.3%	0.2%
	Price discrimination	3	0.1%	0.1%
	Predatory pricing	2	0.1%	0%
	Other exclusionary practices	81	2.7%	1.8%
Vertical Violations	Price fixing in vertical markets	22	0.7%	0.5%
	Resale price maintenance	11	0.4%	0.2%
	Other vertical violation	27	0.9%	0.6%
Merger Violations	Lawsuit to completely block a merger	154	5.0%	3.5%
	Lawsuit to partially block a merger	278	9.1%	6.3%
	Violation of premerger notification	67	2.2%	1.5%
Other Violations	Violation of consent decree	24	0.8%	0.5%
	Other violations	511	16.7%	11.5%
	Unclassified	152	5.0%	3.5%

Table 2

Counts and Frequencies of Geographic Scope and Non-tradable Industry Classification

This table presents counts and frequencies of DOJ antitrust lawsuits. Panel A presents the number and frequency of the geographic scope of antitrust violation for six mutually exclusive possible scopes. Panel B presents the number and frequency of all DOJ antitrust lawsuits for which we were able to determine an industry code, dividing our cases into three mutually exclusive categories: cases that target non-tradable industries, cases that target tradable industries, and cases that target both tradable and non-tradable industries. The classification of industries into tradable and non-tradable is taken from Barkai and Karger (2020). See Sections 2 and 3.1 for further details.

(a) Geographic Scope

Geographic Scope	N	Frequency
City	620	20.3%
State	831	27.2%
Several States	318	10.4%
National	654	21.4%
International	366	12.0%
Unknown	266	8.7%

(b) Industry Classification

Industry Classification	N	Frequency
Non-tradable	1,898	66.3%
Tradable	919	32.1%
Both Tradable and Non-tradable	45	1.6%

Table 3

Effects of Antitrust Enforcement on Industry-Level Employment

This table presents estimates of the effect of antitrust enforcement on log employment, as presented in Equation 2. The analysis sample combines our hand-collected data on DOJ antitrust enforcement with the LBD and consists of annual data for all non-tradable industries that are targeted by a DOJ antitrust lawsuit at some time during the time period 1976–2015, across all 50 states. The unit of observation is an industry-state-year, where industry is defined as a time-consistent four-digit NAICS industry. The regressor Post Antitrust Enforcement$_{j,s,t}$ is an indicator variable equal to one from the first year that an industry-state is targeted by a DOJ antitrust case and remains equal to one in all subsequent years. Column 1 presents results from estimating the regression equation using OLS and the remaining three columns present results from estimating the regression equation using the two-stage estimation procedure of Gardner (2020). Column 3 weights observations by log employment in the year 1985. Column 4 weights observations by employment in 1985. Standard errors are clustered by industry-state. See Section 4 for further details on the empirical design, Section 3 for further details of our analysis sample, and Section 5 for further discussion of the results. ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively.

	Dependent variable: Log Employment			
	(1)	(2)	(3)	(4)
Post Antitrust Enforcement	0.0468***	0.0541***	0.0529***	0.0750***
	(0.0161)	(0.0161)	(0.0152)	(0.0241)
Observations (Rounded)	124,000	124,000	124,000	124,000
Industry × Year	Yes	Yes	Yes	Yes
Industry × State	Yes	Yes	Yes	Yes
State × Year	Yes	Yes	Yes	Yes
Weights	Equal	Equal	$\log \text{Empl}_{1985}$	Empl_{1985}
Implementation	OLS	Gardner	Gardner	Gardner
R^2 (Full)	0.9807	0.9786	0.9785	0.9754
R^2 (Within)	0.0005	0.0031	0.0041	0.0241

Table 4

Effects of Antitrust Enforcement on Industry-Level Business Formation

This table presents estimates of the effect of antitrust enforcement on business formation, as presented in Equation 2. The analysis sample combines our hand-collected data on DOJ antitrust enforcement with the LBD and consists of annual data for all non-tradable industries that are targeted by a DOJ antitrust lawsuit at some time during the time period 1976–2015, across all 50 states. The unit of observation is an industry-state-year, where industry is defined as a time-consistent four-digit NAICS industry. The regressor Post Antitrust Enforcement$_{j,s,t}$ is an indicator variable equal to one from the first year that an industry-state is targeted by a DOJ antitrust case and remains equal to one in all subsequent years. Column 1 presents results for log number of establishments, Column 2 presents results for log number of firms, Column 3 presents results for log of 1 + the number of new establishments, and Column 4 presents results for log of 1 + the number of new firms. The regression equations are all estimated using the two-stage estimation procedure of Gardner (2020). Standard errors are clustered by industry-state. See Section 4 for further details on the empirical design, Section 3 for further details of our analysis sample, and Section 5 for further discussion of the results. ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively.

