Antitrust, Market Power, and U.S. Macroeconomic Outcomes*

Brian Amorim Cabaço† and Morten O. Ravn‡

Click here for the latest version

November 7, 2025

Abstract

We provide a narrative record of U.S. federal antitrust indictments from the mid-1950s to 2023. We document a fundamental shift in antitrust enforcement from civil non-merger and criminal cases toward civil merger cases around 1980. Focusing on publicly listed firms, we estimate significant negative abnormal returns following indictments and use these to construct a measure of antitrust activity. Estimates of the dynamic impact at the 2-digit sector and aggregate levels show that higher antitrust activity stimulates competition, with contrasting effects between case types. Civil non-merger and criminal cases are associated with higher markup dispersion, lower R&D investment, and reduced economic activity at both levels. Civil merger cases are instead associated with lower markup dispersion, and while they reduce activity in treated sectors, they stimulate aggregate GDP, consumption, and R&D investment.

JEL Classification: C32, E30, L16, L40

Keywords: Market power, business cycles, antitrust, dynamic causal effects.

*We are grateful for comments received from seminar audiences at the Hydra 2023 Conference, Society of Economic Dynamics Annual Conference, 2024, the 2023 Spanish Macroeconomics Network Meeting, Paris School of Economics, the University of Texas Austin, SciencesPo, LUISS, Mannheim University, and at the IIES. We thank Alice Avenale, Marina Kotsa or Koca and Diego Almonacid Lovera for excellent research assistance. Ravn acknowledges financial support from ERC Project BUCCAC - DLV 8845598.

†Dept. of Economics, UCL. Email: brian.amorim.17@ucl.ac.uk.

‡Dept. of Economics, UCL and the CEPR. Corresponding author. Email: m.ravn@ucl.ac.uk.

1 Introduction

Much recent empirical evidence has pointed towards increased market power of firms in the U.S. economy. There is comparatively less empirical evidence at a broader level on the impact of this trend. In this paper we aim to provide empirical evidence on this issue. We do so by exploiting the fact that the U.S. has a long history of antitrust legislation and enforcement aimed at addressing the potentially harmful effects of business concentration. Variations in antitrust activity should therefore be related to market power. We construct a dataset of the universe of federal antitrust indictments in the U.S. since the mid-1950s, and, focusing on cases involving publicly listed companies, we estimate their impact on firm valuations. From these estimates, we derive measures of antitrust activity which we utilize to estimate the relationship between antitrust, market power, and macroeconomic outcomes.

Antitrust legislation in the U.S. dates back to the Interstate Commerce Act of 1887 introduced to regulate the U.S. railway industry which was considered to engage in unreasonable business practices. Broader legislation followed swiftly when the U.S. Congress passed the Sherman Act of 1890 which was aimed at preventing monopolization and collusion in U.S. commerce at large. In 1914, further legislation was introduced with the passing of the Clayton Act and the Federal Trade Commission Act. The Clayton Act specified a number of business practices considered anti-competitive while the Federal Trade Commission Act established the Federal Trade Commission. These three acts, and later amendments, still provide the backbone of antitrust legislation in the U.S. while their interpretation has developed over time through the U.S. judicial system. At the federal level, the antitrust laws are enforced by the antitrust authorities, the Antitrust Division of the U.S. Department of Justice (ATR of the USDOJ) and the Federal Trade Commission (FTC). The antitrust cases pursued by these institutions are well documented and offer extensive information about the antitrust authorities' efforts to address anti-competitive behaviors in U.S. commerce.

We exploit the richness of the antitrust enforcement history to investigate the links between antitrust, competition and economic outcomes. We collect data on the universe of antitrust cases launched by the Federal Trade Commission and by the U.S. Department of Justice for sample periods going up to the end of 2023 and starting in 1954 and 1957, re-

spectively. In this sample period, more than 6,300 cases were launched by the FTC and the USDOJ involving close to 16,000 defendants out of which more than 11,500 were corporate firms. We make a broad cut of the antitrust cases into civil non-merger and criminal cases on the one hand, and civil merger cases on the other hand. The former include cases related to the Sherman Act Section 1 violations such as price fixing, bid rigging, market and customer allocations, boycotts and tying and restraints on supply, Sherman Act Section 2 violations related to monopolization, violations of the Clayton Act Section 2 (price discrimination), Section 3 (tying and exclusivity), and Section 8 (interlocking boards), as well as those specified in later amendments. Civil mergers and acquisition cases relate to violations of Section 7 of the Clayton Act, the Hart-Scott-Rodino Improvements Act of 1976, but also to certain violations of the Sherman Act Sections 1 and 2 as well as Section 5 of the Clayton Act. More broadly, civil non-merger and criminal cases relate to the use of unfair business practices while mergers and acquisitions relate to the formation of firms or asset acquisitions that "substantially lessen competition or tend to create monopoly."

An interesting finding from our record of antitrust activity is that over the more than 60-year sample period studied, there has been a fundamental change in antitrust focus with a significant shift away from civil non-merger and criminal cases towards civil merger and acquisitions cases. This change occurs around 1980 and is particularly evident as far as publicly listed firms are concerned. Another interesting fact is that publicly listed firms indicted for antitrust violations are large relative to other publicly listed firms and are concentrated among the top three deciles of the size distribution for such public firms whether measured by sales, employees, or assets.

We exploit the data on antitrust violations to build an indicator of antitrust activity which we relate to market power and outcomes either at the sector-level or at the level of the aggregate economy. The focus on outcomes at a broader level implies that we cannot study each of the antitrust cases with the richness of analysis applied in the empirical industrial organization literature. Instead, we focus on bringing out results at a more aggregated level by taking advantage of the variations in antitrust enforcement across sectors and time. We carry out our analysis in three steps. First, we estimate how antitrust indictments impact on firm valuations from which we construct a measure of antitrust activity from estimates of

abnormal returns on indicted firms' equity in windows around the case opening dates. We then relate these antitrust indicators to outcomes of the U.S. economy at the sector-level and at the aggregate level.

Our estimates of the abnormal returns on firm equity due to antitrust indictments exploit a high frequency event-study approach. For each indicted firm, we first estimate a Fama-French 3-factor model of expected returns using data for sample periods prior to the indictments. Abnormal returns are then measured as realized returns less expected returns in windows around the case opening dates. Our results indicate highly significant negative excess returns on the case opening days as well as on the subsequent trading day. We also find some evidence of negative returns two days ahead of the case opening days. In terms of cumulated excess returns, we find that civil non-merger and criminal cases on average induce a negative excess return around negative one percent at forecast horizons from 50 to 60 trading days, while at this horizon, the average cumulated excess returns for civil merger cases is around -2 percent. Given the size of our sample, more than 1,200 firms for either type of offense, while there are variations across cases, the estimates of the cumulated excess returns are highly statistically significant. From these estimates we derive measures of antitrust activity by summing (weighted by sampling uncertainty) the implied impact on the firms' market valuations across firms and normalizing these measures by the total market cap of U.S. public firms.

We then relate the antitrust activity measures to outcomes in terms of indicators of competition, activity, investment, productivity and markup dispersion at the 2-digit sector-level. For this purpose, we estimate dynamic responses to antitrust activity on the basis of panel local projections. Focusing on the 2-digit level, we find that higher antitrust activity induces a decline in indicators of market power such as markups and market concentration (Herfindahl indices) regardless of the type of antitrust violation that we focus upon. Thus, when a sector is more heavily scrutinized for anti-competitive behavior, market power declines as intended. In the short run, we also find that both types of antitrust indicators induce a decline in the treated sectors' real output and productivity (as measured by estimates of TFPR). However, we find contrasting effects between the two types of antitrust violations on other outcomes. In particular, we find that in response to more intense antitrust activity

related to civil non-merger and criminal cases, capital expenses of publicly listed firms in the relevant sector relative to other sectors decline while markup dispersion rises. In contrast, higher antitrust activity related to civil merger cases, spurs capital expenses and lead to a decline in markup dispersion. We show that the results are robust to measurements of markups, to excluding the early part of the sample which displayed unusually high antitrust activity, and to exclusions of sectors such as Finance and Real Estate or Construction for which competition measures may be harder to estimate. These results are therefore indicative of pro-competitive effects of civil merger related antitrust indictments which also spur investment and generate a decline in misallocation, while civil non-merger and criminal cases appear to deter investment and generate worsening misallocation while stimulating competitive pressures.

An important concern is that the sector-level results hide spillovers and general equilibrium effects due to the "missing constant term." For that reason we then study aggregate outcomes on the basis of local projections by aggregating the antitrust measures across sectors. In this case we study the impact on a rich set of outcomes such as real GDP, consumption and investment, R&D spending, the real wage, unemployment and average labor productivity. Consistently with the sector-level outcomes, we find that increased antitrust activity gives rise to a decline in markups and in real corporate profits, thus indicating that antitrust promotes competition. However, the contrasts between the impact of antitrust activity in terms civil non-merger and criminal cases relative to civil mergers are even more evident. We find that the latter are associated with a persistent rise in aggregate real GDP, consumption and investment, and with a stimulus of R&D spending and productivity whether measured by TFPR or average labor productivity. Accompanying these, we also find a rise in real wages and a temporary decline in unemployment, alongside a short-lived decline in overall markup dispersion. For civil non-merger and criminal cases, we instead find a decline in real activity, consumption, investment and R&D spending while unemployment rises and real wages decline. Moreover, for this type of antitrust violation, we estimate a persistent rise in markup dispersion and a significant decline in productivity. We show that these results are robust to measurement issues related to the estimates of markups and productivity and other issues and we also investigate robustness along the lines of the sources

of frequency of observation. Regarding the latter, all our baseline estimates are based on annual data since we estimate markups and TFPR from Compustat data. An alternative is to proxy markups by the inverse of the labor share of income in which case we estimate aggregate outcomes from quarterly data. One issue that arises when examining quarterly data is that in this case we cannot control for misallocation through markup dispersion nor for market concentration. Nonetheless, we show that many of our results are robust to studying the impact of antitrust on quarterly data.

1.1 Related Literature

A number of papers have studied trends in the number of antitrust cases pursued by the FTC and/or the USDOJ over time, see e.g. Posner (1970), Gallo et al (2000), or Ghosal (2011). The classic study of Posner (1970) is perhaps the most complete of these studies covering both the USDOJ (for the sample period 1890-1969), the FTC (1915-1954) as well as Private Antitrust Cases (1890-1969). Gallo et al (2000) instead focus on the USDOJ and records antitrust cases for the 1955-1997 sample. Our study thus adds 27 years of data on the antitrust cases raised by the USDOJ and 70 years to the data on the FTC.

An existing literature has also used event-study approaches to estimate the impact on firm valuations of antitrust indictments. Bosch and Eckard (1991) estimate average excess return and average cumulated excess returns for 127 firms indicted for price fixing under Section 1 of the Sherman Act in the 1962-1980 period. Firm-by-firm event-study estimators have been applied previously in the antitrust literature. Bosch and Eckard (1991) use a similar event-study approach to estimate stock market responses to 127 USDOJ indictments for price fixing under Section 1 of the Sherman Act in the 1962-1980 sample period.[1] Aguzzoni, Langus and Motta (2013) and Günster and van Dijk (2016) study the impact of "dawn raids" (surprise inspections) and infringement decisions of the European Competition Commission and also adopt an event-study approach to estimate their impact on firm valuations. Our analysis studies a *much* larger set of antitrust cases and for a longer sample period. Moreover, an important difference to the latter two studies is that the U.S. offers a longer history of

[1] Bittlingmayer (1992) instead relate movements in Dow stock price index to the number of antitrust cases raised by the USDOH in the 1904-1945 period. He finds that each case filed has a large impact on Dow.

antitrust regulation and that the success rates of the U.S. antitrust authorities are higher than those of the European Commission in its early years.[2]

A number of studies have found that various indicators of market power have risen over recent decades. This includes studies of markups as measured by the Lerner index estimated using the "production function approach" such as Hall (2018), Traina (2018), Hasenzagl and Pérez (2023), and perhaps most prominently de Loecker, Eeckhout and Unger (2020). Other studies have instead use a "demand-system" approach to generate estimates of markups at the broader economic level, e,g, Döpper et al (2024). Each of these studies document rises in market power over the last few decades although estimates of the size of this change, and of markups themselves, differ across studies. One shared finding of these papers is that the rise in market power as indicated by markups has been accompanied by rising dispersion of markups indicating that changes in misallocation may be an important angle to consider. Barkai (2020) instead document a rise in the profit share while Karabarbounis and Neiman (2019) argue that, while the profit share has risen since the 1980s, it fell during the 1970s and is no higher today than it was in the 1960s. Autor et al (2020) study U.S. Census data for the 1982-2012 sample for six large sectors and document rising concentration within 4-digit industries arguing that this trend reflects mainly reallocation towards large and productive firms. Gutiérrez and Philippon (2017) instead examine estimates of firm concentration at the economy-wide level and argue that it has risen over time, while Rossi-Hansberg, Sarte and Trachter (2021) document that "local market concentration" has declined.[3] Much of this literature has been focused on the measurement of market power, while we study how the impact of antitrust on these indicators and attempt to relate them to aggregate outcomes.

Closest to our efforts is Babina et al (2023) and Besley et al (2021). The latter of these studies exploit an antitrust indicator based on stringency of antitrust laws and policies to examine how antitrust impacts on firm profitability for a ten year sample of a large number of firms in a cross-section of 94 countries. They pursue the hypothesis that antitrust stringency

[2]Event study approach are of course used in many other applications, see e.g. Acemoglu et al (2016) who estimate the impact of political connections on firm performance.

[3]Berry, Gaynor and Scott Morton (2019) warn against interpreting correlations between output, or other outcomes, and market concentration measures as causal evidence on the impact of market power since market concentration can change for many reasons other than market power.

impacts mostly on firms in non-traded sectors and find evidence in favor of this hypothesis.[4] Babina et al (2023), like us, study the U.S. and focus on the cases pursued by the USDOJ in the period 1971-2018, and estimate the impact of antitrust at the state-industry level for non-traded sectors by comparing outcomes in states targeted by the DOJ with those not targeted. They find that antitrust enforcement actions stimulate payroll employment and wages, an increase in the labor share and in business formation. Our study complements this study in a number of dimensions. First, while Babina et al (2023) focus on non-traded sectors, we focus on publicly listed firms which enables us to estimate the impact of antitrust actions at the firm-level from which we construct our antitrust indicators. Secondly, we collect information on antitrust actions for a longer sample period and include both the FTC and the USDOJ. Third, we examine a broader set of antitrust violations. Fourth, we estimate aggregate U.S. outcomes at the sector and aggregate levels. We see the two studies as complementary because they study different aspects of antitrust.

2 U.S. Antitrust

2.1 Legislative background

The main focus of U.S. antitrust legislation is to avoid harm to consumers from anti-competitive behavior of the corporate sector, and to sustain high quality and low prices of goods and services sold on the market. Formal U.S. antitrust legislation dates back to the **Interstate Commerce Act of 1887**. This act was introduced to regulate the U.S. railway industry which was considered to engage in unreasonable business practices resulting in too high prices due to lack of competition and collusion.[5] Broader antitrust legislation followed swiftly in 1890 when U.S. Congress passed the **Sherman Antitrust Act** to combat "business trusts." The next major legislative step in antitrust regulation was taken in 1914 with the passing of the **Clayton Antitrust Act of 1914** and the **Federal Trade Commission Act**. The Sherman and Clayton Acts, and later amendments, still provide the backbone of

[4] Buccirossi et al (2013) similarly relate a proxy of competition policy to outcomes for a panel of (22) countries. They focus on productivity and find that stricter competition policy is associated with higher productivity growth.

[5] In practice, the legislation had ramifications also for non-railroad shipping, see e.g. Gilligan, Marshall and Weingast (1989) for an analysis

Federal U.S. antitrust legislation, but their interpretation and implementation have evolved over time through the U.S. judiciary system.[6]

The Sherman Act prohibits "*every contract, combination, in the form of trust or otherwise, or conspiracy, in restraint of trade or commerce among the several States, or with foreign nations*" (Section 1) and monopolization and attempts to monopolize (Section 2). Of course, not all contracts dealt with in Section 1 are seen as anti-competitive, but some acts are viewed as anticompetitive *per se* and are prohibited in an outright manner. Per se violations include:

- *Price fixing*: Agreements between competitors regarding the prices of products or conditions offered;
- *Bid rigging*: Coordination of bidding behavior among competitors that undermines the bidding process;
- *Market or customer allocation*: Agreements among competitors not to compete for customers;
- *Boycotts*: Refusal to deal with certain customers or restrict the conditions under which they can be served;
- *Tying*: When firms with market power condition the availability of one product on the purchase of (an)other separate good(s).

Other actions are evaluated in a broader context and subject to a "Rule of Reason" principle which applies to *restraints to supply*, agreements in vertical relationships, and to *exclusive dealing*, exclusivity deals between suppliers and retailers. In either case, courts may deem there to be sufficient pro-competitive benefits of certain actions that the Rule of Reason allows for these practices.

Section 2 of the Sherman Act prohibits firms from monopolizing, from attempts to monopolize, or from conspiring to do so. Such behaviors may be taken unilaterally or in a

[6]In addition, U.S. states typically have enacted their own antitrust laws targeted at anti-competitive behaviour within their states, and firms are also subject to foreign entities' antitrust regulation when partaking in business operations under those jurisdictions.

coordinated manner jointly with other firms (like Section 1 violations). While it is not illegal for a firm to be a monopoly as such, Section 2 regulates behaviors aimed at acquiring or maintaining such a position through unreasonable behavior. A *monopoly* is defined as a situation where a firm has "market power" in the sense of being able to control prices or control the level of competition within a "market." The Sherman Act also contains a Section 3 but it does not define separate regulations but instead extends Sections 1 and 2 to the District of Columbia and to U.S. Territories.

