Trading Off Stakeholder Interests: Evidence from Antitrust Investigations

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Abstract

We investigate how firms react when their industry comes under antitrust scrutiny. Using the cases opened by the European Commission between 1991 and 2019, we find that cartel investigations temporarily reduce the operating performance of firms in the affected industry. In response to the shock, firms engage in intense restructuring: they cut investment and sell assets; they undertake mass layoffs and reduce employment; they borrow more from suppliers but do not increase financial debt or change shareholder payouts. The effects are concentrated in non-tradable industries, where intrinsic protection from international competition makes collusion more effective.

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1 Introduction

Collusive agreements among firms are a common tool to extract consumer surplus and boost profits.[1] A growing body of work has analyzed the effects of anti-cartel laws and cartel investigations on corporate strategies and outcomes, such as performance (Günster, Carree, and van Dijk (2011)), capital structure (Dasgupta and Žaldokas (2019)), stock prices (Aguzzoni, Langus, and Motta (2013); Bos, Letterie, and Scherl (2019)) and mergers and acquisitions (Dong, Massa, and Žaldokas (2019)). However, no prior work has studied the impact of antitrust enforcement and the associated restructuring activities across different stakeholders. The redistribution effects of cartel investigations is an important question at a moment when growing anecdotal evidence suggests that consumer welfare (measured in terms of low prices) may come at substantial costs for other stakeholders.

The short- and long-term effects of antitrust shocks on input providers such as employees and suppliers are not clear *a priori*. This is particularly evident in the case of cartel investigations. On the one hand, firms belonging to a previously cartelized industry may increase production as collusive agreements break down. This would boost the demand for inputs and employment. On the other hand, the intensification of competition triggered by an investigation may be a catalyst to cost-cutting and cash-flow boosting activities. This may translate in employee dismissals, increased reliance on trade debt and reduced provision of trade credit. How firms reshape their demand for inputs following antitrust investigations is thus an empirical question, which we address in this paper.

With this goal, we perform an event study exploiting cross-industry variation in the cartel cases investigated by the European Commission from 1991 to 2019. Our sample consists of all the EU firms included in Worldscope from 1988 to 2019. The financial data starts three year before the first cartel case to ensure that all treated firms have three years of data in the pre-treatment period. A cartel investigation can be initiated by the EC or triggered by an application for leniency from a cartel member willing to cooperate with

[1] "A cartel is a voluntary association of legally independent firms that aims to raise their joint profits through explicit agreements," Connor (2020). A collusive outcome may even be sustained without communication among firms, with no explicit agreement or exchange of relevant information: this is labeled as tacit collusion. Motta (2004) discusses the economics of collusive agreements and provides a review of competition policies against collusion in the EU and the US.

the authorities. Once the investigation is closed, the cartel members are subject to hefty fines if deemed guilty by the EC. However, the opening of the investigation represents a shock for *all* firms in the industry, marking the fact that the industry is subject to heightened antitrust scrutiny: the increased attention of competition policy authorities is likely to affect the profitability of both cartel and non-cartel firms, and hence their corporate and financial strategies.

For our analysis, we focus on the 3-digit SIC industries that have at least one 4-digit industry under investigation for anti-cartel law infringements during the sample period. Our treated group consists of all firms belonging to the 4-digit industries under scrutiny. Cartels are normally formed in very granular markets. However, as products outside the relevant market may become substitutes under a regime with high, cartel-driven prices (Inderst, Maier-Rigaud, and Schwalbe (2014)), focusing only on the specific product market may underestimate the impact of the cartel and its breakdown.[2] The control group includes all firms that belong to the same 3-digit, but not the same 4-digit industries as the treated ones. Restricting attention to the 3-digit SIC industry alleviates the concern that the treated and control firms are intrinsically different. To further mitigate any potential bias, we adopt propensity score matching to select a control group with firms that are most similar to the treated ones, and use the matched sample in our main analysis.

The identifying assumption is that the opening of a cartel investigation represents a shock to product market players within the affected industry, irrespective of whether they are indicted by the antitrust authority. There are two justifications for this assumption. First, the cartel breakdown has consequences for the undetected cartel members that are not directly under investigation. Antitrust authorities often carry investigations on a subset of firms involved in the cartel. These are typically the ones named in applications for leniency, while other potential cartel members are left out of the case due to the lack of hard evidence of collusion. Second, even non-cartel members in the industry are hit by the intensification of competition following the cartel breakdown.

[2]Moreover, using 4-digit industries helps the empirical analysis by including firms that are in very similar product markets while still providing enough observations to run the regressions.

To assess whether indeed the opening of the cartel investigation is a shock for the industry, we examine the investigations' impact on the operating performance of non-investigated firms in the cartelized industries. We find that the opening of a cartel investigation in the 4-digit industry is associated with a significant drop in performance: both Return on Assets and EBITDA/Assets decrease by 2 to 3 percentage points in treated firms one year from the start of the cartel investigation. The effects disappears within the next couple of years.

Having established that the cartel investigation is a (short-term) negative shock to firm profitability, we use the event study methodology to analyze the corresponding effects on firms' behavior. First, we uncover a significant negative effect on firms' investment decisions. Investment as a proportion of total assets declines by about 1 percentage point in the treated firms as compared with the control firms. The effect is economically significant as the average investment is 6% of total assets. The reduction in investment happens in the same year of the shock and survives also in the longer run, more than 3 years after the shock. The effect is even stronger when measured in terms of net property, plants and equipment (PPE): treated firms shrink by about 10 percentage points (compared to control firms). The main driver of these changes seem to be an increase in asset sales: the latter ones become between 5 and 8 percentage-point more likely following the shock (which corresponds to an increase between 16 and 26% relative to the average probability of asset sales).

Next, we focus on workforce restructuring, using a measure of mass layoffs as our main outcome. We find that following the opening of a cartel case in their industry, treated firms experience a significant increase in the likelihood of mass layoffs. The effect starts in the year when the investigation is opened, and becomes very pronounced in the second and third year from the start of the investigation, when the probability of a large employee dismissal is 50% higher than the year before the shock. However, the effect wanes after 3 years. We also find that firm employment declines by 13% as the investigation starts, but returns to pre-shock levels after three years. Taken together with the results on asset sales and investment, these findings suggest that the effect on employment is more dramatic

than the effect on assets but is shorter lasting: following antitrust shocks firms engage in mass layoffs not to permanently downsize, but rather to adapt the composition of their labor force (firing, then hiring different types of employees).

We also examine whether cartel investigations have an effect on other stakeholders. To assess whether firm shareholders also share the costs of antitrust shocks, we look at changes in shareholder payouts (cash dividends and share repurchases) and net leverage. We find little or no significant change in either measure. To investigate whether customers and suppliers are affected by the shock, we study how firms manage their working capital: we look at the effect of the cartel investigation on accounts receivable, accounts payable, and inventory. On the one hand, firms may be forced to delay payments to suppliers and limit the amount of trade credit extended, as a consequence of their worsened performance; on the other hand, firms with sufficient financial slack may see trade credit as a strategic tool to gain a competitive edge over their rivals, thus increasing the trade credit provided to customers or reducing the trade debt with their suppliers to take advantage of early payment discounts. We find a short-term increase in accounts payable days (AP days), which, like the effect on layoffs and employment, lasts for less than 3 years. In other words, shocked firms increase their reliance on trade credit from their suppliers. The increase is economically sizeable, corresponding to 12% of the average AP days in the sample. We find no change in accounts receivable days (AR days) or inventory days. This suggests that on average firms do not adjust their trade credit provision to customers, nor become more efficient at managing inventories following the opening of cartel investigations.

Overall, our results indicate that cartel investigations impose an adverse shock to profitability that spurs substantial restructuring in terms of asset sales, mass layoffs, and increased reliance on trade debt, but no change in financial leverage or shareholder payout. This may support the view that firms are becoming more efficient following cartel investigations. To shed more light on this hypothesis, we sort firms depending on their labor productivity before the shock: we label as productive the firms that have higher sales per employee than the industry median and unproductive the remaining ones. We find that the cartel investigation is a negative shock to profitability for both sets of firms

but the effect is greater in magnitude for the unproductive firms. Both sets of firms reduce investment but the effect is larger and longer-lasting for the unproductive firms. Only the unproductive firms reduce employment and engage in mass-layoffs, with both short-run and longer-run effects on employment. None of the sets of firms changes shareholder payouts or financial leverage. Both increase the use of trade debt in the short run but the effect is greater in magnitude for the firms with unproductive employees. These results support the hypothesis that cartel investigations are a catalyst for efficiency changes.

Finally, we explore whether the impact of cartel investigations differs across tradable versus non-tradable industries. Previous research has shown that antitrust policies have a more pronounced effect on the profit margins of firms in non-tradable industries, which are shielded from foreign competition and hence more likely to profit from anticompetitive agreements (see Besley, Fontana, and Limodio (2020)). In line with this hypothesis, we find evidence that the effect of cartel investigations on profitability, labor outcomes (*Mass Layoffs*, *Employment*), and use of trade debt (measured as AP days) is mainly concentrated in non-tradable industries.

Our paper contributes to various lines of research. First, it adds to the literature on the redistributive impact of corporate restructuring. Both mass layoffs and asset sales are documented to improve firms' operating performance in distressed times (Denis and Kruse (2000)). Empirical studies have found that firms tend to resort to different types of restructuring strategies depending on their characteristics. Atanassov and Kim (2009) show that layoffs are more likely in countries with stronger shareholder rights, and less likely where unions have more power. Koh, Durand, Dai, and Chang (2015) show that young firms are more inclined to lay off their employees when in distress while mature firms tend to adopt asset restructuring. We look at the restructuring activities associated with a specific shock to profitability (the breakdown of collusive practices due to the start of a cartel investigation). Hence, we have cleaner identification than previous papers.

Second, we add to research studying the impact of product market competition on firms' real and financial choices (see Sertsios (2020) for a recent survey). Several papers have studied how competition affects investment, capital structure and other corporate

policies (see e.g. Xu (2012) and Frésard and Valta (2016)). Papers looking at the impact of competition on employees have mainly focused on managerial incentives and compensation packages (see e.g. Cuñat and Guadalupe (2005)). We study the impact of increased antitrust scrutiny in an industry on firms' workforce restructuring decisions, exploiting EU cartel investigations as a quasi-natural experiment. We find that antitrust enforcement (i.e. cartel investigations) induces a shift in the labor decisions of listed firms in the industry, which adversely affects firms' workers.

By studying the effect of antitrust investigations on labor adjustment decisions and trade credit policy, our work also fills a gap in the nascent literature on competition policy and corporate strategies. Exploiting the staggered passage of leniency legislation in 63 countries, Dasgupta and Žaldokas (2019) examine how more aggressive anti-cartel laws affect firms' capital structure and in particular financial leverage. They document that firms respond to the passage of leniency laws by increasing investment (possibly to compete for market share) and reducing leverage. Stricter anti-cartel enforcement may also induce firms to shift from price-fixing to other anti-competitive strategies (Bittlingmayer (1985); Mueller (1996)). Indeed, recent studies document an increase in horizontal M&A activity (Hüschelrath and Smuda (2013); Davies, Ormosi, and Graffenberger (2015); Dong, Massa, and Žaldokas (2019)) and in minority share horizontal acquisitions (Heim, Hüschelrath, Laitenberger, and Spiegel (2021)) following the passage of anti-cartel enforcement laws. Other recent papers rely instead on cartel detection data to investigate the behavior of colluding firms *before* being detected. Using the Private International Cartels (PIC) database, Ferrés, Ormazabal, Povel, and Sertsios (2020) find that cartel firms have lower leverage ratios during collusion periods, while González, Schmid, and Yermack (2019) document that managers of cartel firms benefit in several ways from cartel participation: they enjoy greater job security, receive higher cash bonuses, and exercise more aggressively appreciated executive stock options. The results we present suggest that antitrust action does not only lead firms to alter their capital structure and their acquisition strategies; it also spurs firms to embark in an intense restructuring of the labor force.

A recent line of work has investigated the link between product market competition

and macroeconomic outcomes. Eeckhout (2021) argues that limited competition results in lower output, thus adversely affecting employment. Workers would then benefit from antitrust action in product markets. We highlight that the effects of antitrust enforcement on workers are not that obvious, at least in the short run, to the extent that a sudden increase in competition may induce firms to restructure their workforce. We tackle these questions studying the evolution of firm-level outcomes, and we show that cartel investigations trigger in fact mass layoffs, probably aimed at boosting productivity. However, the decline in employment and the increase in productivity we observe are short-lived, which suggests that hiring prevails after the initial workforce restructuring stage.

Our paper also complements the literature on antitrust and labor markets. Recent research has documented the pervasiveness of monopsony in labor markets (see e.g. Azar, Marinescu, Steinbaum, and Taska (2020)), which has led to more antitrust scrutiny of labor markets in recent years. We do not investigate the (still rare) cases of antitrust enforcement against wage-fixing cartels. However, our findings indicate that antitrust actions in product markets may affect workers and other input suppliers. Antitrust authorities should be aware that cartel investigations, by spurring mass layoffs, may have potential spillovers on labor markets and hence on wages.

The rest of the paper proceeds as follows. Section 2 presents the data and the empirical methodology. The main results are presented in Section 3. Cross-sectional tests are reported in Section 4; and robustness tests in Section 5. Section 6 concludes. We describe our sample of cartel cases, the definition of the variables, and the tables of robustness checks in the Appendix.

2 Data and Methodology

In our analysis, we consider all the EU firms (including UK firms) in Worldscope from 1988 to 2019. We combine (1) hand-collected data on EU cartel cases; (2) firm-level financial and restructuring data; and (3) country-level control variables.

2.1 EU Cartel Investigations

We collect data on all EU cartel investigations started by the European Commission in the 1991-2019 period. Information includes the opening and closing date of each investigation, details of firms and industries under scrutiny, as well as the imposed fines. From the opening date of the initial investigation, we identify the year when the industry becomes subject to scrutiny by antitrust authorities for the first time. For cases that were re-adopted after the closure of the initial investigation, or cases with firms failing to settle with the EC at the same time as their "co-conspirators", we consider them as one case. We assign a 4-digit SIC code to each cartel case based on the specific target product market.[3] This procedure allows the identification of 112 cartel cases. The mean and median lengths of the investigation period are four years, the longest investigation in our sample lasted for fifteen years and the shortest took only one year.[4]

To rule out overlapping event windows, we exclude the 4-digit industries with two or more cartel investigations opened in different years in our sample. We keep the cases in which the industries have more than one cartel investigation opened in the same year, since the event windows perfectly coincide in that case. This method yields a sample of 47 4-digit SIC industries/year in which an investigation was opened between 1991 and 2019. We report the details of the cartel cases and their industries from our sample in Table 1. For each investigated industry/year, the column *Investigated Firms* reports the number of the firms that are directly subject to investigation; the column *Treated Firms* lists the number of firms belonging to the investigated 4-digit SIC industries. Note that these two variables report the numbers of investigated firms and their same-industry peers in our Worldscope sample. Given that Worldscope does not have complete coverage of all listed firms and that private firms may also engage in cartels and thus be investigated, the number of cartel firms in our sample is inevitably smaller than the true one.

