The Effect of Corporate Mergers and Acquisitions on Government Subsidies

by

Xuanyu Bai

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Dissertation Committee:

Youchang Wu, Chair
Brandon Julio, Core Member
Diane Del Guercio, Core Member
Van Kolpin, Institutional Representative

University of Oregon

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DISSERTATION ABSTRACT

Xuanyu Bai

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Title: The Effect of Corporate Mergers and Acquisitions on Government Subsidies

What is the effect of corporate mergers and acquisitions on firms' subsidies? I examine this question using U.S. firm-level subsidy data with a stacked difference-in-differences design and find that non-PE-backed merged firms receive 40% less subsidy dollars after the merger, while private equity buyouts have no significant effect on firm subsidies. Declines are stronger in cross-border deals, in deals that lead to improved financial performance in merged firms, and when the buyer of the deal has no political connection prior to the deal, while they are weaker in firms that increase investing activity after merging and mergers between larger firms. The results suggest that improved financials, weakened political connections, and decreased capital investments drive post-deal subsidy reductions.

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CHAPTER I

INTRODUCTION

What happens to firm subsidies after corporate mergers and private equity buyouts? The use of taxpayer money has long been a matter of public scrutiny. In recent years, worry has intensified over subsidies flowing to corporations controlled by wealthy individuals, often at the expense of funding for public services.[1] Criticism towards public money going to corporations has gained even greater traction over the pandemic era of 2020 with the increasing amount of governmental aid. Private equity firms, in particular, were criticized for using public subsidies and the pandemic-driven market sell-off to expand their buyout activity.[2]

Public skepticism towards subsidy allocation and the use of subsidies is not unfounded. Extensive research finds that instead of subsidies being allocated on a by-need or by-merit basis, political connectedness and political power often leads to more allocation of subsidies and government resources. Both Slattery (2024) and Aobdia et al. (2024) find that political incentives rather than economic efficiency frequently drive allocation, with politically connected firms receiving larger awards, while delivering weaker job creation outcomes.

Corporate mergers and PE buyouts have been a focal point in this debate, because of the potential that they reinforce the link between market power and political power. Many studies have found a positive feedback loop between consolidation of market power and political power: As firms acquire more market power, they also tend to amass political power, and vice versa (Cowgill et al. (2024); Callander et al. (2022); Moshary and Slattery (2024)). This feedback loop could

[1] For example, according to Good Jobs First (2023), Democratic State Sen. Rochelle Nguyen was quoted exposing that taxpayer money has gone to funding stadiums for billionaire-owned sports teams, at the cost of cutting summer school funding as well as mental health funding for children, among other community funding.

[2] According to Americans for Financial Reform (2021), private equity has been on a buying spree during the pandemic sell-off, and private equity owned firms has been estimated to have claimed over 5.3 billion of CARES Act loans and grants.

exacerbate subsidy misallocation: firms with more market power gain more political power, and consequently, more public funding.

The relationship change between corporations and government from mergers and acquisitions and its subsequent effect on subsidy allocation is currently under-researched in existing M&A literature. This paper aims to examine the effect of corporate mergers and PE buyouts on corporate subsidies, and provide more insight into the determinants of corporate subsidy allocation.

I find that on average, the non-PE-backed merged firm receives around 40% less subsidy money post-merger, while private equity buyouts show no significant effect on subsidies. The probability of receiving subsidies also decrease significantly after mergers. Asset-scaled subsidies, revenue-scaled subsidies, and employment-scaled subsidies all show significant decrease after mergers: the ratio of subsidies to assets and revenues both decrease by 0.1 percentage points[3], and the amount received decreases by 154 dollars per employee of the firm.

Across subsidy program categories, job training and job retention subsidies exhibit the largest post-merger declines, followed by tax-related subsidies, subsidized grants, and crisis relief subsidies. While the overall dollar amount of subsidies are not significantly affected by private equity buyouts, tax abatement subsidies decline significantly by a large magnitude after PE-backed buyouts. Both state-issued subsidies and federal-issued subsidies decline after mergers, while PE-backed buyouts have a significant negative effect on state-issued subsidies, and no significant effect on federal subsidies.

Four main mechanisms emerge. First, financial improvement to the merged firm reduces the need for government aid. While the overall financial performance metrics of the merged firm do not significantly change after merging, I find that the decrease in subsidies post merger is concentrated in the subset of mergers that lead to improvement in financial metrics such as ROA and ROE.

Second, decreased investment in R&D and capital expenditure makes the merged firm less attractive to subsidy-granting agencies. While the average merged firm significantly decreases in-

[3]The average of subsidy/asset ratio is 0.8%, a 0.1% decrease translates to 12.5% deviation from the mean. The average of subsidy/revenue of the sample is 0.9%, with a 0.1% decrease translating to a 11.1% move from the mean.

vestment into R&D and capital expenditure after the deal, for firms that increase capital expenditure after the deal, the decline in subsidies is much weaker.

Third, the increase of market power after mergers tend to offset the drop in subsidy allocation to merged firms. While subsidy allocation to mergers within the same industry does not significantly differ from the general merged firm, mergers between larger buyers and targets tend to result in a weaker decrease in subsidies post-merger.

Finally, political connections of the merged firm, especially from the buyer side, have a significant effect on subsidies. I find that political connections on average weaken post-merger: political contributions of the merged firm drop by about 34% in likelihood and 37% in dollar amount. Subsidy decline is concentrated in mergers with a buyer that has no prior political contributions before merging. For merged firms that have buyers with prior political connection, the subsidies received may even see an increase.

Additionally, I find that the decline in subsidies after mergers is slightly more pronounced in cross-border transactions. In cross-border deals, both the logarithmic dollar amount of subsidies and the probability of receiving any subsidies are significantly lower than deals with a U.S.-based buyer, suggesting that foreign ownership potentially weakens subsidy access. This further proves that maintenance of political connection after mergers and acquisitions has a strong effect on firms' subsidies.

This paper is closely related to literature on the misallocation of subsidies due to political connections. Current studies such as Slattery (2024) and Aobdia et al. (2024), have established the link between political connections and firms' probability of getting subsidies. They also highlight the misallocation of subsidies due to politicians' incentives to gain political favor and votes through subsidy-giving. Faccio and Hsu (2017) study employment in private equity firms and find similar evidence of political gain being the first order consideration for politicians in decision-making: the authors find that facilities operated by politically connected private equity owners tend to increase employment during election years. This effect is stronger in states with higher levels of corruption, showing that there potentially exists an exchange of favors between

politically connected private equity and politicians. Similarly, Raghunandan (2024) also discovers a connection between misconduct, political power, and subsidies: The author finds that firms tend to have higher misconduct penalty in the state that they receive subsidies, possibly caused by reduced state-level oversight in the subsidy-granting state.

This study also contributes to research in subsidies to corporations. To the best of my knowledge, there are currently no studies on the effect of mergers on firm subsidies. Recent research in corporate subsidies finds that subsidy-receiving firms tend to enjoy a positive effect on financials, as well as increased research spending. This further establishes that firms have financial incentive to capture subsidies. For example, Pérez Amuedo et al. (2024a) study the effect of subsidies on firm operations, and find that recipients of cost-saving subsidies[4] tend to have improved profits, while receiving subsidies that encourage adding new streams of revenue such as grants, will encourage firm R&D spending. The authors also find that the positive effect on profits and investments are solely driven by local and state level subsidies, while federal level subsidies has no effect on firm behavior. In another paper by Pérez Amuedo et al. (2024b), the authors explore the effect of subsidy receiving on firm CEO pay structure, and find that CEOs of subsidy receiving firms tend to shift from cash-intense pay packages to options and equity compensation. The authors conjecture that this is likely because subsidies encourage the firms to take risks.

This paper is also closely related to studies on the effect of mergers and buyouts on firm financials. While I do not directly observe significant improvement in financials in the merged firm sample, unlike Erel et al. (2015) and El Ghoul et al. (2025), I do find that the effect of reduced subsidies received is stronger in firms that improve financial performance after merging.

This study adds to literature on the effect of private equity buyouts– an area that has been debated due to the growing market size of private equity and the mixed findings on the effect of PE. Studies including Fracassi et al. (2022), Bernstein and Sheen (2016) , Agrawal and Tambe (2016), Davis et al. (2014), and Cohn et al. (2021) have all found that PE buyouts yield beneficial results

[4]The authors define “cost-reducing subsidies" as subsidies that aim to cover expenses, such as job training subsidies and tax abatement subsidies. While subsidies that aim to open up new avenues of income, such as subsidized loans and subsidized grants, are defined as revenue-generating subsidies.

for target firms. Both Agrawal and Tambe (2016) and Cohn et al. (2021) find that PE improves employee skills and employability after takeover, while Davis et al. (2014) and Fracassi et al. (2022) find that firms are more competitive or improve in operations and financials. Bernstein and Sheen (2016) find a decrease in health violations after a PE buyout. However, many studies also discover a detrimental effect from PE takeover. Eaton et al. (2020), Gornall et al. (2025), Sheen et al. (2022), and Gupta et al. (2024), Zhong (2024) have all found that PE does significant damage to the target firm. Eaton et al. (2020) found that hospitals have worse patient care and survival rates after PE takeover. Both Gornall et al. (2025) and Zhong (2024) document that PE takeover makes employees worse off, with the studies revealing job quality declines and declines in contribution to employee pension plans, respectively. Sheen et al. (2022) find that PE leads to increased misconduct in financial advisers. Eaton et al. (2020) find that PE buyouts in higher education leads to decline in education quality.