The Sherman Act was left deliberately vague in many of its details so that U.S. courts could develop its implementation. This aspect of the act was eventually seen as a weakness and antitrust reform became a topic of the 1912 U.S. Presidential election. After the election of President Woodrow Wilson, the Congress passed in 1914 the Federal Trade Commission Act and the Clayton Antitrust Act. These acts were meant to make the legislation more specific and forward-looking, and to improve antitrust enforcement. The Clayton Antitrust Act prohibits specific actions when they *may* lessen competition. Actions include:

- *Price discrimination* between different buyers when it substantially lessens competition or tends to create monopoly (Section 2);
- *Tying and exclusivity* of sales when these substantially lessen competition (Section 3);
- *Mergers and acquisitions* that either (may) significantly lessen competition or create monopoly (Section 7).
- *Forming interlocking boards* on competing companies when a merger of those would violate antitrust (Section 8).

In an amendment to the Clayton Act that extends Section 2, the **Robinson-Patman Act of 1936** further bans a set of price discriminatory actions. Section 7 of the Clayton Act was expanded in the **1950 Celler-Kefauver Amendment**: Section 7 of the Clayton Act specifically prohibits stock acquisitions that have the likely effect of lessening competition, but did not apply to asset acquisitions nor, somewhat surprisingly, to actual mergers; The Celler-Kefauver amendment extended the legislation to asset purchases and to deal with mergers.

In an important later addition to the formal antitrust legislation, the **Hart-Scott-Rodino Antitrust Improvements Act of 1976** introduced a number of amendments to U.S. antitrust legislation. Principally, the act requires firms to pay a filing fee and to pre-notify and register mergers and acquisitions that affect U.S. commerce and exceed certain limits on the size of the transactions, assets, or net sales of the parties involved. Companies involved in such transactions must subsequently await antitrust authorities to approve the transaction before they can implement any integration of their business activities. Failure to pre-notify or completion of the transaction prior to approval can trigger a civil penalty.

2.2 Enforcement

Antitrust enforcement was initially under the remit of the U.S. Attorneys and the Attorney General. These days, federal antitrust legislation in the U.S. is enforced by the **Antitrust Division** (the ATR) of the U.S. Department of Justice (the USDOJ), and by the **Federal Trade Commission (FTC)**. At the state level, antitrust is enforced by State attorneys general, and private parties can also seek enforcement against competitors. Our analysis is focused on the enforcement of Federal antitrust laws by the FTC and the USDOJ.

The USDOJ was involved with antitrust enforcement from the passing of the Sherman Act, but with scarce resources and without a dedicated antitrust division, its role was limited. In 1919, A. Mitchell Palmer, who was the Attorney General, took the initiative for a reorganization of the USDOJ which led to the formation of the ATR with the intention of having a dedicated antitrust enforcement team, see Werden (2018). The mission of the ATR is to "*promote economic competition through enforcing and providing guidance on antitrust laws and principles.*"

The ATR can prosecute individuals and firms deemed in violation of U.S. antitrust laws by filing criminal lawsuits and it can instigate civil action. In pursuing violators of antitrust laws and regulations, it can request courts to forbid future violations and culprits to remedy past violations. For certain violations, culprits may face large fines, and, in the case of individuals, up to 10 years of prison. The choice of whether to pursue violations as criminal cases or a civil case depends on the prosecutor's views about the seriousness of the alleged offense and on the strength of the evidence. Furthermore, only certain offenses are viewed

as criminal and some have only more recently become felonies (cartelization, for example, became a felony in 1974). In many cases, there is therefore a somewhat fluid distinction between civil non-merger cases and criminal cases. The ATR also functions as an advocate for competition and provides guidance on antitrust laws to firms through business reviews. The ATR has permission to fill more than a thousand positions and currently employs more than 50 PhD economists and fewer than 400 attorneys.

The FTC was created in 1914 with the passing of the Federal Trade Commission Act. It is an independent agency of the U.S. government and enforces non-criminal antitrust law and consumer protection. Its mission is to "*prevent business practices that are anticompetitive or deceptive or unfair to consumers.*" The FTC investigates competition issues raised to them by reports from consumers or firms, from pre-merger notification filings, from congressional inquiries, or from reports in the media. It can seek voluntary compliance, file administrative complaints, and initiate federal litigations. The FTC has more than a thousand staff including around 300 lawyers and it employs approximately 75 PhD economists.

Jointly, the ATR and the FTC therefore possess significant analytical and technical expertise in the area of antitrust although their joint technical manpower and budgets are not large when compared to e.g. the Federal Reserve system.[7]

The USDOJ enforces criminal antitrust cases related to Sections 1 and 2 of the Sherman Act, and it can also initiate civil actions requesting injunctive relief and damages. In case of litigation, the USDOJ also enforces Sections 2 and 3 of the Clayton Act, but the FTC takes the lead in case of administrative proceedings. Sections 7 and 8 of the Clayton Act are jointly enforced by the FTC and the USDOJ. The FTC may also enforce Sherman Act Sections 1 and 2 violations, but not if they are criminal cases. The HSR Act of 1976 is also jointly enforced with both agencies engaging in pre-consummation reviews (and requests for further details); For such cases, the agencies coordinate between them which agency carries out the review and pursues potential violations. The FTC may also investigate cases under Section 5 of the Federal Trade Commission Act which deals with unfair methods of competition which adds to the USDOJ's competencies under the Sherman Act.

[7] In comparison, the Federal Reserve Board employs more than 400 PhD economists

2.3 Sources and Methodology

We collect data on the universe of federal antitrust investigations carried out by the FTC and the ATR for sample periods that cover 1954-2023 and 1957-2023, respectively. The primary source of information about antitrust investigations is the Commerce Clearing House Trade Regulation Reporter (the CCH Trade Reporter), also referred to as the CCH Bluebook. Whenever possible, we double-check the information from the CCH Trade Reporter with antitrust case documents published on the websites of the USDOJ and the FTC. In certain cases, we further use information from Company Form 10-K, 8-K, and 10-Q submissions to the SEC.

Following Posner (1970) and Gallo et al (2000), we make several adjustments to the raw CCH Trade Reporter data. First, the CCH Trade Regulation Reporter frequently contains several updates on the same investigation as it proceeds through different legal stages. Counting such updates as separate cases would lead to a substantial double-counting. In some cases, though, further defendants are added during the process of a case. To deal with these issues, unique cases are counted from the date at which it was initially filed, but if further defendants are added during the process, we date the investigation against these by this later date. Secondly, it is not uncommon that one case may trigger further separate cases. This happens, in particular, in cases where employees are investigated for criminal offenses related to investigations of corporations. We choose not to control for such possible double-counting.

For each antitrust case filed by the ATR or the FTC we record the following information:

- the docket number;
- firms or individuals involved;
- the case type: civil non-merger, criminal or merger;
- the type of antitrust legislation violation(s) alleged;
- the court where the case was filed;
- the names of firms and/or individuals involved;

- the dates of significant case developments.

For cases that involve firms rather than individuals ("natural persons"), we check whether the companies involved were publicly traded at the time of the case opening and, if that is the case, we link them to their stock market ticker and to Compustat accounting data. This mapping of firms named in antitrust cases to publicly traded firms is very labor intensive because firms are often referred to using slightly different names, and because of typos in antitrust filings.

2.4 Broad Trends

Table 1 contains summary statistics for the history of antitrust cases. In the sample period that we examine, the antitrust authorities initiated a total of 6,387 investigations with the USDOJ accounting for approximately two thirds of these in total. Panel A of Figure 1 illustrates the total number of Federal antitrust cases over the 1954-2023 sample for the FTC and the 1957-2023 sample for the ATR separately, as well as the sum of the two. As is evident, the period spanning the late 1950s to the early 1960s was a period with very high antitrust activity relative to the rest of the sample. Apart from this early period, the total number of cases have been rather stable over time with two exceptions. First, the USDOJ was very active for a period from the mid-1970s to the mid-1980s. Secondly, there appears to have been some decline in antitrust activity at the very end of the sample.

Antitrust cases often involve multiple defendants, and defendants may be either firms or individuals ("natural persons"). Furthermore, firms may be either publicly traded ("listed") or not. In total, the 6,387 antitrust cases in our sample involve 15,829 defendants, the majority of which are corporations (11,568 in total). among corporations, publicly traded firms account for approximately 34 percent of the total number of antitrust investigations. Thus, publicly traded firms, which tend to dominate the right tail of the firm-size distribution, are significantly over-represented given that they account for only a tiny fraction of the total number of businesses in the U.S.

Panel B illustrates the trends over time of the total number of firms or individuals indicted for antitrust violations. As is evident, there is considerable time variation in the number of corporate firms investigated and, in contrast to the number of cases, there is a marked

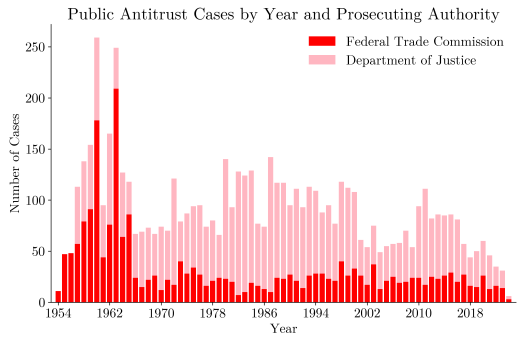
Table 1: Antitrust Investigations: Summary Statistics

		Defendants				
				Firms		
	Cases	**Total**	**Individuals**	Total	Non-Listed	Listed
Total	6,387	15,829	4,261	11,568	8,564	3,004
FTC	2,266	3,946	635	3,311	2,284	1,027
USDOJ	4,121	11,833	3,626	8,257	6,280	1,977
Civil non-merger	2,267	5,582	936	4,646	3,670	976
Criminal	2,577	7,250	3,205	4,045	3,407	638
Mergers and Acquisitions	1,524	2,935	108	2,827	1,451	1,376
Mixed	19	62	12	50	36	14

Notes: For the FTC the sample covers the sample period 1954-2023. For the USDOJ it covers the period 1957-2023. The first column reports the number of antitrust cases initiated by the antitrust authorities. The other columns report the number of defendants indicted in the antitrust cases. "Listed firms" refers to firms that at the time of the indictment were listed either on the SEC or on foreign stock exchanges. Source: Own calculations on the basis of the CCH Trade Reporter and information published by the FTC and the USDOJ on their websites.

negative trend over time in the number of firms indicted: In the period 1957-1964, an average of 379 firms were involved in antitrust investigations each year, a number that declines to 238 firms per year for the 1965-1980 sample, and further to 81 firms per year post-2000. The number of individuals investigated for antitrust violations instead displays relative stability over the sample apart from peaking at 116 persons in the 1957-1964 period.

Panel A: By Authority



Panel B: By Defendant Type

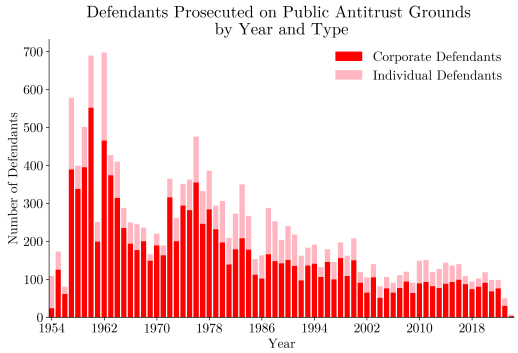
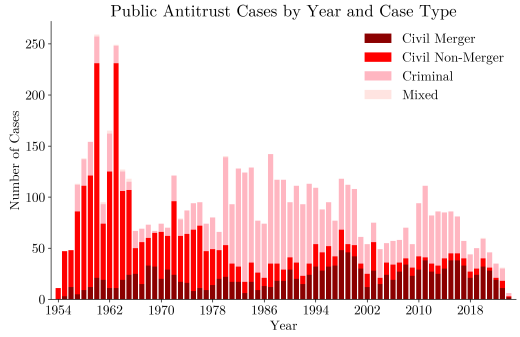


Figure 1: Antitrust Trends

Another important consideration regards the type of antitrust violation involved. 2,267

out of the 6,387 antitrust cases in the sample relate to civil non-merger antitrust violations, while 2,577 are criminal cases (which can only be pursued by the USDOJ), 1,524 were related to civil merger violations, and a tiny number (19) were mixed. Focusing on corporate antitrust defendants, civil non-merger violations account for approximately 40 percent of the cases, criminal cases for 35 percent, and M&A for the remaining 25 percent. For listed firms, civil merger cases instead dominate the indictments and account for 46 percent of the defendants (1,376 defendants), while civil non-merger violations account for 32 percent, and criminal cases for the remaining 21 percent.

Panel A: All Cases



Panel B: Firms Prosecuted

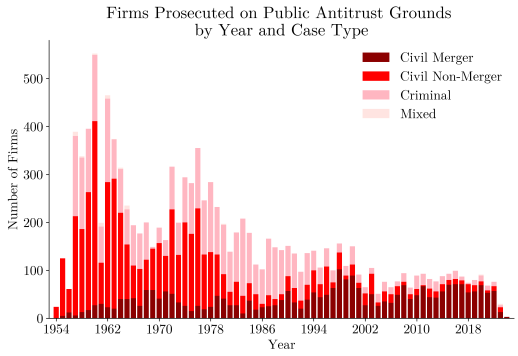


Figure 2: Antitrust Trends by Case Type

Panel A of Figure 2 illustrates the development in the number of antitrust *cases* over time across the four broad categories of antitrust violations, while Panel B illustrates the same time series for the number of *firms* indicted. This figure demonstrates a remarkable change in antitrust focus in the U.S. over the 70-year period that we examine. In the period from 1954 up to the late 1970s/early 1980s, the antitrust authorities frequently initiated antitrust investigations related to civil non-merger violations of antitrust, but starting from the early 1980s, such civil non-merger related antitrust cases become rare. As a share of the total number of cases, in the period 1957-1980, civil non-merger type violations account for on average 63 percent of all the cases, while the corresponding share from 1981 to the end of the sample is only 14 percent. The decline in the relative frequency of civil non-merger case violations is accounted for by a combination of an increase in the importance of both civil merger cases, which accounted for 16 percent of all cases in the 1957-1980 period

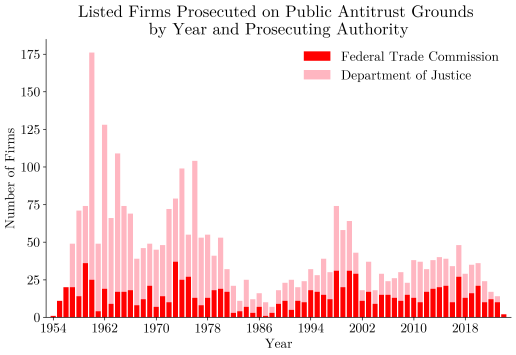
but 30 percent of all cases post-1980, and an increase in the share of criminal cases, which over those two sub-samples increases from 21 percent to 56 percent. As mentioned earlier, whether an offense is deemed criminal or not depends both on the type of offense and on an evaluation of the seriousness of the alleged offense and the robustness of the evidence. Thus, the decline in the importance of civil non-merger type cases appears to be related to a combination of growing importance of civil merger cases and a shift from civil non-merger cases to criminal cases. Consistently with this, Ghosal (2011) test for a structural break in antitrust enforcement in the 1958-2002 period and concludes that there was a structural break in 1979 with shift towards criminal cases. New to our evidence is the significant growth in the importance of civil merger related cases.

The corresponding trends at the level of firms shown in Panel B illustrates a similar decline in importance of civil non-merger violations which accounted for 53 percent of all firms indicted for antitrust violations up to 1980, but for only 19 percent of firms indicted for the post-1980 sample. An important difference relative to Panel A is that, as far as the number of firms pursued for antitrust violations is concerned, there is also a declining importance of criminal cases which accounted for 34 percent of the total number of firms indicted in the 1957-1980 sample, but only 19 percent since 2000. Thus, civil merger cases have become the main reason for antitrust cases raised against firms.

In Figure 3 we focus on the antitrust cases raised against *publicly listed companies*. For these large firms, the structural change in the composition of the antitrust violations that the firms are prosecuted for is both very visible and very significant. In the 1954-1980 sample, M&A related violations accounted for 22 percent of the total number of firms indicted for antitrust violations, while in the 1981-2023 sample, the corresponding share is 79 percent. Thus, the changing focus of antitrust discussed above, is particularly evident for public firms. Furthermore, for this subset of defendants, the frequency of antitrust case openings also displays a negative trend over the sample which occurs around the same time as the change in the focus of the antitrust authorities towards M&A related violations. In particular, the number of publicly listed firms prosecuted for antitrust violations declines from around 71 per year in the 1954-1980 sample to 30 firms per year post-1980. This decline appears to be mainly the product of fewer investigations against publicly listed firms initiated by the

USDOJ while the number of firms investigated by the FTC remains roughly unchanged.