	Dependent variable:			
	Log Estab (1)	Log Firms (2)	Log (1+New Estab) (3)	Log (1+New Firms) (4)
Post Antitrust Enforcement	0.0294***	0.0410***	0.0431***	0.0513***
	(0.0097)	(0.0108)	(0.0130)	(0.0138)
Observations (Rounded)	124,000	124,000	124,000	124,000
Industry × Year	Yes	Yes	Yes	Yes
Industry × State	Yes	Yes	Yes	Yes
State × Year	Yes	Yes	Yes	Yes
R^2 (Full)	0.9918	0.9917	0.9553	0.9553
R^2 (Within)	0.0024	0.0046	0.0010	0.0013

Table 5

Effects of Antitrust Enforcement on Industry-Level Employment, Payroll, Sales, and Labor Share

This table presents estimates of the effect of antitrust enforcement on log employment, log payroll, log sales, and log labor share, as presented in Equation 2. The analysis sample combines our hand-collected data on DOJ antitrust enforcement with the Economic Census and consists of data measured every five years for non-tradable industries in the covered sectors that are targeted by a DOJ antitrust lawsuit at some time during the time period 1977–2012, across all 50 states. The unit of observation is an industry-state-year, where industry is defined as a time-consistent four-digit NAICS industry. The regressor Post Antitrust Enforcement$_{j,s,t}$ is an indicator variable equal to one from the first year that an industry-state is targeted by a DOJ antitrust case and remains equal to one in all subsequent years. The labor share is the ratio of payroll to sales. Column 1 presents results for log employment, Column 2 presents results for log payroll, Column 3 presents results for log sales, and Column 4 presents results for log labor share. The regression equations are all estimated using the two-stage estimation approach of Gardner (2020). Standard errors are clustered by industry-state. See Section 4 for further details on the empirical design, Section 3 for further details of our analysis sample, and Section 5 for further discussion of the results. ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively.

	Dependent variable:			
	Log Employment	Log Payroll	Log Sales	Log Labor Share
	(1)	(2)	(3)	(4)
Post Antitrust Enforcement	0.0411**	0.0592***	0.0245	0.0347***
	(0.0191)	(0.0208)	(0.0217)	(0.0092)
Observations (Rounded)	19,000	19,000	19,000	19,000
Industry × Year	Yes	Yes	Yes	Yes
Industry × State	Yes	Yes	Yes	Yes
State × Year	Yes	Yes	Yes	Yes
R^2 (Full)	0.9753	0.9758	0.9787	0.9548
R^2 (Within)	0.0009	0.0015	0.0002	0.0017

A Antitrust Data Appendix: For Online Publication

This appendix provides additional details of the construction of our comprehensive database of all Department of Justice antitrust lawsuits that occurred between 1971 and 2018. We classify 3,055 Department of Justice (DOJ) antitrust lawsuits provided by the Commerce Clearing House (CCH) Trade Regulation Reporter. For each case, two research assistants classify every variable independently, and a third party reviews each disagreement.

A.1 Data Codebook

Table A.3 presents a list and description of the variables in our hand-collected antitrust data.

In 337 cases, the filing year is not recorded in the case summary.[26] In these cases we take advantage of the sequential ordering of case numbers (assigned based on the filing date) to fill in the missing filing years.

A.2 Additional Data Statistics

For the purpose of creating summary statistics, we impose the following restrictions on the data. First, we restrict the sample to cases where we could determine a four-digit NAICS industry code. This reduces the sample by 156 cases from 3,055 to 2,899. Second, we restrict the sample to cases where we could determine an alleged antitrust violation (inclusive of challenged mergers, exclusive of violations of past consent decrees). This reduces the sample by 212 cases from 2,899 to 2,687.

Statistics by Sector and Industry. Table A.4 presents counts of antitrust cases by NAICS sectors. The sum of the number of cases in the table exceeds the total number of cases because there are a small number of cases that cover seller firms in more than one sector (a total of 58 out of 2,899 cases cover seller firms in two different sectors – the most common combination in these cases with two sectors being Manufacturing and Wholesale

[26] In 149 of these 337 cases, the case summary does not contain information on alleged legal violations.

Trade). Over our sample period, Manufacturing had the greatest number of cases (852) and Education Services had the fewest (3). The table further presents counts of cases in each sector separately for conduct cases and for M&A cases.

Table A.5 presents the 20 four-digit NAICS industries with the greatest number of antitrust cases. Some examples of the top industries include Highway, Street, and Bridge Construction (NAICS 2373, 300 cases), Grocery and Related Product Merchant Wholesalers (NAICS 4244, 120 cases), Motor Vehicle Parts Manufacturing (NAICS 3363, 60 cases), Lumber and Other Construction Materials Merchant Wholesalers (NAICS 4233, 38 cases), and Securities and Commodity Contracts Intermediation and Brokerage (NAICS 5231, 34 cases). The table further presents counts of cases in each industry separately for conduct cases and for M&A cases.