CHAPTER II

HYPOTHESIS DEVELOPMENT

II.1 Competing predictions on post-deal subsidy allocation

Whether subsidy allocation increases or decreases after mergers and buyouts is an empirical question. On one hand, mergers and buyouts may improve firms' financial positions and operating efficiency, reducing their need for government aid. If post-deal firms become less financially constrained or more operationally efficient, they may rely less on subsidies after the transaction. This leads to the following prediction:

H1a: Firms' subsidy amounts decrease after mergers and buyouts.

On the other hand, mergers and buyouts may also increase firms' ability to obtain subsidies. Post-deal firms may become larger, more politically connected, or more important to local economies, which can increase governments' incentives to allocate subsidies to them. This leads to the competing prediction:

H1b: Firms' subsidy amounts increase after mergers and buyouts.

II.2 Changes in firm requirement for aid

For firms that achieve more substantial financial and operation improvements, the need for aid would likely further decrease.

A substantial body of research documents changes in firms' financial positions after mergers and buyouts. This change is a potential channel that could alter firms' need for subsidies, as improvement in financial performance could lead to firms requiring less government aid, whereas deterioration in finances could result in the opposite.

The majority of studies on mergers and buyouts document an improvement rather than deterioration in financial performance, and this result is present across different time periods, as

well as different types of mergers. According to Erel et al. (2015), acquisitions alleviate financial constraints in the target firm, and that the effects are stronger in smaller firms and firms with weak financials pre-acquisition. Similar effects have been found in private firm buyouts and large public firms: El Ghoul et al. (2025) study a sample of private firms and find that target firms' financial constraints after buyout are improved through better access to both internal financing and external financing. In an earlier study by Healy et al. (1992), the authors studied the 50 largest US firms and found that merged firms have higher asset productivity and operating cash flow return compared with the industry baseline. The effect of financial improvement in targets is also present when the acquisition stake is small: Liao (2014) studies minority stake acquisitions, and finds that targets are often financially constrained before acquisition, and targets increase investment expenditure after acquisition, additionally, 27% of targets issue new equity and increase market cap within two years of acquisition.

Based on this literature, mergers and private equity buyouts may improve operating efficiency and financial performance. If these improvements reduce firms' reliance on government aid, the decline in subsidies should be stronger among firms that experience larger post-deal improvements. Therefore, I propose the following hypothesis:

H2: Firms' subsidies decrease more after mergers and buyouts when deals improve financial performance.

II.3 Changes to government's incentive to allocate

While firms' need for government aid could change after mergers and buyouts, the willingness to allocate subsidies of government agencies could also change due to the new firms' political and market behavior, such as shifts in firm political connections, market power, and the firms' effect on the economy.

One channel that could lead to a change in subsidy allocation is a shift in firm political and market power after mergers and buyouts: Many studies have discovered a potential feedback loop between firms' consolidation of market power and accumulation of political power, while

political power is confirmed by many scholars to be conducive to higher subsidies, among many other benefits to the connected firm.

Studies have established that politically connected firms have greater probabilities of receiving subsidies and government contracts, while also receiving larger amounts of subsidies. Slattery (2024) finds that politicians have the incentive to use subsidy-giving as a saliency tool to gain votes in re-elections, this phenomenon is further confirmed by Aobdia et al. (2024), whose study reveals that subsidies are not effectively allocated. The authors find that, instead of allocating subsidies based on the potential for job creation, state level economic incentive awards are granted disproportionally to politically connected firms, with firms that are connected to state politicians more likely to receive an award and also receive larger award amounts, but generate less future job growth compared with otherwise unconnected recipient firms. Dong et al. (2024) study a sample of US publicly traded firms receiving subsidies that disclose job creation goal achievement information. They find that while about 63% of recipients meet job creation targets, firms that are politically connected and receive subsides during election years perform poorly. Brogaard et al. (2020) study firms' likelihood to receive governmental contracts in close elections, and finds that firms that contribute to the winning politician are more likely to receive contracts, and also receive larger awards with more lenient completion dates.

Aside from financial benefits from political connection, firms that amass political power also face lax regulation enforcements and more favorable antitrust ruling and takeover premiums in mergers compared with their otherwise unconnected peers. Heitz et al. (2023) studies enforcement and penalties of the Clean Air Act for politically connected firms, and finds that firms with political connections face less regulatory enforcement and lower penalties. In an earlier study, Yu and Yu (2011) find that firms that engage in lobbying activities tend to evade fraud detection for longer times and be less likely to be detected by regulators. Mehta et al. (2020) uncover a causal relation between merger party political connection and the favorability in antitrust review outcomes in mergers. Similarly, Croci et al. (2017) examine the effect of political contribution and lobbying of target firms in mergers, and finds that firms with political activity are less likely to be targeted in

mergers, and if they are targeted, the takeover is lengthier, and the target firm demands a higher takeover premium if the buyer has less political expertise. While the buyer, if politically connected, is less likely to overpay for the target. Ferris et al. (2016) examine political connections from the buyer side of mergers, and finds that buyers that have political connections are more likely to succeed in acquisitions and bypass regulation, as well as make more bids on larger target firms.

The aforementioned misallocation of subsidies and potentially harmful effects of political connectedness could be further exacerbated by acquisitions, as many studies discovered a feedback loop between consolidation of market power and political power: A model developed by Cowgill et al. (2024) shows that, consolidation of market power also leads to more political power. The study finds that the merging of public firms are followed by increased lobbying activity and political contribution, especially in mergers between larger firms and within-industry mergers. Similarly, Callander et al. (2022) also discover a feedback loop between market and political power, and warned that it could harm competition. Moshary and Slattery (2024) study mergers within the auto-industry to explore the effect of mergers on political activity and outcomes of lobbying, they find that mergers within the same political market and mergers in states where political and product markets are aligned result in increased lobbying. More notably, they find that after mergers, bills more favorable to the auto industry has increased enactment rates. Similarly, the linkage between acquisition and increased political activity is observed in private equity target firms. Hollenbach and Szakonyi (2022) studies a sample of US PE targets and find that companies purchased by private equity increase politically lobbying efforts, and are five times more likely to lobby in the same areas as their PE acquirers.

Based on this literature, firms that gain market power after mergers and buyouts may have greater bargaining power with governments and may be more likely to receive subsidies. Therefore, I propose the following hypothesis:

H3: Subsidy increases are stronger for firms that increase market size after mergers and buyouts.

Similarly, if political connections affect subsidy allocation, then firms that lose political

connections after mergers and buyouts should experience weaker access to subsidies. Therefore, I propose the following hypothesis:

H4: Subsidy decreases are stronger for firms that weaken political connections after mergers and buyouts.

Politicians are motivated to use subsidies to gain votes according to the findings of Slattery (2024). This suggests that there is potentially geographic favoritism in subsidy-giving, since politicians are motivated to use subsidies as a tool to gain popularity among voters. Therefore, buyer firms that have headquarters located outside the US are less likely to access government subsidies after merging. For this reason, I propose the following hypothesis:

H5: Cross-border mergers have a stronger decline in subsidies after the deal.

Furthermore, politicians often have the goal of job creation or encouraging innovation, which suggests that firms that increase hiring or pursue more R&D could potentially attract subsidies:

H6: Firms that increase hiring or increase investments receive more subsidies after the deal.

CHAPTER III

DATA

III.1 Sample construction

This section covers the data used in this study, all tables and figures of summary statistics and results are collected in Appendix A. Figures are presented first, followed by tables.

III.1.1 Subsidies data

Records of corporate subsidy transactions are obtained from the Subsidy Tracker database, provided by Good Jobs First, a nonprofit policy resource center that compiles information on public subsidies to private businesses, corporate misconduct, and tax abatement. Subsidy Tracker covers all subsidies to corporations that are disclosed by the government or media, or available through FOIA (Freedom of Information Act) requests. The Subsidy Tracker data is collected at the federal, state, and local level, with many of the state and local level subsidies hand collected from official government sites and information gathered directly from governmental agencies.

The raw data obtained from Subsidy Tracker is at the transaction level, meaning that there could be multiple transactions under different subsidy programs to the same company in a year, and that companies that receive no subsidies will not appear in the database. To clean up the data structure, I aggregated transaction-level subsidy records to the firm–year level, and then create a panel starting from the firm's founding date (or first year of financial reporting in CIQ) and ending on the date of firm liquidation.

To ensure subsidy receiving companies in my sample are for-profit firms that are in operation, I first match subsidy recipient firms to S&P Capital IQ (CIQ) entities using CIQ's company name-matching excel tool, followed by manual verification to remove incorrect matches, nonprofit entities, and religious organizations. After identifying 43,164 unique firms from the aforementioned step, I

then further restrict the observation sample to firm-years with revenue or net income data available in the CIQ database, which leaves me with 9,077 unique firms.

The sample period selected for this study is from 2004-2022. While transactions recorded in Subsidy Tracker start as early as 1990, the coverage on subsidies to corporations improves substantially from 2000 onward. Furthermore, following S&P CIQ's 2004 acquisition by McGraw-Hill Financial, the database's coverage of bankruptcy and liquidation records of firms also improves. I therefore set the data start date as 2004 for this study, as bankruptcy and liquidation event dates are essential in identifying operational firms and deciding the cutoff year for observations if a firm liquidates.

III.1.2 Buyout and mergers data

Mergers and acquisitions are sourced from S&P Capital IQ using the screener tool following studies such as Boyarchenko and Elias (2023), Siegel and Wang (2013), and Kline and Brown (2019), while private equity leveraged buyouts (LBOs) are obtained from PitchBook and supplemented with CIQ deal records to improve coverage. I match PitchBook targets to Subsidy Tracker–CIQ matched firms using a fuzzy-matching algorithm, followed by manual verification through online searches. For mergers and acquisitions, CIQ firm IDs are used to link target and buyer firms to M&A events.