Panel A: Firms by Antitrust Authority



Panel B: Firms by Type of Violation

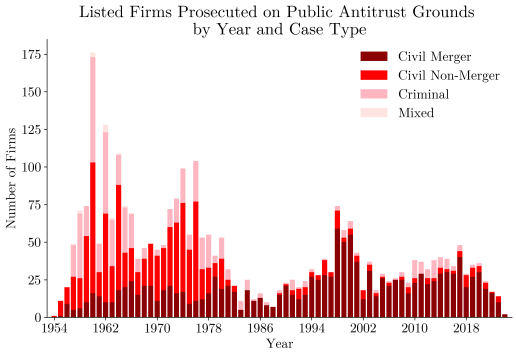


Figure 3: Publicly Listed Firms

An important question is the extent to which firms prosecuted for antitrust violations differ in any observable ways from other firms. In Table 2 we report several statistics comparing publicly listed firms prosecuted for antitrust violations with their competitors. We define competitors as publicly listed firms that operate within the same 3-digit NAICS sector as the prosecuted firm. By construction, publicly listed firms are large relative to all firms in the economy, but prosecuted firms also tend to be large relative to other publicly listed firms. We find that the prosecuted firms are larger than their competitors regardless of whether measured by their market value, operating income, capital expenses, R&D expenses, number of employees, or their market share. On the other hand, in terms of sales per employee or relative to their fixed assets the prosecuted firms appear to be less productive.[8]

Figure 4 illustrates the prosecution rate computed over the entire sample for publicly listed firms across deciles of the firm size distribution as measured by either employment or 3-digit NAICS market shares. There is a very strong size dependence in the prosecution rates: Firms in the top decile of the employee distribution (market share distribution) have a 17 percent (24 percent) frequency of having been prosecuted at least once in the sample that we examine. By contrast, firms in the bottom half of the size distributions have close to zero chance of being observed in the sample. It is important to stress that these numbers refer to the size distributions of publicly listed firms which already is right-skewed. Thus,

[8]Using production-function based measures of TFPR, prosecuted firms are instead more productive than their competitors.

Table 2: Comparison of Means of Prosecuted Firms and their Competitors

	Prosecuted	Competitors	Difference	t-stat	p-value
Equity value	18,863.5	4,543.8	14,320.7	3.729	0.000
Operating income	1,929.3	405.1	1,524.2	6.899	0.000
Capital exp.	874.5	148.4	726.1	7.147	0.000
R&D Expenditure	844.7	189.1	655.6	2.708	0.000
No. of employees	45,196	7,746	37,448	10.483	0.000
Market share	0.210	0.018	0.193	23.936	0.000
Sales/employees	258.6	392.2	-133.6	-2.423	0.015
Sales/fixed assets	5.176	14.884	-9.708	-16.509	0.000

Notes: Equity value, operating income, capital expenditure, R&D expenditure, and sales/employees are measured in millions of constant 1957 dollars converted using the CPI. Equity value is the market value of the firm's equity. Operating income is operating income before depreciation. The market share is computed at the NAICS 3-digit level. All numbers refer to means.

as far as the corporate sector is concerned, antitrust indictments are heavily concentrated among large firms.

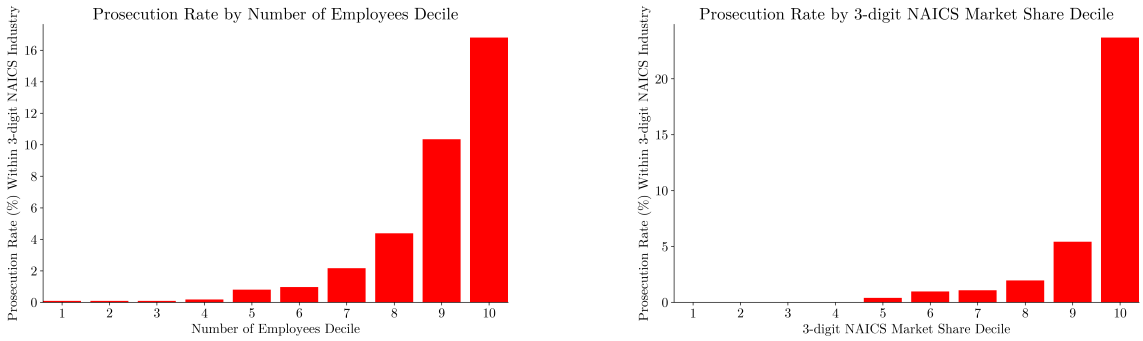


Figure 4: Prosecution Rates

3 The Impact of Antitrust Investigations on Firm Valuations

We now turn to estimating the impact of antitrust indictments on firm valuations. The fact that a firm is prosecuted for violations of antitrust legislation is, of course, not a random event: To the extent that the allegations are correct, such firms are likely to have earned abnormal profits prior to the case opening date. Therefore, difference-in-differences type estimators cannot be applied. Moreover, when the prosecutions lead to changes in business

practices, antitrust can impact on both the firms that are pursued for antitrust violations *and* on their competitors implying that the SUTVA (Stable Unit Treatment Value Assumption) condition may be invalid which rules out the use of cross-sectional estimators unless one is willing to explicitly model the spillovers between firms.

For that reason, we adopt an event-study approach. In particular, focusing on publicly listed firms, we exploit information on equity returns to evaluate the impact of antitrust prosecutions on firm valuations by comparing realized equity returns after a firm has been indicted for violations of antitrust legislation with the predicted returns in the absence of such an indictment. For the latter, we use a parametric model of equity returns estimated on data prior to the indictment. We interpret these estimates as measuring the impact of a loss of the benefits of violating antitrust regulations for market power related motives on prosecuted firms' expected profits. The underlying idea is that (i) firms engage in anti-competitive practices in pursuit of profit margins, and (ii) when these practices violate antitrust laws and the firm is prosecuted, it leads to an expectation among market participants that part of these practices may be terminated therefore reducing expected firm profits as measured by the market valuation of its equity.

3.1 Methodology

There are three steps to the procedure. In the first step, for each listed firm indicted for antitrust violations, we estimate the determinants of equity returns on a pre-treatment sample ahead of the case opening date. We apply a simple three-factor Fama-French model and estimate the loadings on the return determinants using daily equity return data for a 12-month period starting 13 months before the case opening date and ending one month prior to this date. We leave one month in between the treatment (the case opening) and the estimation sample in order to eliminate concerns about contamination with the subsequent antitrust case opening. In the second step, we compute daily excess and cumulated daily excess returns on the treated firms' equity in windows around the case opening date. Excess returns are defined as the differences between the realized returns at the firm-level and those implied by the pre-treatment estimates of the loadings on the factors determining the firms equity returns. Third, we convert the cumulated excess returns into dollar equivalents for

each case by combining them with firm market capitalizations at the time of the antitrust case opening.

We include all firms pursued for antitrust violations that at the time of the antitrust case opening were listed either in the U.S. or on foreign stock exchanges.[9] For companies listed in the U.S., we obtain daily equity prices from the Center for Research in Security Prices (CRSP) and for companies listed on foreign stock exchanges we use equity price data from LSEG (formerly Refinitiv).

We date the antitrust cases by the case opening dates. Posner (1970) and Gallo et al (2000) show that the success rates of the USDOJ have been very high since the 1950s so we work under the assumption that the equity market assumes that cases will lead to antitrust intervention of commercial activities. Let $t^a_{i,j}$ denote the day of the announcement of antitrust investigation i against firm j, and let $r_{j,t}$ denote the equity returns of firm j at date t. The first stage then consists of least squares estimation of the Fama-French three factor model:

$$r_{j,t} = \alpha_{i,j} X_t + \nu_{j,t}, t \in (t^a_{i,j} - T_1, t^a_{i,j} - T_2) \tag{1}$$

where $X_t = (1, smb_t, hml_t, r_{m,t})$ denotes a constant and the three standard Fama-French factors: smb_t is the "small minus big" factor meant to capture a size premium, hml_t is the "high minus low" factor capturing a value premium, and $r_{m,t}$ is the equity market excess return measured as the return on the market equity portfolio less the risk-free rate.[10] $T_1 = 13$ months and $T_2 = 1$ month define the window in which the factor loadings are estimated.

In the second stage we then calculate excess equity returns for each prosecuted firms in windows around the case opening date. The excess returns at horizon s are defined as the difference between realized returns and those predicted on the basis of the factor loadings estimated in equation (1):

$$R_{i,j,s} = r_{j,t^a_{i,j}+s} - \widehat{\alpha}_{i,j} X_{t^a_{i,j}+s}, s \in (s_1, s_2) \tag{2}$$

[9] We have some attrition for firms listed outside the U.S. when they have changed ownership post the antitrust investigation.

[10] Alternatively, one might use a Fama-French 5-factor specification, but data for the two additional factors are not available for the entire sample period that we study. For the sample period that allows the use of the 5-factor model, results are very similar to the 3-factor model.

where $\widehat{\alpha}_{i,j}$ denotes the least squares estimate of $\alpha_{i,j}$ and (s_1, s_2) denotes the width of the window that we evaluate. We then use these estimates to compute cumulated excess returns:

$$AR_{i,j,s} = \sum_{h=s_1}^{s} R_{i,j,h} \tag{3}$$

Finally, we convert the cumulated excess returns into dollar values by multiplying with firm j's market capitalization at date $s(i,j)$:

$$Q^M_{i,j,s} = AR_{i,j,s} \times cap_{j,t^a_{i,j}} \tag{4}$$

where $cap_{j,t^a_{i,j}}$ is the market capitalization of firm j at date $t^a_{i,j}$.

3.2 Results

It is important for our analysis that the first-stage regressions of the daily returns based on the estimations of the Fama-French 3-factor model have at least some explanatory power. Across all treatments (2,707 cases), the mean and median R^2-statistics equal 20.9 percent and 17.2 percent, respectively, estimates that we find satisfactory. Figure 5 illustrates the distribution of R^2-statistics indicating that the fit is good apart from the bottom 20 percent of cases where the R^2 is below 7.5 percent. Given the noisy nature of daily returns, we believe that these results are very encouraging.

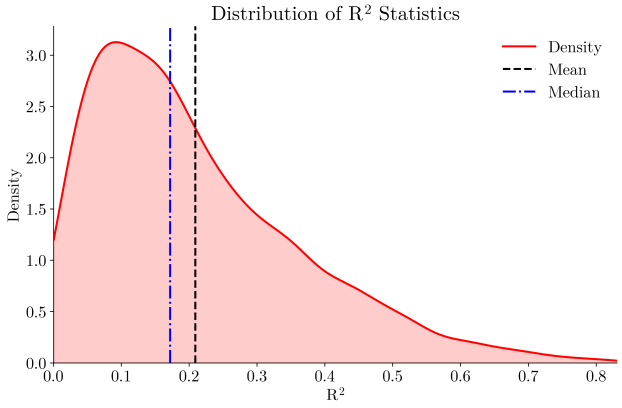


Figure 5: Distribution of R^2 Statistics Associated with Equation 1

We report average firm-level estimates of excess returns and cumulated excess, $R_{i,j,s}$ and

$AR_{i,j,s}$, for each horizon (measured in trading days):

$$\overline{R}^{A}_{z,s} = \frac{1}{N_z} \sum_{v(i,j)\in V_z} R(i,j,s) \tag{5}$$

$$\overline{AR}^{A}_{z,s} = \frac{1}{N_z} \sum_{v(i,j)\in V_z} AR(i,j,s) \tag{6}$$

where V_z denotes a selection criterion such as the violation type or the subsample, and $v(i,j)$ is an indicator of the violation type for case i against firm j. N_z is the number of cases of type z.

Tables 3 and 4 contain the estimates of the average excess returns and average cumulated excess returns, respectively. We report the estimates for all cases, and for civil non-merger and criminal cases and civil merger cases separately. The excess return estimates are reported for horizons starting from 5 trading days prior to the case opening date to 60 trading days after the case opening date. For the estimates of $\overline{AR}_{z,s}$, for reasons that will become clear below, we report results cumulating the excess returns from two days prior to the case opening day. Care must be taken when conducting inference in this environment because many investigations involve multiple firms which induces cross-sectional correlation of excess returns and of cumulated excess returns. Moreover, equity returns may be subject to high event-day volatility due to market uncertainty about the impact of antitrust cases. To take these issues into account, we report p-values of the null hypothesis that the (cumulated) excess returns equal zero using the test statistic proposed by Kolari and Pynnönen (2010). This test statistic modifies the t-test statistic of Boehmer, Musumeci and Poulsen (1991) (developed to address volatility) additionally taking into account cross-sectional correlation of abnormal (cumulated) returns.[11]

We find a spike in negative excess returns on the case opening day for all cases, and for civil non-merger and criminal cases and merger cases separately. The negative excess returns persist on the day after the case opening which could be due to either the timing of

[11] We computed a battery of other test statistics including the Kolari and Pynnönen (2011) non-parametric test statistic that, on top of cross-sectional correlation and event-day volatility is also robust to serial correlation and non-normality. In terms of significance of the estimated returns, most results are identical.

Table 3: The Impact of Antitrust Case Filings on Excess Returns

	All Cases			**Civ. non-merger & crim.**			**Civil merger**		
s	N_{all}	$\overline{R}_{\text{all},s}$	*p-val*	$N_{non-mer}$	$\overline{R}_{\text{non-mer},s}$	*p-val*	N_{mer}	$\overline{R}_{\text{mer},s}$	*p-val*
-5	2,707	0.00	0.45	1,262	0.02	1.00	1,430	-0.01	0.28
-4	2,707	-0.01	0.77	1,262	-0.01	0.69	1,430	-0.01	0.97
-3	2,707	0.03	0.73	1,262	-0.01	0.36	1,430	0.05	0.77
-2	2,707	-0.11	0.00	1,262	-0.06	0.03	1,430	-0.16	0.02
-1	2,707	-0.03	0.70	1,262	-0.05	0.46	1,430	-0.02	0.93
0	2,707	-0.44	0.00	1,262	-0.45	0.00	1,430	-0.42	0.00
1	2,686	-0.30	0.00	1,262	-0.27	0.00	1,409	-0.31	0.00
2	2,665	0.02	0.83	1,262	-0.02	0.43	1,388	0.05	0.77
3	2,650	-0.02	0.49	1,262	0.01	0.35	1,373	-0.04	0.94
4	2,643	-0.04	0.04	1,262	0.01	0.46	1,366	-0.08	0.04
5	2,636	-0.02	0.73	1,262	0.02	0.42	1,359	-0.04	0.27
10	2,606	-0.03	0.46	1,260	0.04	0.59	1,331	-0.11	0.13
20	2,583	-0.06	0.19	1,259	-0.11	0.06	1,310	0.00	0.84
30	2,569	0.01	0.90	1,259	0.03	0.70	1,296	-0.02	0.91
40	2,556	-0.01	0.58	1,259	-0.04	0.14	1,283	-0.03	0.51
50	2,544	0.06	0.08	1,259	0.06	0.09	1,271	0.05	0.39
60	2,532	0.01	0.61	1,256	-0.02	0.27	1,262	0.04	0.91

Notes: s denotes the number of days from the case opening, and N is the number of treated firms. $\overline{R}_{\text{z},s}$ is the average excess return at horizon s as defined in (5). *p-val* is the p-value associated with the Kolari and Pynnönen (2010) test statistic for the null that the average excess returns are zero.

the announcement of the antitrust case filing within a trading day or to short run persistence in the updating of investors' return beliefs. For both of these trading days, we can firmly reject the null that the average excess returns are zero for all cases as well as for civil non-merger and criminal cases and mergers separately.[12] After the first two trading days, excess returns are close to zero, and insignificantly different from zero with very few exceptions. Prior to the case opening date, we find significant negative excess returns two days ahead of the case filing for civil merger (and for all cases) while the evidence is slightly less clear for civil non-merger and criminal cases. It is unclear why excess returns should be negative prior to the case opening date for these cases, and while the estimated abnormal returns at this horizon are much smaller than those on the case opening date and the trading day after, we can reject that they are zero.

[12]The non-parametric test-statistic of Kolari and Pynnönen (2011) also rejects the null for all cases and for civil non-merger and criminal cases.

Table 4: The Impact of Antitrust Case Filings on Cumulated Excess Returns

	All Cases			**Civ. non-merger & crim.**			**Civil merger**		
$[s_1, s_2]$	N	$\overline{AR}_{s_2}$	p	N	$\overline{AR}_{s_2}$	p	N	$\overline{AR}_{s_2}$	p
$[-2,-1]$	2,707	-0.14	0.01	1,262	-0.11	0.04	1,430	-0.17	0.12
$[-2,0]$	2,707	-0.58	0.00	1,262	-0.56	0.00	1,430	-0.59	0.00
$[-2,1]$	2,686	-0.90	0.00	1,262	-0.84	0.00	1,409	-0.94	0.00
$[-2,2]$	2,665	0.90	0.00	1,262	-0.86	0.00	1,388	-0.92	0.00
$[-2,3]$	2,650	-0.99	0.00	1,262	-0.86	0.00	1,373	-1.01	0.00
$[-2,4]$	2,643	-0.99	0.00	1,262	-0.84	0.00	1,366	-1.11	0.00
$[-2,5]$	2,636	-1.01	0.00	1,262	-0.82	0.00	1,359	-1.16	0.00
$[-2,10]$	2,606	-1.18	0.00	1,260	-0.82	0.00	1,331	-1.60	0.00
$[-2,20]$	2,583	-1.15	0.00	1,259	-0.69	0.00	1,310	-1.80	0.00
$[-2,30]$	2,569	-1.41	0.00	1,259	-1.04	0.00	1,296	-2.02	0.00
$[-2,40]$	2,556	-1.38	0.00	1,259	-0.77	0.01	1,283	-2.02	0.00
$[-2,50]$	2,544	-1.38	0.00	1,259	-0.64	0.01	1,271	-2.12	0.00
$[-2,60]$	2,532	-1.60	0.00	1,256	-0.98	0.00	1,262	-2.26	0.00

Notes: $[s_1, s_2]$ denotes the window over which the excess returns are calculated, and N denotes the number of treated firms. $\overline{AR}_{z,s}$ is the average cumulated excess return from trading day -2 to trading day s after the case opening date as defined in (6). *p-val* is the p-value associated with the Kolari and Pynnönen (2010) test statistic for the null that the average excess returns are zero.