[3] To ensure the accuracy of the data, we omit the cases where we are not confident about the information, such as the SIC code. Details on our procedure are described in Appendix A.

[4] As four cartel investigations in our sample haven't been closed yet, the summary statistics of the investigation lengths are based on the remaining 108 cases.

2.2 Firm-Level Data

We rely on Worldscope to collect financial information for all the EU (including UK) publicly listed firms.

We use two measures of firm performance: *ROA* and *EBITDA over Total Assets*. We define ROA as a firm's net income before extraordinary items and preferred dividends over total assets. We employ three measures of investment activity: *Investment*, which is measured as capital expenditures over total assets, *Asset Growth*, which is measured as the annual percentage change in net Property Plant and Equipment (PPE), and *Asset Sales*, which is a dummy variable suggested by Atanassov and Kim (2009) that takes value one if the firm experiences a decrease in net PPE greater than 15% over the previous two years, and zero otherwise. We use two measures of labor restructuring. The variable *Mass Layoff* takes the value one in year t if the firm experiences a decrease in the number of employees greater than 20% over one year or two years.[5] *log(Employees)* is the natural logarithm of the total number of employees. To measure the impact on financial decisions, we compute two variables: *Shareholder Payout* is the sum of cash dividends and share repurchases over sales; and *Net Leverage* is the sum of interest-paying debt minus cash divided by total assets. We use three standard ratios to measure the management of the firm's working capital: *AP Days* is the ratio of accounts payable over the cost of goods sold (COGS) multiplied by 365; similarly, *AR Days* is the ratio of accounts receivable over Sales multiplied by 365, and *Inventory Days* is the ratio of inventories over COGS multiplied by 365.

In our regressions, we include the standard firm-level measures of size as controls: the natural logarithms of total assets and of sales. All firm-level controls are lagged for one year in the regressions and are winsorized at the 1% level.

[5]See Atanassov and Kim (2009) for a brief discussion of the choice of the 20% cutoff based on previous evidence on large layoffs. As in their paper, we use the change in number of employees over one year/two years as the data on the number of employees may not be updated as frequently as financial data.

2.3 Identification Strategy and Descriptive Statistics

We employ a difference-in-difference methodology that exploits the opening of cartel investigations as a quasi-natural experiment. We label all firms whose primary 4-digit SIC code corresponds to a 4-digit industry in which an EU cartel investigation has been opened as treated firms. As a robustness check, we also exploit a firm's primary product's segment classification to identify whether the firm operates in the investigated industry. The results are both qualitatively and quantitatively similar to our baseline findings, and are reported in the Appendix. If more than one cartel investigation is opened in the same industry in one year, we consider them as one shock and treat them as one cartel investigation.

The underlying assumption is that the opening of a cartel investigation is likely to have an impact on all industry participants, whether directly subject or not to the antitrust investigation. This may happen for two reasons. First, the cartel breakdown will affect the many undetected cartel members that are not directly under investigation due to lack of hard evidence. Antitrust authorities often carry investigations on a subset of firms involved in the cartel. These are likely to be the ones named in the leniency application,[6] while leaving other cartel members out of the case due to the lack of hard evidence of collusion. Secondly, as non-cartel members often benefit indirectly from a cartel operating in their industry, they will be affected by the intensification of competition following the cartel breakdown. The effect of the investigation may somehow be mitigated for non-investigated firms, as they will not bear the financial burden of potential fines. However, the existing evidence on antitrust enforcement actions suggests that fines account for a small part of the impact on firms' values. Aguzzoni, Langus, and Motta (2013) document a 2.89% reduction in firms' share prices when a surprise inspection is carried out, but find that fines account for no more than 8.9% of the loss in firms' market value caused by the antitrust action.

In our analysis we exclude from our sample all cartel members directly subject to the

[6]Leniency programs have long been considered as a powerful tool in detecting cartels, with the ultimate goal of deterring firms from colluding. The EC leniency policy (which was introduced in 1996 and reformed in 2002) grants complete immunity from fines to the first firm reporting the cartel and providing key evidence to the EC.

investigation (including those applying for leniency), and study the response of the other firms in the 4-digit SIC industry (the "treated firms"). This partly mitigates endogeneity concerns due to the fact that applications for leniency and cartel investigations may be triggered by the actions of cartel members.

We label as (clean) controls the never-treated firms whose core business is in the same 3-digit (but not the same 4-digit) SIC industry as the treated firms, and never experience a cartel investigation in their industry during the sample period. Given the staggered nature of our shocks, the treated firms in the years outside the treatment period also act as controls. This procedure gives us our full sample with 1,405 treated firms and 1,243 control firms.

To assuage concerns that treated firms may be inherently different from control firms in the pre-investigation period due to the presence of cartels in their industries, we match treated and control firms using a multivariate propensity score matching method. We exploit a logit model to estimate the probability of being treated using firm size (measured by the natural logarithms of total assets and sales) as explanatory variables. We then accomplish the matching by selecting control firms with the nearest propensity scores to the treated firms. The approach yields a matched sample with 490 control firms and 614 treated firms. As a robustness check, we also ran our main regressions including *all* the firms in the 3-digit SIC industries that have at least one subsequent 4-digit SIC industry under investigation. Results are qualitatively and quantitatively similar and are available upon request.

The sample construction process only requires firms to be active for at least one year before and one year after the cartel investigation, and thus does not necessarily yield a balanced panel. It is possible that some treated firms are forced to exit the market due to intensified competition. However, a close inspection of our data shows that there are only 16 out of 614 treated firms that exit within five years after the investigation begins.

Table 2 presents descriptive statistics of the firm-level controls and outcomes, measured one year before the shock, separately for treated/control firms in our baseline analysis, after matching. Treated and control firms are similar in terms of size (measured by sales

and assets), and do not differ across the outcome variables that we will examine, with the exception of Investment, Asset Growth (at 10% level), AP Days, and Inventory Days. Investment and Asset Growth one year before the shock are larger in the treated group, suggesting that firms in the affected industries were investing and growing more before the shock. AP Days are larger for the treated firms suggesting a greater use of trade debt relative to the control firms. Inventory Days are lower for the treated firms, indicating a more efficient management of inventories. To mitigate concerns that the samples are not comparable, we will pay particular attention in our empirical analysis to rule out the possibility that key firm outcomes display a different pre-treatment trend for treated firms versus never-treated firms.

2.4 Empirical Specification

We implement a pooled event study, which exploits the staggered nature of cartel investigations. We denote as 0 the year in which an investigation opens in a 4-digit industry s, and estimate the following dynamic specification:

$$y_{i(s)t} = \sum_{\tau=-3,\tau\neq-1}^{+3} \alpha_\tau I_{\tau i(s)t} + \alpha_{4+} I_{4+,i(s)t} + \beta \cdot X_{it-1} + \lambda_{i(s)} + \gamma_{kt} + \epsilon_{i(s)t}, \tag{1}$$

where y_{it} is an outcome observed for firm i (active in industry s) at time t. The term $I_{\tau i(s)t}$ is a treatment indicator equal to 1 if in year t firm i is τ years away from the event, i.e. the opening of a cartel investigation in industry s, with $\tau \in [-3, +3]$. The term $I_{4+,i(s)t}$ is a treatment indicator equal to 1 if in year t firm i is 4+ years past the opening of a cartel investigation in its industry, thus putting into the same bin all longer term effects. As a normalization we exclude the first lead, $I_{-1i(s)t}$. We do not include the leading terms before $\tau = -4$, thus assuming away anticipation effects four or more periods before treatment. We include lagged firm-level controls, calendar year indicators, and firm fixed effects in our regressions. In all specifications, we also control for country×year fixed effects (γ_{kt}) to filter out possible country-specific trends that could simultaneously affect firms' corporate strategies and antitrust enforcement. We cluster our standard errors at the 4-digit SIC

industry level in all regressions, because the investigation shocks are defined at this level. Since our main indicators of corporate restructuring (*Mass Layoff* and *Asset Sales*) are dummy variables, as a robustness check, in the Appendix we also estimate a logit model of specification (1) controlling for firm, country and year fixed effects.

Our difference-in-difference approach identifies the causal effect of a cartel investigation under the assumption that outcomes in treated and untreated firms display parallel trends in the absence of treatment. While this assumption cannot be tested directly, the coefficients on the leads will give us an indication of its plausibility. The coefficients on lags allow us instead to study the dynamics of the average firm response to a cartel investigation.

One concern with our identification strategy is whether industry-level downturns may be driving applications for leniency (and the subsequent antitrust investigations) *and* firm restructurings. This would be the case if cartel members are more prone to defect from the cartel and apply for leniency when their industry is facing a bust, a time when the benefit of receiving immunity from the antitrust authority outweighs the potential profit from collusion. To address this issue we identify periods of industrial booms and busts following the method used in Braun and Larrain (2005) and Boutin, Cestone, Fumagalli, Pica, and Serrano-Velarde (2013). Table 3 reports the percentages of treated and control industries that are facing a downturn prior to cartel investigations starting. For treated industries, 11.9% are identified as experiencing a downturn one year before the shock, compared with 15.0% in control firms. The percentage of treated (control) industries in a bust increases to 19.0% (21.4%) when we extend the period to include two years before the cartel investigations. Hence, if anything treated firms are slightly *less* likely to experience a downturn before the investigation start, as compared with non-treated 4-digit industries in their same 3-digit sector (this is in line with the fact that treated firms have higher investment pre-event compared with controls). This mitigates in part the concern that industry downturns are driving antitrust investigations and mass layoffs. As a further check, in the Appendix we provide a robustness test excluding from the sample firms operating in industries that are identified as being in a downturn one year

before the cartel investigation starts.

The two-way fixed effects estimator in equation (1) has been shown to be a weighted average of all possible 2×2 DiD estimators that compare firms in treated cohorts to never-treated firms, and firms in different treated cohorts to each other. In the presence of heterogeneous treatment effects, the 2×2 DiD components using already-treated units as controls may have negative weights (see Goodman-Bacon (2021)). To assess whether this issue biases our TWFE estimator, we verify the robustness of all our results to the use of the alternative "interaction-weighted" estimator proposed by Sun and Abraham (2021). The results are discussed in Section 5, and figures are reported in the Appendix.

3 The Effect of Cartel Investigations

In this section, we explore how the opening of a cartel investigation in an industry affects firms belonging to that industry. We start from the impact on firm performance and then we examine firms' changes in investment, operation, and financial policies.

3.1 Impact on Firm Performance

In Section 2.3 we argued that the opening of cartel investigations represents a shock for *all* firms in the affected industry. In other words, not only the indicted firms in the cartel case, but also other firms in the industry (not directly under investigation) are hit by the shock. To support this claim, we investigate whether the opening of cartel cases affects the performance of non-investigated firms in the cartelized industries.

We start in Figure 1 by plotting our two measures of performance (ROA, defined as the ratio of net income before extraordinary items and preferred dividends over total assets; and the ratio of EBITDA over total assets) in the ten year window around the cartel investigation. We plot separately the average performance for treated and control firms. The figure shows that that treated firms underperform compared to control firms one year after the shock. At all other times the difference in the performance between the two sets of firms is not statistically significant. This indicates that the cartel investigation

led to a reduction in firm performance that lasted for a short period of time.[7]

To confirm this results in Table 4, we estimate equation (1) using the two measures of firms' performance (ROA and EBIT/Assets). We find that cartel investigations have an adverse effect on the performance of treated firms. Return on Assets and EBITDA/Assets decline by about 2-3 percentage points in the first three years of the cartel investigation. This suggests that the investigations trigger an intensification of competition, probably due to heightened antitrust scrutiny over the whole industry and the cartel breakdown. We find no evidence of a permanent decrease in profitability: the effect is concentrated in the three years following the start of the investigation (years 0-2), while the long term effect as captured by the coefficient α_{4+} is never statistically different from zero.

Regressions include firm and country×year fixed effects. Across all the columns, the coefficients of leading terms are not significant, indicating the plausibility of the parallel trend assumption. The results in Table 4 thus support the identifying assumption in our analysis: cartel investigations represent an adverse shock to profitability even for firms in the cartelized industry that are not directly targeted by the investigation.

Table 4 demonstrates that the investigation is a negative shock to profitability. The question for the remaining of this section is: how does the firm react to this shock? In basic cash flow analysis, a negative shock to profitability will have to be reflected in one or more of these changes: a reduction in net investment (investment - asset sales), a reduction in operation costs (both in terms of inputs as well as in terms of working capital), an increase in net leverage (debt - cash), and a decrease in net shareholder payouts (dividend and share-repurchases).

3.2 Impact on Investment Policy

First, we consider the impact of cartel investigations on investment. In Table 5 we uncover a significant negative effect on firms' investment decisions. Column 1 shows that investment as a proportion of total assets decline by about 1 percentage point in the treated firms as compared with the control firms. The effect is economically significant

[7]Figure 1 also shows no evidence of differences between treated and control firms in the years before the shock, in support of the parallel trend assumption needed to perform a difference-in-difference analysis.

as the average investment is 6% of total assets. The reduction in investment happens in the same year of the shock ($\tau = 0$), continues in the next year ($\tau = +1$) and survives also in the longer run ($\tau > +3$), more than 3 years after the shock.

In column 2, we follow Atanassov and Kim (2009) and compute asset sales as a dummy variable that takes value one when the firm experiences a drop of more than 15% in net PPE over the previous 2 years. According to this variable, asset sales become between 5 and 8 percentage points more likely following the shock (which corresponds to an increase between 16 and 26% relative to the average probability of asset sales in the sample). The increase in asset sales starts one year after the shock ($\tau = +1$) and continues even more than 3 years after the shock ($\tau > +3$). We confirm this finding in column 3, where we find that net PPE shrink in treated firms by about 10 percentage points (compared to control firms); and they grow 7.6 percentage points less than control firms even more than 3 years after the shock.

These results indicate that a strong effect of the reduction in profitability is reflected in a reduction of net investment. We turn next to the effect of cartel investigations on employment.