To capture the absorbed targets' potentially receiving subsidies through the buyer, I combine the buyer and target firm by summing the observed subsidy amounts and financial data of both firms, thus creating a buyer-target combined entity. This approach captures: (i) subsidies flowing to the target indirectly via the buyer post-acquisition and (ii) the overall change in combined subsidy-receiving capacity. To construct merged firms, I further require that the mergers and PE buyouts have a clearly identifiable target firm and buyer firm, and that the target firm is fully merged into the buyer and no longer existing as a separate firm after the deal. Because of the nature of PE buyouts, most LBO deals result in a change of ownership that leaves the target firm remaining as a separate entity owned by various buyers after the LBO. For this reason, I only include platform deals with a clearly identifiable platform firm, which are deals that involve acquiring a company to merge into

an existing portfolio firm.

To ensure that the two firms combined are comparable, I require the target's size (three years pre-deal) to be within 0.2-5 times of the buyer firms' size. This ensures that the merger or buyout poses a material effect on the merging firms' operations, instead of being an inconsequential purchase among many for the buyer firm. [5]

After the aforementioned parsing procedure, the combined sample consists of 315 unique combined firm pairs that went through a mergers and acquisition event, and 65 unique combined firm pairs that went through a private equity leveraged buyout event.

III.1.3 Political contribution data

I obtain political contribution data of firms from the DIME database by Bonica (2024)(Database on Ideology, Money in Politics, and Elections). I match firms recorded in the DIME database to CIQ firms, and aggregate contributions to the firm year level.

Political contribution is measured using dollar amount of annual contributions, log of dollar amount contributions plus one, and a dummy indicator for political contributions.

6,616 firms in the sample have made at least one political contribution throughout the sample period. 414 firms that went through mergers or buyouts have made at least one donation to politicians during our sample period. Out of these, 123 firms have contributed at least once after the merger or buyout event, while 409 firms have made no contributions after the event. 388 firms had no records of political contributions before being involved in the merger or buyout, and 377 out of those firms never made a political contribution before or after the event.

Like subsidies, political contributions also vary widely across firms. Most firms do not contribute consistently every year. Over the entire sample period, the average contribution amount per year is around 64,000 dollars. In firm-years that have political contributions, the average contributing amount is around 132,000 dollars per year, with the median contribution amount at

[5] Papers such as Gokkaya et al. (2021) set a limit of 1% of acquirer market cap or 1 million market value of purchase for their target sample. Due to limited sample size and a lack of acquired percentage data in my sample, I use 20% of buyer firm size as the cutoff for significant acquisitions in the main test, and 1% of acquirer size in the robustness tests.

around 3,000 dollars per year.

III.2 Variables and Summary Statistics

Table A.1 shows the firm-year panel of summary statistics of the entire sample, and Table A.2 and Table A.3 show the cross-section summary of targets and buyers one year before treatment.

III.2.1 Subsidy variables

The main variable used to measure subsidies is total dollar amount of subsidies received per year. To measure the amount of subsidies received each year, the transaction level subsidies data is aggregated to the firm year level. In addition to total dollar amount, the log of subsidy dollar amount plus one, the count of unique subsidy transactions received per year, and a dummy indicator that equals one for years when a firm receives subsidies are used to measure firm subsidies. Most firms in the sample do not consistently receive subsidy aid every year, and there is a large spread in the dollar amount and frequency of subsidies. The average firm throughout the sample period receives about 220 thousand dollars worth of subsidies, with a 33% chance of receiving subsidies per year. In firm years when firms are granted a subsidy, the median amount received per year is around 200 thousand dollars, with the average annual amount at 1.1 million dollars.

Scaled subsidy variables are constructed to capture the proportion of subsidy change that results from firm financial performance and structure change after merging, I use the following scaled-subsidy measures: subsidy dollar amount scaled by total assets, subsidy dollar amount scaled by total revenues, and subsidy dollar amount scaled by number of employees. Firms generally do not receive a substantial subsidy amount compared with their asset or revenue size, with the average subsidy to asset ratio at 0.8% throughout the sample period, and average subsidy to revenue ratio sits at 0.9%, and the average firm receives about 340 dollars per employee.

A series of sub-category subsidy variables grouped by issuance agency and subsidy programs are used to track the changes within different types of subsidies. Federal subsidy and state subsidy amounts are studied to measure potential heterogeneity in subsidy receiving behavior de-

pending on the subsidy issuer. On average, federal subsidies have much larger dollar amounts compared with state subsidies, with the average federal subsidy received at 87 thousand dollars per year throughout the sample period, and state subsidies per year at roughly half that amount.

In addition, I also identify job training subsidies, tax abatement subsidies, subsidized loans, and grants through the program names in the subsidies transaction data. Among the sub-categories of subsidy programs, subsidized loans and tax abatement subsidies are the largest in size, with the average firm receiving around 61 thousand dollars of subsidized loans, and 32 thousand dollars of tax abatement subsidies each year. Subsidized grants and job training subsidies on the other hand, are relatively small in size. The average firm receives only around 500 dollars in job training subsidies and 6,000 dollars in grants each year.

III.2.2 Financial variables

Financials are obtained from S&P CIQ. I use a series of financial ratios to measure the financial composition and performance change of companies before and after merging. Leverage, debt to equity ratio, equity to assets ratio, and cash to assets ratios are used as measures for composition change of the firm. And ROA, ROE, turnover, cashflow growth, sales growth, and operating income to assets ratios are used as measures for financial performance.

The average firm in the sample has a ROA at 2.3% and ROE of 9.8%, both with high dispersions over the sample. When it comes to buyer firms and target firms in merger and buyout deals, the buyer firm is clearly in better financial shape than the target before the deal: The average target firm in merger and buyout deals have negative ROA and ROE one year before deal, while buyer firms have ROA and ROE of 1% and 4.5%, respectively.

In the mechanisms tests, I use a dummy indicator for financial performance improvement after deal to capture the effect of financial improvement on subsidy receiving. For mergers that have higher ROA, ROE, and cash flow growth after merging compared with the three-year average before merging, the dummy indicator for financial improvement equals one. Additionally, I use a series of employment and investment variables to measure changes to firm alignment with government

goals, as increases in investments and hiring could potentially attract subsidies. The measures include revenue per employee, employment growth, capital expenditure ratio, and R&D expense ratio.

CHAPTER IV

EMPIRICAL DESIGN

IV.1 Main analysis

In the main analysis, I aggregate each target firm and its acquirer into a single combined entity. This approach allows continued observation of targets that are absorbed into the buyer or platform firm post-transaction and captures potential spillovers in subsidy access from target to buyer after the change in ownership.

A stacked difference in difference design following Cengiz et al. (2019) is used for all tests in this paper aside from the robustness tests using TWFE and local projection DiD. This method mitigates potential biases of staggered treatment with TWFE as found in Goodman-Bacon (2021). The treatment group of combined target and buyer firms are split into cohorts based on event year, and the control group are never-treated firms that have never been targeted in PE-backed buyouts or non-PE merger acquisitions. The baseline specification used to estimate the effect of mergers and buyouts on subsidies is as follows:

$$y_{i,c,t} = \beta Treated_{i,c} \times Post_{t,c} + \gamma X_{i,t,c} + \lambda_{i,c} + \sigma_{t,j,c} + \epsilon_{i,c,t} \tag{1}$$

Where $y_{i,j,c,t}$ is the outcome variable for firm i belonging to cohort c in year t; $X_{i,t}$ is firm level controls; $\lambda_{i,c}$ is firm-cohort fixed effects that captures firm fixed effects with each cohort; $\sigma_{t,j,c}$ is year-industry-cohort fixed effects that captures year times industry fixed effect within each cohort. $Post_{t,c}$ is a dummy variable that equals one in the post-deal years, and $Treated_{i,c}$ is a variable that indicates treatment and equals one for combined firm i of cohort c that has gone through buyout or merger. Both $Treated_{i,c}$ and $Post_{t,c}$ are subsumed in the estimation from controlling for firm-cohort fixed effects and year-industry-cohort fixed effects.

If the effect of the buyer firm taking advantage of target firms' access to subsidies is stronger in the overall effect, the coefficient β should be positive and significant. If the effect of financial improvement from mergers and buyouts is stronger and the outcome variable declines after the event, the coefficient β should be negative and significant.

To capture potentially differing effects between mergers and buyouts, I split the treatment indicators based on the type of deal, and create separate indicators for mergers and buyouts:

$$y_{i,c,t} = \beta_1 Treat_{i,c_{MA}} \times Post_{t,c} + \beta_2 Treat_{,c_{LBO}} \times Post_{t,c} + \gamma X_{i,t,c} + \lambda_{i,c} + \sigma_{t,j,c} + \epsilon_{i,c,t} \quad (2)$$

Where $Treat_{i,c_{MA}}$ is a dummy that equals one for firm i in cohort c that went through a merger acquisition; $Treat_{i,c_{LBO}}$ is a dummy that equals one for firm i in cohort c that went through a private equity leveraged buyout; $Post_{i,c_{MA}}$ and $Post_{i,c_{LBO}}$ are dummy variables that equal one in the post-deal years for mergers and LBOs, respectively. The following F-test is used in addition to the estimation to test the potential differing effects between the two types of deals:

$$F - test : \beta_1 - \beta_2 = 0 \quad (3)$$

IV.2 Regression model choices

Cohn et al. (2022) show that using a $Log(N + 1)$ specification for count or count-like variables is not optimal in estimation and has no economic meaning for interpretation. Instead, the authors suggested using a Poisson model with fixed effects, which will give consistent estimates and is equivalent to using a log-linear model.

The main dependent variables $Subsidy(\$)$ fits the definition of a count-like variable due to the large number of zeros in years when the firm in question does not receive any subsidies. While I use a Poisson model with fixed effects in the main analysis on $Subsidy(\$)$ and count variables, a $Log(N + 1)$ of subsidy dollar amount and a winsorized subsidy dollar amount is estimated on a

linear model with fixed effects alongside the Poisson analysis for comparison.