Given the evidence on abnormal returns two days ahead of the case opening, Table 4 reports average cumulated excess returns from this trading day. We find large negative cumulated excess returns that we can reject equal zero at very high significance levels at all horizons from two days prior to the case opening up to 60 trading days after the case opening. Quantitatively, the impact on average cumulated excess returns are very similar for the two types of antitrust violations at short horizons going up 2-3 trading days after the case opening. After this, the average cumulated excess returns stabilize for civil non-merger and criminal cases at a level corresponding to around -0.9 to -1 percent, while they rise gradually with the forecast horizon for mergers until 50 trading days after the case opening date, and stabilize at a level closer to -2 percent. Bosch and Eckard (1991) estimate average excess return and average cumulated excess returns for 127 firms indicted for price fixing under Section 1 of the Sherman Act in the 1962-1980 period. They estimate a spike in negative abnormal returns on the indictment date of -0.75 percent and of -0.33 percent the day before with the former (latter) of these being significant at the 99 percent (90 percent level). These estimates are larger than those that we estimate for civil non-merger and criminal cases

which could be due to their selection relating to more severe violations. However, in terms of cumulated excess returns, the point estimates of the abnormal returns in their study and in ours are remarkably similar: Bosch and Eckard (1991) estimate that the cumulated excess returns from 5 trading days prior to the indictment to 5 days after is -0.78 percent while the estimate over this window is -0.81 percent for the civil non-merger and criminal cases in our sample. Our study, however, includes an order of magnitude more firms and covers a much longer sample period.

As discussed above, we find a remarkable shift in antitrust focus around 1980 after which civil non-merger cases become very rare for publicly listed firms while merger cases if anything become more common. For that reason, in Figure 6 we show the estimated average cumulated excess returns for the two types of antitrust cases comparing the pre-1980 sample with a sample that starts in 1980. We show the average cumulated excess returns together with two standard error bands (which should be interpreted with care due to cross-sectional dependence and volatility). As is clear, average cumulated excess returns are significantly negative in the pre-1980 sample for both types of antitrust violations. In the post-1980 sample, we find significant negative cumulated excess returns for civil non-merger and criminal cases only for a few trading days after the case opening after which they hover around zero.[13] For merger cases instead, the estimates for the post-1980 sample indicate significant negative cumulated excess returns although they take longer to cumulate and stabilize at a lower level than in the pre-1980 sample.

One might speculate about the underlying reasons for this structural change. One hypothesis is that it is a result of the introduction of the HSR Act of 1976 which meant that the antitrust authorities had to redirect a significant amount of their resources to pre-consummation reviews of large merger cases. Another possibility is that it reflects a change in antitrust orientation. In this respect, the structural change coincides with the publication of Bork (1978) who argued that antitrust should be focused on how market power impacts on allocative efficiency rather than on large-scale business power (see e.g. Crane (2014)).

[13] A formal test of the null hypothesis that cumulated excess returns are zero that takes cross-sectional correlation and volatility into account fails to reject this hypothesis beyond four trading days. Taking serial correlation into account as well, the cumulated excess returns are significant for 5 trading days but only at the 10 percent level.

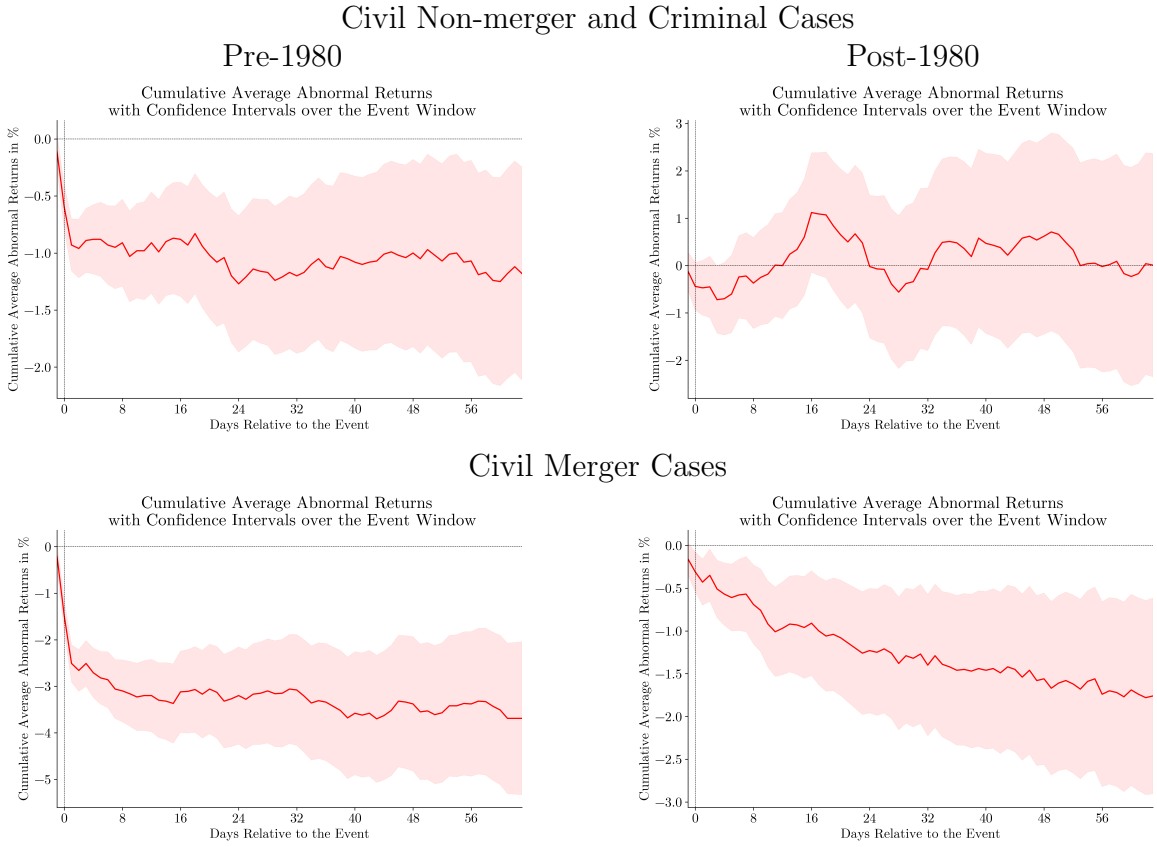


Figure 6: Average Cumulated Excess Returns on Prosecuted Firms' Daily Equity Returns Pre- and Post-1980

Robert Bork went on to state that "Congress designed the Sherman Act as a 'consumer welfare prescription,' " a statement that subsequently was often quoted in federal antitrust cases indicating potential impact of Bork's arguments. Third, starting in 1976, and perhaps related to Bork's arguments, the Manne Economics Institute for Federal Judges provided intensive economic training for federal judges which Ash, Chen and Naidu (2025) argue had a deep impact on their rulings. Here we simply note the result, but it clearly deserves attention in future work. Lancieri et al (2022) argue that weaker enforcement has occurred due the influence of big business which supported light enforcement for the best of its own interests. Here we simply note the trends, but clearly the results deserve further research.

The average cumulated excess returns obscure differences across cases that are inevitable given the amount of heterogeneity in both the seriousness of the antitrust cases and the amount of volatility in daily equity returns. Figure A.1 in the Appendix illustrates the

distributions of cumulated excess returns pre- and post-1980 at the 21 trading days horizon. Pre-1980, the mean is significant for both types of antitrust cases and the distribution of cumulated excess returns at this horizon is skewed to the left. For mergers, the post-1980 distribution is relatively similar to the pre-1980 distribution, while for civil non-mergers and criminal cases the post-1980 distribution is close to symmetric around 0 and with a notably higher cross-case variance.

Given the differences in the responsibilities for antitrust enforcement across the FTC and the USDOJ, it is also interesting to examine whether there tends to be significant differences in the impact of investigations by these two authorities on excess returns. We find that the point estimates of the average cumulated excess returns are larger for USDOJ cases than for FTC cases for all horizons that we estimate. For the USDOJ, the average cumulated excess returns stabilize at around -1.6 percent after 20 trading days while for FTC cases it stabilizes around -1 percent (also after 20 days). Taking sampling uncertainty into account, the differences are statistically significant for at least the first 8-10 trading days. This difference in the impact of antitrust investigations may be due to the USDOJ being in charge of more serious criminal cases while the FTC can only raise cases associated with civil litigation.

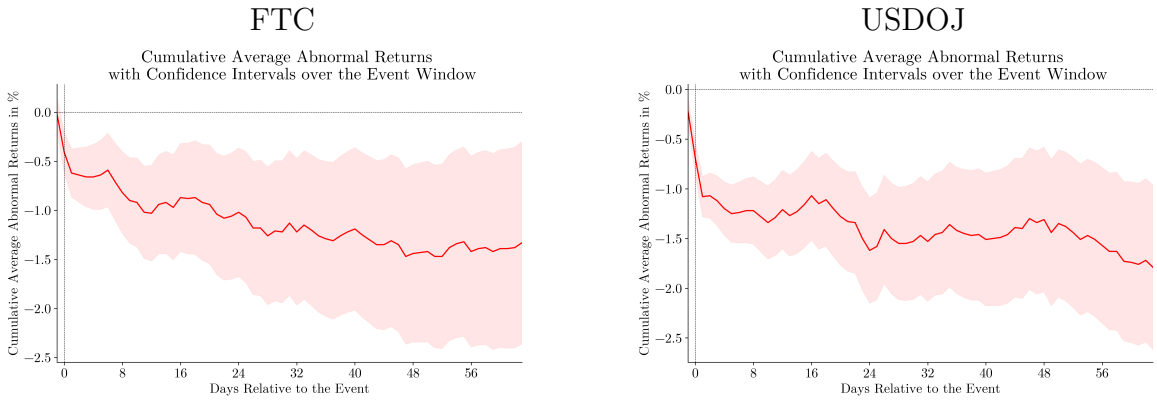


Figure 7: Average Cumulated Excess Returns: FTC vs USDOJ

Using the estimates of the cumulated excess returns at the firm level for the prosecuted firms, we derive an indicator for antitrust activity by converting these estimates into dollar amounts as in equation (4). We evaluate the excess returns at 10 trading days, and we

then sum over firms at each date. In order to minimize noise, we weight each of the firm level estimates by the (inverse of the) variance of the excess returns. Practically, this re-weights the measure towards those units for which the event study regressions are, in relative terms, estimated with higher precision. Note that many event study test statistics, e.g. Patell (1976), perform the same adjustment as this improves the statistical power of the tests. Given the high volatility of equity returns, we eliminate observations for which the estimated excess returns are positive after 5 trading days so as to minimize concerns about contamination of non-antitrust related events; discarding such mostly irrelevant events helps to manage measurement error in the antitrust indicator. Finally, we normalize the estimate by the total market capitalization of the listed firms, and we aggregate to the quarterly (or annual frequency).

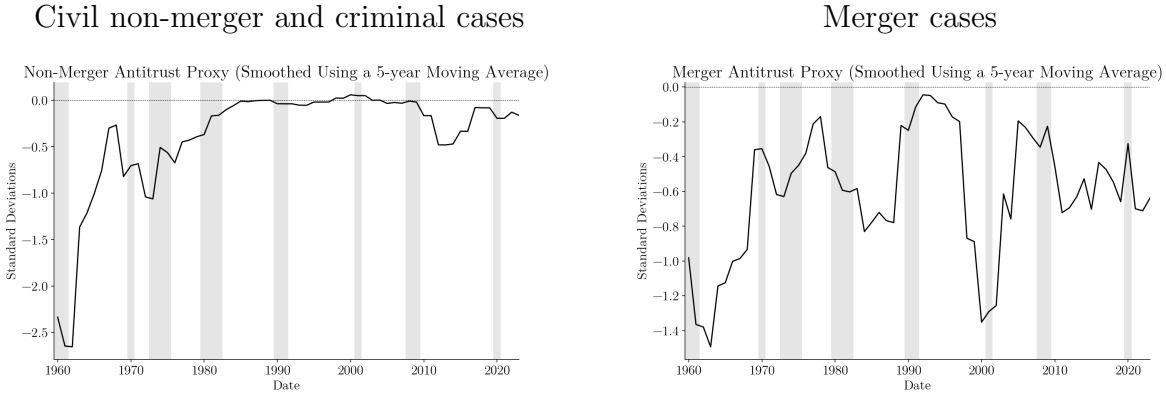


Figure 8: Estimated Indicator for Antitrust Activity

In the analysis below we will use this indicator of antitrust activity evaluated at the 2-digit industry level. However, it is still of interest to inspect its path over time at the aggregate level. Figure 8 illustrates a five year moving average of the indicator of antitrust activity for civil non-merger and criminal cases, and for merger cases. We indicate NBER recessions by the grey shaded areas. Notably, despite the fact that equity returns are cyclical, we do not find any strong evidence of cyclicality of the antitrust activity measures.

For civil non-merger and criminal cases we find three waves of high antitrust activity. The first wave takes place in the late 1950s up to the early 1960s during the Dwight D. Eisenhower and John F. Kennedy administrations. Measured in terms of standard deviations, this is

the period of most intense antitrust activity against publicly listed firms in the sample. For instance, in October 1958, the USDOJ launched a case against nine oil companies and independent marketers of gasoline price oil for fixing the retail price of gasoline. In December 1958, the USDOJ launched a case against several of the successors to the Standard Oil Company for engaging in territorial allocation and tying arrangements regarding the sale of refined oil products. In 1960, the USDOJ initiated multiple cases against dozens of manufacturers of electrical equipment for price fixing and bid rigging (notably, General Electric was listed as a defendant in 31 different criminal and civil non-merger cases during that year). The second wave is in the late 1960s to mid-1970s during the Lyndon B. Johnson and Richard Nixon administrations. Relative to later periods, this period also stands out as having intense antitrust activity. Notably in 1972, the USDOJ launched a case for intellectual property abuses and that included 17 publicly-listed companies from the defense industry as defendants.

Starting in the early 1980s, a 15-year period of antitrust tranquility that starts and lasts until the mid-1990s when the USDOJ launched well-known investigations such as the case against Nasdaq market makers for having colluded on quoting conventions (launched in July 1996), the case against Akzo Nobel Chemicals and Glucona for price fixing and market allocation for sodium gluconate (launched in September 1997), and cases against VISA International Corp. and Mastercard International Inc. (launched in October 1998) for restraints to competition in the market for general purpose credit card products and services. We do not, however, find a large impact of these cases in terms of abnormal returns. Instead, we do find some impact of cases raised for such violations in the early 2010s under the Barack Obama administration when the USDOJ started antitrust investigations against Apple Inc. and a large group of commercial publishers for conspiring to limit competition in the e-book markets (in April 2012), and against Verizon and a group of other suppliers of cable TV for bundling of offerings in violation of Section 1 of the Sherman Act.

The behavior of the civil merger case antitrust indicator is very different. First, while this indicator does display considerable variation over time, it does not show any signs of a structural change in terms of a decline (or increase) in merger relative antitrust activity. Similarly to the indicator for civil non-merger and criminal cases, we find high antitrust

activity in the late 1950s to mid-1960s. A second wave of high intensity occurs in the mid-1980s during the Reagan administration, and a third wave in the late 1990s during the Clinton administration. But overall, this antitrust activity indicator for merger related cases instigated against public firms demonstrates much less of a change over the sample period than the indicator for civil non-merger and criminal cases against public firms.

4 Estimating the Lerner Index

Below we will estimate the impact of antitrust activity on sector-level and aggregate outcomes. One of the outcomes that we will examine is the Lerner index. We follow much recent work in the literature and estimate this measure of markups for listed firms using a production-function approach applied to the Compustat accounting data. Our focus on Compustat data is natural given our estimates of antitrust activity discussed above are based on equity returns.

Following Hall (1988), de Loecker and Warzynski (2012), de Loecker, Eeckhout and Unger (2020) and many other recent papers, we exploit cost-minimization to express the Lerner index as:

$$\mu_{j,t} = \frac{\theta^X_{j,t}}{\alpha^X_{j,t}} \tag{7}$$

$$\theta^X_{j,t} = \frac{\partial Q_{j,t}}{\partial X^V_{j,t}} \frac{X^V_{j,t}}{Q_{j,t}} \tag{8}$$

$$\alpha^X_{j,t} = \frac{P^X_{j,t} X_{j,t}}{P_{j,t} Q_{j,t}} \tag{9}$$

where $\mu_{j,t}$ is the markup of product prices over marginal costs of firm j at date t, $\theta^X_{j,t}$ is the firm's output elasticity to the variable input X_v, and $\alpha^X_{j,t}$ is the spending share out of revenue on the variable input. Finally, $Q_{j,t}$ denotes the quantity of output of firm j, $P_{j,t}$ is the price of the product, and $P^X_{j,t}$ is the variable input price.

Subject to assumptions on which factor(s) are variable, the factor spending share, $\alpha^X_{j,t}$, is observable in accounting data such as Compustat. The output elasticity to the variable input instead needs to be estimated. For this purpose, we use the Ackerberg, Caves and

Frazer (2015) two-step version of the Olley and Pakes (1996) estimator. We assume that production technologies are identical within 2-digit NAICS industries and specify these either by translog or Cobb-Douglas technologies:

$$q_{j,t} = \theta_{0,l} + \theta_{1,l}x^d_{j,t} + \theta_{2,l}x^v_{j,t} + \theta_{3,l}(x^d_{j,t})^2 + \theta_{4,l}(x^v_{j,t})^2 + \theta_{5,l}x^d_{j,t}x^v_{j,t} + z^p_{j,t} + z^{tr}_{j,t}, j \in N_l \quad (10)$$

$$q_{j,t} = \theta_{0,l} + \theta_{1,l}x^d_{j,t} + \theta_{2,l}x^v_{j,t} + z^p_{j,t} + z^{tr}_{j,t}, j \in N_l \quad (11)$$

where $q_{j,t}$ denotes the logarithm of firm j output at date t, $x^d_{j,t}$ denotes the input of dynamic inputs, $x^v_{j,t}$ are variable (static) inputs, $z^p_{j,t}$ is a persistent productivity factor, and $z^{tr}_{j,t}$ is a non-persistent productivity factor. N_l is a set of firm indices that determines the industry membership,l, of firm j.