3.3 Impact on Employment Policy

Figure 2 provides some preliminary unconditional evidence on the evolution of the firms' workforce, in the 5-year window around the start of a cartel investigation. This is shown separately for (a) treated firms (whose core-business is in the 4-digit SIC industries that experience a cartel investigation in our sample period); (b) their matched clean controls (never-treated firms that operate in the same 3-digit but not same 4-digit industry as the treated). The number of employees in the treated group shows a small decline in the first year of the cartel investigation, with treated and clean controls displaying similar trends afterwards. The right hand side panel plots the trend for the *Mass Layoff* indicator. While mass layoffs are stable in the clean control firms, the plot is hump-shaped for treated firms, displaying a sharp increase in layoffs in the first three years of the investigation, followed by a sharp decrease.

Table 6 present the estimated coefficients of specification (1) where the the outcome variables are the *Mass Layoffs* indicator (in column 1) and *log(Employment)* (in column 2). The effect of a cartel investigation kicks in the year when the investigation is opened; the probability of mass layoffs increases at $\tau = 0$ (with respect to $\tau = -1$) and continues to be significantly larger for two more years. The magnitude of these effects is important: for instance, one year after the opening of the investigation, the likelihood of a large employee dismissal increases by 7.5 percentage points, adding to a pre-event likelihood equal to 11.5% in treated firms (see Table 2). The effect wanes after three years: the coefficient on the long-run indicator ($\tau > +3$) is not significant in any of our specifications.

Employment declines when the investigation starts, but returns to pre-shock levels after three years. Hence, antitrust shocks cause firms to engage in a restructuring of the labor force, but do not cause long term harm to firm employment. Firms probably engage in mass layoffs to adapt the composition of their labor force (firing, then hiring different types of employees), not to downsize.

3.4 Impact on Financing Policy

Part of the shortfall in profits could be covered by changes in the firm financing policy. Specifically, firms may transfer some of the effects of the antitrust shock onto shareholders by reducing their payouts or by raising debt (and thus increasing equity risk). In Table 7, we report the estimated coefficients from specification (1) when the outcome variables are *Shareholder Payout* (measured as cash dividends and share repurchases over sales) and *Net Leverage* (measured as debt minus cash over total assets).

In column 1, we find no significant effect on net leverage, except for a small long-term effect ($\tau > 3$): an increase of 3.5 percentage point in net leverage that is significant at the 10% level. In column 2, we find that the average treatment effect is significantly different from zero only at $\tau = 0$, where we observe a 0.7 percentage point drop in shareholder payout. However, the effect disappears immediately after. In both regressions, there is no sign of pre-treatment trends.

These results indicate that firms do not significantly alter their capital structure as the

result of the cartel investigation. Effectively, shareholders are shielded from the effects of the cartel investigation.

3.5 Impact on Working Capital Management

Another way in which firms may counter the reduction in profitability is by managing working capital more effectively. In this section, we study the impact of cartel investigations on accounts receivable, accounts payable and inventories.

While reducing inventory days is a clear-cut way to reduce working capital needs, it is not clear a priori how intensified competition may affect the demand and the supply of trade credit. A worsened performance following the antitrust shock may force firms to increase (decrease) their sources (uses) of cash, e.g. by increasing their demand of trade credit from suppliers and reducing trade credit offered to customers. For instance, Ershain, Giannetti, and Huang (2021) document that firms hit by adverse shocks (i.e. natural disasters) obtain more trade credit from suppliers. However, in a more competitive environment financially strong firms might view trade credit as a strategic tool to gain a competitive edge over their rivals. This may call for an increase in their trade credit provision (to attract customers), and a reduction of trade credit use (to take advantage of early-payment discounts on inputs from suppliers).[8]

To investigate this question, we define *AR Days* as a firm's accounts receivable divided by its sales and multiplied by 365 (so that the *AR Days* captures how many days, on average, it takes the supplier to receive payments for their sales). Similarly, the variable *AP Days* is defined as a firm's accounts payable divided by its Costs of Good Sold and multiplied by 365; and *Inventory Days* is defined as a firm's inventory divided by its COGS and multiplied by 365.

In Table 8, we report the results. In column 1, we find a strong, short-term increase in AP days, which, like the effect on layoffs and employment, lasts for less than 3 years.

[8]Barrot (2016) finds that long payment terms offered by financially strong suppliers undermine the entry and survival of their liquidity constrained product market rivals. However, Chod, Lyandres, and Yang (2019) show that the relation between supplier competition and trade credit is complex: in their model, a supplier's trade credit provision to a cash-constrained buyer generates a positive demand externality on rival suppliers, as more cash becomes available to pay the latter.

The increase is economically sizeable, corresponding to 12% of the average AP days in the sample. In columns 2 and 3, we find no change in either AR days or inventory days. These findings suggest that firms do not adjust their trade credit provision nor become more efficient at managing inventories following the opening of cartel investigations; but they increase their reliance on credit from their suppliers (thus reducing by about 11 days the length of their cash conversion cycle), for a period of 3 years following the shock.

4 Cross-Sectional Results

So far, our analysis indicates that the cartel investigations represent a negative shock to profitability with real impacts on investment decisions, employment policies, and the use of trade credit. In this section we take advantage of the cross-sectional differences to shed more light on the mechanism at work.

4.1 Financial Distress

It stands to reason to expect that the consequences of a negative shock to profitability should be more severe for firms closer to financial distress: after all, they have less margin to maneuver. To test this hypothesis, in Table 9, we estimate the effect of cartel investigations on *ROA*, *Asset Sales*, *Mass Layoffs*, *Shareholder Payout*, and *AP Days* separately for firms with an EBIT interest coverage ratio smaller or larger than 2. The assumption is that firms with a smaller coverage ratio face a greater risk of financial distress.[9]

As expected, we find that the negative effects on ROA are greater in magnitude for firms close to financial distress; and so is the increase in asset sales (in the Appendix, we also find a similar negative effect on investment and growth of assets). Firms away from financial distress see a limited effect on their profitability and virtually no effect on asset sales.[10] However, the effects on employment (measured as mass layoffs, in Table 9, and as changes in overall employment, in the Appendix) are instead present in both sets of

[9] In the Appendix, we report the findings for all outcome variables used in Section 3.

[10] The difference between coefficients in the subsample of firms close to distress vs firms far from distress is significant. We have tested the significance of difference between coefficients in all our split regressions.

firms. In other words, even firms that have financial slack and could react to the shock by raising more external finance while hoarding labor prefer not do so, and rather take the opportunity to reduce labor costs. This suggests that firms engage in mass layoffs not as a way to cut uses of cash to accommodate the shock, but rather to seek efficiency changes once their industries are subject to antitrust scrutiny.

Interestingly, we find that only firms that are not financially distressed increase their AP days: this indicates that suppliers are unwilling to extend credit to firms close to financial distress; while firms away from financial distress choose to save on working capital by extending their reliance on trade debt.

While payouts are usually sticky, we would expect firms closer to financial distress to be more likely to cut payouts when subject to antitrust shocks. However, we find that both firms with a coverage ratio larger than 2 (likely not at risk of financial distress) and low-coverage ratio firms do not reduce payouts, thus completely shielding their shareholders from the impact of the antitrust shock. In the Appendix we find no effect on leverage for either group of firms.

4.2 Efficiency Gains

The findings so far indicate that the negative shock to profitability associated with cartel investigations seems to be associated with an increase in restructuring activity by the firm aimed at saving costs, particularly those associated with employment. If that is indeed the case, we would expect to see more restructuring in firms that have less productive labor to start with.

To test this hypothesis, in Table 10, we estimate equation (1) separately in firms with high or low labor productivity. We classify firms into productive and unproductive based on whether the (log of) sales per employee is greater or smaller than the industry median. The dependent variables in the table are ROA, asset sales, mass layoffs, shareholder payout, and AP days. The results for the remaining outcome variables (EBITDA/assets, investment, asset growth, number of employees, net leverage, AR days and inventory days) are in the Appendix.

The results indicate that the effects are largely concentrated in the firms that are relatively unproductive. This is particularly true for the mass layoffs. This is strong indication that the outcome of cartel investigations is a push to increase labor productivity.

4.3 Tradable versus Non-Tradable Industries

The identifying assumption in our analysis is that cartel investigations trigger an increase in competition in the affected industries by leading to cartel breakdowns and more generally by attracting antitrust scrutiny. If indeed cartel investigations are a shock to product market competition (as we argue), the magnitude of the shock is likely to vary across industries depending on how competitive they are to start with. Besley, Fontana, and Limodio (2020) argue that collusion and other anticompetitive agreements are more effective in industries that are protected from international competition, such as non-tradable industries. In line with this argument, they find that tougher antitrust policy adversely affects profitability mainly in non-tradable industries.

Using the same logic, we test the prediction that the impact of cartel investigations is larger in non-tradable industries. To this aim, in Table 11 we estimate equation (1) for all outcomes (ROA, asset sales, mass layoffs, shareholder payout, and AP days) separately in non-tradable versus tradable industries. Following Besley, Fontana, and Limodio (2020), we define as tradable industries Agriculture, Forestry and Fishing (A), Mining (B), and Manufacturing (D); all other industries are labeled as non-tradable.

Consistent with our prediction, we find that firm performance in non-tradable industries is more negatively affected by the antitrust shocks. Furthermore, the adverse impact on asset sales, mass layoffs and AP days is more pronounced in non-tradable industries, while shareholder payouts are not affected by antitrust investigations irrespectively of the industry.[11]

[11]The coefficients on ROA, asset sales, mass layoffs and AP days are statistically different across the two subsamples.

5 Robustness Tests

In this section, we present a number of robustness tests. First, we focus on a subset of investigations that explicitly mention price-fixing as their type of collusive agreement; second, we estimate logit models for the dependent variables that are binary; third, we offer an alternative industry classification based on product segments; fourth, we control for industry-level business cycles; and finally we discuss the results for the variables that were excluded from the cross-sectional analysis presented in Section 4.

5.1 Price-Fixing Cartels

Cartels can be categorized in different types based on the collusive agreements between colluding firms. The most common cartel types include price-fixing, market-sharing, and bid-rigging, etc. Considering that firms employ different strategies when engaging in different types of cartels, cartel investigations may have heterogeneous effects on affected firms subject to the cartel types.

According to the disclosed information on cartel cases detected by the European Commission, a large proportion of the cartels have more than one type of collusive agreements. For instance, in many cases, firms collude by simultaneously fixing prices and allocating market shares. As shown in our previous findings, firms carry out restructuring activities in response to the deteriorated performance associated with the cartel investigations. Following this line of argument, price-fixing cartels should provide a cleaner setting as firms' markup and profitability are directly impaired by the cartel investigations.

Out of the 42 investigated industries in our matched sample only 19 of them were identified by the antitrust authority as pure price-fixing cartels. We conduct our analysis using only the cartel cases detected in these 19 industries as a robustness check. Table 12 presents the results. Although we lose a large number of observations by restricting our attention to this cartel type, the results remain consistent with our baseline findings. Firms that are hit by antitrust investigations on price-fixing adjust in ways that affect workers (and other suppliers of inputs), while limiting the negative effects on shareholders.

5.2 Logit Model

Mass Layoff and *Asset Sales* are dummy variables that take only values 0 and 1. Therefore, the use of linear regression models might be inappropriate, and a logit regression model might be preferable. In Table A.1 of the Appendix, we present the results of estimating specification (1) with a logit model. The cost of adopting logit is that we cannot include country × year fixed effects across our specifications due to data limitations. As we believe that these controls are critical to our identification strategy, we used OLS in our baseline analysis and present the logit model only as a robustness check. Across all specifications, we control for firm, year and country fixed effects. We include firm-level controls (the natural logarithms of total assets and sales) in columns 2, 3, 5, and 6, and country-level controls (GDP growth, GDP per capita and unemployment rate) in column 3 and 6.

The results in Table A.1 confirm that the likelihood of mass layoffs and asset sales increases significantly following the cartel investigations in the treated (as compared to the control) group. The effect on mass layoffs is concentrated in the three years following the start of the investigation, as the coefficient on the long term binned-lag is not statistically significant. However, the effect on asset sales appears to be long lasting. Overall, the results in Table A.1 confirm the findings of our baseline analysis.

5.3 Industry Classification using Product Segments

For some firms, it is possible that their primary SIC codes do not reflect the industry classification of their main product lines. To address this concern, we exploit the product segment data from Worldscope to identify firms that are affected by cartel investigations and conduct a robustness check. Specifically, we categorize firms into different product segments based on the product segment SIC code provided by Worldscope and define treated firms as the ones that locate in four-digit product SIC industries where at least one cartel investigation begins in that year.[12] We then perform the same matching method and estimate the coefficients in (1). In Table A.2 of the Appendix, we present the results.

[12]For firms with missing product segment SIC code, we use the primary SIC code.

Across all specifications, we control for firm and country*year fixed effects as well as include firm-level controls (the natural logarithms of total assets and sales).

The results in Table A.2 coincide with our baseline findings, which alleviates concerns over the accuracy of industry classification. Firms' performance, investment, and employment experience a significant drop following the start of the investigation, while treated firms increase their reliance on trade credit from suppliers. We do not find a reduction in firms' payout policies, which further confirms our finding that firms protect shareholders from the impact of antitrust scrutiny.

5.4 Industry Booms and Busts

Colluding firms are more inclined to defect from the cartel when the industry they operate in is experiencing a downturn. As an industry-level downturn may also induce deteriorated performance and restructuring, our results may suffer from an omitted variable bias. To mitigate this concern, we follow Braun and Larrain (2005) and Boutin, Cestone, Fumagalli, Pica, and Serrano-Velarde (2013) and identify the boom and bust periods of each industry in our sample. We then exclude all the firms operating in industries that are in recession one year before the cartel investigation begins. We re-run the baseline analysis using this cleaned sample and report our results in A.3.

Our findings are both qualitatively and quantitatively similar to the baseline results. Firms that operate in the affected industries undergo a deterioration in their performance and a reduction in their long-term investment. In response to this, firms actively seek labor restructuring in order to boost productivity. The pattern of employment reconfirms the short-term characteristics of the workforce restructuring. In contrast, firms limit the negative impact of antitrust scrutiny on shareholders and customers.

5.5 Ancillary Results on Cross-Sectional Analysis

In Tables 9 –12, we excluded several outcome variables from the cross-sectional analysis: EBITDA/Assets, Investment, Asset Growth, Net Leverage, AR Days, Inventory Days, and log(Employees). The results for these variables are reported in the Appendix. The

findings in Tables A.4- A.7 are qualitatively similar to the corresponding variables reported in Tables 9 –12.