Statistics by State. For the purpose of creating summary statistics by state, we impose a third restriction on the data, on top of the two described above (contains information on industry and alleged antitrust violation). Specifically, we restrict to cases that have a geographic scope of City, State, or Several States (thus excluding cases with a National, International, or Unknown scope). This reduces the sample by 1,023 cases from 2,687 to 1,664 cases.

There are cases in which the case summary does not explicitly list the location(s) of the seller firms or product market(s). Of the 1,664 cases, 66 do not contain information on the location(s) of the product market(s) and 181 do not contain information on the location(s) of the seller firms. In many cases these locations can be imputed. When a case is missing the location of the product market but contains a single location of seller firms, we can impute the location of the product market as the location of the seller firms (this approach can fill in 41 of the missing 66 product markets). The cases with an unknown location after the imputation either have seller firms located in multiple states or are missing information on the location(s) of both the product market(s) and the seller firms.[27] Similarly, when cases

[27]There are some cases that are missing information on the locations of both the product markets and

are missing the location(s) of the seller firms but contains a single location of a product market, we can impute the location of the seller firms as the location of the product market (this approach can fill in 127 of the missing 181 seller firm locations).

Note: Out of an abundance of caution, we do not use these imputations in our empirical analysis.

Table A.6 presents counts of local antitrust cases by state for the 1,664 antitrust cases with a geographic scope of City, State, or Several States. Column 2 provides counts of the location of seller firms. Column 3 provides counts of the location of seller firms after imputing missing values as described above. Column 4 provides counts of the location of product markets. Column 5 provides counts of the location of product markets after imputing missing values as described above. The table further provides counts for the District of Columbia and U.S. territories (these are not used in the empirical analysis).

A few patterns are clear from the data. First, larger states have more cases. The states with the largest number of cases (measured as baseline seller state) are New York (172), Texas (158), California (131), Pennsylvania (101), and Florida (100). The states with the fewest number of cases (measured as baseline seller state) are Maine (3), Vermont (3), Montana (4), Nevada (5), New Mexico (5), and Alaska (7).

Second, as we would expect from cases that are local in nature, the locations of the sellers and the locations of the products very closely align. The counts of baseline seller state and baseline product state (which exclude imputations) have a Spearman rank-rank correlation above 95%. Similarly, when we regress baseline product state counts on baseline seller state counts we get a slope coefficient of 1.01 and an R-squared of 97%.

Statistics for Regression Sample. As described in Section 3.2, our regression analysis focuses on a subsample of cases that meet several criteria, including having a local scope, af-

the seller firms where nonetheless we managed to determine that the geographic scope of the violations is limited to a City, State, or Several States. One example is in the case United States v. William Holman, whose case summary stipulates that the product markets cover "seven states," though the names of these states are not provided.

fecting a nontradable industry, and alleging past anticompetitive conduct (no merger cases).

Tables A.1 and A.2 provide additional breakdowns of these cases. As in the full sample, lawsuits in the regression sample focus mainly on bid-rigging (62% of cases), price fixing (31%), and other market allocation-related violations (19%). Merger-related lawsuits are excluded from the economic analysis because they concern *future* potential anticompetitive activity and therefore concern different economic dynamics. We additionally limit the regression analysis to subnational cases in order to study variation across states within industry. In the regression sample, all lawsuits are local in scope, focusing on violations by firms operating in a city (37% of cases), state (51%), or several states (12%).

A.3 Comparison to DOJ Website Data

The DOJ Antitrust Division website provides an alternative potential source of data on DOJ antitrust lawsuits.[28] The information available on the website includes the date of the filing, the case name (e.g., United States v. San Diego County Veterinary Medical Association), the type of case (e.g., criminal), the alleged violation (e.g., price fixing), an industry name that corresponds to one or several six-digit NAICS industries (in earlier years, four-digit SIC industries), the name of the product, and links to supporting documents (e.g., complaint or plea agreement).

Our hand-collected data provide three significant advantages over the DOJ website. First, our data provide complete coverage of DOJ antitrust lawsuits whereas the DOJ website has limited coverage, especially prior to the mid-1990s. Second, our data provide additional variables not readily available on the DOJ website, including those necessary for our empirical analysis. Third, even when accompanying documents are provided on the DOJ website in machine-readable format, we find that automated attempts to extraction of additional information from these documents falls short. We demonstrate this through our attempt to extract geographic information. In summary, there is no way to compile a comprehensive

[28] https://www.justice.gov/atr/antitrust-case-filings-alpha

dataset without manually reading each case.