The persistent productivity factor is assumed to be first-order Markovian with a distribution that is known to firms and stochastically increasing in $z^p_{j,t}$, while $\mathbb{E}_{j,t}z^{tr}_{j,t} = 0$. It is assumed that there are no news shocks to the persistent productivity component. Along with the literature, we will assume that the dynamic input is "capital" which is accumulated over time by combining existing capital, $x^d_{j,t-1}$ with "investment" at date $t-1$, $i_{j,t-1}$. Both inputs are assumed to be chosen prior to the realization of $z^{tr}_{j,t}$. The key distinction between the capital and variable (or static) factors of production is that the former are chosen at date $t-1$ prior to the firm having any information about $z^p_{j,t}$ while the latter may be chosen anytime from period $t-1$ up until just prior to the realization of $z^{tr}_{j,t}$.

With these assumptions, following Ackerberg, Caves and Frazer (2015), the input elasticities can be estimated in two stages using the control function variable approach of Olley and Pakes (1996). We apply the control function to the variable input which is assumed increasing in $z^p_{j,t}$. Following de Loecker, Eeckhout and Unger (2020), we allow the control function to depend on market shares in order to address the "omitted price problem" that arises due to the fact that product prices are not available in Compustat data. De Ridder, Grassi and Morzenti (2025) study the seriousness of the omitted price problem and argue that appropriate choices of the econometric framework, congruent with our choices, still allow for meaningful estimates of trends in markups and in markup dispersion.

The estimation allows for time variation in factor prices at the sector level but not for

firm-specific persistent factor price components, for capital adjustment costs but only to the extent that they are not firm-specific. We measure variable costs by Costs of Goods Sold (COGS) which consist of spending on labor, raw materials, as well as items such as manufacturing overheads. We estimate the production function using annual data for the sample period 1954 to 2023 and use standard selection criteria, see e.g. Hasenzagl and Pérez (2023). Our baseline estimates relate to the translog specification because of the flexibility it offers in approximating production functions, see also De Ridder, Grassi and Morzenti (2025), but as we will discuss later, none of our results are very sensitive to alternatively assuming a Cobb-Douglas technology.

Our analysis below will study markup estimates at the sector level. In order to aggregate markups across firms, we compute sales-weighted harmonic means:

$$\mu_{l,t} = \left(\sum_{j \in N_l} \frac{P_{j,t} Q_{j,t}}{P_{l,t} Q_{l,t}} \frac{1}{\mu_{j,t}} \right)^{-1} \tag{12}$$

where $P_{j,t}Q_{j,t}$ are firm j's sales and $P_{l,t}Q_{l,t}$ are total sales of firms in the sector. Likewise, we aggregate to the total Computstat "aggregate" level using sales-weighted sector-level harmonic means. Arguments for the use of the sales weighted harmonic mean measure of the sector or aggregate markup can be found in Grassi (2017), Baqaee and Farhi (2020), Edmond, Midrigan and Xu (2023) or Hasenzagl and Pérez (2023) and follows from the aggregation of the Lerner index across firms.[14]

Figure 9 illustrates our estimates of the aggregate markup assuming either translog production functions (Panel A) or Cobb-Douglas production functions (Panel B). The levels of the estimated aggregated markups and their time paths are very similar for the two alternative specifications. For the translog specification, we find a 15 percentage point increase in the aggregate markup over the sample from around 10 percent at the beginning of the sample, to around 25 percent in the 2020s, while for the Cobb-Douglas specification it rises 18 percentage points from 15 percent at the beginning of the sample to around 33 percent by the 2020s. The two estimates also agree on the variations over time both displaying a

[14] Equivalently, sector level markups can be computed as employment-weighted arithmetic means, see Edmond, Midrigan and Xu (2023)

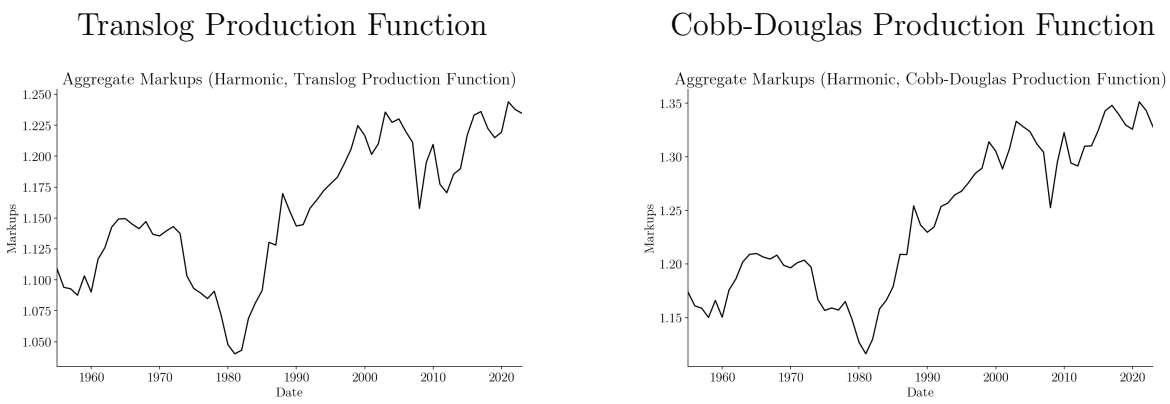


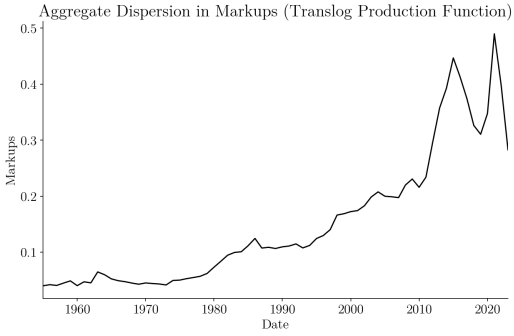
Figure 9: Aggregate markup Estimates

rise in markups from the early 1960s to the early 1970s followed by a marked decline in the 1970s up to the early 1980s. From the early 1980s, as has been much discussed, aggregate markups display strong and sustained growth for two decades rising from levels of 5 percent (12 percent) in the early 1980s to 24 percent (33 percent) by the mid 2000s for the translog (Cobb-Douglas) specification.

Our aggregate markup estimates agree with those of Hasenzagl and Pérez (2023) who impose Cobb-Doublas technologies, but use investment when constructing the control function. However, the rise in the aggregate markup that we estimate is much smaller than the one in de Loecker, Eeckhout and Unger (2020) who find a rise of more than 40 percentage points over the same period. As discussed in detail by Hasenzagl and Pérez (2023), the main source of difference derives from the use of harmonic means when aggregating. Figure A.2 in the appendix shows that our estimates are similar to those of de Loecker, Eeckhout and Unger (2020) when using arithmetic averages.

In Figure 10 we illustrate the dispersion of markups across firms. Regardless of the production function specification, we find a strong rise in the dispersion of markups across Compustat firms that starts in the late 1970s/early 1980s and continues up to around 2010 when it accelerates significantly until 2015 after which it becomes very volatile. In models with linear pricing, this rise in dispersion can be interpreted in terms of increasing misallocation. An important consideration regards the relationship to misallocation. In environments with linear pricing, markup dispersion is a sufficient statistic for factor misallocation because

Translog Production Function



Cobb-Douglas Production Function

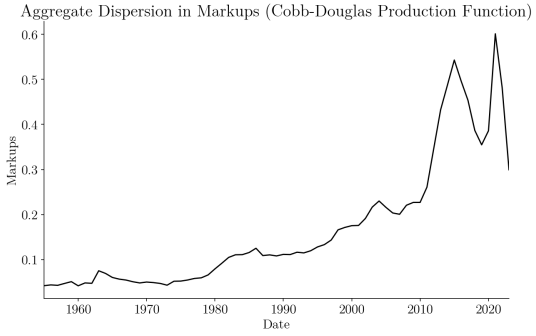


Figure 10: markup Dispersion

firms with high markups sell "too little" relative to the case in which factors are efficiently allocated across firms, see e.g. Baqaee and Farhi (2020), Edmond, Midrigan and Xu (2023).[15]

5 The Dynamic Impact of Antitrust Activity: Sector Level Evidence

5.1 Approach

We now exploit the indicators of antitrust activity constructed in Section 3 to estimate the dynamic impact of antitrust. We initially focus on sector-level evidence and aggregate the firm-level estimates to the 2-digit sector level. With these indicators in hand, the dynamic effects of antitrust are estimated using a panel local projection (LP) approach, see e.g. Dube et al (2025).

The estimating relationships are specified as:

$$y_{h,t+s} - y_{h,t-1} = \delta_{t,s} + \psi_{h,s} + \beta_{h,s} \triangle \nu_{h,t} + \Gamma_{h,s} \triangle X_{h,t-1} + e_{h,t+s} \tag{13}$$

where $y_{h,t} \in X_{h,t}$ denotes the outcome in question for sector $h \in H$, and $\nu_{h,t}$ is the estimated impact of antitrust cases against firms in sector h at date t. In order to control for other shocks, we allow for an aggregate time-fixed effect, $\delta_{t,h}$, for sector-specific linear trends

[15] Bornstein and Peter (2025) show that this result may be sensitive to non-linear pricing which induces misallocation across consumers.

through $\psi_{h,s}$, and for a rich set of controls. We specify the latter by controlling for lags of the entire set of outcomes that we consider. Because of differencing, sector-fixed effects do not appear in the panel LP regressions but are present in the implicit levels specifications. We compute standard errors with the Driscoll and Kraay (1998) method using 8 lags. Moreover, due to high persistence of the data, we correct for the Nickell-bias using a split-panel Jackknife estimator, see Dhaene and Jochmans (2015).

We focus on 2-digit BEA/NAICS sector-level aggregation. The data are annual and cover the sample period 1957-2023. We examine the dynamic impact on the following vector of outcomes: real GDP and price levels obtained from the BEA, operating profits measured from Compustat, the sales share of the treated sector measured as total sales of Compustat firms in the respective sector relative to total sales of Compustat firms, the 2-digit concentration measured by the Herfindahl index (concentration), markups and markup dispersion as estimated in Section 4, and TFPR. All variables are measured in natural logarithms. We measure TFPR from the production function estimates in Section 4 and aggregate within sectors. In the baseline regressions we use the trans-log production function estimates for markups and productivity, but we also show results for the Cobb-Douglas specification. We include various indicators of competition in the analysis because focusing on a single measure is potentially misleading. As discussed by e.g. Syverson (2019), market concentration is an outcome determined by the conditions of competition and a high level of concentration may not necessarily be indicative of a less competitive environment. markups are in principle a better indicator, but are harder to measure. By looking at a set of indicators, we hope to be able to make better conclusions on how antitrust affects market power and competition.

5.2 Results

Figure 11 illustrates the dynamic responses of indicators of competition and market power in response to the antitrust indicator related to civil non-merger and criminal cases. These variables are the operating profits, the markup and the Herfindahl index. We show the point estimates for a forecast horizon going up to 5 years along with 68 percent and 90 percent confidence bands indicated by the dark and light shaded areas, respectively.

We find that when a sector is subject to more intense antitrust activity, the operating

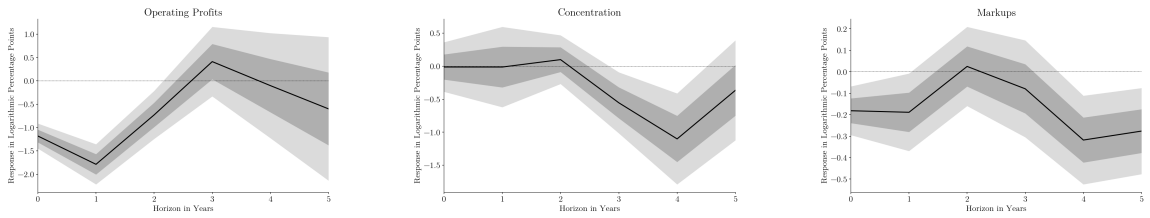


Figure 11: The Dynamic Impact of Civil Non-merger and Criminal Cases on Competition

profits of the Compustat firms in the sector fall abruptly upon impact and remain depressed for an additional two years after which they recover. Accompanying the decline in profits, we also find a persistent decline in markups which is significant at the 90 percent level upon impact, and in the first, fourth and fifth year thereafter. The signs of increased competition are also reflected in the Herfindahl index but occurs with some delay. Thus, on the basis of these indicators, we find that more intense antitrust activity against civil non-merger and criminal offenses induce a decline in market power of firms in the treated sector relative to other sectors.

Figure 12 illustrates the impact of the antitrust measure on other outcomes. Most significantly, we find an abrupt decline in the Compustat firms' capital expenditures which takes place upon impact and lasts for the first two years thereafter. Moreover, although there is some recovery of capital expenditures in the third and fourth years, the results indicate a significant decline in investments. Alongside the decline in investment, we also estimate an increase in markup dispersion of the treated sector which persists for four years and is significant at the 68 percent level upon impact and in the first and fourth year. The decline in capital expenses and the increase in markup dispersion is accompanied by a persistent decline in BEA real value added of the sector which is significant at the 68 percent level for the first four years. The decline in real value added is not reflected in the sales share of the Compustat firms in the sector apart from in the very first year. This may indicate either an impact on costs or that a part of the transmission occurs through non-listed firms. In favor of the cost channel, our results indicate a decline in TFPR until four years after the treatment. Finally, we find a temporary reduction in prices upon impact which is reversed at longer forecast horizons.

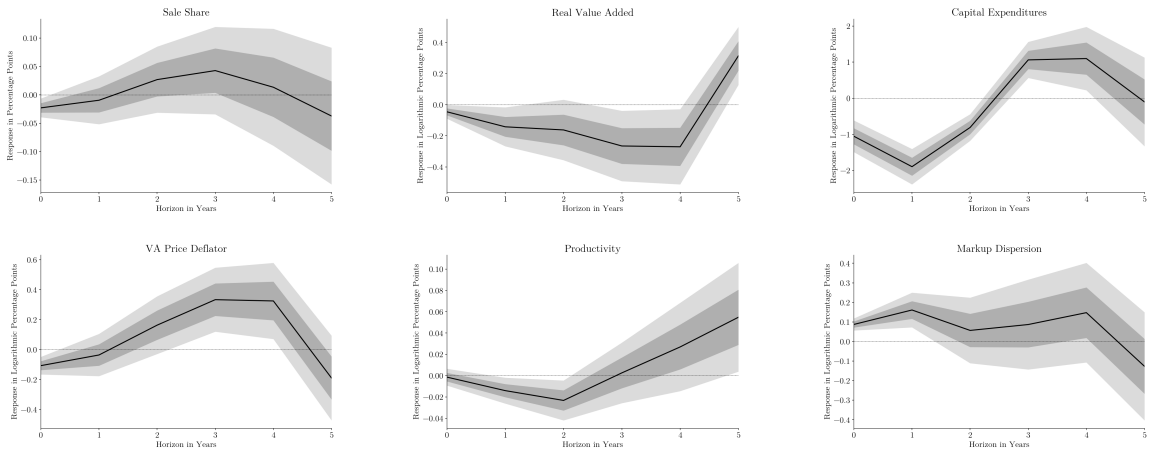


Figure 12: The Dynamic Impact of Civil Non-Merger and Criminal Cases on Sector-Level Outcomes

These results indicate that although increased antitrust activity related to civil non-merger and criminal violations of antitrust laws, is associated with declining market power, there is little evidence of beneficial effects on the economy in the short to medium-run. Importantly, our results indicate detrimental effects in terms of capital investment, misallocation and productivity. Interestingly, Werden (2014) argues that the rule of reason is focused solely on competition rather than on efficiency and welfare. Nonetheless, given that our estimates are ultimately based on cross-industry comparisons, it is unclear whether the results mask the impact of spillovers on other industries (i.e. whether there is a "missing intercept" issue). Below we look at aggregate effects to examine these issues in more detail.

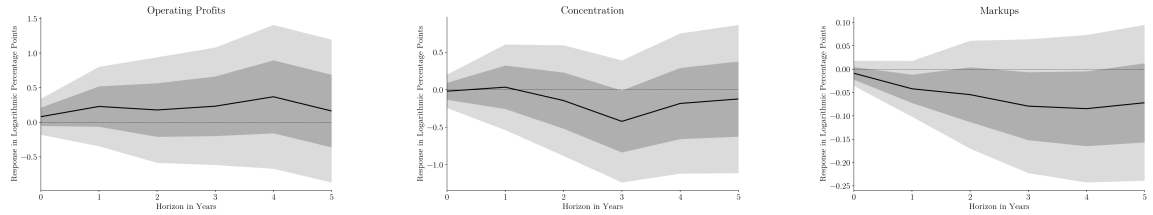


Figure 13: The Dynamic Impact of Civil Merger Cases on Competition Measures

Figure 13 illustrates the dynamic impact of the merger-case based antitrust measure on the market power indicators. Consistently with the results for civil non-merger and criminal cases, we find that firm concentration and markups decline persistently in the treated sector

relative to other ones. The impact on markups occur more gradually over time than for civil-merger and criminal cases, but also appears more persistent. The decline in the Herfindahl index is less precisely estimated than for civil non-merger and criminal cases but is significant at the 68 percent level at the 3 year horizon. We fail to find a significant impact of merger related antitrust on operating profits which, if anything, rise. Nonetheless, the fact that firm concentration and markups both decline make the results broadly consistent with merger cases stimulating competition. In this respect, it also has to be taken into account that many M&A cases may impact on competition measures over the longer run which is harder to estimate precisely.