5.6 Interaction-Weighted (IW) Method

Finally, we report in the Appendix the estimates obtained by implementing the alternative "interaction-weighted" (IW) method proposed by Sun and Abraham (2021), using the STATA package `eventstudyinteract`. The results are in Figures A.1- A.5. Across all specifications the findings are both qualitatively and quantitatively similar to our baseline analysis.

6 Conclusion

This paper investigates how antitrust action against cartels affects different firm stakeholders in firms operating in cartelized industries. We focus on firms that operate in the same industry as the cartel members under investigation, and document that investigations lead to a temporary decline in their performance. The adverse effect on these (non-investigated) firms is likely a consequence of the increased antitrust scrutiny and the enhanced competition in the industry (due to the breakdown of the cartel). We investigate how this adverse event changes firms' corporate and financial strategy. Our findings suggest that firms react by restructuring their assets and their labor force.

Exploiting the differential timing of cartel investigations initiated by the European Commission within a difference-in-differences setup, we show that antitrust enforcement spurs a significant increase in asset sales and mass layoffs among firms operating in the affected industry. It also leads to an increase in trade debt (as measured by AP days). The impact of antitrust investigations manifests immediately after the case opens and dies away after three years. Employment initially declines but returns to its pre-shock levels within three years. Similarly, AP Days increase and then return to the pre-shock levels within the same time horizon. In sum, antitrust shocks cause firms to engage in a sharp restructuring of the labor force and trade debt, but do not cause long term effects

on either. The more inefficient firms (i.e. those with lower labor productivty) are the more affected by this restructuring activity.

By contrast, we find little evidence of a significant change in firm payouts to shareholders or changes in financial leverage after the cartel investigation is opened. This indicates that when exposed to competition policy shocks, firms shield shareholders precisely while engaging in large employee dismissals and changes in trade debt practices. These findings are evidence of (short-term) conflicts between stakeholders and provide support for a wider scope for corporate governance: managers should keep the welfare of various stakeholders (including workers and other suppliers) into account when designing their corporate strategies (see Tirole (2001) for an early discussion).

Finally, our results suggest that antitrust action against anti-competitive infringements in product markets has spillovers on the labor market, by driving a rise in mass layoffs among firms in the industry. After starting cartel investigations in product markets, the authorities should be alert to possible changes in labor market power which may facilitate abuse of dominance and collusion to fix wages. This is especially important in view of recent calls for more antitrust litigation related to labor market abuse (see Marinescu and Posner (2019)).

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Figure 1: Cartel investigations and performance

This figure plots the trends of firms' performance in the 5-year window around the start of a cartel investigation for: (a) treated firms (firms whose core business is in the 4-digit SIC industries that experience a cartel investigation in our sample period); (b) their matched controls (firms that never experience cartel investigations but operate in the same 3-digit SIC industries as the treated). We use ROA and EBITDA/Assets as our two measures of performance. The two measures are both winsorized at 1% level and then averaged at the 4-digit SIC industry level.

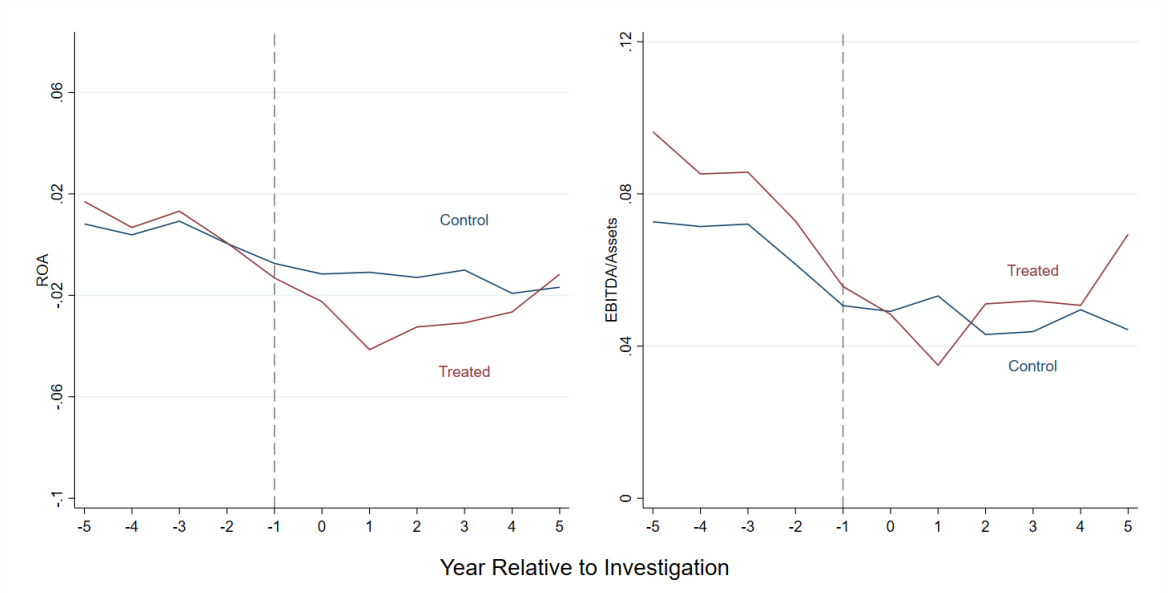

Figure 2: Cartel investigations and employment

The figure plots the trends of firms' employment and mass layoffs in the 5-year window around the start of a cartel investigation for: (a) treated firms (firms whose core business is in the 4-digit SIC industries that experience a cartel investigation in our sample period); (b) their matched controls (firms that never experience cartel investigations but operate in the same 3-digit SIC industries as the treated). The l.h.s. panel plots the log value of the number of employees; the r.h.s. panel plots our *Mass Layoffs* indicator. We average these two variables at 4-digit SIC industry level based on the relative year to the opening of cartel investigations.

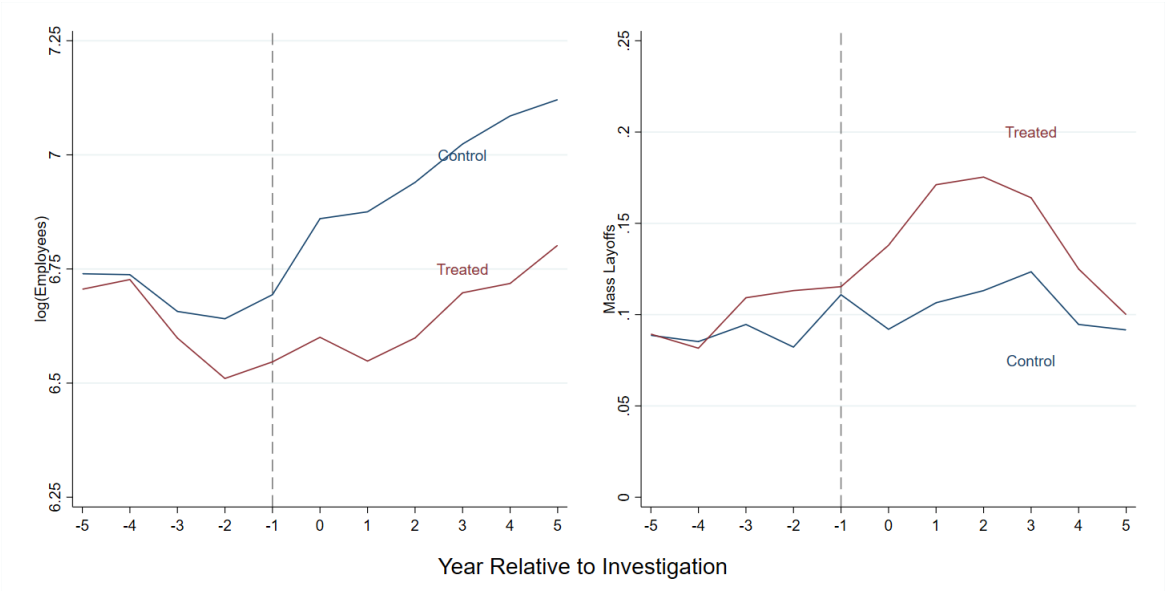

Table 1. Description of Cartel Cases

This table provides details of the EU cartel cases in our sample. The specific dates of opening investigations, cartel industries and firms under investigations can be found on the European Commission website. *Investigated Firms* is the number of public firms in our sample that were under cartel investigations in their own 4-digit SIC industries. *Treated Firms* reports the number of firms that are in the same 4-digit SIC industries as the investigated ones.

SIC	Industry Description	Open Year	Investigated Firms	Treated Firms
1742	Plstrng,Drywall,Insultn Work	2001	0	2
1791	Structural Steel Erection	2002	0	5
2033	Can Fruit, Veg, Presrv, Jam, Jel	2013	1	11
2048	Prep Feed Anmls-Ex Dogs,Cats	1996	1	20
2082	Malt Beverages	2000	2	70
2087	Flavoring Extract,Syrup, Nec	1999	0	4
2284	Thread Mills	2001	1	4
2673	Plastic,Foil,Coatd Papr Bags	2001	1	7
2677	Envelopes	2010	1	2
2812	Alkalies and Chlorine	1999	0	6
2813	Industrial Gases	1997	3	8
2834	Pharmaceutical Preparations	1999	2	212
2841	Soap and Other Detergents	2008	0	10
2911	Petroleum Refining	2005	7	32
2951	Asphalt Paving, Blocks	2002	0	2
3052	Rubber,Plastics Hose and Belts	2007	0	7
3211	Flat Glass	2005	1	10
3261	Vitreous China Plumb Fixture	2004	1	13
3291	Abrasive Products	2010	0	5
3351	Rollng,Drawng,Extrude Copper	2001	1	5
3442	Metal Doors,Frames,Mold,Trim	2007	0	17
3446	Architect, Ornamentl Metal Wk	1991	0	8
3534	Elevators and Moving Stairways	2004	1	9
3537	Indl Trucks,Tractors,Trailrs	2010	0	16
3562	Ball and Roller Bearings	2011	1	6
3585	Air-Cond, Heating, Refrig Eq	2009	1	22
3612	Pwr,Distr,Specl Transformers	2007	0	18
3641	Electric Lamp Bulbs and Tubes	2009	0	9
3648	Lighting Equipment, Nec	2012	0	4
3671	Electron Tubes	2007	0	2
3674	Semiconductor,Related Device	2002	2	95
3675	Electronic Capacitors	2013	0	2
3691	Storage Batteries	2012	0	14
3695	Magnetc,Optic Recordng Media	2009	0	1
3711	Motor Vehicles and Car Bodies	2017	5	52
4013	RR Switching, Terminal Estab	2013	0	2
4412	Deep Sea Frn Trans-Freight	2012	1	87
4731	Arrange Trans-Freight, Cargo	2003	1	17
4911	Electric Services	2012	2	239
4922	Natural Gas Transmission	2006	0	6
4941	Water Supply	2008	0	36
5093	Scrap and Waste Materials-Whsl	2012	0	2
5148	Fresh Fruit and Vegetable-Whsl	2005	0	4
5411	Grocery Stores	2019	1	71
6021	National Commercial Banks	1999	2	176
6035	Savings Instn,Fed Chartered	1997	1	25
7812	Motion Pic, Videotape Prodtn	2002	0	80
Total			40	1455

Table 2. Descriptive Statistics

This table shows the descriptive statistics for the matched sample. All variables are measured at $\tau = -1$, i.e. one year before the opening of an investigation in the treated industry. We divide the matched sample into two groups of firms: *Treated Firms* and *Control Firms*. *Treated Firms* are the firms whose core business is in the 4-digit SIC industries that experience a cartel investigation in our sample period. *Control Firms* are their matched never-treated counterparts. Our sample period spans from 1988 to 2019. The full sample on which we perform the matching consists of all firms in the 3-digit SIC industries that have at least one 4-digit SIC industry under cartel investigation. This table reports the descriptive statistics for all firm-level variables (after winsorization). Differences in means between the treated and controls and their corresponding p-values are presented in the last two columns.

	Treated Firms				Control Firms				Difference	
	Mean	Median	SD	N	Mean	Median	SD	N	Difference	P-Value
log(Total Assets)	12.992	12.613	2.535	614	12.880	12.353	2.820	614	0.112	0.464
log(Sales)	11.960	11.811	2.465	614	11.932	11.782	2.657	614	0.027	0.852
ROA	-0.013	0.014	0.170	614	-0.007	0.011	0.174	614	-0.006	0.561
EBITDA/Assets	0.056	0.073	0.184	548	0.051	0.060	0.174	560	0.005	0.640
Investment	0.063	0.045	0.070	561	0.040	0.022	0.055	540	0.023***	0.000
Asset Sales	0.312	0.000	0.464	577	0.308	0.000	0.462	591	0.004	0.882
Asset Growth	0.249	-0.013	1.417	576	0.129	-0.052	0.950	586	0.120*	0.092
Net Leverage	0.119	0.147	0.316	479	0.135	0.163	0.276	427	-0.015	0.439
Shareholder Payout	0.039	0.006	0.144	614	0.037	0.009	0.158	614	0.002	0.829
AR Days	104.270	75.539	85.946	489	108.746	88.524	74.124	431	-4.475	0.397
AP Days	100.255	60.294	100.908	422	84.604	60.849	76.240	364	15.651**	0.014
Inventory Days	67.192	43.446	74.657	436	115.237	85.224	104.188	373	-48.045***	0.000
Layoff	0.115	0.000	0.320	529	0.111	0.000	0.314	550	0.004	0.820
log(Employees)	6.547	6.426	2.156	556	6.694	6.654	2.233	576	-0.147	0.259

Table 3. Boom & Bust Industries before the Cartel Investigations

This table reports the percentage of industries that are in boom or bust before the cartel investigations begin, for both the treated sample and the control sample. We identify periods of industrial booms and busts following the method used in Braun and Larrain (2005) and Boutin, Cestone, Fumagalli, Pica, and Serrano-Velarde (2013). Booms and busts are identified from the fluctuations of industry sales based on a peak-to-trough criterion. We first estimate the cyclical component of the industry-level sales as a proxy for the state of industry, where the cyclical industry sales is measured as the difference between the actual sales and a trend computed using a Hodrick-Prescott filter with a smoothing parameter of 100. A trough occurs when the log of industry sales is below the trend by more than one standard deviation, where the standard deviation is calculated using the cyclical industry sales. For each trough, we go back in time until we find a local peak, which is defined as the closest preceding year in which cyclical industry sales is higher than in both the previous and posterior years. A bust goes from the year after the local peak to the year of the trough. Similarly, a peak occurs when the cyclical industry sales is more than one standard deviation above zero. Once a peak is identified, we go back in time until we find a local trough (the closest proceeding year in which the cyclical industry sales in lower than both the previous and posterior year). The boom goes from the year after the local trough until the year of the peak.