Data download and processing. We download all cases that appear on the DOJ Antitrust Case Filings webpage and extract from each page the available standardized information. Figure A.1 presents an example case from the DOJ Antitrust Case Filings webpage. In line with this example, we extract the filing date (June 25, 1996), the name of the case (United States v. American National Can Co. and KMK Maschinen AG), the type of case (Civil-Merger), the alleged violations (Agreements Not to Compete, Customer or Territorial Allocation or Restrictive Resale Practice, Exclusive Dealings and Requirements Contracts, Intellectual Property Abuses, Other Restraint of Trade, and Technology Restrictions), the product market (Laminated Tubes), the verbal description that can be matched to an industry code (Laminated Plastic Plate, Sheet, and Profile Shapes and Laminated Plastics Plate, Sheet, and Shape Manufacturing), and the names and URLs of the attached case documents (Final Judgment, [Proposed] Final Judgment, Competitive Impact Statement, Stipulation, and Complaint). After restricting the data to cases filed between 1971 and 2018, we have 2,053 cases.

While most of the cases on the DOJ website are indeed DOJ antitrust lawsuits, the website does include some additional cases. These include instances in which the DOJ Antitrust Division provides a Statement of Interest even though the U.S. is not a party.[29] We determine the set of DOJ lawsuits using the name of the case (U.S. plaintiff) and the URL of the case (where "us-v-" indicates a U.S. plaintiff). Including the URL in our determination of DOJ antitrust cases is necessary because there are several cases in which the name on the DOJ webpage is incomplete and fails to include the United States.[30] After this first filter we are left with 1,937 cases.

The DOJ assigns a case type to each of the cases that can take one of the following values:

[29] See, for example, Danielle Seaman v. Duke University and Duke University Health System available at `https://www.justice.gov/atr/case/danielle-seaman-v-duke-university-et-al`.

[30] See, for example, the case named "AB Electrolux, Electrolux North America, Inc., and General Electric Company" available at `https://www.justice.gov/atr/case/us-v-ab-electrolux-electrolux-north-america-inc-and-general-electric-company`.

Civil Non-Merger, Civil Merger, Criminal, and Other. We remove cases with a case type Other or with a missing case type. These cases that we remove all appear to be lawsuits that resulted from a DOJ Antitrust Division investigation but in which no antitrust violations are alleged. Indeed, in 40 out of 43 such cases the DOJ website does not contain any alleged violation and even when an antitrust violation is listed it is not related to the specific court proceeding.[31] After this second filter we are left with 1,894 cases.

Comparison of Coverage. We attempt to match each and every antitrust case in our hand-collected data to a case on the DOJ website. We start by matching on case name and filing date, but we expand through manual searches. The reasons to expand beyond automated matching by case name and filing date are (1) there can be differences in case names either due to abbreviations or due to the combination of several lawsuits into one case and (2) the filing date is not always accurate.[32] To ensure maximal coverage, we allow many-to-one matches and carry out the matching procedure in both directions. Furthermore, when we match from our data to the DOJ website we do not filter the DOJ website data prior to matching.

Our hand-collected data appear to be complete. Our data covers 1,893 out of the 1,894 of the cases on the DOJ website with a U.S. plaintiff and a non-Other case type.[33]

The DOJ website is missing many cases included in our hand-collected data. Figure A.2 presents the number of cases in our data in each year, where the cases are split into those that appear on the DOJ website and those that are missing from the DOJ website. The DOJ website is missing nearly half of the DOJ antitrust lawsuits in our data: of the 3,055 cases in our data, only 1,693 (55%) appear on the DOJ website. Almost all of the missing data

[31] See, for example, United States v. Hsuan Bin Chen available at `https://www.justice.gov/atr/case/us-v-hsuan-bin-chen`.

[32] See, for example, U.S. v. Essex Group, Inc., et al. available at `https://www.justice.gov/atr/case/us-v-essex-group-inc-et-al`. In this case, the filing date listed on the DOJ website (January 16, 1980) corresponds to the Competitive Impact Statement. The actual filing date was over a year earlier (September 21, 1978).

[33] The one case we are missing is United States v. Halliburton Company, available at `https://www.justice.gov/atr/case/us-v-halliburton-company`.

are from the early years of the sample (1971–1995). From 1996 onward, the DOJ website is missing only 73 (6%) of the cases out of a total of 1,251.

Limitations to Automated Extraction Attempts. We attempt to extract additional information on each case from the accompanying documents that are provided on the DOJ website. We note that these documents are always available and even when they are available they are not always available in machine-readable format.

We focus on attempting to extract the geographic location of the alleged violations. This variable is necessary for our analysis that compares outcomes of the same non-tradable industry located in different U.S. states. Another advantage of focusing on the extraction of geography is that there is a fixed set of locations and these can be identified through proper nouns (and abbreviations of state names).

We attempt to gather geographic information from supplementary *Complaint* and *Information* files on the DOJ website. In order to give the extraction attempt the best possible chance, we limit our extraction attempts to cases that contain Complaint or Information documents in HTML format. This ensures that there are no errors in the reading of the text.

Almost all cases contain the name of a U.S. state (e.g., New York). This is not surprising since case documents list the name of the court (e.g., Southern District of New York). For this reason, a simple search for state names will likely result in at least one match. This does not however indicate a successful match and the reason is that the name of the court does not always match the states in which the geographic violations occurred.