CHAPTER V

EFFECT ON SUBSIDIES AFTER MERGERS AND BUYOUTS: BASELINE RESULTS

This section presents the baseline estimates of the effect of mergers and buyouts on combined buyer–target firms' subsidy receipts, using the stacked DiD method specified in equations (1) and (2).

V.1 Baseline results

Table A.4 reports the stacked DiD results for the combined firm sample. Panel A estimates the average post-deal effect without distinguishing between deal types, while Panel B estimates separate effects for mergers and private equity buyouts and tests for differences between the two deal types using an F-test.

The dependent variables are a logarithmic of subsidy dollar amount plus one in column (1), winsorized dollar amount of subsidies in columns (2) and (3), Count of number of subsidy transactions in column (4), and a dummy indicator that equals one if a firm receives any subsidies in a given year in column (5). A linear regression model with fixed effects is used in columns (1), (2), and (5), while columns (3) and (4) use a Poisson regression model with fixed effects.

Results in Panel A show that on average, after mergers and buyouts, the buyer and target combined have both a lower probability of receiving subsidies and receive lower dollar amount of subsidies. On average, the probability of receiving a subsidy reduces by 9.8%, and the number of transactions per year reduces by 50%.[6] Subsidy dollar amount received on average sees in total a 40.3% drop after mergers and buyouts. [7]

[6] A note on percentage calculations for coefficients in log-linear and Poisson analysis: The estimates are reported as is in all tables, while real percentage changes are calculated using the following equation: Percentage change= $e^{\beta} - 1$.

[7] A note on estimations with subsidy dollar amount as dependent variable: The annual subsidy dollar amount

Results in Panel B show that the decrease in subsidies are mainly driven by non-PE merged firms, with subsidy dollars received by merged firms declining 41.4% post-deal. PE platform deals appear to have no significant effect on the combined firms' dollar amount of subsidies, or the probability of receiving subsidies, while the number of subsidies claimed per year decreases by a slightly lower magnitude compared with non-PE merged firms. The F-tests for difference between the merger effect and PE platform deal effect shows a significant difference in probability of receiving subsidies after deal, but there appears to be no statistical difference in subsidy transaction count or subsidies dollar recipient amount between the two deal types.

Table A.5 shows the results for stacked DiD analysis on changes in subsidies scaled by firm assets, revenues, and employment after mergers and buyouts. Similar to Table A.4, I first estimate without distinguishing between deal types, then perform a test with separate indicators for mergers and buyouts. The dependent variables are subsidy dollar amount scaled by total assets in column (1) and (4), subsidy dollar amount scaled by total revenue in columns (2) and (5), logarithmic of total assets in columns (3) and (6). A linear regression model with fixed effects is used in all columns for this test. Results in columns (1)-(3) shows that relative to firm size and revenue, subsidies of target and buyer combined firms decline by 0.1% relative to firm assets and revenues, while per capita subsidies decline by 154 dollars per employee. Columns (4)-(6) shows that, similar to the main test, the effect of subsidy change is mainly driven by non-PE merged firms. While the asset-scaled subsidy change is insignificant in PE LBO deals, the coefficient estimate is negative and there also appears to be no statistical difference from the significant coefficient of merger deals.

V.2 Pre-trends and treatment effect dynamics

To test whether the parallel trend assumption holds for DiD analysis, I use the following dynamic estimation models with a dummy variable for each event-time in the 11-year window to interact

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received is a count-like variable, according to Cohn et al. (2022), a Poisson model should be the best fit for analysis of count or count-like variables. While we report results from models using linear regressions with fixed effects using log subsidies plus one and raw winsorized dollar amount as dependent variables, the estimate from the Poisson model is likely the most accurate estimate.

with the treatment indicator:

$$y_{i,c,t} = \Sigma_{\tau=-5}^{\tau=5} \beta_{1\tau} Treat_{i,c_{MA}} \times T_{\tau,c} + \gamma X_{i,t} + \lambda_{i,c} + \sigma_{t,j,c} + \epsilon_{i,c,t}, \tag{4}$$

$$y_{i,c,t} = \Sigma_{\tau=-5}^{\tau=5} \beta_{2\tau} Treat_{i,c_{LBO}} \times T_{\tau,c} + \gamma X_{i,t} + \lambda_{i,c} + \sigma_{t,j,c} + \epsilon_{i,c,t}, \tag{5}$$

where $T_{\tau,c}$ is a dummy variable that equals one for observations in the year τ relative to the deal year and zero for all others. The outcome variable $y_{i,c,t}$ is subsidy dollar amount, log of subsidy dollar amount plus one, count of subsidies, and a dummy indicator for receiving a subsidy in panels A, B, C, and D, respectively.

I use the year $\tau = -1$ as the baseline year for both types of deals, and plot the coefficients on the interaction terms $\beta_{1\tau}$ in Figure A.1, and the interaction terms $\beta_{2\tau}$ are plotted in Figure A.2.

All the panels in the Figure A.1 show that there is no apparent pre-trend before the merger event, while there is a strong decline in all subsidy measures after merger events. Figure A.2 shows that for PE-backed merger deals, while there is no apparent pre-trend, subsidies do not change significantly after the event.

CHAPTER VI

MECHANISMS

This section investigates potential channels through which mergers reduce firm subsidies. The literature suggests two main mechanisms: (i) improvements in financial capacity, which can reduce the need for subsidies, and (ii) changes in political connections, which can alter firms' access to public funds. Given that the baseline results show that only non-PE mergers have significant effects on merged firm subsidies, I only explore the mechanisms in non-PE mergers.

VI.1 Financial improvement channel

To explore whether financial changes are driving the decline in subsidies after mergers, I estimate a diff-in-diff model of the effect of merger events on merged-firm financials, followed by a triple-difference subsample analysis focusing on deals where the financial performances of the merged firm improve after the deal.

Table A.6 Panel A reports results on financial structure changes. After the deal, the merged firm increases the proportion of debt financing, as indicated by the significant increase in leverage and decrease in equity to asset ratio. The merged firm also holds significantly less cash relative to asset size, this could either be a result of cash-mergers or an indicator for improved financial risk leading to less reliance on cash reserves.

Table A.6 Panel B shows the effect of mergers on firm financial performance. As seen from the test, most performance indicators including ROA, ROE, cash flow to assets, cash flow growth, and sales growth are not significantly affected by merging. Column (1) shows that turnover sees a significant decrease, which potentially indicates a decrease in operating efficiency following the merger.

While financial performance do not exhibit significant changes after mergers, for the subset

of firms that do see a substantial financial improvement, the subsidy decrease is stronger and larger in magnitude. Table A.7 columns (1)-(3) reports triple-difference estimates for the subset of deals that leads to improvement in financial performance indicators. As shown in columns (1) and (2), merged firms with stronger ROA and ROE ratio sees a significantly greater decrease in subsidies received, Firms with improved cash flow growth, however, do not seem to receive a significantly different amount of subsidies compared with the general merged firm.

Overall, these results suggest that part of the post-merger subsidy decline is likely also driven by improvements in the merged firms' financial performance, which leads to less reliance on government aid on the firms' part.

VI.2 Government incentives channel

Government officials and politicians often have more incentive to give out subsidies based on employment growth and investments in research and new facilities. In this section, I look at whether firms' employment and investments change significantly after mergers, and if the growth in employment and investments are linked to receiving more subsidies.

Table A.8 shows the effect of mergers on firm employment growth, R&D ratios, and capital expenditure ratios. While employment growth does not change significantly after the merge, revenue per worker decreases significantly, indicating lower employee efficiency after mergers. Merged firms also significantly decrease investment activities as shown by the decrease in capital expenditure and R&D expense ratios.

Table A.9 reports triple-difference estimates for the subset of deals that leads to increased investing and hiring. As shown in column (1), while the average merged firm could lose up to 48% in subsidies, firms that increase employment growth after merging experience up to 50% increase in subsidies instead. Similarly, firms that increase R&D expenditure ratio also experience an increase in subsidies post-merger rather than a decrease. Increases in capital expenditure ratio, on the other hand, appears to have no significant effect on merged firm subsidies, as shown in column (2).

Overall, the results in this section show that decreasing investments in Capex or R&D, as well

as drops in employee efficiency, result in lower subsidy allocations, while increasing investments or employment growth could win the firm more subsidies. This is likely because the decrease in investments into R&D and new projects makes the firm less attractive to the subsidy-granting government agencies.

VI.3 Market power channel

Firms that increase in size and market power after merging are also more likely to attract subsidies, as increasing market power and importance in the industry could lead to the firm garnering both political power and attention from government officials. In this section, I explore the effect of larger mergers and within-industry mergers to determine whether market power also increase subsidy allocation.

Table A.10 shows the effect of above median buyer firms, above median target firms, and within-industry mergers. Compared with the average merged firm, when the merger involves either a larger target firm or buyer firm, the resulting merged firm experiences a smaller drop in subsidy allocation. This effect is stronger when the buyer in the deal is above median size, with a reduced subsidy decrease of 21% after merging compared with the 41% of the general sample. Mergers with larger target firms experience 30% subsidy allocation decrease. As shown in columns (5) and (6) of Table A.10, merging within the same industry does not appear to affect subsidy allocation significantly.

VI.4 Political connections channel

In past literature, political connections are well established as a determinant for subsidy access. To test whether changes in political engagement contribute to the subsidy decline, I first estimate the effect of deals on political contribution, then I perform subsample tests on the subset of mergers that have weaker political connections, as well as a subsample test on cross-border mergers.