	Percentage of Industries in Booms and Busts	
	Treated Firms	Control Firms
1 Year before Cartel Investigations (Bust)	11.9%	15.0%
1 Year before Cartel Investigations (Boom)	16.7%	18.7%
1-2 Years before Cartel Investigations (Bust)	19.0%	21.4%
1-2 Years before Cartel Investigations (Boom)	28.6%	27.2%

Table 4. Impact of Cartel Investigations on Performance

This table reports the estimated coefficients from equation (1), focusing on the impact of cartel investigations on ROA (measured as net income before extraordinary items and preferred dividends / total assets) and EBITDA/total assets. We use the matched sample which excludes the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	ROA	EBITDA/Assets
	(1)	(2)
Shock($\tau = -3$)	0.008	0.005
	(0.005)	(0.005)
Shock($\tau = -2$)	0.000	0.000
	(0.005)	(0.005)
Shock($\tau = 0$)	-0.019***	-0.019***
	(0.007)	(0.007)
Shock($\tau = +1$)	-0.034***	-0.028***
	(0.010)	(0.009)
Shock($\tau = +2$)	-0.026**	-0.014
	(0.011)	(0.009)
Shock($\tau = +3$)	-0.008	0.001
	(0.011)	(0.013)
Shock($\tau > +3$)	-0.006	0.002
	(0.008)	(0.008)
Observations	21273	19410
R^2	0.476	0.545
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Country*Year FE	Yes	Yes

Table 5. Impact of Cartel Investigations on Investment

This table reports the estimated coefficients from equation (1), focusing on the impact of cartel investigations on investment (measured as capex/total assets), asset sales (measured as a dummy variable that identifies a drop in net PPE greater than 15% over the previous 2 years), and asset growth (measured as the growth rate of net PPE). We use the matched sample which excludes the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Investment	Asset Sales	Asset Growth
	(1)	(2)	(3)
Shock($\tau = -3$)	-0.002	-0.016	0.057
	(0.004)	(0.022)	(0.060)
Shock($\tau = -2$)	-0.003	-0.019	0.086
	(0.006)	(0.021)	(0.072)
Shock($\tau = 0$)	-0.011*	0.023	-0.097***
	(0.006)	(0.031)	(0.031)
Shock($\tau = +1$)	-0.013*	0.060*	-0.108
	(0.007)	(0.031)	(0.065)
Shock($\tau = +2$)	-0.010	0.083**	-0.080
	(0.007)	(0.033)	(0.055)
Shock($\tau = +3$)	-0.009	0.055*	-0.102***
	(0.006)	(0.030)	(0.039)
Shock($\tau > +3$)	-0.015**	0.050**	-0.076**
	(0.006)	(0.022)	(0.036)
Observations	19138	21038	21002
R^2	0.563	0.316	0.203
Control Variables	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes

Table 6. Impact of Cartel Investigations on Labor

This table reports the estimated coefficients from equation (1), focusing on the impact of cartel investigations on mass layoffs (measured as a dummy variable that identifies drops by more than 20% in the number of employees over the previous 2 years), and firm employment (measured as the logarithm of the number of employees). We use the matched sample which excludes the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Mass Layoffs	log(Employees)
	(1)	(2)
Shock($\tau = -3$)	0.008	-0.045
	(0.016)	(0.031)
Shock($\tau = -2$)	0.018	-0.063
	(0.018)	(0.044)
Shock($\tau = 0$)	0.049***	-0.128**
	(0.017)	(0.050)
Shock($\tau = +1$)	0.075***	-0.122**
	(0.022)	(0.052)
Shock($\tau = +2$)	0.079***	-0.137**
	(0.024)	(0.057)
Shock($\tau = +3$)	0.052*	-0.100
	(0.028)	(0.066)
Shock($\tau > +3$)	0.013	-0.084
	(0.013)	(0.066)
Observations	18921	19207
R^2	0.244	0.966
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Country*Year FE	Yes	Yes

Table 7. Impact of Cartel Investigations on Financing Strategies

This table reports the estimated coefficients from equation (1), focusing on the impact of cartel investigations on Net Leverage (measured as (debt - cash)/total assets) and Shareholder Payout (measured as (cash dividends + share repurchases)/sales). We use the matched sample which excludes the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Net Leverage	Shareholder Payout
	(1)	(2)
Shock($\tau = -3$)	-0.002	0.001
	(0.012)	(0.004)
Shock($\tau = -2$)	0.004	-0.005
	(0.012)	(0.005)
Shock($\tau = 0$)	0.004	-0.007**
	(0.015)	(0.003)
Shock($\tau = +1$)	0.016	-0.000
	(0.014)	(0.005)
Shock($\tau = +2$)	0.022	0.003
	(0.016)	(0.006)
Shock($\tau = +3$)	0.021	0.002
	(0.019)	(0.006)
Shock($\tau > +3$)	0.035*	0.007
	(0.021)	(0.007)
Observations	15274	21266
R^2	0.643	0.544
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Country*Year FE	Yes	Yes

Table 8. Impact of Cartel Investigations on Working Capital

This table reports the estimated coefficients from equation (1), focusing on the impact of cartel investigations on firms' working capital management, measured as AP days (measured as accounts payable×365/COGS), AR days (measured as accounts receivable×365/Sales), and inventory days (measured as inventory×365/COGS). We use the matched sample which excludes the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	AP Days	AR Days	Inventory Days
	(1)	(2)	(3)
Shock($\tau = -3$)	6.014*	2.239	-1.273
	(3.384)	(3.265)	(2.760)
Shock($\tau = -2$)	4.928	3.326	-0.754
	(3.489)	(3.126)	(2.751)
Shock($\tau = 0$)	10.887**	-0.011	-0.076
	(4.316)	(2.871)	(3.333)
Shock($\tau = +1$)	11.981**	-2.636	-1.867
	(5.005)	(5.172)	(4.072)
Shock($\tau = +2$)	11.832**	-2.908	-5.531
	(4.775)	(4.930)	(3.835)
Shock($\tau = +3$)	6.338	1.079	-1.716
	(4.765)	(4.607)	(4.360)
Shock($\tau > +3$)	-0.155	-0.659	1.450
	(7.899)	(5.271)	(7.158)
Observations	14053	15473	14411
R^2	0.664	0.677	0.794
Control Variables	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes

Table 9. Financial Distress

This table reports the coefficients from equation (1) estimated separately for firms close to financial distress (columns 1, 3, 5, 7, 9) and away from it (columns 2, 4, 6, 8, 10). We define a firm as financially-distressed if its EBIT interest coverage ratio is smaller than 2. The dependent variable is ROA (in columns 1 and 2), asset sales (in columns 3 and 4), mass layoffs (in columns 5 and 6), shareholder payout (in columns 7 and 8) and AP days (in columns 9 and 10). We use the matched sample which excludes the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	ROA		Asset Sales		Mass Layoffs		Shareholder Payout		AP Days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock($\tau = -3$)	0.022	0.000	-0.044	0.003	0.003	0.005	-0.007	0.004	8.235	5.802
	(0.015)	(0.004)	(0.042)	(0.028)	(0.027)	(0.018)	(0.013)	(0.006)	(5.917)	(3.584)
Shock($\tau = -2$)	0.004	0.000	-0.004	-0.003	-0.011	0.040*	-0.012*	0.001	4.950	5.716
	(0.017)	(0.005)	(0.047)	(0.021)	(0.039)	(0.021)	(0.006)	(0.006)	(7.924)	(4.398)
Shock($\tau = 0$)	-0.053***	-0.000	0.154**	-0.040	0.110**	0.019	-0.016	-0.002	6.421	14.633***
	(0.018)	(0.004)	(0.063)	(0.032)	(0.042)	(0.021)	(0.010)	(0.004)	(9.554)	(4.584)
Shock($\tau = +1$)	-0.072***	-0.008*	0.177***	-0.006	0.114**	0.059**	-0.006	0.005	9.743	9.682*
	(0.020)	(0.005)	(0.058)	(0.028)	(0.053)	(0.026)	(0.015)	(0.007)	(9.262)	(5.233)
Shock($\tau = +2$)	-0.040*	-0.013*	0.170***	0.051	0.080	0.086**	-0.003	0.008	10.144	8.698*
	(0.024)	(0.007)	(0.064)	(0.036)	(0.054)	(0.034)	(0.012)	(0.007)	(10.024)	(5.030)
Shock($\tau = +3$)	-0.014	-0.004	0.138**	0.030	-0.007	0.083**	0.012	-0.004	9.384	3.304
	(0.019)	(0.011)	(0.067)	(0.035)	(0.038)	(0.036)	(0.014)	(0.005)	(9.729)	(4.708)
Shock($\tau > +3$)	0.008	-0.010	0.068	0.066**	0.005	0.030	0.008	0.010	-10.554	4.749
	(0.022)	(0.006)	(0.045)	(0.030)	(0.033)	(0.019)	(0.013)	(0.008)	(13.231)	(7.386)
Observations	6660	11726	6583	11592	5488	10781	6646	11736	4682	8538
R^2	0.487	0.401	0.344	0.332	0.305	0.243	0.602	0.549	0.656	0.700
Interest Coverage ≤ 2	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10. Productive versus Unproductive Firms

This table reports the coefficients from equation (1) estimated separately for productive firms (columns 1, 3, 5, 7, 9) and unproductive firms (columns 2, 4, 6, 8, 10). We use firms' pre-shock productivity level to split the sample. A firm is considered as productive if its sales per employee is higher than or equal to the industry median one year prior to the cartel investigation. The dependent variable is ROA (in columns 1 and 2), asset sales (in columns 3 and 4), mass layoffs (in columns 5 and 6), shareholder payout (in columns 7 and 8), and AP days (in columns 9 and 10). We use the matched sample which excludes the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	ROA		Asset Sales		Mass Layoffs		Shareholder Payout		AP Days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock($\tau = -3$)	0.001	0.017*	-0.016	0.009	0.006	0.012	-0.005	0.002	4.912	2.154
	(0.004)	(0.009)	(0.030)	(0.028)	(0.022)	(0.023)	(0.005)	(0.007)	(4.143)	(4.812)
Shock($\tau = -2$)	0.007	-0.002	-0.017	-0.013	0.014	0.015	-0.006	-0.003	-0.139	7.014
	(0.006)	(0.010)	(0.028)	(0.026)	(0.025)	(0.026)	(0.007)	(0.005)	(4.180)	(6.102)
Shock($\tau = 0$)	-0.021*	-0.025**	0.016	0.034	0.012	0.088***	-0.008	-0.006	9.225*	10.588*
	(0.013)	(0.012)	(0.048)	(0.030)	(0.022)	(0.022)	(0.006)	(0.005)	(4.945)	(5.916)
Shock($\tau = +1$)	-0.021	-0.044**	0.062	0.058	0.035	0.126***	-0.009	0.010	5.582	11.431*
	(0.016)	(0.017)	(0.047)	(0.038)	(0.032)	(0.030)	(0.006)	(0.009)	(6.218)	(6.721)
Shock($\tau = +2$)	-0.016	-0.022	0.069**	0.103*	0.038	0.129***	0.003	0.006	7.816	8.490
	(0.014)	(0.016)	(0.033)	(0.057)	(0.031)	(0.033)	(0.010)	(0.008)	(4.887)	(7.115)
Shock($\tau = +3$)	0.009	-0.012	0.062	0.018	0.027	0.078*	0.000	0.006	3.406	4.737
	(0.011)	(0.018)	(0.043)	(0.035)	(0.028)	(0.041)	(0.011)	(0.010)	(6.078)	(7.632)
Shock($\tau > +3$)	-0.000	-0.010	0.021	0.067**	-0.014	0.038*	0.004	0.011	0.409	0.097
	(0.008)	(0.010)	(0.031)	(0.026)	(0.020)	(0.023)	(0.007)	(0.010)	(8.924)	(10.209)
Observations	10860	9070	10814	8982	10209	8056	10868	9064	7358	5756
R^2	0.403	0.545	0.322	0.358	0.238	0.290	0.338	0.716	0.702	0.672
High Productivity	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 11. Tradable versus Non-Tradable Industries

This table reports the coefficients from equation (1) estimated separately for firms operating in non-tradable (columns 1, 3, 5, 7, 9) and tradable (columns 2, 4, 6, 8, 10) industries. We define tradable/non-tradable industries following Besley, Fontana, and Limodio (2020): tradable industries are Agriculture, forestry and fishing (A), Mining (B), and Manufacturing (D); all other industries are labeled as non-tradable. The dependent variable is ROA (in columns 1 and 2), asset sales (in columns 3 and 4), mass layoffs (in columns 5 and 6), shareholder payout (in columns 7 and 8) and AP days (in columns 9 and 10). We use the matched sample which excludes the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	ROA		Asset Sales		Mass Layoffs		Shareholder Payout		AP Days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock($\tau = -3$)	0.006	0.000	-0.012	0.010	0.017	0.037	0.005	0.001	12.407	2.012
	(0.005)	(0.007)	(0.025)	(0.034)	(0.018)	(0.023)	(0.008)	(0.007)	(8.635)	(3.121)
Shock($\tau = -2$)	-0.000	-0.001	-0.038	0.026	0.032	0.019	-0.003	-0.002	9.636	1.225
	(0.004)	(0.009)	(0.024)	(0.028)	(0.022)	(0.028)	(0.006)	(0.008)	(8.634)	(3.241)
Shock($\tau = 0$)	-0.024*	-0.011	0.022	0.009	0.079***	0.010	-0.006	-0.001	18.851**	2.670
	(0.012)	(0.010)	(0.049)	(0.028)	(0.024)	(0.025)	(0.005)	(0.005)	(8.124)	(4.626)
Shock($\tau = +1$)	-0.049***	-0.004	0.111**	-0.010	0.099**	0.036	-0.005	0.002	18.942***	-0.095
	(0.017)	(0.010)	(0.050)	(0.040)	(0.038)	(0.024)	(0.006)	(0.009)	(5.177)	(5.629)
Shock($\tau = +2$)	-0.048**	0.005	0.150***	0.005	0.102**	0.034	-0.001	0.002	20.363***	3.984
	(0.018)	(0.008)	(0.031)	(0.051)	(0.045)	(0.026)	(0.008)	(0.009)	(4.652)	(6.309)
Shock($\tau = +3$)	-0.026**	0.020	0.063	-0.000	0.088**	0.003	-0.001	-0.003	12.179	-2.721
	(0.011)	(0.014)	(0.044)	(0.050)	(0.039)	(0.027)	(0.007)	(0.007)	(7.329)	(5.442)
Shock($\tau > +3$)	-0.005	-0.004	0.059*	0.017	0.014	-0.008	0.001	0.006	4.669	-4.224
	(0.009)	(0.010)	(0.035)	(0.032)	(0.016)	(0.023)	(0.007)	(0.009)	(20.342)	(8.926)
Observations	10669	10494	10549	10378	9560	9233	10662	10493	4538	9391
R^2	0.365	0.558	0.347	0.350	0.292	0.267	0.408	0.661	0.701	0.655
Non-Tradable	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 12. Price-Fixing Cartels