Our attempt to extract geographic location proceeds in two steps.

In the first step, we attempt to identify cases with a national scope. To do so, we search for phrases that indicate a national aspect. The best match that we found is the phrase "Throughout the United States". This phrase does correlate with our hand-collected classification of national cases, but it has some false positives and many false negatives.

One example of a false positive is "U.S. v. Michael Beberman,"[34] in which the phrase "Throughout the United States" refers to the location of the firm's suppliers. In other examples of false positives, the phrase "Throughout the United States" refers to the location of the owners of the firm.

More importantly, there are many false negatives. These are many cases that we are able to manually classify as national in our hand-collected data, but that do not contain the phrase "Throughout the United States". In such cases, we looked for other phrases to help with the classification, but we do not find any reliable way of determining whether a case is of national scope.

In the second step, we attempt to identify the state or set of states that match the geographic location(s) of the seller or the geographic location of the violation. When a case is limited to a single state our extraction of state names leads to successful match of over 80%. When our hand-collected data indicates multiple states, we are able to match these states in the DOJ website data in around 70% of the cases. It is worth noting again that we are conditioning on the set of cases that are not national in scope, even though we can't determine this in the DOJ data.

In summary, even when accompanying documents are provided on the DOJ website in machine-readable format, we find that attempts to extract geographic information falls short. From this we conclude that there is no way to compile a comprehensive dataset without the manually reading each case.

[34] https://www.justice.gov/atr/case/us-v-michael-beberman

Table A.1

Counts and Frequencies of Alleged Violations in Regression Sample

This table presents counts and frequencies of DOJ antitrust lawsuits in the regression sample. A single case can allege multiple violations and for this reason the sum of counts exceeds the total count of DOJ antitrust lawsuits and the sum of frequencies exceeds 100%. See Sections 2 and 3 for further details.

Violation Category	Violation	N	Frequency
Horizontal Violation	Bid rigging	291	62%
	Price fixing	144	30.7%
	Market allocation	89	19%
	Boycott, refusal to deal, or exclusive dealings	7	1.5%
	Reciprocity	5	1.1%
	Other horizontal violation	19	4.1%
Exclusionary Practices	Tying and bundling	1	0.2%
	Price discrimination	2	0.4%
	Patent or other IP misuse	-	-
	Predatory pricing	-	-
	Other exclusionary practices	15	3.2%
Vertical Violations	Price fixing in vertical markets	6	1.3%
	Resale price maintenance	3	0.6%
	Other vertical violation	3	0.6%
Merger Violations	-	-	-
Other Violations	Other violations	62	13.2%
	Violation of consent decree	-	-
	Unclassified	-	-

Table A.2

Counts and Frequencies of Geographic Scope in Regression Sample

This table presents counts and frequencies of DOJ antitrust lawsuits in the regression sample. There are three mutually exclusive possible scopes: city, state, and several states. By construction, cases with national, international or unknown scope are excluded from the regression sample. Note also that by construction, only nontradable industries are included in the regression. See Sections 2 and 3 for further details.

Geographic Scope	N	Frequency
City	173	36.9%
State	238	50.7%
Several States	58	12.4%

Table A.3: Data Codebook

Variable Category	Variable	Description
Legal Identifier	Case number	Commerce Clearing House case ID number. Note: These numbers are assigned sequentially based on case filings.
Legal Identifier	Case name	Name of the court case.
Legal Identifier	Filing date	Date the case was filed in court.
Legal Identifier	Name of district court	Name of the district court where the case was filed.
Legal Identifier	Docket number	Docket number of the case.
Legal Classification	Type of case	Takes one of the following values: Criminal, Civil, Other, or No Information.
Legal Classification	Legal code	The legal act and section of the act under which the case is brought. Legal act takes one of the following values: Sherman Act, Clayton Act, Robinson-Patman Act, Hart-Scott-Rodino Act, Other, or No Information.

Continued on next page

Table A.3: Data Codebook (continued from previous page)

Variable Category	Variable	Description
Legal Classification	Alleged violation	Alleged legal violation. See Table 1 for a complete list of alleged violations.
Legal Outcome	Outcome of district court	Takes one of the following values: Pleaded Guilty, Nolo Contendere, Dismissed, Dropped, Enjoined, Plea Agreement, Found Guilty, Found Not Guilty, Consent Decree, Other, or No Information.
Legal Outcome	Decision date	Date on which the outcome of the district court case was decided.
Legal Outcome	Fines imposed	Dollar value of the fines imposed. Note: When a case contains multiple fines, we separately collect each fine.
Legal Outcome	Jail sentence imposed	Jail sentence imposed, measured in months. Note: When a case contains multiple jail sentences, we separately collect each jail sentence.
Legal Outcome	Probation imposed	Probation sentence imposed, measured in months. Note: When a case contains multiple probation sentences, we separately collect each probation sentence.
Geography	Seller state	Location of the seller or sellers. Measured as U.S. state or states.