The wider sector-level impacts of the antitrust civil merger indicator are shown in Figure 14. In contrast to the results for civil non-merger and criminal cases, we find that prevention of mergers stimulate capital expenses of firms in the treated sector relative to other sectors. The rise in capital expenditures is estimated to be both large and persistent. Moreover, more intense antitrust activity related to merger-related antitrust offenses is associated with a decline in markup dispersion which is statistically significant at the 90 percent level in the third, fourth and fifth year. These results are in stark contrast to the results reported earlier for civil non-merger and criminal cases. More in line with the results in Figure 12, we find a negative impact on real value added, but it is quantitatively small and only significant at the 68 percent level at the 3 year forecast horizon. In the case of merger cases, a key consideration always regards the trade-off between productivity and market power effects of M&A. Thus, it is perhaps not surprising that we find that the sector-level relative TFPR declines in the short to medium run in the aftermath of an increase in civil merger related antitrust measure.

5.3 Robustness

It is important to establish robustness of our results to various measurement-related issues.

5.3.1 The Technology Specification

A first issue that we will examine concerns the measurement of productivity and markups. The results reported above build on translog production function estimations as in equation

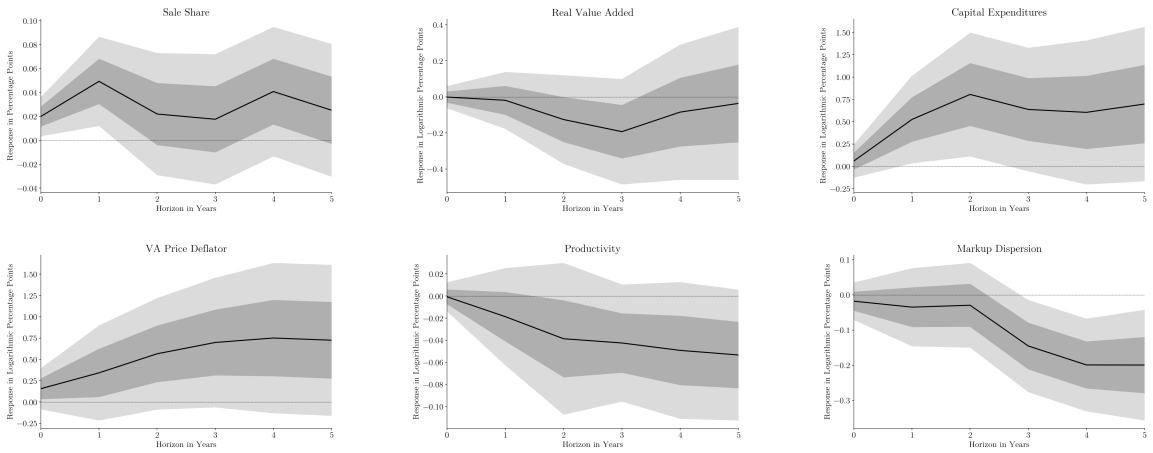


Figure 14: The Dynamic Impact of Civil Merger Cases on Sector Level Outcomes

(10). The translog-specification has the advantage that it allows one flexibly to allow for differences in output elasticities across firms, sectors, and over time. On the other hand, because of this flexibility, the translog specification may also some induce instability in estimates of markups and productivity at the firm-level. For that reason we now examine robustness to estimating markups and TFPR from Cobb-Douglas production function specification as in equation (11) imposing parameter homogeneity at the 2 digit level.

Table A.1 in the Appendix reports the estimates of the elasticity of output to the variable inputs (θ_v), the elasticity to the dynamic inputs (θ_d), and the implied returns to scale (ξ) at the 2-digit NAICS level for the translog and Cobb-Douglas production function specifications. For the translog case we report the coefficients evaluated at the mean factor inputs. The estimates are very similar across the two technologies apart from construction (sector 23) where the Cobb-Douglas specification implies a smaller variable input share than the translog case, FIRE (sector 52) and Education services (61), where the variable input elasticities are estimated to be higher for the Cobb-Douglas specifications. However, the parameter estimates are very similar in the great majority of cases.

Figure 15 illustrates the estimates of the dynamic impact of antitrust on markups, markup dispersion and on TFPR when assuming a Cobb-Douglas technology. Given the evidence discussed above, it is not surprising that the results are very similar to those discussed earlier for the translog case: Higher antitrust activity leads to a decline in markups while the impact

on markup dispersion depends on the antitrust violations with dispersion declining in the case of M&A type violations while it rises for civil non-merger and criminal cases. The impact on TFPR for M&A violations is also very similar to the estimates from the translog case. The only instability in the results concern the impact on TFPR for civil non-merger and criminal cases. For this variable, the translog case indicates a decline over the first 3 years while the Cobb-Douglas case implies a rise in TFPR. TFPR is inherently difficult to estimate and to interpret, and this result probably shows that these responses must be interpreted with due care. For all other outcomes discussed for the baseline specification, the dynamic responses are very similar for the Cobb-Douglas technology.

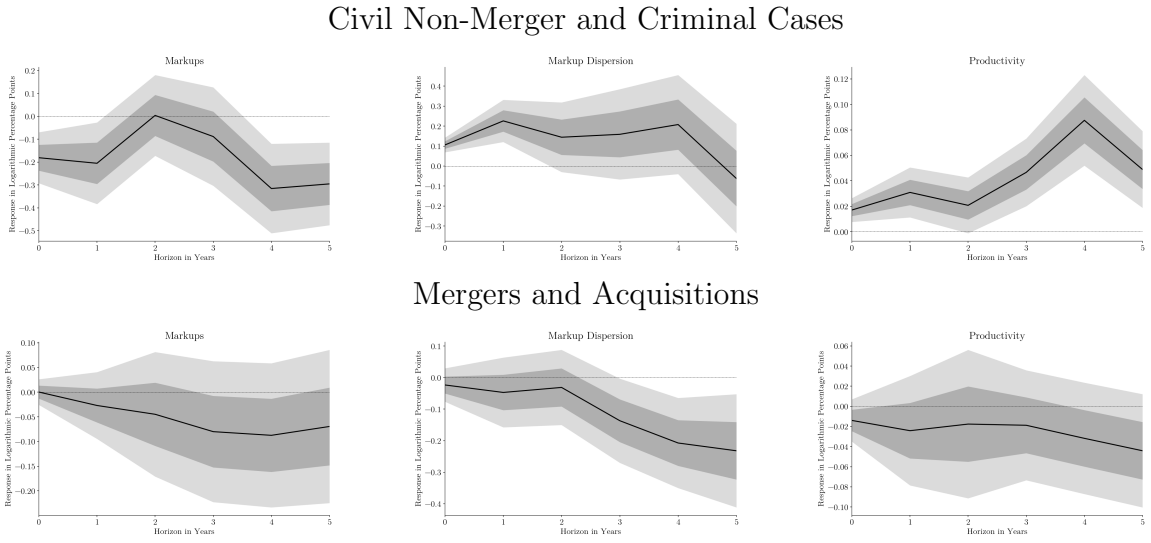


Figure 15: Cobb-Douglas Production Function

5.3.2 Excluding FIRE

A second concern with our results is that we have included FIRE in the baseline estimates. Many analyses involving estimates of markups or productivity based on the production-function approach exclude FIRE because of problems estimating production functions, and therefore markups and TFPR, for this sector.

In Figure 16 we report the impact of antitrust activity on three key outcomes, capital expenses, markups and markup dispersion when we exclude FIRE from the analysis. The responses of these variables are very similar to the baseline estimates apart from the decline

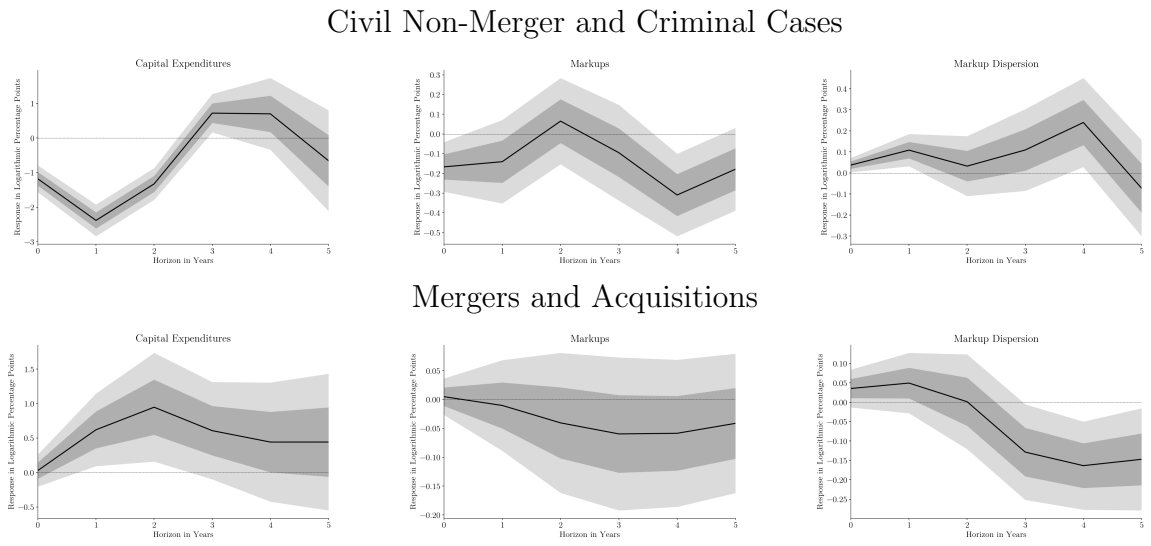


Figure 16: Excluding FIRE

in markup dispersion appearing with a delay of two years for civil mergers and acquisitions. The robustness of the results hold for all other outcomes too.

One may similarly consider excluding utilities from the analysis. In the Appendix Figure A.3 illustrates the outcomes for a specification of the panel LP regressions where we exclude this sector. The results are almost identical to those of the baseline specification.

5.3.3 Excluding the Early Sample

A third consideration relates to the sample period. As we discussed in Section 2, the period from the late 1950s to the early 1960s witnessed very elevated levels of antitrust activity especially on the part of the FTC. One might be concerned that our results therefore are driven by the experiences of this part of the sample period and not representative of the whole sample.

We illustrate the results for the key variables when starting the sample in 1962 in Figure 17. Again, we find that the key results are robust, although there is some impact on the size of the impact on capital expenditures, and a more pronounced decline in markups in response to the antitrust indicator for civil non-merger and criminal cases. For mergers and acquisitions, we similarly find a more muted rise in capital expenditures. On all other outcomes, the results are as good as unchanged.

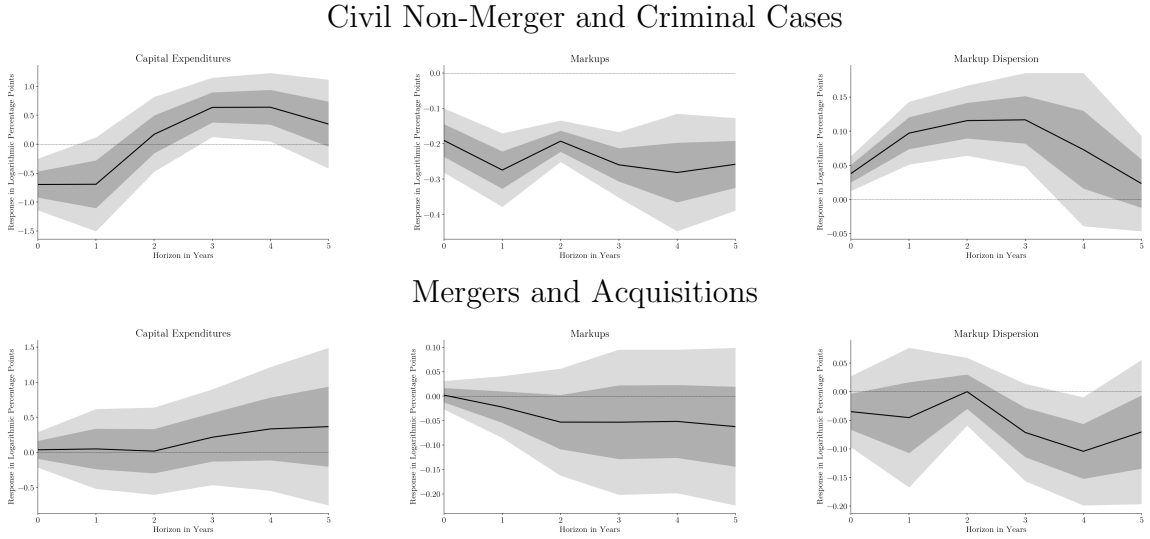


Figure 17: Estimates for the 1962-2023 Sample Period

We have also examined robustness along a number of other dimensions such as eliminating the rich set of controls, not allowing for sector-specific trends, evaluating the cumulated excess returns at shorter or longer horizons when building the antitrust activity indicators, and changing the cut-off level for the cumulated excess returns when constructing the antitrust indicators. We find robustness of all the results in these dimensions too.

6 Aggregate Results

The results presented in the previous section suggest that the two types of antitrust cases both stimulate competition but have contrasting impacts on sector-level outcomes. This leaves open the question of their aggregate effects due to the "missing intercept" problem which arises due to general equilibrium effects and spillovers. Thus, we now turn to estimates of the dynamic macroeconomic effects by aggregating the antitrust indicators across sectors and exploring how they affect aggregate U.S. economic outcomes.

For this purpose we estimate local projections of the form:

$$y_{t+h} = \alpha_h + \delta_h t + \beta_h \nu_t + \Gamma_h X_{t-1} + u_{t+h} \tag{14}$$

where ν_t is the antitrust indicator aggregated across firms and sectors.

We estimate the local projections for a rich set of outcomes. As above we examine the impact on markups, markup dispersion, and TFPR estimated from the translog specification of the technology. To this we add BEA data on real profits, real GDP, real consumption, real investment, real R&D spending, the unemployment rate, real hourly earnings, and labor productivity. We also examine the impact on the fed funds rate, CPI inflation, and real stock prices. All variables apart from the unemployment rate and the federal funds rate are measured in natural logarithms. Whenever estimating the outcome for a variable y, we include two lags of this variable among the controls in X_{t-1}. Furthermore, in the vector of controls we include two lags of markups, real GDP, inflation, the fed funds rate, real stock prices, R&D, real earnings, the unemployment rate, and real corporate profits. We apply the bias correction for short time series by Herbst and Johannsen (2024), and we compute Newey-West standard errors (using eight lags).

6.1 Civil Non-Merger and Criminal Cases

Figure 18 reports the responses of markups, real profits and markup dispersion to the civil non-merger and criminal case antitrust indicator. We show the point estimates along with 68 and 90 percent confidence bands. The results echo those that we found for the sector level analysis: An increase in antitrust activity depresses real profits and gives rise to a persistent decline in markups while markup dispersion rises. Thus, in line with the results in Section 5, we find signs of increased competition, but also more misallocation indicating economy-wide impact beyond the sector-level results discussed earlier.

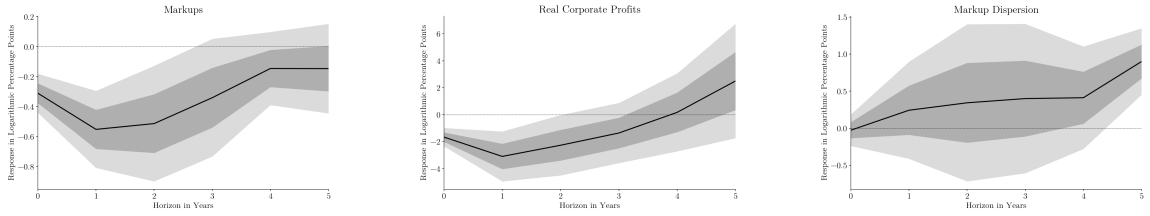


Figure 18: The Dynamic Impact of Civil Non-Merger and Criminal Cases on Competition Measures

In Figure 19 we report the results for real outcomes. The results indicate persistent negative effects on the economy of civil non-merger and criminal case antitrust activity. In particular, we find a persistent and significant impact on real GDP, consumption, investment and R&D which are accompanied by declining labor earnings, and a rise in unemployment. The negative effects are particularly pronounced for real investment and R&D spending and appear to be very persistent without any signs of recovery within the forecast horizon that we examine apart from unemployment which shows signs of recovery after 4 years. Furthermore, we also find a persistent decline in TFPR while labor productivity hardly moves.