This table reports the coefficients from equation (1) using the matched sample but including only the price-fixing cartels. The dependent variable is ROA (in column 1), asset sales (in column 2), mass layoffs (in column 3), shareholder payout (in column 4), and AP days (in columns 5). We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the natural logarithms of total assets and sales as firm-level controls, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	ROA	Asset Sales	Mass Layoffs	Shareholder Payout	AP Days
	(1)	(2)	(3)	(4)	(5)
Shock($\tau = -3$)	0.000	-0.026	-0.018	-0.010*	6.606
	(0.008)	(0.030)	(0.020)	(0.006)	(8.022)
Shock($\tau = -2$)	-0.003	-0.016	0.016	-0.016***	3.085
	(0.006)	(0.027)	(0.026)	(0.005)	(7.348)
Shock($\tau = 0$)	-0.018	0.046	0.073**	-0.018***	9.258
	(0.013)	(0.030)	(0.029)	(0.006)	(9.975)
Shock($\tau = +1$)	-0.037	0.087	0.107***	-0.014**	4.742
	(0.023)	(0.065)	(0.036)	(0.007)	(9.243)
Shock($\tau = +2$)	-0.037*	0.072	0.121***	-0.008	13.158
	(0.021)	(0.044)	(0.038)	(0.010)	(8.711)
Shock($\tau = +3$)	-0.017	0.087**	0.048	-0.007	8.980
	(0.018)	(0.043)	(0.029)	(0.010)	(9.011)
Shock($\tau > +3$)	-0.004	0.049	0.007	-0.011	-13.765
	(0.012)	(0.043)	(0.015)	(0.009)	(19.714)
Observations	8620	8548	7958	8628	3848
R^2	0.546	0.344	0.253	0.542	0.697
Control Variables	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes

Appendix

A.1 Cartel Case Collection

The cartel cases exploited in this paper are documented on the European Commission website. See https://ec.europa.eu/competition/cartels/cases/cases.html. We collect all the cases that were closed between 2000 and 2019. The EC website arranges the cases using the closing year rather than the opening year of the investigation. We use the opening year as the shock year. For each case, we read thoroughly through the decision files. We start from the EC's *Summary decision*. When the information we need is not disclosed in the *Summary decision* file, we resort to the *Non-confidential version of the decision*, which is the detailed version of the *Summary decision*. From these sources, we collect data on when the investigation was started, the cartel firms, the cartel periods, the cartel industries, any immunity or fine reduction applications, and the granted cases of immunity and fine reductions.

The opening year of an investigation is the year when the EC first initiated the case. In many cases, the initiation is provoked by the leniency application submitted by one of the cartel firms. We take the year of the first-submitted leniency application as the opening year in those cases. Regarding the cartel firms, we note down all the legal entities (not just undertakings) and then assign them ISIN codes if they are public firms, or EIKON PermID if they are private firms. For cartel periods, given the complexity of the relations among all the legal entities and undertakings – some legal entities may be acquired by another parent company or merge with other legal entities during the collusion period – we take the collusion periods of the undertakings and assign them to their subsequent legal entities. Similarly, the application and granting of immunity or fine reduction are also documented at undertaking level. As for the cartel industries, the EC discloses a three-digit or four-digit NACE Rev.2 code to each case. In order to merge this with our Worldscope dataset, we first manually search for the four-digit SIC codes based on the product description in each decision file, we then double check the accuracy of the SIC codes through the industry codes matching table. All the cases where we are uncertain about the industry codes are excluded from our sample.

A.2 Variable Definitions

Variable	Definition	Source
Outcome Variables		
ROA	A firm's net income before extraordinary items and preferred dividends normalized by total assets.	Worldscope
EBITDA/Assets	A firm's EBITDA normalized by total assets.	Worldscope
Net Leverage	Firm's net debt (net debt is defined as debt minus cash and short-term investment) normalized by assets.	Worldscope
Shareholder Payout	The sum of cash dividends and share repurchases over sales.	Worldscope
AP Days	AP Days are calculated as accounts payable divided by COGS, times 365.	Worldscope
AR Days	AR Days are calculated as accounts receivable divided by sales, times 365.	Worldscope
Inventory Days	Inventory Days are calculated as a firm's inventory divided by COGS, times 365.	Worldscope
Investment	A firm's CAPEX normalized by total assets.	Worldscope
Asset Sales	A dummy variable that equals one if a firm's net PPE experiences a drop of at least 15% over the last year or last two years.	Worldscope
Asset Growth	The growth rate of a firm's net PPE.	Worldscope
Mass Layoffs	A dummy variable that equals one if a firm's number of employees experiences a drop of at least 20% over the last year or last two years.	Worldscope
log(Employees)	The log value of a firm's number of employees.	Worldscope
Control Variables		
Total Assets	The log value of a firm's total assets.	Worldscope
Size	The log value of a firm's sales.	Worldscope
Non-Tradable	A dummy variable that equals one if a firm belongs to the non-tradable sector. Specifically, tradable industries include Agriculture, Forestry and Fishing (A), Mining (B), and Manufacturing (D); all other industries are non-tradable ones.	Worldscope
Interest Coverage	The ratio of EBIT divided by firms' total interest expense.	Worldscope
GDP Growth	A country's GDP growth in that year.	World Development Indicators
Unemployment Rate	The country-level unemployment rate in that year.	World Development Indicators
GDP per capita	The log value of a country's GDP per capita.	World Development Indicators

A.3 Robustness Tests

Table A.1. Logit Estimates

This table reports the estimated coefficients from equation (1) obtained with logit regressions, using the *Mass Layoff* and *Asset Sales* indicators as the outcome. We control for firm, year and country fixed effects. The natural logarithms of total assets and sales are included as firm controls in column 2, 3, 5, and 6. In column 3 and 6 we also include country-level controls. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Mass Layoffs			*Asset Sales*		
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	-0.014	0.017	-0.038	-0.106	-0.084	-0.120
	(0.195)	(0.196)	(0.198)	(0.143)	(0.144)	(0.147)
Shock($\tau = -2$)	0.166	0.154	0.094	-0.068	-0.077	-0.093
	(0.184)	(0.185)	(0.188)	(0.131)	(0.132)	(0.135)
Shock($\tau = 0$)	0.392**	0.393**	0.371**	0.130	0.124	0.153
	(0.175)	(0.176)	(0.179)	(0.126)	(0.126)	(0.128)
Shock($\tau = +1$)	0.622***	0.607***	0.550***	0.410***	0.406***	0.434***
	(0.178)	(0.179)	(0.182)	(0.127)	(0.127)	(0.129)
Shock($\tau = +2$)	0.616***	0.617***	0.575***	0.577***	0.566***	0.613***
	(0.182)	(0.183)	(0.185)	(0.131)	(0.132)	(0.134)
Shock($\tau = +3$)	0.464**	0.472**	0.366*	0.359***	0.366***	0.384***
	(0.193)	(0.194)	(0.198)	(0.133)	(0.133)	(0.135)
Shock($\tau > +3$)	-0.104	-0.097	-0.169	0.281***	0.282***	0.314***
	(0.141)	(0.142)	(0.146)	(0.091)	(0.092)	(0.095)
Observations	11065	10900	10094	20195	19955	18779
R^2	0.029	0.029	0.025	0.131	0.133	0.128
Firm-Level Controls	No	Yes	Yes	No	Yes	Yes
Country-Level Controls	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A.2. Alternative Industry Classification

This table reports the estimated coefficients from equation (1). We exploit an alternative industry classification, product segment SIC, to identify the treated and control firms. We control for firm, country×year fixed effects. The natural logarithms of total assets and sales are included as firm controls in all columns. All standard errors are clustered at 4-digit code product SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	ROA	EBITDA/Assets	Net Leverage	Shareholder Payout	AP Days	AR Days	Inventory Days	Investment	Asset Sales	Asset Growth	Mass Layoffs	log(Employees)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Shock($\tau = -3$)	0.010	0.007	0.005	0.005	5.327	2.431	-2.504	-0.001	-0.026	0.027	-0.008	-0.049
	(0.006)	(0.006)	(0.011)	(0.005)	(3.371)	(2.658)	(2.722)	(0.004)	(0.018)	(0.046)	(0.015)	(0.030)
Shock($\tau = -2$)	0.003	0.003	0.003	-0.003	4.122	6.647*	0.253	-0.003	-0.021	0.058	0.016	-0.071
	(0.006)	(0.006)	(0.012)	(0.004)	(3.225)	(3.358)	(2.942)	(0.006)	(0.021)	(0.074)	(0.019)	(0.045)
Shock($\tau = 0$)	-0.015**	-0.015**	0.011	-0.005	12.195***	0.574	-0.413	-0.011*	0.020	-0.122***	0.039**	-0.122**
	(0.007)	(0.007)	(0.014)	(0.005)	(4.370)	(2.738)	(3.897)	(0.006)	(0.033)	(0.035)	(0.018)	(0.048)
Shock($\tau = +1$)	-0.033***	-0.026***	0.019	-0.003	12.416**	-3.232	-2.452	-0.012*	0.057*	-0.140*	0.062***	-0.129**
	(0.011)	(0.009)	(0.013)	(0.006)	(4.903)	(4.810)	(4.159)	(0.007)	(0.030)	(0.072)	(0.023)	(0.054)
Shock($\tau = +2$)	-0.021*	-0.010	0.022	0.000	12.324***	0.222	-5.684	-0.010	0.086**	-0.094	0.085***	-0.146**
	(0.011)	(0.008)	(0.014)	(0.006)	(4.632)	(4.622)	(4.103)	(0.006)	(0.035)	(0.069)	(0.028)	(0.059)
Shock($\tau = +3$)	-0.002	0.007	0.017	-0.002	6.798	1.783	-3.986	-0.009	0.068**	-0.097**	0.062**	-0.118*
	(0.014)	(0.015)	(0.017)	(0.007)	(4.568)	(4.680)	(5.523)	(0.006)	(0.030)	(0.043)	(0.030)	(0.065)
Shock($\tau > +3$)	-0.001	0.007	0.035*	0.002	2.104	1.881	0.658	-0.015**	0.058**	-0.058	0.022	-0.113*
	(0.007)	(0.008)	(0.019)	(0.007)	(7.883)	(5.158)	(9.049)	(0.006)	(0.022)	(0.050)	(0.015)	(0.067)
Observations	21042	19192	15046	21033	13864	15164	14277	18916	20832	20807	18799	19075
R^2	0.505	0.567	0.639	0.369	0.678	0.680	0.784	0.564	0.322	0.207	0.238	0.962
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.3. Excluding Busting Industries

This table reports the coefficients from equation (1) using the matched sample, where all the industries that are in a recession one year before the cartel investigations begin are excluded. The regressions include the natural logarithms of total assets and sales as firm-level controls. We control for firm, country*year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	ROA	EBITDA/Assets	Net Leverage	Shareholder Payout	AP Days	AR Days	Inventory Days	Investment	Asset Sales	Asset Growth	Mass Layoffs	log(Employees)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Shock($\tau = -3$)	0.008	0.006	-0.002	0.002	2.803	0.628	-4.628*	-0.002	-0.036*	0.095	-0.007	-0.056
	(0.006)	(0.006)	(0.016)	(0.006)	(4.233)	(4.106)	(2.773)	(0.005)	(0.021)	(0.067)	(0.017)	(0.035)
Shock($\tau = -2$)	0.003	0.003	0.005	-0.004	1.248	3.322	-3.127	0.000	-0.036*	0.129	0.010	-0.063
	(0.006)	(0.006)	(0.015)	(0.005)	(4.093)	(3.772)	(3.255)	(0.006)	(0.021)	(0.093)	(0.020)	(0.049)
Shock($\tau = 0$)	-0.011	-0.012	0.010	-0.008**	9.719**	-0.736	-3.055	-0.011	0.015	-0.079**	0.047**	-0.137**
	(0.008)	(0.008)	(0.017)	(0.004)	(4.556)	(3.592)	(4.024)	(0.007)	(0.040)	(0.035)	(0.022)	(0.053)
Shock($\tau = +1$)	-0.034***	-0.027**	0.013	-0.003	11.196**	-5.448	-4.092	-0.011	0.052	-0.075	0.082***	-0.135**
	(0.012)	(0.011)	(0.019)	(0.006)	(4.664)	(6.544)	(4.796)	(0.007)	(0.037)	(0.086)	(0.029)	(0.060)
Shock($\tau = +2$)	-0.033**	-0.020*	0.013	0.006	11.161**	-5.006	-8.167*	-0.010	0.108***	-0.123***	0.080**	-0.163***
	(0.013)	(0.010)	(0.021)	(0.007)	(4.556)	(6.010)	(4.178)	(0.008)	(0.030)	(0.033)	(0.034)	(0.059)
Shock($\tau = +3$)	-0.015	-0.005	0.010	0.003	6.985	-2.221	-6.883	-0.010	0.073**	-0.075**	0.062*	-0.083
	(0.012)	(0.013)	(0.024)	(0.008)	(5.210)	(5.382)	(4.675)	(0.007)	(0.028)	(0.034)	(0.035)	(0.075)
Shock($\tau > +3$)	-0.005	0.002	0.019	0.002	-4.564	-3.491	-8.675*	-0.014**	0.034	-0.067	0.000	-0.085
	(0.009)	(0.010)	(0.028)	(0.008)	(9.792)	(5.588)	(4.780)	(0.007)	(0.026)	(0.041)	(0.016)	(0.082)
Observations	17319	15588	11636	17317	10770	11787	11065	15463	17128	17097	15204	15453
R^2	0.432	0.502	0.629	0.530	0.683	0.699	0.803	0.572	0.323	0.216	0.254	0.965
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.4. Financial Distress