VI.4.1 Political contributions change

I use political contributions made by firm and employee PACs to measure political connections of a firm. Table A.11 shows the results of the change in political contribution behavior after mergers. After mergers, both the dollar amount of political contributions and the probability of making political contributions decrease significantly. As shown in columns (1)-(3) and columns (4)-(6), the magnitude holds stable after controlling for firm size. The probability of making political contributions declines by around 34% after mergers, and the dollar amount contributed drops by around 37% after mergers.

I continue to test whether political connections drive the effect of subsidy change after mergers. Table A.12 Panel A shows the results of a triple diff-in-diff on the subset of merged firms that have never contributed to political campaigns. As shown in Panel A, for merged firms that never established political connections in the sample period, subsidies decrease by a larger magnitude compared with the baseline test: a close to 57% decrease in subsidy dollars. The decreasing magnitude on probability of receiving and number of transactions received is also substantially lower than the baseline.

Table A.12 Panel B shows the results of a triple diff-in-diff on the subset of merged firms that have made political contributions both before and after the deal. Panel B shows that firms that made political contributions both before the deal and after the deal have a much weaker decline in number of subsidies received and probability of receiving subsidies, while the dollar amount of subsidies also do not significantly differ from the average merged firm in the Poisson model.

In Table A.13, I further test whether pre-deal political connections from the buyer side or target side in mergers have more effect on subsidies.

In Panel A of Table A.13, I run a triple diff-in-diff on the subset of deals where the buyer firm has not made any political contributions before the deal. As shown in Panel A, the results of subsidy decrease after merger is solely driven by deals with buyer firms that come into the deal without any political connections. For deals that involve buyers with prior political contributions, there may even be an increase in subsidies received.

Panel B of A.13 shows a triple diff-in-diff on the subsample of merged firms with targets that never made political contributions before the deal. As shown in Panel B, target political connections appears to have no determining effect on subsidy receiving at all, with the subset of deals with non-contributing targets having no significant difference from the general merged firm.

VI.4.2 Cross-border acquisitions

I estimate a triple diff-in-diff for cross-border merger and acquisition deals to explore the effects of geographical heterogeneity on firm subsidies. The results on cross-border acquisitions are shown in Table A.14.

Overall, I find that the effect of decreased subsidies is slightly stronger for cross-border deals. The change in subsidy counts and Poisson estimates for subsidy amounts are not statistically different from the general M&A sample, but all coefficients remain negative, and both log-transformed subsidy amounts and probability of receiving subsidies are significantly lower for cross-border deals.

The slightly stronger decline in cross-border deals suggests that the buyer being foreign-owned in a deal likely reduces the accessibility to US subsidies, likely due to reduced local, federal, and state connections.

Taken together, the evidence indicates that diminished political engagement from the buyer side is a key driver of the subsidy decline after mergers. For buyer firms that fail to maintain connection with politicians by contributing to campaigns, and for firms that have never established political connections, subsidies received will decline by a greater magnitude, and the firm will be less likely to receive any subsidies. While for buyer firms that maintain political connections after deal, the merged firm will instead see a slight increase in subsidies received.

CHAPTER VII

HETEROGENEITY IN SUBSIDY TYPES

In this section, I classify subsidies into different sub-types based on issuing government agency and subsidy program names, and explore the responses to mergers between different categories of subsidies.

VII.1 Issuing agency levels: State and federal subsidies

Table A.15 shows the results using the dollar amount and count of state-issued and federal-issued subsidies as the dependent variable. The coefficients are estimated using equation **??** on a Poisson regression with fixed effects.

State subsidies decrease significantly for both mergers and PE buyouts both in dollar amount and number of subsidies. With the effect being stronger in PE deals: merged PE platform and targets receive 84.7% less subsidies in dollar amount per year, and 71.9% fewer in the number of subsidies. The decrease for non-PE merged firms is smaller but still substantial, at about 50% decrease for both dollar amount received and the number of subsidies received.

Federal subsidies only decrease for non-PE merged firms after merger, while reacting insignificantly to PE buyouts. For non-PE merged firms, subsidies received from the federal government decreases by 60% in dollar amount, and 80% in subsidy frequency.

VII.2 Subsidy program types

There are a variety of subsidy programs targeting different types of firms for different purposes. In this section, I group subsidies by keywords in their program names into the following categories: crisis relief subsidies, subsidized loans, subsidized grants, investment and expansion subsidies, job training and retention subsidies, and tax-related subsidies.

Table A.16 shows the results using the dollar amount of each sub-category as the dependent variable, where the coefficients are estimated using equation **??** on a Poisson regression with fixed effects.

For merging firms, subsidized grants and job training subsidies all drop by around half after merging, while crisis relief subsidies and tax-related subsidies both drop by around 40%. Subsidized loans on the other hand, appear to be unaffected by either mergers or private equity deals.

While the total subsidy dollar amount has insignificant change after PE LBO deals as shown in Table A.4, dollar amount of subsidies from tax-related subsidies decrease significantly after PE LBO deals. For PE LBO deals, grant subsidies increase by a large but insignificant magnitude after deals, which likely indicates increased R&D activity. This change is likely due to PE backed platform firms taking on add-on firms that expand the platform firms' business. All other subsidy types appear to have no significant changes from PE LBO deals.

Table A.17 shows that the post-M&A decline in subsidies varies across subsidy purposes and firm characteristics. The baseline effect is generally negative across all subsidy types, however, the interaction terms suggest that this decline is partially offset for firms with stronger political connections at the general and state-level, and larger buyer size: firms that continue to make political contributions after merging experience significantly weaker declines in crisis relief and jobs subsidies, while state-level contributions only result in slightly smaller declines in crisis relief and jobs subsidies. Mergers with larger buyers also experience weaker declines in crisis relief, investment-expansion, and property-tax subsidies. Financial and employment changes post-merger produce mixed patterns: Improvements in cash flow growth are associated with weaker declines in investment, grant, and property-tax subsidies, while employment growth improvements are associated with weaker declines in crisis relief and investment-expansion subsidies but stronger declines in property-tax subsidies.

CHAPTER VIII

ROBUSTNESS TESTS

In this section, I perform several robustness checks on the baseline results presented in main analysis, including matching between treated and control firms, alternative event-window and sample period cutoffs, alternative target to buyer asset size ratio cutoff for the treated sample, and alternative regression models. Table B.1 reports covariate balance for the CEM and Mahalanobis matched samples.

VIII.1 Matching

I use two different matching methods with stacking based on the pre-treatment subsidy amounts and trends, as well as firm size as the matching criteria. I construct matched samples using both coarsened exact matching (CEM) and nearest-neighbor Mahalanobis matching.

Table B.2 shows the results from using coarsened exact matching with stack DiD. In this test, treated firms are matched at event time t=-1 to firms in the same industry and baseline year, with exact matching imposed on coarsened bins of the three-year pre-treatment average subsidy amount, average subsidy count, subsidy trend (calculated as a slope between years t=-2 and t=-1), and firm size.

Table B.3 shows the results from using a nearest-neighbor Mahalanobis matching with stacked DiD. In this specification, treated firms at t=-1 are matched to the three nearest control firms within the same baseline comparison set using the Mahalanobis distance over the same set of pre-treatment covariates.

Matching is conducted separately for mergers and buyouts, after which the matched firm pairs are mapped back into the full panel to form stacked treated-control cohorts used in the main regressions. As shown in the results in Table B.2 and Table B.3, the directions of estimates remain

robust, suggesting that the results are not driven by a particular matching algorithm.

VIII.2 Alternative event window and sample cut-offs

In this section, I repeat the baseline analysis on a seven-year event window, relaxed target to buyer size ratio cutoff, and a sample period that excludes the 2020 COVID pandemic and onward.

Table B.4 shows the robustness results from using an alternative seven-year window stacked DiD (three years pre-event and three years post-event). The results of this robustness align with the main baseline results, and estimates similar magnitudes of subsidy decrease at 45% subsidy dollar amount decrease after non-PE mergers and acquisitions, while PE-backed buyouts have no effect on firm subsidies.

Table B.5 shows the robustness test results using an alternative cutoff for target firm to buyer firm size. In the main analysis, I use a 20% minimal target to buyer firm size as the cutoff point for significant acquisition. Here, I relax the cutoff and use 1% target to buyer size ratio as the definition for a significant acquisition. The results here are slightly weaker and the estimates have smaller magnitudes compared with the main analysis, with Panel B of Table B.5 showing a 28% decrease in subsidy dollar amount.

Table B.6 shows the robustness test results of truncating the years 2020-2022 from the sample. As shown in the table, the coefficient estimates remain similar to the baseline analysis, suggesting that the results are not driven by the increased governmental aid starting around the 2020 COVID pandemic.

VIII.3 Alternative models

In this section, I use TWFE and local projection DiD as an alternative to the stacked DiD model used in the main analysis of this paper.

Table B.7 shows the robustness test results using TWFE instead of a stacked DiD for model estimation. Again, the results are similar to the baseline, and give an estimated 40% decrease in subsidy dollar amount after non-PE merger and acquisitions.

Figure B.1 shows a dynamic plot using local projection DiD following Dube et al. (2023). As shown in the figures, similar to the parallel trend test in Figure A.1, there is no clear pre-trend and an apparent decrease in subsidy dollar, number, and probability of receiving after non-PE merger and acquisitions.

To conclude, as shown in Appendix B, the post-merger decline in subsidies remains statistically and economically significant across all specifications, with effect magnitudes comparable to the baseline.

CHAPTER IX

CONCLUSION

This paper examines how corporate mergers and PE-backed buyouts affect firms' subsidies. I document a decline in subsidies received by merged firms after non-PE-backed mergers, but not in private equity buyouts. State level subsidies and tax abatement subsidies are the types of subsidies that experience the greatest decline after the deal.

Four main channels emerge from the subsample tests: the financial performance channel, the political connections channel, market power channel, and the government incentives channel.