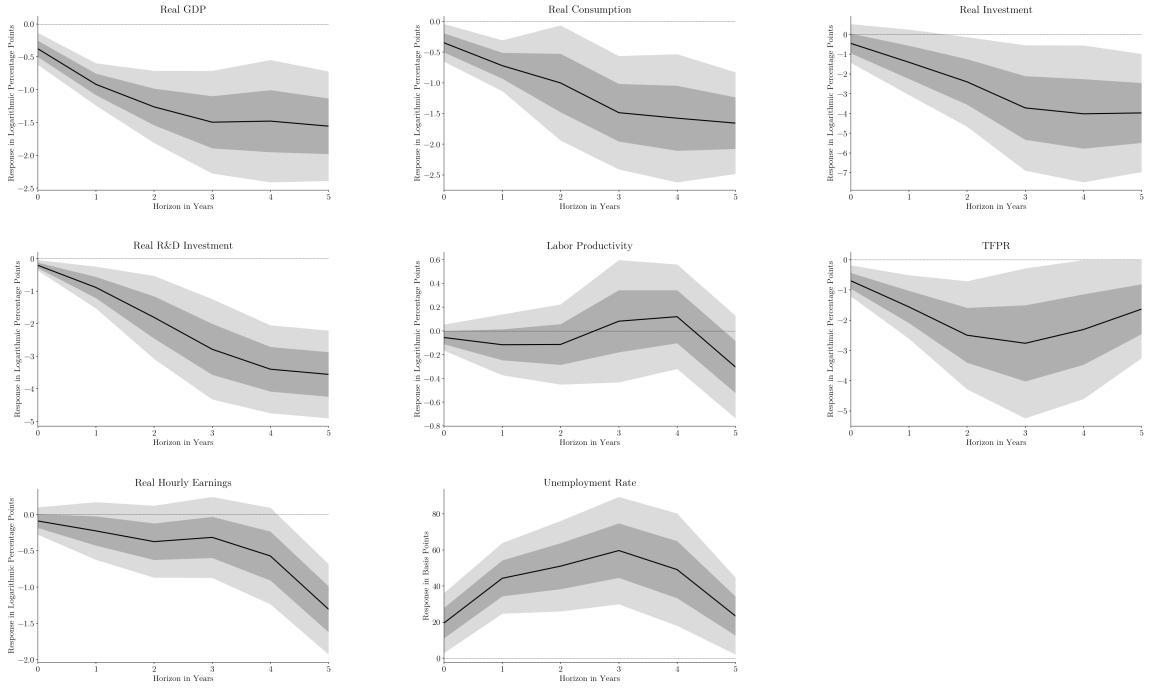


Figure 19: The Dynamic Impact of Civil Non-Merger and Criminal Cases on the Real Economy

Finally, Figure 20 shows the impact of the antitrust indicator on the federal funds rate, CPI inflation and real stock prices. The results indicate that the Fed leans against the wind and reduces the short-term nominal interest rate in response to the impact of the antitrust indicator on the economy. Hence, there are conflicting pressures on inflation given the decline in markups and the real economy combined with a monetary stimulus. We find the net effect to be a temporary drop in inflation. Last, we find that real stock prices decline indicating

that the decline in real interest rates do not offset the impact on expected stock returns.

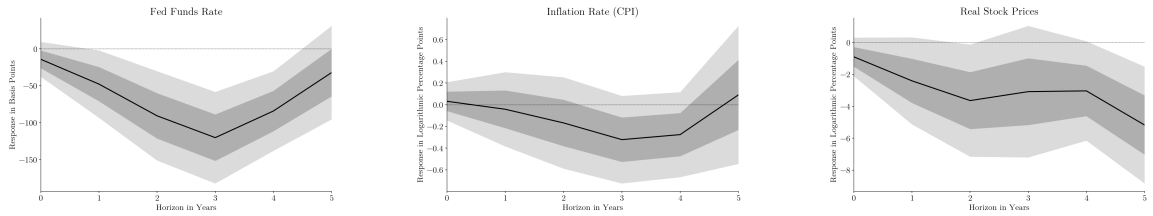


Figure 20: The Dynamic Impact of Civil Non-Merger and Criminal Cases on Asset Markets and Inflation

To sum up, our results indicate that more intense antitrust activity in terms of civil non-merger and criminal cases raised against publicly traded firms depresses market power, but at the cost of a negative impact on the economy. We have investigated the robustness of these results to the measurement of markups and TFPR using the results from the Cobb-Douglas specification of the production function, to elimination of the time-trend in the LP regressions, and to the measurement of the antitrust indicator using either different forecast horizons for the cumulated excess returns or different filters for the minimum impact on abnormal returns. We find remarkable robustness with the only exception, as discussed earlier for the sector-level results, that the TFPR response is sensitive to the specification of the production function: When assuming a Cobb-Douglas technology, we find only a mild decline in TFPR.

6.2 Civil Mergers and Acquisitions

Next, we turn to the aggregate impact of civil merger antitrust investigations. Figure 21 shows the impulse responses of markups, corporate sector profits and markup dispersion to the merger-based antitrust indicator. We find that more intense antitrust activity induces a decline in markups and in corporate sector profits. Both of these variables respond with some delay and with a lower elasticity than for the civil non-merger and criminal case indicator but are more indicative of pro-competitive effects than the corresponding sector-level estimates. As far as markup dispersion is concerned, we find some decline in the short run which reverses at longer forecast horizons but standard errors are large and sampling uncertainty does not

permit us to reach a clear conclusion on misallocation.

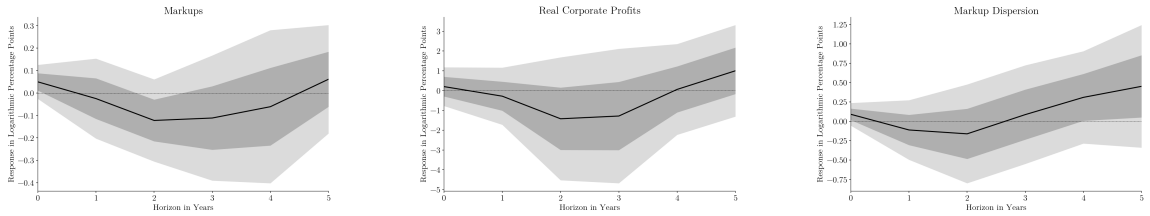


Figure 21: The Dynamic Impact of Civil Merger Cases on Competition Measures

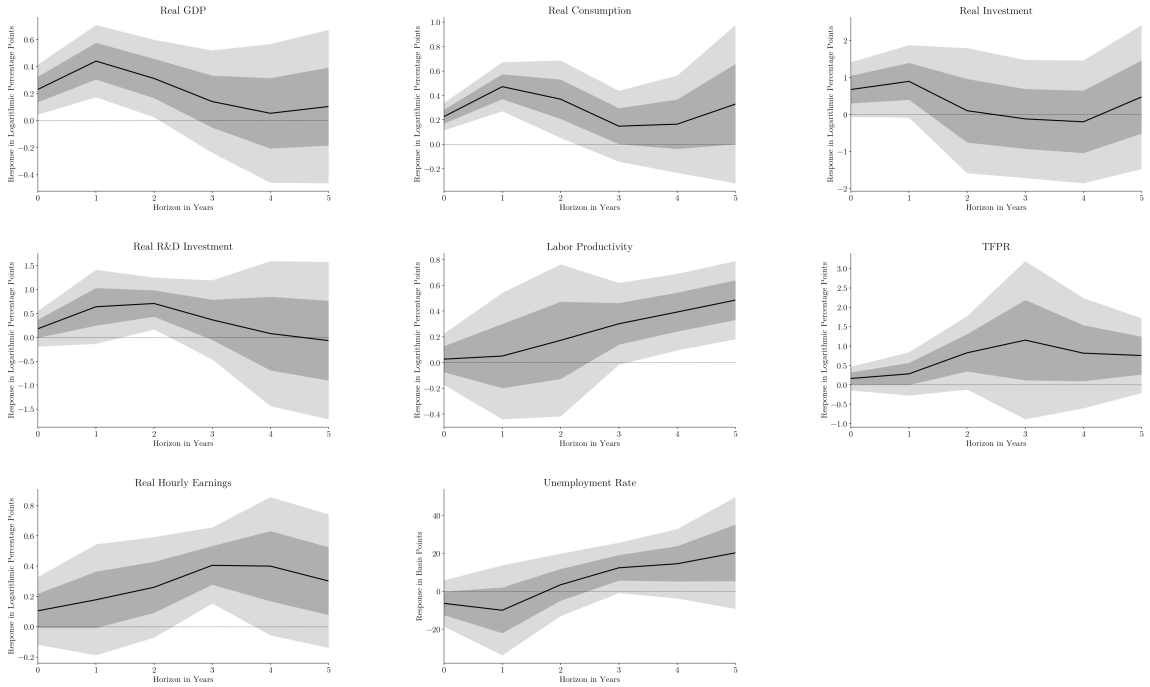


Figure 22: The Dynamic Impact of Civil Merger Cases on the Real Economy

The impact on the real economy is illustrated in Figure 22. We find that more intense antitrust merger enforcement have a stimulatory impact on the economy. In particular we find a rise in real GDP which is significant on impact and two (three) years thereafter at the 90 percent (68 percent) level. Accompanying the rise in real GDP we find that private sector consumption, real investment and real R&D spending all rise. The short-run impact on investment is large but imprecisely estimated while the rise in consumption is more moderate but more precisely estimated with significance at the 90 percent level for three years.

We also find positive responses of real hourly earnings and of labor productivity in response to an increase in the merger case related antitrust indicator while unemployment declines, albeit only very temporarily. Finally, we find an increase in TFPR but sampling uncertainty is admittedly high for this variable.

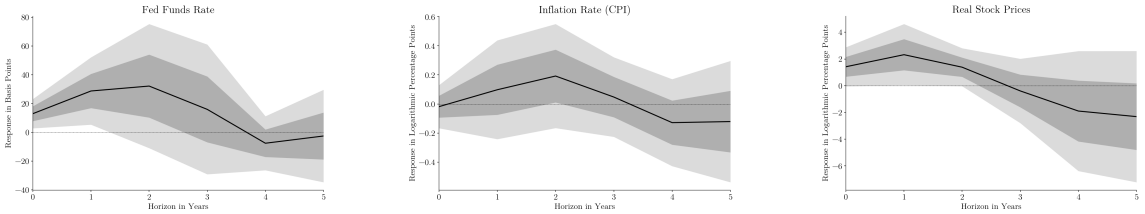


Figure 23: The Dynamic Impact of Civil Non-Merger and Criminal Cases on Asset Markets and Inflation

Finally, in Figure 23 we show the impact on the fed funds rate, CPI inflation and real stock prices. Most likely due to the impact of antitrust activity on aggregate activity, we find that the fed funds rate and CPI inflation rise marginally in response to more intense civil merger related antitrust activity for the first 3-4 years after which the effect dies out. In terms of the stock market, we estimate a positive effect on firm valuations on impact and in the first two years thereafter suggesting that preventing mergers have beneficial effects on the valuations of other firms in the economy.

The contrast between the results estimated for civil non-mergers and criminal cases and those for civil merger cases is striking. Our results suggest that prevention of anti-competitive mergers have benefitted the economy, while we fail to uncover such evidence for civil non-merger and criminal cases. These results also indicate that the change in antitrust activity that occurred around 1980 which witnessed a stark decline in the intensity of indictments for civil non-merger and criminal cases and increased focus on mergers may have been for the better in terms of protecting the economy against the harmful aspects of firm concentration.

6.3 Quarterly Results

These result discussed so far are all based on annual data. All other things equal, it would be preferable to study quarterly data since this gives us many more observations and can

address issues related to time-aggregation. The main issue here is that the markup, markup dispersion and TFPR estimates are derived from annual Compustat data. Quarterly Compustat data are only available for a much shorter sample and for a subset of the variables for a subset of the firms. As an alternative, we now consider proxying markups by the inverse of the labor share which permits one to generate quarterly estimates of the impact of antitrust. This does not come without costs since it does not allow us to control for markup dispersion or to study market concentration. Furthermore, the (inverse) labor share only proxies for the Lerner index under special circumstances, so our results should be taken with a grain of salt.

We show the results in Figure 24 for a forecast horizon going up to 20 quarters. The qualitative results for the civil non-merger and criminal cases are very similar to the annual results discussed above. We find that the labor share increases in response to more intense antitrust activity which is consistent with the drop markups discussed above. In the annual data, we found that the drop in markups persists for four years and in the quarterly data we find that the rise in the labor share persists for 15 quarters. As in the annual data, we also find a negative and very persistent impact on real GDP, consumption, investment, and R&D spending. The main difference relative to the annual results is that we fail to find a significant impact on real hourly earnings. Nonetheless, the quarterly results are remarkably similar to those we found in the annual data. For civil merger cases, we instead fail to find an impact on the labor share. This may indicate that the labor share is poor proxy for the markup in this case and/or that the lack of control for markup dispersion and market concentration impair the estimation. For this reason, the quarterly results are hard to interpret in this case. Interestingly, though, there is still a stark contrast to the results for the civil non-merger and criminal cases because we find a positive impact of the civil merger antitrust indicator on labor productivity and real hourly earnings while the estimated responses of real output, investment and R&D spending are insignificantly different from zero.

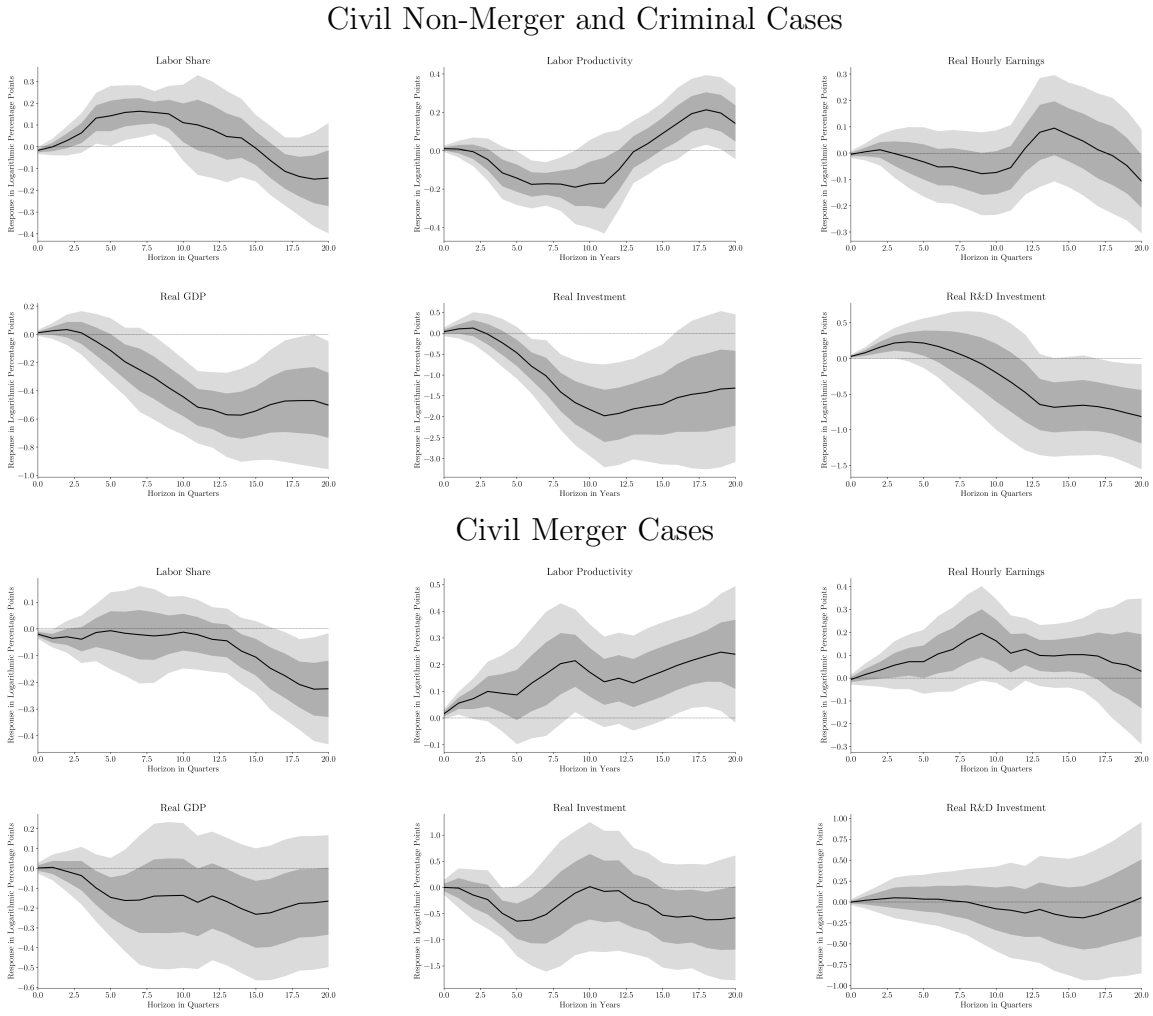


Figure 24: Quarterly Results

7 Conclusion

In this paper we have examined the link between antitrust activity, competition and aggregate outcomes. We collected data on U.S federal antitrust investigations since the mid-1950s. We documented a fundamental shift in the focus of antitrust away from civil non-merger and criminal cases towards civil merger related violations of antitrust laws, a shift that is particularly evident for publicly listed firms. We also documented that publicly listed firms indicted for antitrust violations are heavily selected from the right tail of the firm-size distribution even when comparing only to other publicly listed firms.

We map corporate firms involved in these cases to Compustat data and estimate how

firm valuations are affected by antitrust activity. We demonstrate that, on average, firms that are indicted for antitrust violations suffer significant negative abnormal stock returns after the antitrust cases are opened. The abnormal returns are larger for civil merger related violations than for civil non-merger and criminal cases. We exploit the firm-level estimates to build indicators of antitrust activity measured as the impact on firm valuations within a certain sector or across the economy. These indicators show meaningful variation over time and sectors which allow us to estimate their impact on the economy.

We found that when a sector is more intensively subjected to antitrust activity, indicators of market power decline. However, the impact on other outcomes differ markedly across the type of antitrust violation in question. In particular, we find that increased antitrust activity related to civil merger related violations of antitrust stimulate the economy including investment in capital goods, spending on R&D, labor productivity, and real wages, while increased antitrust activity related to civil non-merger and criminal cases has the opposite effects. Consistently with this, we also find that markup dispersion declines in the former case but increases for civil non-mergers and criminal cases.