This table reports the coefficients from equation (1) focusing on the impact of cartel investigations on various firm outcomes estimated separately for financially-constrained firms and financially-unconstrained firms. We define a firm as financially-constrained if its interest coverage ratio is smaller than 2. The regressions include the natural logarithms of total assets and sales as firm-level controls. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EBITDA/Assets		Investment		Asset Growth		Net Leverage		AR Days		Inventory Days		log(Employees)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Shock($\tau = -3$)	0.008	0.001	-0.005	-0.001	0.304	-0.083***	0.005	-0.004	-3.809	7.344**	1.573	-1.056	-0.036	-0.054
	(0.013)	(0.004)	(0.010)	(0.004)	(0.237)	(0.031)	(0.020)	(0.011)	(6.237)	(3.006)	(4.472)	(3.021)	(0.066)	(0.035)
Shock($\tau = -2$)	0.010	-0.004	-0.008	0.001	0.405**	-0.033	0.014	-0.002	5.237	5.216	3.779	-0.260	-0.094	-0.080
	(0.015)	(0.005)	(0.010)	(0.006)	(0.199)	(0.047)	(0.019)	(0.014)	(5.572)	(3.730)	(4.983)	(3.158)	(0.058)	(0.054)
Shock($\tau = 0$)	-0.044**	-0.007	-0.027***	-0.002	-0.279***	-0.013	0.003	-0.010	-5.474	4.778	-4.249	3.571	-0.152**	-0.132***
	(0.017)	(0.006)	(0.010)	(0.005)	(0.088)	(0.045)	(0.029)	(0.013)	(4.872)	(3.550)	(4.704)	(5.638)	(0.070)	(0.045)
Shock($\tau = +1$)	-0.050***	-0.009	-0.024**	-0.007	-0.294**	-0.064	0.007	0.004	-13.133*	3.852	-11.104**	0.563	-0.189**	-0.131***
	(0.018)	(0.006)	(0.012)	(0.006)	(0.116)	(0.046)	(0.027)	(0.015)	(7.437)	(4.600)	(5.364)	(5.146)	(0.081)	(0.043)
Shock($\tau = +2$)	-0.011	-0.013*	-0.019	-0.005	-0.048	-0.115**	-0.001	0.011	-4.037	3.285	-9.007	-5.583	-0.171*	-0.166***
	(0.022)	(0.007)	(0.012)	(0.005)	(0.157)	(0.046)	(0.030)	(0.017)	(6.452)	(4.793)	(6.228)	(5.906)	(0.089)	(0.055)
Shock($\tau = +3$)	-0.002	0.004	-0.024**	-0.000	-0.302***	-0.027	-0.020	0.014	-2.535	5.445	3.831	-5.229	-0.147	-0.119*
	(0.020)	(0.011)	(0.011)	(0.004)	(0.095)	(0.056)	(0.039)	(0.016)	(5.339)	(5.101)	(7.348)	(6.458)	(0.110)	(0.061)
Shock($\tau > +3$)	0.021	-0.005	-0.022**	-0.011**	-0.187*	-0.066*	0.018	0.035*	-4.134	5.335	5.823	-1.162	-0.121	-0.121*
	(0.019)	(0.007)	(0.010)	(0.005)	(0.098)	(0.037)	(0.040)	(0.021)	(6.873)	(5.697)	(7.418)	(9.097)	(0.121)	(0.068)
Observations	6384	11242	5874	10862	6564	11577	5224	9136	5398	9146	4844	8711	5620	10913
R^2	0.536	0.536	0.558	0.584	0.273	0.202	0.657	0.626	0.677	0.704	0.782	0.821	0.959	0.969
Interest Coverage ≤ 2	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.5. Productive versus Unproductive Firms

This table reports the coefficients from equation (1) focusing on the impact of cartel investigations on various firm outcomes estimated separately for productive and unproductive firms. We use firms' pre-shock productivity level to split the sample. A firm is considered as productive if its sales per employee is higher than or equal to the industry median one year prior to the cartel investigation. The regressions include the natural logarithms of total assets and sales as firm-level controls. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EBITDA/Assets		Investment		Asset Growth		Net Leverage		AR Days		Inventory Days		log(Employees)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Shock($\tau = -3$)	-0.006	0.016	0.002	-0.004	0.133	-0.041	-0.004	0.000	1.906	-3.372	-4.228	1.031	-0.089*	0.018
	(0.005)	(0.010)	(0.005)	(0.005)	(0.098)	(0.037)	(0.014)	(0.022)	(3.142)	(4.239)	(3.520)	(4.886)	(0.045)	(0.034)
Shock($\tau = -2$)	0.004	-0.001	-0.002	-0.001	0.072	0.139	-0.002	0.010	-2.742	8.802	0.776	-3.838	-0.106**	-0.016
	(0.007)	(0.010)	(0.007)	(0.006)	(0.130)	(0.094)	(0.017)	(0.020)	(2.634)	(5.980)	(3.575)	(4.144)	(0.043)	(0.047)
Shock($\tau = 0$)	-0.017	-0.029**	-0.010	-0.012***	-0.174***	-0.059	0.032**	-0.020	-5.330	3.434	4.673	-6.189	-0.166***	-0.072
	(0.010)	(0.013)	(0.007)	(0.004)	(0.052)	(0.065)	(0.015)	(0.022)	(3.849)	(3.596)	(5.867)	(4.283)	(0.049)	(0.049)
Shock($\tau = +1$)	-0.009	-0.039***	-0.007	-0.018***	-0.212***	0.030	0.039**	-0.001	-6.036	5.175	-0.068	-3.817	-0.141**	-0.102**
	(0.012)	(0.014)	(0.007)	(0.007)	(0.070)	(0.082)	(0.018)	(0.016)	(5.471)	(4.894)	(5.710)	(5.897)	(0.063)	(0.047)
Shock($\tau = +2$)	0.000	-0.011	-0.011*	-0.009	-0.121	0.009	0.037*	0.004	-8.017	3.554	-8.659	-4.227	-0.177***	-0.102*
	(0.009)	(0.018)	(0.007)	(0.007)	(0.080)	(0.086)	(0.019)	(0.020)	(5.115)	(5.587)	(5.463)	(5.517)	(0.060)	(0.053)
Shock($\tau = +3$)	0.013	-0.010	-0.009	-0.010*	-0.092*	-0.053	0.031	-0.002	-6.538	7.039	-2.031	0.450	-0.106	-0.119**
	(0.011)	(0.019)	(0.007)	(0.006)	(0.052)	(0.055)	(0.025)	(0.022)	(5.148)	(6.221)	(6.014)	(6.126)	(0.080)	(0.048)
Shock($\tau > +3$)	0.005	-0.001	-0.011*	-0.017***	-0.064	-0.054	0.049**	0.013	-3.353	1.335	0.659	1.268	0.006	-0.199***
	(0.009)	(0.011)	(0.007)	(0.005)	(0.045)	(0.053)	(0.023)	(0.028)	(5.826)	(6.842)	(8.443)	(7.341)	(0.083)	(0.053)
Observations	9976	8227	9924	8084	10806	8963	7873	6328	7902	6404	7482	5934	10296	8198
R^2	0.519	0.607	0.573	0.607	0.233	0.233	0.666	0.668	0.711	0.692	0.833	0.780	0.969	0.968
High Productivity	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.6. Tradable versus Non-Tradable Industries

This table reports the coefficients from equation (1) focusing on the impact of cartel investigations on various firm outcomes estimated separately for firms operating in tradable/non-tradable industries. We define tradable/non-tradable industries following Besley, Fontana, and Limodio (2020): tradable industries are Agriculture, forestry and fishing (A), Mining (B), and Manufacturing (D); all other industries are labeled as non-tradable. The regressions include the natural logarithms of total assets and sales as firm-level controls. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EBITDA/Assets		Investment		Asset Growth		Net Leverage		AR Days		Inventory Days		log(Employees)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Shock($\tau = -3$)	-0.002	-0.001	-0.001	-0.002	0.130*	-0.084	-0.013	0.009	-5.067	-0.900	-0.867	0.494	-0.061	-0.039
	(0.004)	(0.008)	(0.006)	(0.002)	(0.073)	(0.069)	(0.024)	(0.013)	(5.722)	(2.876)	(3.211)	(4.126)	(0.045)	(0.037)
Shock($\tau = -2$)	-0.006	0.005	-0.002	-0.007	0.237*	-0.072	-0.017	0.001	-3.187	-0.142	1.343	0.477	-0.061	-0.030
	(0.004)	(0.008)	(0.008)	(0.005)	(0.117)	(0.059)	(0.018)	(0.020)	(6.073)	(3.603)	(5.135)	(3.220)	(0.061)	(0.034)
Shock($\tau = 0$)	-0.028**	-0.008	-0.017**	-0.004	-0.099**	-0.035	0.023	-0.026	-13.603*	-0.014	1.651	0.864	-0.171***	-0.030
	(0.012)	(0.010)	(0.008)	(0.005)	(0.038)	(0.066)	(0.025)	(0.019)	(6.934)	(2.881)	(6.710)	(4.295)	(0.063)	(0.038)
Shock($\tau = +1$)	-0.042***	-0.003	-0.017	-0.007	-0.175	-0.027	0.041	-0.010	-20.438*	0.152	-7.448	2.428	-0.147**	-0.051
	(0.015)	(0.009)	(0.010)	(0.005)	(0.119)	(0.107)	(0.030)	(0.019)	(11.279)	(4.362)	(6.387)	(5.219)	(0.070)	(0.042)
Shock($\tau = +2$)	-0.044***	0.016	-0.015	-0.005	-0.165***	-0.014	0.029	0.004	-19.287	2.111	-5.988	-3.675	-0.183**	-0.063
	(0.014)	(0.010)	(0.010)	(0.003)	(0.037)	(0.088)	(0.030)	(0.023)	(11.906)	(4.046)	(5.111)	(5.159)	(0.072)	(0.045)
Shock($\tau = +3$)	-0.024*	0.033***	-0.020*	0.003	-0.093*	-0.079	0.029	0.007	-11.038	3.390	-4.973	1.834	-0.124	-0.035
	(0.013)	(0.012)	(0.010)	(0.005)	(0.051)	(0.068)	(0.037)	(0.026)	(12.078)	(4.399)	(6.330)	(5.625)	(0.088)	(0.056)
Shock($\tau > +3$)	0.002	0.009	-0.022**	-0.005	-0.149***	-0.013	0.063*	0.017	-11.780	2.422	-2.990	4.725	-0.136	-0.006
	(0.008)	(0.011)	(0.009)	(0.004)	(0.045)	(0.062)	(0.036)	(0.028)	(12.481)	(5.156)	(4.612)	(9.054)	(0.097)	(0.061)
Observations	9041	10261	9282	9746	10535	10356	5012	10139	5058	10291	4731	9562	9699	9385
R^2	0.431	0.618	0.647	0.481	0.242	0.234	0.671	0.649	0.712	0.679	0.641	0.801	0.969	0.965
Non-Tradable	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.7. Price-Fixing Cartels

This table reports the coefficients from equation (1) using the matched sample, where only the price-fixing cartels are included. The regressions include the natural logarithms of total assets and sales as firm-level controls. We control for firm, country*year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EBITDA/Assets	Investment	Asset Growth	Net Leverage	AR Days	Inventory Days	log(Employees)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shock($\tau = -3$)	0.001	-0.001	0.074	0.030	-7.691*	-3.488	-0.002
	(0.008)	(0.003)	(0.087)	(0.021)	(4.348)	(2.797)	(0.035)
Shock($\tau = -2$)	-0.003	-0.005	0.109	0.015	-2.852	-0.956	-0.008
	(0.008)	(0.003)	(0.134)	(0.026)	(4.674)	(3.993)	(0.028)
Shock($\tau = 0$)	-0.020	-0.006	-0.051	0.032	-3.785	-2.252	-0.096**
	(0.012)	(0.004)	(0.052)	(0.030)	(5.921)	(4.478)	(0.042)
Shock($\tau = +1$)	-0.039*	-0.010**	-0.127	0.027	-19.486*	-3.538	-0.118*
	(0.023)	(0.004)	(0.156)	(0.032)	(10.593)	(5.991)	(0.063)
Shock($\tau = +2$)	-0.035**	-0.006	-0.132**	0.031	-5.186	-2.136	-0.138***
	(0.016)	(0.004)	(0.051)	(0.034)	(12.653)	(5.276)	(0.052)
Shock($\tau = +3$)	-0.000	-0.007	-0.076	0.027	-7.655	-9.009	-0.034
	(0.017)	(0.006)	(0.065)	(0.039)	(11.119)	(5.678)	(0.078)
Shock($\tau > +3$)	0.007	-0.008	-0.093*	0.060	-11.556	-5.836	-0.067
	(0.012)	(0.006)	(0.054)	(0.046)	(9.083)	(6.573)	(0.077)
Observations	7272	7401	8536	4281	4234	3963	8034
R^2	0.580	0.670	0.215	0.625	0.736	0.821	0.974
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Figure A.1: The impact of cartel investigations on performance: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021) "interaction weighted" estimator, together with 95% confidence intervals. We control for firm and country × year fixed effects as well as firm-level controls. In the top panel we use ROA as the outcome variable; in the bottom panel we use EBITDA/Assets.