Continued on next page

Table A.3: Data Codebook (continued from previous page)

Variable Category	Variable	Description
Geography	Product state	Location where the products are sold. Measured as U.S. state or states.
Geography	Geographic scope	Geographic scope of the alleged violation. Takes one of the following values: City, State, Several States, National, International, or No Information.
Industry	NAICS4	Four-digit NAICS industry code of the seller. Note 1: When a case contains multiple industries, we separately collect each industry. Note 2: The collection of this variable is based on a manual comparison of the case summary to industry descriptions as provided by the official U.S. government NAICS manual.
Appellate Court	Appeal of verdict to appellate court	Binary variable indicating whether the final verdict of the district court was appealed to an appellate court.
Appellate Court	Name of appellate court	Name of the appellate court to which the final verdict of the district court was appealed.

Continued on next page

Table A.3: Data Codebook (continued from previous page)

Variable Category	Variable	Description
Appellate Court	Date of appeal to appellate court	Date on which the final verdict of the district court was appealed to an appellate court.
Appellate Court	Who appealed verdict to appellate court	Takes one of the following values: U.S., Defendant, Other, or No Information.
Appellate Court	Appellate court decision	Text describing the decision of the appellate court.
Appellate Court	Other appeal to appellate court	Binary variable indicating whether the case involves an appeal to an appellate court that was not an appeal of the final verdict.
Supreme Court	Appeal of verdict to Supreme Court	Binary variable indicating whether the final verdict of the appellate court was appealed to the Supreme Court.
Supreme Court	Date of appeal to Supreme Court	Date on which the final verdict of the appellate court was appealed to the Supreme Court.
Supreme Court	Who appealed verdict to Supreme Court	Takes one of the following values: U.S., Defendant, Other, or No Information.

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Table A.3: Data Codebook (continued from previous page)

Variable Category	Variable	Description
Supreme Court	Supreme Court decision	Text describing the decision of the Supreme Court.
Supreme Court	Other appeal to Supreme Court	Binary variable indicating whether the case involves an appeal to the Supreme Court that was not an appeal of the verdict of an appellate court.

Table A.4: Antitrust Cases by Sector

Sector	Description	All Cases	Conduct Cases	M&A Cases
11	Agriculture, Forestry, Fishing and Hunting	31	25	6
21	Mining, Quarrying, and Oil and Gas Extraction	41	19	22
22	Utilities	27	19	8
23	Construction	532	525	7
31-33	Manufacturing	852	663	189
42	Wholesale Trade	289	271	18
44-45	Retail Trade	134	123	11
48-49	Transportation and Warehousing	147	127	20
51	Information	160	62	98
52	Finance and Insurance	133	76	57
53	Real Estate and Rental and Leasing	135	133	2
54	Professional, Scientific, and Technical Services	66	50	16
56	Administrative, Support, and Waste Management	81	56	25
61	Educational Services	3	3	0
62	Health Care and Social Assistance	47	38	9
71	Arts, Entertainment, and Recreation	11	4	7
72	Accommodation and Food Services	11	8	3
81	Other Services (except Public Administration)	31	24	7
92	Public Administration	20	18	2

Table A.5: NAICS Industries with Greatest Number of Antitrust Cases

NAICS4	Description	All Cases	Conduct Cases	M&A Cases
2373	Highway, Street, and Bridge Construction	300	300	0
4244	Grocery and Related Product Merchant Wholesalers	120	117	3
2382	Building Equipment Contractors	97	96	1
5312	Offices of Real Estate Agents and Brokers	91	91	0
3115	Dairy Product Manufacturing	60	55	5
3363	Motor Vehicle Parts Manufacturing	60	54	6
5621	Waste Collection	45	27	18
5121	Motion Picture and Video Industries	43	26	17
5313	Activities Related to Real Estate	43	43	0
3251	Basic Chemical Manufacturing	42	36	6
3273	Cement and Concrete Product Manufacturing	40	36	4
5221	Depository Credit Intermediation	40	7	33
2371	Utility System Construction	38	34	4
3344	Semiconductor and Other Electronic Component Manufacturing	38	36	2
4233	Lumber and Other Construction Materials Merchant Wholesalers	38	34	4
4811	Scheduled Air Transportation	37	29	8
3121	Beverage Manufacturing	35	26	9
2379	Other Heavy and Civil Engineering Construction	34	33	1
5231	Securities and Commodity Contracts Intermediation and Brokerage	34	33	1
4238	Machinery, Equipment, and Supplies Merchant Wholesalers	33	30	3