First, the effect of subsidy decline is stronger in the subset of firms that see a strong financial performance improvement after merging. When the merger results in improved financial performance metrics, subsidies claimed by the firm significantly decreases compared with the general merged firm, likely due to lesser reliance on government aid from stronger financial performance.

Second, political connections of the buyer firm in deals have a determining effect on subsides. Firms exhibit a strong decline in political contributing activity and amounts after the merger. In the subset of merged firms where the buyer has political connections prior to the deal, instead of seeing a drop in subsidies, the amount of subsidies given to the merged firm could even increase. The decline in subsidies received after merging appears to be solely driven by merged firms with buyers that have no political connections. Additionally, the effect of decline is slightly stronger in cross-border deals, which is likely a result of weaker political connection with U.S. politicians after shifting to foreign ownership.

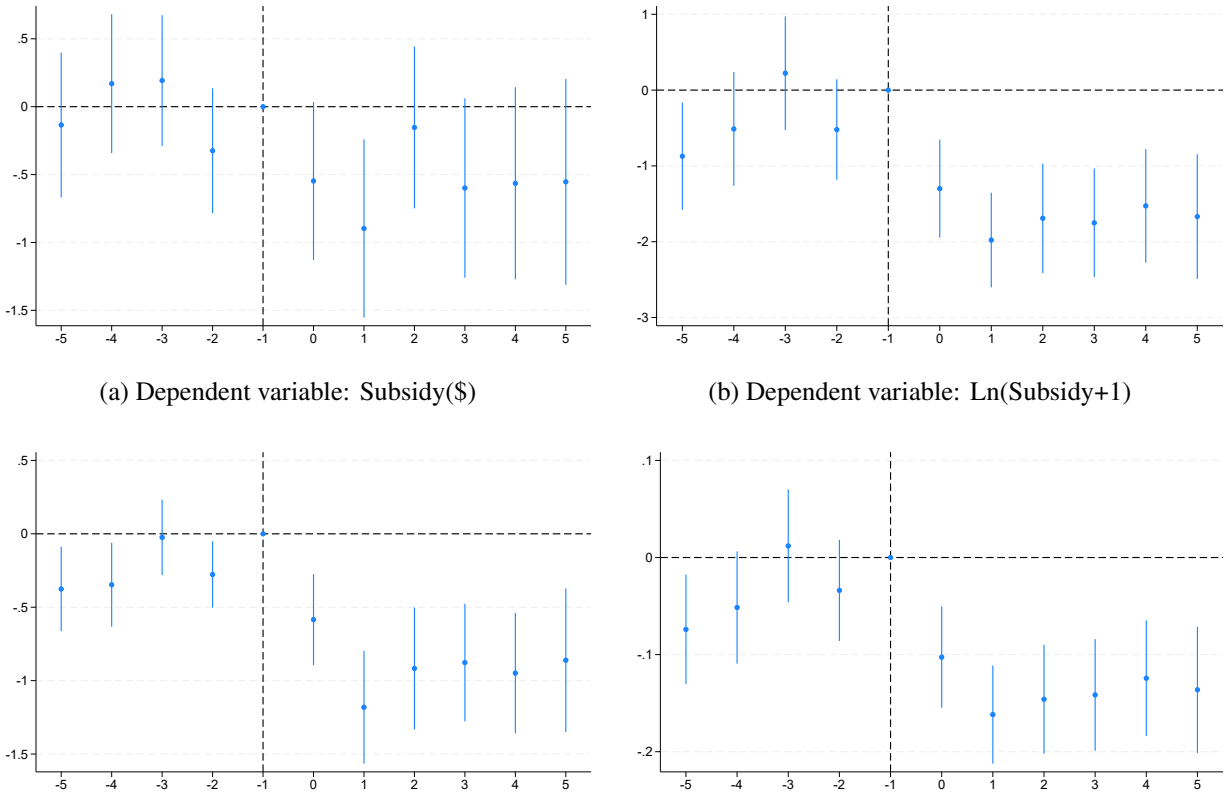
Furthermore, the increase in market power after mergers could potentially offset some of the decrease in subsidy allocation post-merger. While merging within the same industry has no significant effect on subsidies, mergers between firms of larger sizes tend to have a significantly

smaller decrease compared to the general merged firm, suggesting that market power is conducive to subsidy allocation.

Finally, the decline in subsidies is weaker in firms that increase capital expenditures. This is likely because politicians are often incentivized to distribute subsidies with the goal of employment growth, job creation, and creation of new projects and facilities. Firms in the sample on average show less investments in R&D rate and capital expenditure rates upon merging, with no change in employment growth rate, which is likely the reason for the drop in subsidies received after the merger. For the subset of firms that increase capital expenditures, the decline in subsidies after merging is much weaker.

APPENDIX A

TABLES AND FIGURES



(a) Dependent variable: Subsidy($)

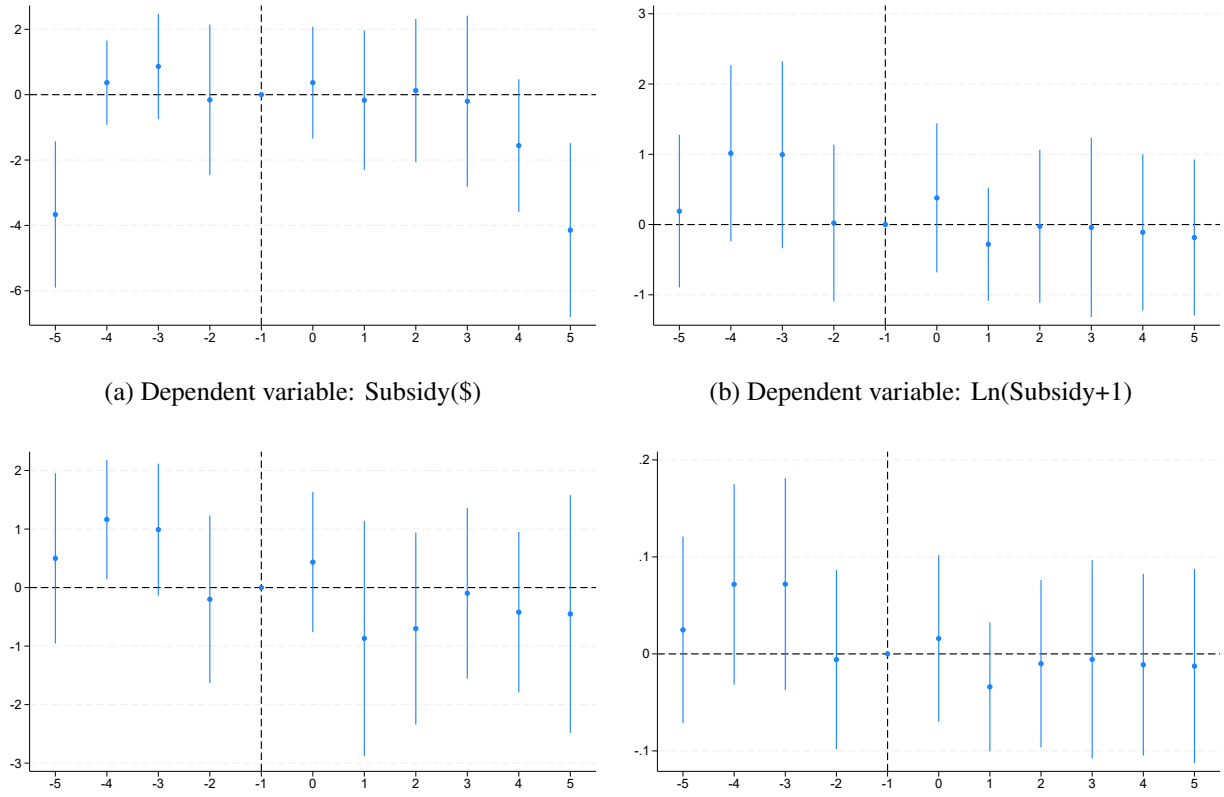
(b) Dependent variable: Ln(Subsidy+1)

(c) Dependent variable: Count

(d) Dependent variable: D_sub

Figure A.1: **Coefficient plot for stacked DiD regressions**

This figure shows the point estimates and 95% confidence intervals of the coefficients for merger deals, where the x-axis numbers represent the year relative to the buyout year (year 0). The dependent variables are winsorized dollar amount of subsidies, a logarithmic of subsidy dollar amount plus one, count of number of subsidy transactions, and a dummy indicator that equals one for non-zero subsidies, respectively, in panels A to D. Panels A and C are based on Poisson models, while panels B and D are based on linear models.



(a) Dependent variable: Subsidy($)

(b) Dependent variable: Ln(Subsidy+1)

(c) Dependent variable: Count

(d) Dependent variable: D_sub

Figure A.2: **Coefficient plot for stacked DiD regressions**

This figure shows the point estimates and 95% confidence intervals of the coefficients for PE LBO deals, where the x-axis numbers represent the year relative to the buyout year (year 0). The dependent variables are winsorized dollar amount of subsidies, a logarithmic of subsidy dollar amount plus one, Count of number of subsidy transactions, and a dummy indicator that equals one for non-zero subsidies, respectively, in panels A to D. Panels A and C are based on Poisson models, while panels B and D are based on linear models.