One interpretation of our results is that antitrust activity related to civil non-merger and criminal cases potentially has been counterproductive because, while stimulating competition, it has had negative consequences on the economy. To the extent that one can consider the antitrust activity indicator as proxying for changes in competition, the results are also consistent with the fact that certain types of changes in market power may be good for the economy. Given the fact that this type of antitrust activity relates mainly to the pre-1980 sample, it would be consistent with this sample being subject to forces that at the same time stimulated the economy and lead to increased market power. In contrast, preventing anti-competitive mergers appear to stimulate both competition and the overall economy. Thus, the rise in merger activity that occurs around 1980 may have been associated with "bad concentration" which the antitrust authorities attempted to lean against. Nonetheless, given the nature of our analysis, we leave a full analysis of this to further research. These interpretations would be consistent with the "good" vs. "bad" concentration hypothesis of Covarrubias, Gutiérrez, and Philippon (2020), perhaps combined with issues related to the implementation of antitrust laws in terms of the importance of competition vs. efficiency

considerations.

Investigating the underlying structural interpretation of the results in more detail is clearly important and is the focus of our ongoing research. In this respect, Amorim Cabaço (2025) examines the impact of mergers on the economy using structural firm-dynamics model and argues that the anti-competitive effects of mergers dominate the pro-productivity effects. It would follow from this that the change in antitrust focus that we have documented in the data has been warranted, but perhaps also that enforcement has been too lax to prevent rising market power.

References

Acemoglu, Daron, Simon Johnson, Amir Kermani, James Kwak, and Todd Mitton (2016): "The Value of Connections in Turbulent Times: Evidence from the United States," *Journal of Financial Economics*, 121, 368-91.

Ackerberg, Dan, K. Caves, and Garth Frazer (2015): "Identification Properties of Recent Production Function Estimators," *Econometrica*, 83(6), 2411-51.

Aguzzoni, Luca, Gregor Langus, and Massimo Motta (2013) "The Effect of EU Antitrust Investigations and Fines on a Firm's Valuation," *The Journal of Industrial Economics*, 61(2), 290-338.

Amorim Cabaço, Brian (2025): "The Macroeconomic Effects of Mergers and Acquisitions in the United States," manuscript, University College London.

Ash, Elliott, Daniel L. Chen and Suresh Naidu (2025): "Ideas Have Consequences: The Impact of Law and Economics on American Justice," *Quarterly Journal of Economics*, forthcoming.

Autor, David, David Dorn, Lawrence F. Katz, Christina Patterson, and John van Reenen (2020): "The Fall and Rise of the Labor Share and the Rise of Superstar Firms," *Quarterly Journal of Economics*, 645-708.

Babina, Tania, Simcha Barkai, Jessica Jeffers, Ezra Karger, and Ekaterina Volkova (2023): "Antitrust Enforcement Increases Economic Activity," NBER Working Paper, no.31597.

Baqaee, David Rezza, and Emmanuel Farhi (2020): "Productivity and Misallocation in General Equilibrium," *Quarterly Journal of Economics*, 135(1), 105-63.

Barkai, Simcha (2020: "Declining Labor and Capital Shares," *Journal of Finance*, 75(5), 2421-2463.

Berry, Stephen, Martin Gaynor, and Fiona Scott Morton (2019): "Do Increasing Markups Matter? Lessons from Empirical Industrial Organization," *Journal of Economic Perspectives*, 33(3), 44-68.

Besley, Timothy, Nicola Fontana, and Nicola Limodio (2021) "Antitrust Policies and Profitability in Nontradable Sectors," *American Economic Review: Insights*, 3(2), 251-65.

Bittlingmayer, George (1992): "Stock Returns, Real Activity, and the Trust Question," *Journal of Finance*, 1992, 47:5, 1701-30.

Bornstein, Gideon, and Alessandra Peter (2025): "Non-Linear Pricing and Misallocation," *American Economic Review*, forthcoming.

Boehmer, Ekkehart, Jim Masumeci, and Annette B. Poulsen (1991): "Event-study Methodology under Conditions of Event-Induced Volatility," *Journal of Financial Economics*, 30(2), 253-72.

Bork, Robert H. (1978): "The Antitrust Paradox," New York: Basic Books.

Bosch, Jean-Claude, and E. Woodrow Eckard Jr. (1991): "The Profitability of Price Fixing: Evidence from Stock Market Rection to Federal Indictments," *Review of Economics and Statistics*, 73(2), 309-17.

Buccirossi, Paolo, Lorenzo Ciari, Tomaso Duso, Giancarlo Spagnolo, and Cristiana Vitale (2013): "Competition Policy and Productivity Growth: An Empirical Assessment," Review of Economics and Statistics, 95(4), 1324-1336.

Cavenaile, Laurent, Murat Alp Celik, and Xu Tian (2021): "The Dynamic Effects of Antitrust on Growth and Welfare," *Journal of Monetary Economics*, 121, 42-59.

Covarrubias, Matias, Germàn Gutiérrez, and Thomas Philippon (2020): "From Good to Bad Concentration? U.S. Industries over the past 30 Years," *NBER Macroeconomics Annual*, 34(1), 1-46.

Crane, Daniel A. (2014): "The Tempting of Antitrust: Robert Bork and the Goals of Antitrust Policies," *Antitrust Law Journal*, 79(3), 835-53.

de Loecker, Jan, and Jan Eeckhout (2018): "Global Market Power," NBER Working Paper, no.24768.

de Loecker, Jan, Jan Eeckhout, and Gabriel Unger (2020): "The Rise of Market Power and the Macroeconomic Implications," *Quarterly Journal of Economics*, 135(2), 561-644.

de Loecker, Jan, Pinelopi K. Goldberg, Amit K. Khandelwal, and Nina Pavcnik (2016): "Prices, Markups and Trade Reform," *Econometrica*, 84(2), 445-510.

de Loecker, Jan, and Frederic Warzynski (2012): "Markups and Firm-Level Export Status," *American Economic Review*, 102(6), 2437-71.

De Ridder, Maarten, Basile Grassi, and Giovanni Morzenti (2025): "The Hitchhiker's Guide to Markup Estimation: Assessing Estimates from Financial Data," forthcoming, *Econometrica*.

Dhaene, Geert, and Koen Jochmans (2015): "Split-panel Jackknife Estimation of Fixed-Effects Models," *Review of Economic Studies*, 82(3), 991-1030.

Driscoll, John C., and Aart C. Kraay (1998): "Consistence Covariance Matrix Estimation with Spatially Dependent Panel Data," *Review of Economics and Statistics*, 80(4), 549-60.

Dube, Arindrajit, Daniele Girardi, Òscar Jordà, and Alan M. Taylor (2025): "A Local Projections Approach to Differences-in-Differences," *Journal of Applied Econometrics*, forthcoming.

Döpper, Hendrik, Alexander McKay, Nathan H. Miller, and Joel Stibale (2024): "Rising Markups and the Role of Consumer Preferences," Manuscript, Georgetown University.

Edmond, Chris, Virgiliu Midrigan, and Daniel Yi Xu (2023): "How Costly Are Markups?" *Journal of Political Economy*, 131(7), 1619-75.

Gallo, Joseph C., Kenneth Dau-Schmidt, Joseph L. Craycraft, and Charles J. Parker (2000): "Department of Justice Antitrust Enforcement, 1955-1997", *Review of Industrial Organization*, 17(1), 75-133.

Ghosal, Vivek (2011): "Regime Shift in Antitrust Laws, Economics, and Enforcement," *Journal of Competition Law and Economics*, 7(4), 733–74.

Gilligan, Thomas W., William J. Marshall, and Barry R. Weingast (1989): "Regulation and the Theory of Legislative Choice: The Interstate commerce Act of 1887," *Journal of Law & Economics*, 32(April), 35-61.

Grassi, Basile (2017): "IO in I-O: Size, Industrial Organization, and the Input-Output Network Make a Firm Structurally Important," manuscript, Bocconi University.

Günster, Andreas, and Mathijs van Dijk (2016): "The Impact of European Antitrust Policy: Evidence from the Stock Market," *International Review of Law and Economics*, 46, 20-33.

Gutiérrez, Germán, and Thomas Philippon (2017): "Declining competition and Investment in the U.S.," NBER Working paper, no.23583.

Hall, Robert E. (1988): "The Relation Between Price and Marginal Cost in U.S. Industry," *Journal of Political Economy*, 96, 921-47.

Hall, Robert E. (2018): "Using Empirical Marginal Cost to Measure Market Power in the US Economy," NBER Working paper, no.25251.

Hasenzagl, Thomas, and Luis Pérez (2023): "The Micro-Aggregated Profit Share," working paper, University of Minnesota.

Herbst, Edward P., and Benjamin K. Johannsen (2024): "Bias in Local Projections," *Journal of Econometrics*, 240(1), 1-24.

Hoberg, Gerard, and Gordon Phillips (2016): "Text-Based Network Industries and Endogenous Product Differentiation," *Journal of Political Economy*, 124(5), 1423-65.

Karabarbounis, Loukas, and Brent Neiman (2019): "Accounting for Factorless Income," *NBER Macroeconomics Annual*, 33(1), 167-228.

Kolari, James W., and Seppo Pynnönen (2010): "Event Study Testing with Cross-sectional Correlation of Abnormal Returns," *Review of Financial studies*, 23(11), 3996-4025.

Kolari, James W., and Seppo Pynnönen (2011): "Nonparametric Rank Tests for Event Studies," *Journal of Empirical Finance*, 18(5), 953-71.

Lancieri, Filippo, Eric A. Posner, and Luigi Zingales (2022): "The Political Economy of the Decline of Antitrust Enforcement in the United States," Working paper no.2022-104, Becker Friedman Institute, University of Chicago.

Miller, Nathan H. (2024): "Industrial Organization and the Rise of Market Power," Manuscript, Georgetown University.

Newey, Whitney K., and Kenneth D. West. (1987): "Hypothesis testing with efficient method of moments estimation," *International Economic Review*, 777-787.

Olley, Steve g., and Ariel Pakes (1996): "The Dynamics of Productivity in the Telecommunications Equipment Industry," *Econometrica*, 64(6), 1263-97.

Patell, James M. (1976): "Corporate Forecasts of Earnings Per Share and Stock Price Behavior: Empirical Test," *Journal of Accounting Research*, 246-276.

Posner, Richard A. (1970): "A Statistical Study of Antitrust Enforcement," *The Journal of Law and Economics*, 13(2), 365–419.

Rossi-Hansberg, Esteban, Pierre-Daniel Sarte, and Nicholas Trachter (2021): "Diverging Trends in National and Local Concentration," *NBER Macroeconomics Annual*, 35, 115-50.

Syverson, Chad (2019): "Macroeconomics and Market Power: Context, Implications, and Open Questions," *Journal of Economic Perspectives*, 33(3), 23-43.

Traina, James (2018): "Is Aggregate Market Power Increasing? Production Trends Using Financial Statements," working paper, University of Chicago.

Werden, Gregory J. (2014): "Antitrust's Rule of Reason: Only Competition Matters" *Antitrust Law Journal*, 79(2), 713-59.

Werden, Gregory J. (2018): "Establishment of the Antitrust Division of the U.S. Department of Justice" *St. John's Law Review*, 92(3), 419-30.

A Additional Results

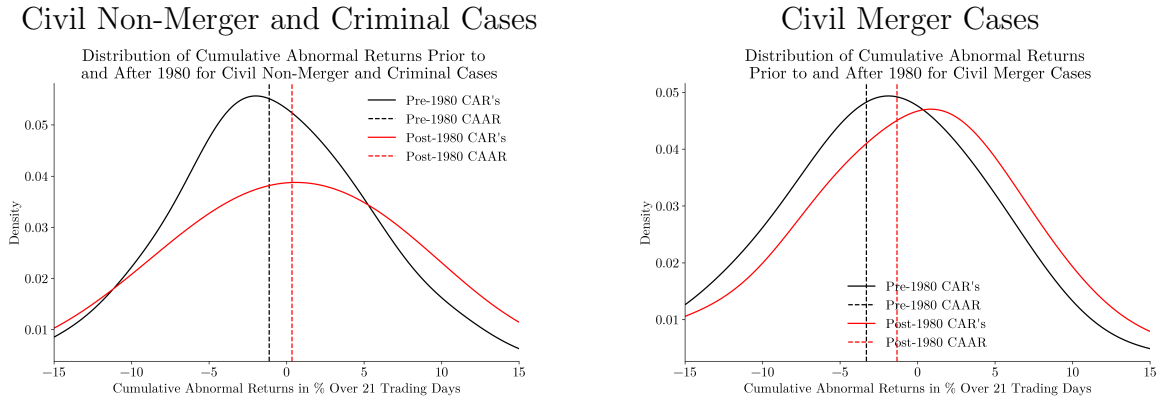
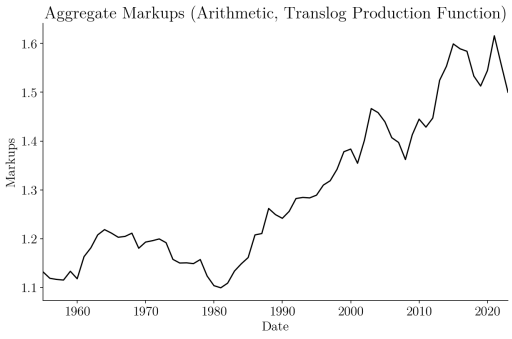


Figure A.1: Distribution of Cumulated Excess Returns (21 days post case opening)

Table A.1: Production Function Estimates

	Translog			**Cobb-Douglas**		
Industry	$\bar{\theta}_v$	$\bar{\theta}_d$	$\bar{\xi}$	$\bar{\theta}_v$	$\bar{\theta}_d$	$\bar{\xi}$
11	0.82	0.13	0.94	0.86	0.09	0.95
21	0.71	0.28	0.99	0.78	0.20	0.99
22	0.80	0.16	0.97	0.84	0.11	0.96
23	0.94	0.03	0.97	0.77	0.10	0.87
31	0.83	0.14	0.97	0.86	0.11	0.97
32	0.82	0.13	0.96	0.83	0.12	0.95
33	0.86	0.10	0.96	0.85	0.10	0.95
42	0.89	0.07	0.96	0.88	0.07	0.95
44	0.83	0.12	0.95	0.82	0.13	0.95
45	0.87	0.09	0.96	0.87	0.09	0.95
48	0.84	0.13	0.97	0.86	0.10	0.96
49	0.90	0.08	0.98	0.89	0.07	0.96
51	0.76	0.16	0.92	0.74	0.17	0.91
52	0.62	0.30	0.92	0.72	0.26	0.97
53	0.79	0.11	0.89	0.78	0.11	0.88
54	0.80	0.15	0.95	0.77	0.16	0.93
56	0.85	0.10	0.95	0.85	0.10	0.95
61	0.70	0.17	0.88	0.80	0.13	0.93
62	0.86	0.06	0.92	0.81	0.08	0.89
71	0.84	0.10	0.94	0.82	0.09	0.91
72	0.84	0.13	0.97	0.87	0.11	0.98
81	0.77	0.08	0.86	0.76	0.10	0.85

Translog



Cobb-Douglas

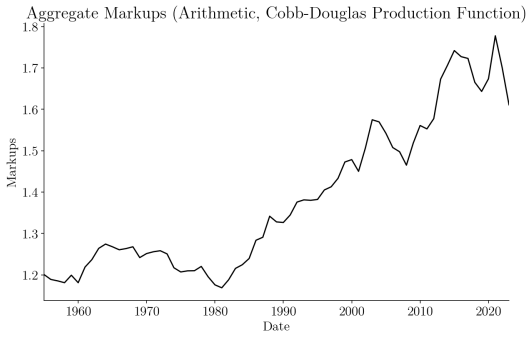


Figure A.2: Aggregate Markup Estimates: Arithmetic Means

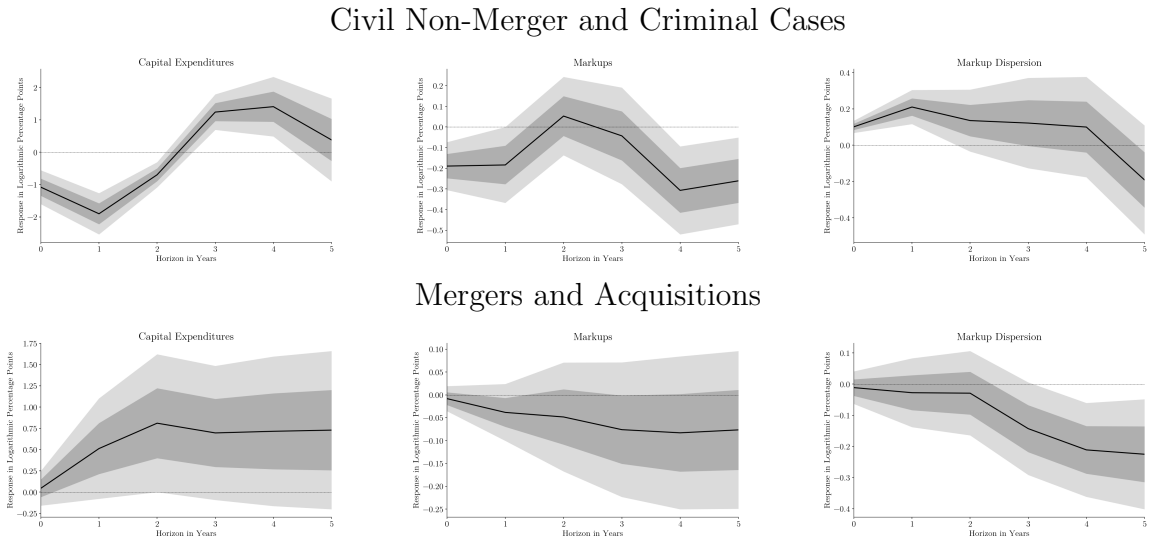


Figure A.3: Sector-Level Results when Excluding Utilities