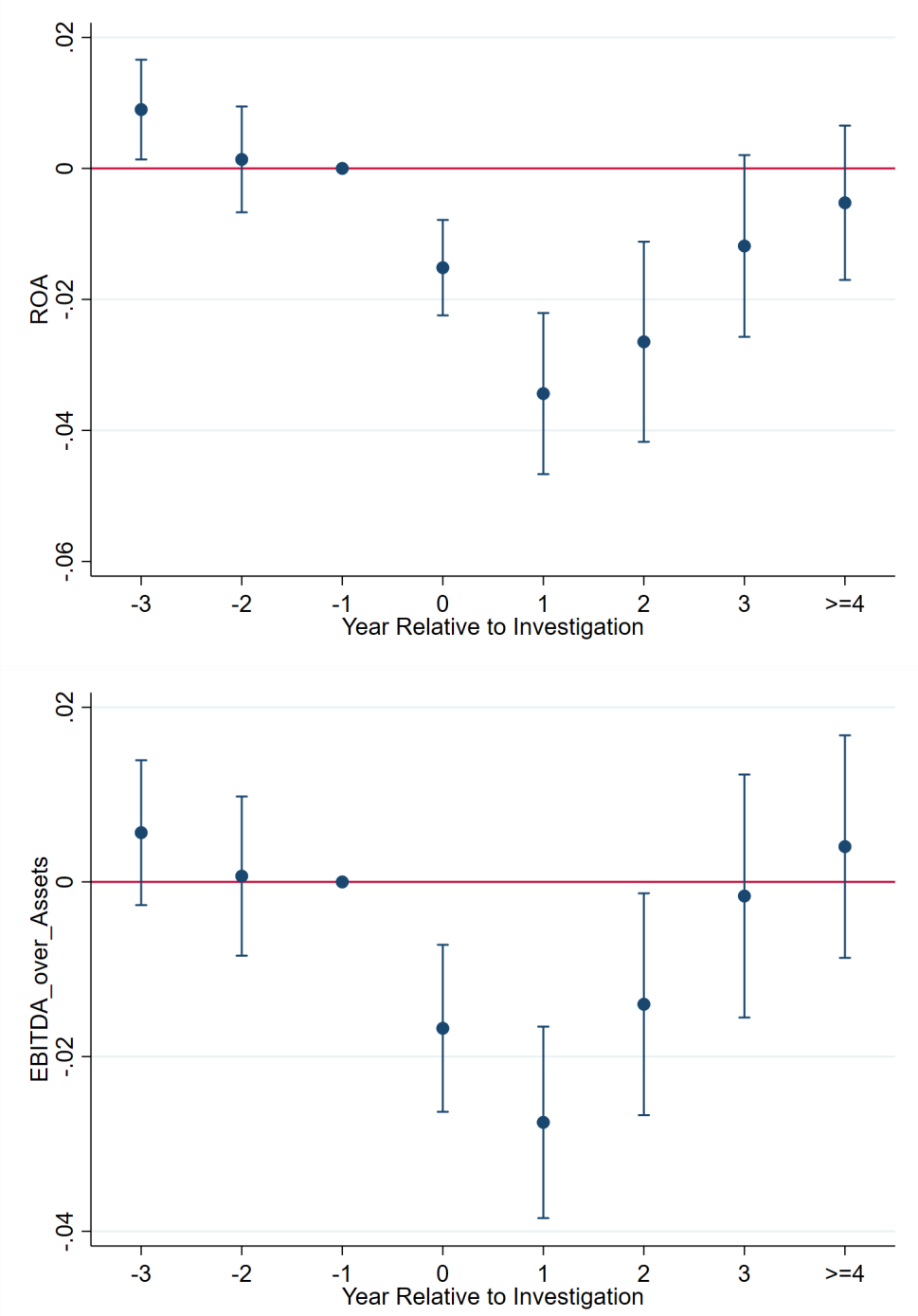

Figure A.2: The impact of cartel investigations on investment: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021) "interaction weighted" estimator, together with 95% confidence intervals. The top panel reports estimates for *Investment*; the middle panel for *Asset Sales*; and the bottom panel for *Asset Growth*. We control for firm and country × year fixed effects as well as firm-level controls.

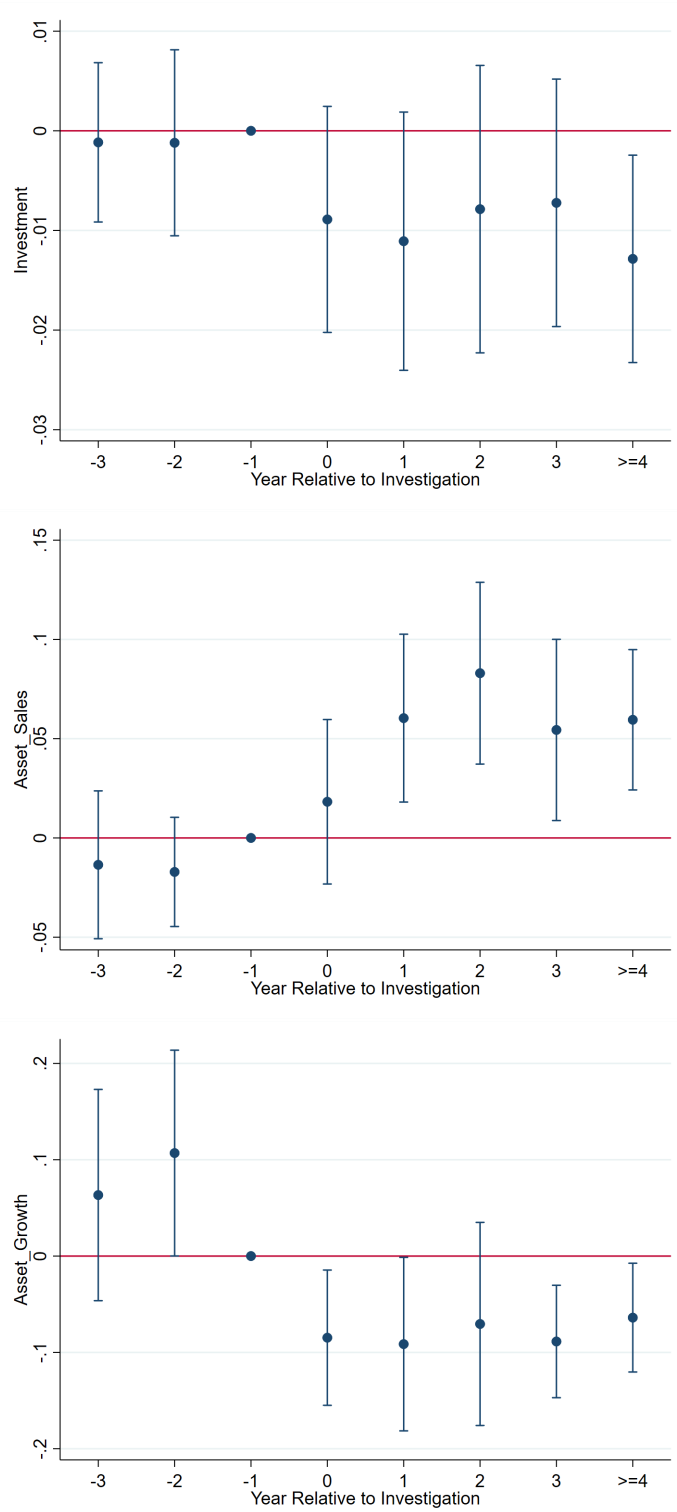

Figure A.3: The impact of cartel investigations on labor: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021) "interaction weighted" estimator, together with 95% confidence intervals. The top panel reports estimates for *Mass Layoffs*; and the bottom panel for *Employment*. We control for firm and country × year fixed effects as well as firm-level controls.

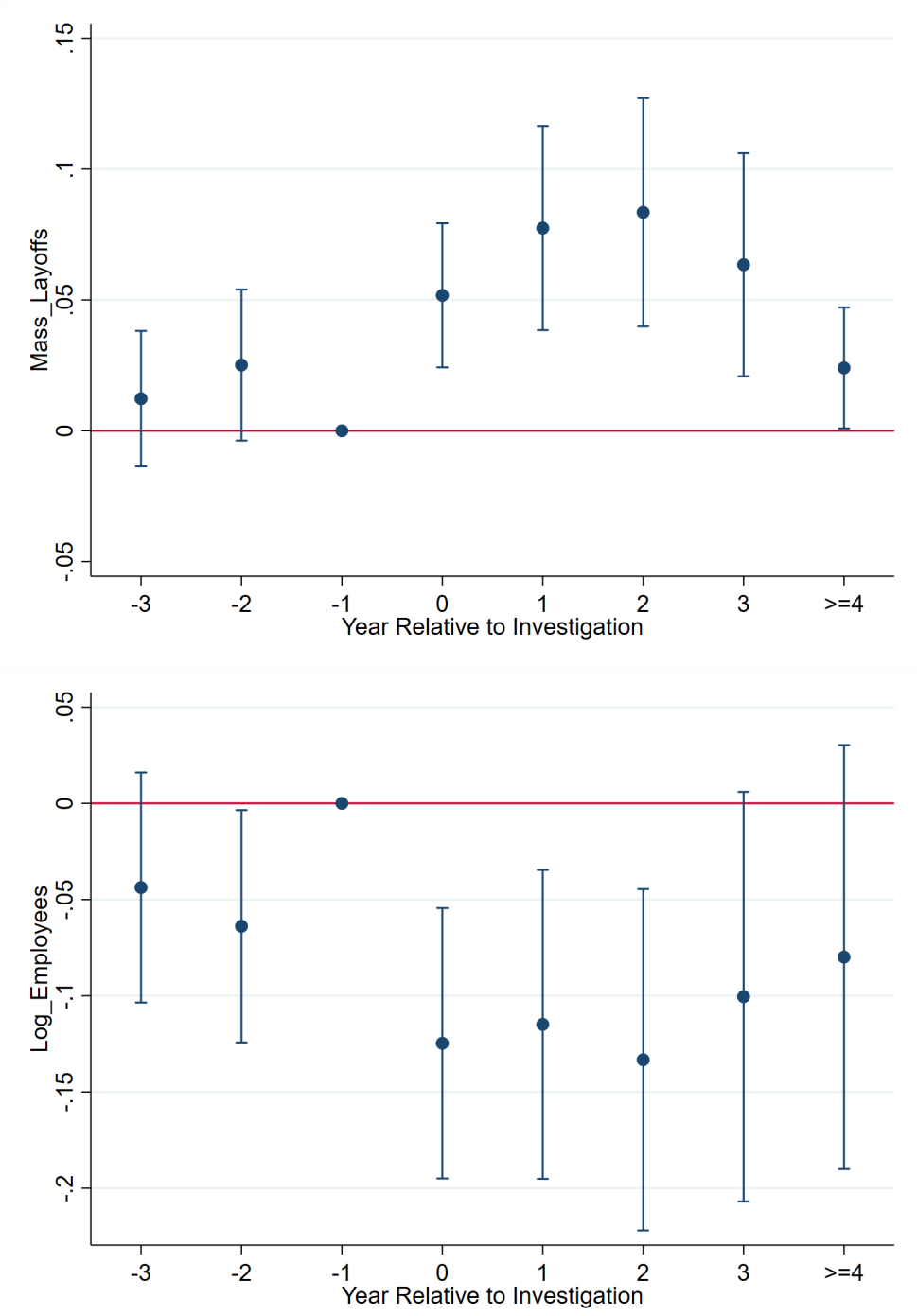

Figure A.4: The impact of cartel investigations on financing: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021) "interaction weighted" estimator, together with 95% confidence intervals. The top panel reports estimates for *Net Leverage*; and the bottom panel for *Shareholder Payout*. We control for firm and country × year fixed effects as well as firm-level controls.

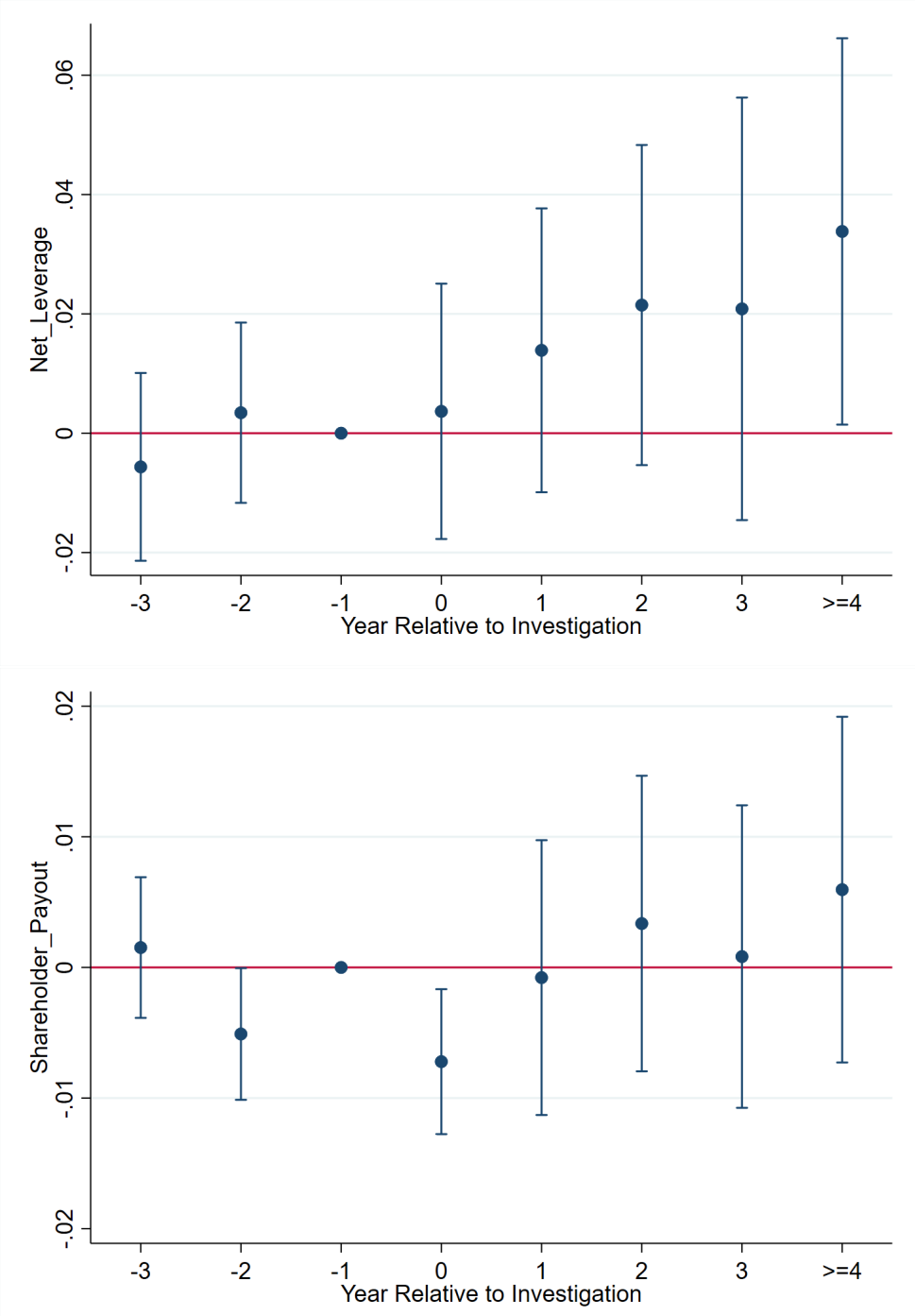

Figure A.5: The impact of cartel investigations on working capital: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021) "interaction weighted" estimator, together with 95% confidence intervals. The top panel reports estimates for *AP Days*; the middle panel for *AR Days*; and the bottom panel for *Inventory Days*. We control for firm and country × year fixed effects as well as firm-level controls.