Table A.1: **Firm-year panel summary statistics**

This table shows the summary statistics of the full sample of the firm-year panel of subsidy-receiving target firms and their corresponding buyer firms.

	count	mean	p50	sd	min	max
ln(Subsidy+1)	58,144	2.379	0.000	4.925	0.000	15.384
Subsidy($)	58,144	212,647.024	0.000	850,932.671	0.000	4,799,201.000
Subsidy/Asset	57,716	0.008	0.000	0.050	0.000	0.434
Subsidy/Revenue	50,019	0.009	0.000	0.058	0.000	0.510
Subsidy/Employee	21,129	339.150	0.000	1,922.105	0.000	16,197.183
Count	58,144	0.263	0.000	0.607	0.000	3.000
D_sub	58,144	0.196	0.000	0.397	0.000	1.000
Federal($)	58,144	87,093.606	0.000	472,866.761	0.000	3,151,000.000
State($)	58,144	41,491.288	0.000	190,482.382	0.000	1,208,956.000
Federal Count	58,144	0.054	0.000	0.227	0.000	1.000
State Count	58,144	0.152	0.000	0.429	0.000	2.000
Subsidized Loan	58,144	60,585.523	0.000	372,714.740	0.000	2,691,000.000
Grant Subsidy	58,144	7,356.985	0.000	43,632.197	0.000	299,959.000
Tax Subsidy	58,144	31,560.965	0.000	155,176.068	0.000	1,000,000.000
Job Subsidy	58,144	3,437.428	0.000	20,712.086	0.000	148,765.000
Crisis Subsidy	58,144	93,950.682	0.000	461,606.292	0.000	3,000,000.000
Investment Subsidy	58,144	904.514	0.000	6,947.299	0.000	59,157.000
Property Tax Subsidy	58,144	791.946	0.000	6,197.341	0.000	52,675.000
Pol_Contrib($)	58,144	62,351.015	0.000	933,096.281	0.000	181,148,087.437
ln(Contrib($)+1)	58,144	3.876	0.000	4.478	0.000	19.015
D_Contribute	54,906	0.516	1.000	0.500	0.000	1.000
Leverage	25,476	0.260	0.199	0.273	0.000	1.623
Equity/Asset	55,343	0.458	0.437	0.283	0.031	1.000
Debt/Equity	23,984	1.316	0.533	3.076	0.000	23.890
Cash/Asset	56,934	0.160	0.079	0.212	0.000	1.000
Turnover	49,695	1.581	1.078	1.695	0.014	9.943
OperatingIncome/Asset	25,798	-0.015	0.042	0.293	-1.833	0.364
ROA	57,675	0.023	0.023	0.232	-1.261	0.820
ROE	55,342	0.098	0.088	0.497	-2.352	2.408

Table A.1: **Firm-year panel summary statistics, continued**

This table shows the summary statistics of the full sample of the firm-year panel of subsidy-receiving target firms and their corresponding buyer firms.

	count	mean	p50	sd	min	max
Δ CashFlow	17,737	0.423	0.036	4.590	-18.087	28.868
Δ Sales	36,752	0.576	0.066	3.595	-0.978	32.430
Δ Employees	18,677	0.074	0.028	0.231	-0.433	1.274
Capex/AT	20,934	0.046	0.026	0.063	0.000	0.411
Capex/PPE	20,832	0.330	0.181	0.527	0.006	3.894
R&D/PPE	42,188	0.393	0.000	1.728	0.000	13.656
R&D/AT	43,657	0.021	0.000	0.073	0.000	0.504
ln(Asset)	57,716	18.522	18.410	3.591	6.928	34.568
ln(Revenue)	50,016	18.182	17.782	3.236	2.303	32.167
ln(Equity+1)	55,384	4.001	3.513	2.951	0.000	18.759
ln(Debt+1)	25,493	5.086	5.425	3.408	0.000	19.065
ln(Employees)	21,129	7.595	7.685	2.329	0.000	14.648
Assets	57,716	431,047.289	98.934	13,519,932.260	0.001	1029836868.000
Revenue	50,016	33,306.887	52.797	836,617.282	0.000	93,304,592.000
Equity	55,384	48,255.500	32.556	1,580,463.876	0.000	140,197,662.000
Debt	25,493	164,961.532	226.049	3,288,690.933	0.000	190,470,395.000
Total Employees	21,129	16,717.262	2,176.000	52,622.667	1.000	2,300,000.000
Operating Income	26,115	13,478.204	53.940	320,121.731	-389,021.000	20,543,441.000
Net Income	58,103	4,527.893	1.122	177,266.329	-1,468,040.000	18,312,054.000
Cash	56,982	37,614.729	7.152	1,438,205.062	0.000	124,657,856.000
Cash Flow	19,989	852.207	31.075	27,878.176	-2,821,410.625	1,509,121.500

Table A.2: **Summary statistics: Target firms at T=-1 summary statistics**

This tables shows the summary statistics of the cross-section of treated target firms one year before merger or buyout.

	count	mean	p50	sd	min	max
ln(Subsidy+1)	427	2.828	0.000	5.331	0.000	15.384
Subsidy($)	427	302,530.044	0.000	1,037,793.481	0.000	4,799,201.000
Subsidy/Asset	427	0.001	0.000	0.004	0.000	0.028
Subsidy/Revenue	427	0.007	0.000	0.048	0.000	0.449
sub_emply	427	683.793	0.000	5,353.095	0.000	60,749.379
Count	427	0.293	0.000	0.598	0.000	3.000
D_sub	427	0.239	0.000	0.427	0.000	1.000
Federal($)	427	147,010.234	0.000	643,108.399	0.000	3,151,000.000
State($)	427	61,970.136	0.000	225,594.732	0.000	1,208,956.000
Count_fed	427	0.063	0.000	0.244	0.000	1.000
Count_state	427	0.187	0.000	0.436	0.000	2.000
Subsidized Loan	427	109,299.766	0.000	524,732.867	0.000	2,691,000.000
Grant Subsidy	427	7,899.199	0.000	46,421.162	0.000	299,959.000
Tax Subsidy	427	34,948.459	0.000	155,115.935	0.000	1,000,000.000
Training Subsidy	427	829.822	0.000	4,869.540	0.000	31,337.000
Pol_Contrib($)	427	19,297.349	0.000	95,383.851	0.000	1,191,276.382
ln(Contrib($)+1)	427	3.296	0.000	4.360	0.000	13.991
D_Contribute	427	0.389	0.000	0.488	0.000	1.000
ln(Asset)	273	19.907	20.361	2.373	10.926	26.330
ln(Revenue)	251	18.967	18.705	2.481	12.231	23.974
ln(Equity+1)	264	4.923	4.824	2.141	0.020	10.061
ln(Debt+1)	228	4.328	4.835	2.689	0.000	11.444
ln(Employees)	193	7.167	7.090	1.936	3.219	11.695
ROE	264	0.050	0.082	0.221	-1.106	0.661
ROA	273	0.009	0.012	0.108	-0.557	0.226
Assets	273	4,394.585	696.327	21,149.287	0.056	272,153.000
Revenue	251	1,613.799	132.861	3,770.303	0.205	25,801.121
Equity	264	1,086.581	123.493	3,265.184	0.020	23,406.000
Debt	228	1,267.101	124.898	6,880.655	0.000	93,356.000
netppe	268	556.672	25.867	2,006.906	0.001	22,039.000
rd	365	27.487	0.000	204.850	0.000	3,340.084
TotalEmployees	193	6,440.684	1,200.000	13,835.150	25.000	120,000.000
OperatingIncome	239	212.231	16.976	738.510	-672.000	6,946.234
Netincome	284	77.394	4.656	607.864	-6,001.000	4,758.000
Cash	273	204.066	29.589	784.817	0.000	10,015.877
CashFlow	148	188.052	25.038	604.229	-474.053	3,778.610
capex	226	104.021	9.462	349.505	0.009	3,193.000

Table A.3: **Summary statistics: Buyer firms at T=-1 summary statistics**

This tables shows the summary statistics of the cross-section of treated buyer firms one year before merger or buyout.

	count	mean	p50	sd	min	max
ln(Subsidy+1)	427	0.682	0.000	2.952	0.000	15.384
Subsidy($)	427	72,133.527	0.000	485,996.555	0.000	4,799,201.000
Subsidy/Asset	427	0.000	0.000	0.000	0.000	0.001
Subsidy/Revenue	427	0.000	0.000	0.000	0.000	0.002
sub_emply	427	10.331	0.000	82.382	0.000	927.152
Count	427	0.082	0.000	0.388	0.000	3.000
D_sub	427	0.052	0.000	0.221	0.000	1.000
Federal($)	427	15,180.354	0.000	215,543.895	0.000	3,151,000.000
State($)	427	26,658.377	0.000	161,024.318	0.000	1,208,956.000
Count_fed	427	0.007	0.000	0.084	0.000	1.000
Count_state	427	0.061	0.000	0.308	0.000	2.000
Subsidized Loan	427	12,604.215	0.000	183,951.962	0.000	2,691,000.000
Grant Subsidy	427	2,684.754	0.000	25,069.499	0.000	299,959.000
Tax Subsidy	427	21,147.824	0.000	133,159.995	0.000	1,000,000.000
Training Subsidy	427	366.944	0.000	3,375.048	0.000	31,337.000
Pol_Contrib($)	427	97,207.699	0.000	799,929.886	0.000	13,058,577.432
ln(Contrib($)+1)	427	2.110	0.000	4.170	0.000	16.385
D_Contribute	427	0.225	0.000	0.418	0.000	1.000
ln(Asset)	389	20.870	21.017	2.330	13.536	27.908
ln(Revenue)	363	19.960	19.828	2.317	12.792	25.902
ln(Equity+1)	373	5.933	5.997	2.157	0.006	11.586
ln(Debt+1)	343	5.291	5.721	2.668	0.000	11.229
ln(Employees)	291	7.657	7.427	2.025	1.609	12.402
ROE	373	0.048	0.088	0.413	-2.171	1.635
ROA	389	0.012	0.019	0.136	-0.767	0.262
Assets	389	12,215.633	1,341.889	71,801.515	0.756	1,318,888.000
Revenue	363	4,120.303	408.500	13,683.283	0.359	177,526.000
Equity	373	3,031.999	401.300	9,653.855	0.006	107,614.000
Debt	343	1,991.070	304.338	5,776.722	0.000	75,314.000
netppe	385	1,444.033	75.200	6,994.477	0.021	110,968.000
rd	389	108.701	0.000	607.472	0.000	7,512.000
TotalEmployees	291	12,667.357	1,681.000	30,113.997	5.000	243,360.000
OperatingIncome	351	610.361	66.300	2,198.796	-299.800	30,752.000
Netincome	393	373.969	22.549	1,380.346	-750.400	18,418.000
Cash	386	1,182.468	74.172	14,718.216	0.000	288,853.000
CashFlow	241	646.833	68.433	2,089.040	-1,160.625	17,089.625
capex	315	263.903	17.058	1,325.938	0.019	21,228.000

Table A.4: **Effect of buyout and acquisition on firm subsidies: Baseline stacked DiD regressions**

This table shows stacked DiD regression main baseline results. Panel A shows the test using a $Post \times Treat$ variable that does not distinguish between the types of acquisition event, and Panel B shows the test with merger acquisitions and private equity leverage buyouts separated as two event types.