Table A.6: Local Antitrust Cases by State

	Seller State		Product State	
State	Baseline	Including Imputation	Baseline	Including Imputation
Alabama	45	46	49	49
Alaska	7	7	13	13
Arizona	20	22	19	19
Arkansas	9	9	13	14
California	131	139	139	140
Colorado	26	27	29	29
Connecticut	22	24	36	36
Delaware	18	18	16	16
Florida	100	105	120	122
Georgia	88	97	102	103
Hawaii	11	12	10	10
Idaho	7	7	8	8
Illinois	69	75	70	72
Indiana	26	30	56	56
Iowa	25	27	37	37
Kansas	27	30	37	37
Kentucky	31	33	44	44
Louisiana	21	26	49	53
Maine	3	3	6	6
Maryland	38	39	34	34
Massachusetts	31	31	25	25

Continued on next page

Table A.6: Local Antitrust Cases by State (continued from previous page)

	Seller State		Product State	
State	Baseline	Including Imputation	Baseline	Including Imputation
Michigan	31	34	46	49
Minnesota	18	18	17	17
Mississippi	16	17	27	28
Missouri	34	39	36	37
Montana	4	5	9	9
Nebraska	26	26	29	29
Nevada	5	5	14	14
New Hampshire	7	7	10	10
New Jersey	71	73	93	97
New Mexico	5	5	10	10
New York	172	185	180	189
North Carolina	95	100	104	104
North Dakota	7	8	10	10
Ohio	63	67	70	70
Oklahoma	21	23	32	32
Oregon	9	9	17	17
Pennsylvania	101	111	111	113
Rhode Island	7	7	12	12
South Carolina	35	38	48	48
South Dakota	8	10	16	16
Tennessee	63	66	67	69

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Table A.6: Local Antitrust Cases by State (continued from previous page)

	Seller State		Product State	
State	Baseline	Including Imputation	Baseline	Including Imputation
Texas	158	168	148	152
Utah	11	13	14	14
Vermont	3	3	10	10
Virginia	72	73	87	88
Washington	16	17	29	31
West Virginia	10	10	15	15
Wisconsin	15	15	23	23
Wyoming	8	9	11	11
District of Columbia	11	14	17	18
American Samoa, Guam, Puerto Rico, and Virgin Islands	25	27	13	13
Unknown	181	54	66	25

U.S. V. AMERICAN NATIONAL CAN CO. AND KMK MASCHINEN AG

Final Judgment (December 12, 1996)

[Proposed] Final Judgment (June 25, 1996)

Competitive Impact Statement (June 25, 1996)

Stipulation (June 25, 1996)

Complaint (June 25, 1996)

Case Open Date:
Tuesday, June 25, 1996

Case Name:
United States v. American National Can Co. and KMK Maschinen AG

Case Type:
Civil Non-Merger

Case Violation:
Agreements Not to Compete
Customer or Territorial Allocation or Restrictive Resale Practice
Exclusive Dealings and Requirements Contracts
Intellectual Property Abuses
Other Restraint of Trade
Technology Restrictions

Market:
LAMINATED TUBES (I.E, TOOTHPASTE TUBES)

Industry Code:
Laminated Plastic Plate, Sheet, and Profile Shapes
Laminated Plastics Plate, Sheet, and Shape Manufacturing

Component:
Antitrust Division

Case Documents:
Final Judgment
[Proposed] Final Judgment
Competitive Impact Statement
Stipulation
Complaint

Updated July 13, 2015

Figure A.1: **Example Case from the DOJ Antitrust Case Filings Webpage**

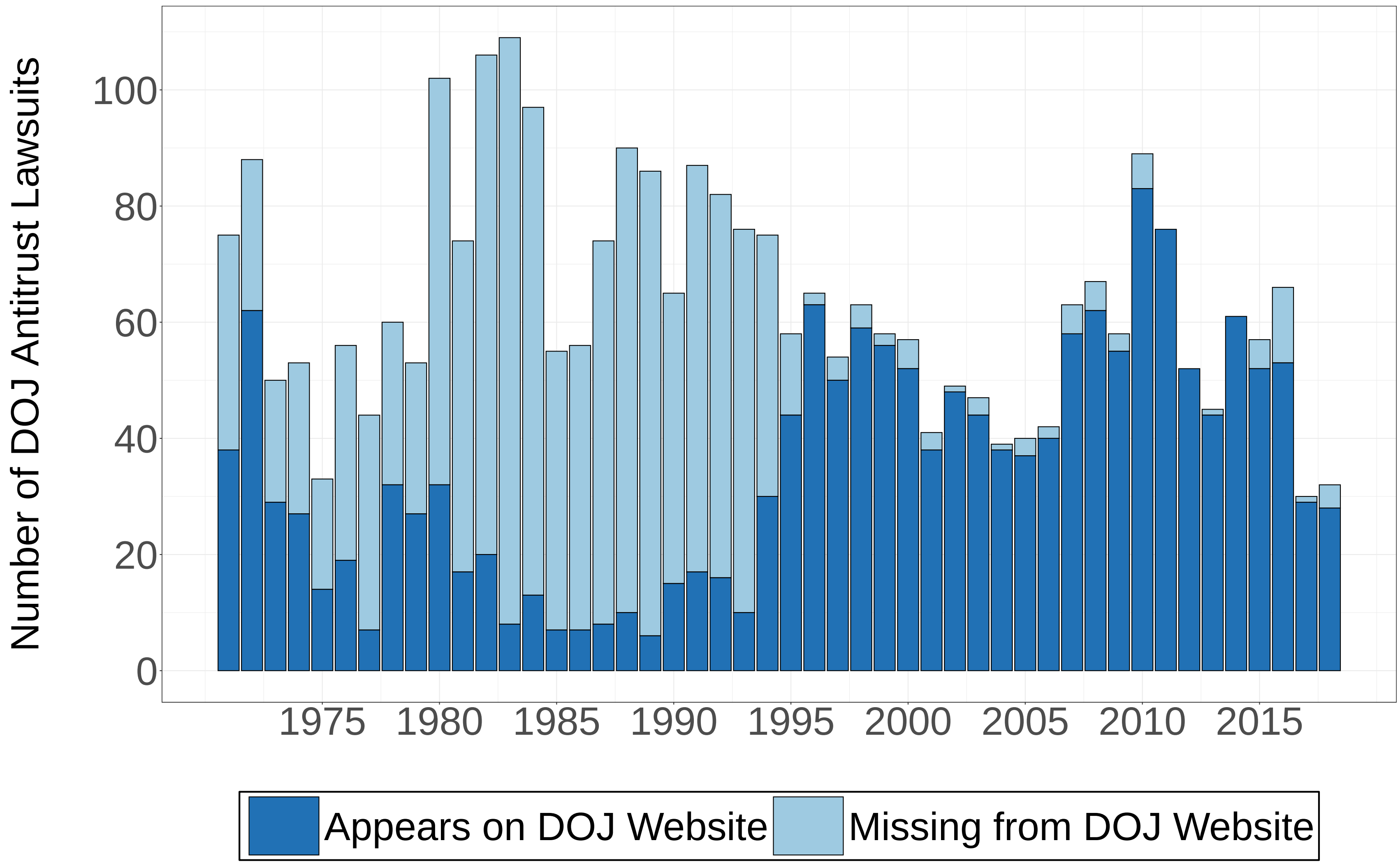

Figure A.2: **Comparison of Our Data to DOJ Antitrust Case Filings Webpage**

B CBP Analysis

The U.S. Census publicly provides aggregated, noise-infused employment data via the County Business Patterns (CBP) series. The CBP does not allow for time-consistent industry classification (an issue solved by our use of the LBD for main results). Also, data-masking through noise infusion and cell suppression make the CBP suboptimal for analysis. Nonetheless, in this appendix we rerun our results in this public dataset and find effects in-line with our results using restricted data. Furthermore, we use the CBP to address some subsample questions.[35] Specifically, we show that employment results are not driven by bid-rigging cases or by post-1993 leniency cases. The noisiness of CBP precludes us from using it for narrower subsample analysis.

[35]The Census Bureau limits the number of coefficients that may be disclosed for a given project, complicating subsample analysis in the microdata.

Table B.1

Effects of Antitrust Enforcement on Industry-Level Employment: Subsamples Using CBP

This table presents estimates of the effect of antitrust enforcement on log employment, based on Equation 2 and using CBP data rather than LBD microdata. The regressor Post Antitrust Enforcement$_{j,s,t}$ is an indicator variable equal to one from the first year that an industry-state is targeted by a DOJ antitrust case and remains equal to one in all subsequent years. Columns 1 and 2 present results using the same set of cases as in Table 3. Columns 3 and 4 drop bid-rigging cases. Columns 5 and 6 restrict the sample to pre-1993, prior to the current cartel leniency policy. Odd columns use an OLS estimation and even columns use the two-stage estimation procedure of Gardner (2020). Standard errors are clustered by industry-state, where industry is four-digit NAICS code (though we are unable to implement a time-consistent definition as in our main regressions). ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively.

	Dependent variable: Log Employment					
	(1)	(2)	(3)	(4)	(5)	(6)
Post Antitrust Enforcement	0.0438***	0.0367**	0.0584***	0.0704***	0.0331*	0.0365*
	(0.0151)	(0.0157)	(0.0195)	(0.0202)	(0.0200)	(0.0196)
Observations (Rounded)	109900	109900	84100	84100	81300	81300
Industry × Year	Yes	Yes	Yes	Yes	Yes	Yes
Industry × State	Yes	Yes	Yes	Yes	Yes	Yes
State × Year	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Equal	Equal	Equal	Equal	Equal	Equal
Implementation	OLS	Gardner	OLS	Gardner	OLS	Gardner
Sample	All cases	All cases	Drop bid-rigging	Drop bid-rigging	Pre-1993	Pre-1993
R^2 (Full)	0.9727	0.9706	0.9759	0.974	0.9769	0.9746
R^2 (Within)	0.0003	0.001	0.0004	0.0026	0.0001	0.0012