The dependent variables are a logarithmic of subsidy dollar amount plus one in column (1), winsorized dollar amount of subsidies in columns (2) and (3), Count of number of subsidy transactions in column (4), and a dummy indicator that equals one for non-zero subsidies in column (5). A linear regression model with fixed effects is used in columns (1), (2), and (5), while columns (3) and (4) use a Poisson regression model with fixed effects.

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Combined firm subsidies

	(1) Ln(Sub+1)	(2) Subsidy($)	(3) Subsidy($)	(4) Count	(5) D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post×Treat	-1.209***	-99381***	-0.516***	-0.703***	-0.098***
	(-6.35)	(-2.74)	(-2.65)	(-5.51)	(-6.50)
Observations	310,061	310,061	208,256	208,256	310,061
R^2 / Pseudo R^2	0.411	0.397	0.556	0.232	0.411
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Panel B: Combined firm subsidies: Separate dealtypes

	(1) Ln(Sub+1)	(2) Subsidy($)	(3) Subsidy($)	(4) Count	(5) D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma×Treat_ma	-1.342***	-116029.623***	-0.535***	-0.702***	-0.108***
	(-6.28)	(-2.90)	(-2.65)	(-5.29)	(-6.37)
Post_lbo×Treat_lbo	-0.404	1389.079	-0.301	-0.714*	-0.037
	(-1.24)	(0.02)	(-0.44)	(-1.70)	(-1.49)
Observations	310,061	310,061	208,256	208,256	310,061
R^2 / Pseudo R^2	0.411	0.397	0.556	0.232	0.411
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes
F-test: Post_ma = Post_lbo	5.85**	1.78	0.11	0.00	5.74**
p-value	0.016	0.182	0.744	0.979	0.017

SE adjusted for clustering at firm level

Table A.5: **Effect of buyout and acquisition on firm size and revenue scaled subsidies: Stacked DiD**

This table shows stacked DiD regressions on scaled-subsidies. Columns (1) to (3) shows the test using a $Post \times Treat$ variable that does not distinguish between the types of acquisition event, while Columns (4) to (6) shows the test with merger acquisitions and private equity leverage buyouts separated as two event types.

The dependent variables are subsidy dollar amount scaled by total assets in columns (1) and (4), subsidy dollar amount scaled by total revenue in columns (2) and (5), subsidy dollar amount scaled by total employment in columns (3) and (6). A linear regression model with fixed effects is used in all columns.

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Scaled by	Subsidy/ Assets	Subsidy/ Revenues	Subsidy/ Employees	Subsidy/ Assets	Subsidy/ Revenues	Subsidy/ Employees
Model Type	*OLS*	*OLS*	*OLS*	*OLS*	*OLS*	*OLS*
Post × Treat	-0.001***	-0.001**	-154.050***			
	(-2.68)	(-2.14)	(-2.61)			
Post_ma × Treat_ma				-0.001**	-0.001*	-160.595***
				(-2.52)	(-1.70)	(-2.63)
Post_lbo × Treat_lbo				-0.003	-0.003*	-65.101
				(-1.25)	(-1.75)	(-0.28)
Observations	308,653	260,346	140,586	308,653	260,346	140,586
R^2	0.377	0.317	0.327	0.377	0.317	0.327
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes	Yes
F-test: Post_ma = Post_lbo				0.51	0.80	0.16
p-value				0.475	0.370	0.692

SE adjusted for clustering at firm level

Table A.6: **Mechanisms: Effect of mergers on combined firm financials**

This table shows a stacked DiD regression exploring effect of merger acquisitions on merged firm financial composition and performance. All models are tested using OLS with fixed effects. The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Financial Structure Change

	(1) Lvg	(2) Equity/AT	(3) Debt/Equity	(4) Cash/AT
Model Type	*OLS*	*OLS*	*OLS*	*OLS*
Post_ma × Treat_ma	0.023**	-0.030***	0.103	-0.037***
	(2.10)	(-3.31)	(1.11)	(-5.99)
Observations	121257	225588	114771	231643
R^2	0.763	0.819	0.715	0.626
IndustryYearCohortFE	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Panel B: Financial Performance Change

	(1) Turnover	(2) ROA	(3) ROE	(4) ΔCashflow	(5) ΔSales	(6) OperatingIncome/AT
Model Type	OLS	OLS	OLS	OLS	OLS	OLS
Post_ma × Treat_ma	-0.066**	0.005	-0.009	0.307	-0.107	-0.001
	(-2.17)	(0.61)	(-0.56)	(1.28)	(-1.31)	(-0.17)
Observations	197309	233998	225558	91450	166974	122870
R^2	0.785	0.649	0.525	0.144	0.231	0.788
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.7: **Mechanisms: Financial performance improved after deal**

This table shows a stacked triple DiD regression exploring the subset of merger acquisitions that result in an improvement in merged firm financial performance. All models are tested using Poisson model with fixed effects. The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1) Subsidy($)	(2) Subsidy($)	(3) Subsidy($)
Model Type	PPML	PPML	PPML
Post_ma × Treat_ma	-0.495**	-0.504**	-0.581**
	(-2.13)	(-2.18)	(-2.42)
Post_ma × Treat_ma × improve_roa	-1.905***		
	(-2.77)		
Post_ma × Treat_ma × improve_roe		-2.042**	
		(-1.99)	
Post_ma × Treat_ma × improve_cf_growth			0.511
			(0.70)
Observations	162779	162779	162779
Pseudo R^2	0.558	0.558	0.558
IndustryYearCohortFE	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.8: **Mechanisms: Effect of mergers on combined firm investment and hiring**

This table shows a stacked DiD regression exploring effect of merger acquisitions on merged firm investment and hiring activity. All models are tested using OLS with fixed effects. The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)
	ΔEmploy	Rev/Employ	Capex/AT	RD/AT	RD/PPE
Model Type	OLS	OLS	OLS	OLS	OLS
Post_ma × Treat_ma	-0.010	-0.086**	-0.007**	-0.009***	-0.109
	(-0.60)	(-2.34)	(-2.23)	(-3.47)	(-1.48)
Observations	95088	101344	104716	202009	196349
R^2	0.303	0.972	0.621	0.842	0.795
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.9: **Mechanisms: Investment or employment improved after deal**

This table shows a stacked triple DiD regression exploring the subset of merger acquisitions that result in an increase in merged firm hiring or investment activity. All models are tested using Poisson model with fixed effects. The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1) Subsidy($)	(2) Subsidy($)	(3) Subsidy($)
Model Type	PPML	PPML	PPML
Post_ma × Treat_ma	-0.642***	-0.673***	-0.534**
	(-2.70)	(-2.77)	(-2.34)
Post_ma × Treat_ma × improve_emply_growth	1.045*		
	(1.86)		
Post_ma × Treat_ma × improve_capex_at		0.739	
		(1.29)	
Post_ma × Treat_ma × improve_rd_ppe			1.451***
			(3.19)
Observations	162779	162779	162779
Pseudo R^2	0.558	0.558	0.558
IndustryYearCohortFE	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.10: **Mechanisms: Effect of Market Power**

This table reports stacked DiD regression results examining whether the effect of mergers and acquisitions on government subsidies varies with deal characteristics related to market power and buyer-target scale.

The dependent variables are subsidy dollar amounts in columns (1), (3), and (5), and subsidy counts in columns (2), (4), and (6). Columns (1) and (2) interact the M&A treatment effect with an indicator for large buyers. Columns (3) and (4) interact the M&A treatment effect with an indicator for large targets. Columns (5) and (6) interact the M&A treatment effect with an indicator for within-industry acquisitions.

The event window is 11 years, from $t-5$ to $t+5$, with $t=0$ defined as the year of acquisition. $Post_ma$ is a dummy variable equal to one for post-acquisition years and zero otherwise. $Treat_ma$ is a dummy variable equal to one for acquired firms and zero for control firms. All specifications are estimated using PPML and include firm-by-cohort and industry-year-by-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Subsidy($)	Count	Subsidy($)	Count	Subsidy($)	Count
Model Type	*PPML*	*PPML*	*PPML*	*PPML*	*PPML*	*PPML*
Post_ma × Treat_ma	-1.402***	-1.321***	-0.997***	-0.931***	-0.283	-0.627**
	(-3.58)	(-5.68)	(-3.31)	(-4.70)	(-0.61)	(-2.18)
Post_ma × Treat_ma × D_large_buyer	1.163**	1.022***				
	(2.51)	(3.66)				
Post_ma × Treat_ma × D_large_target			0.724*	0.547**		
			(1.76)	(2.06)		
Post_ma × Treat_ma × D_within_ind					-0.317	0.027
					(-0.60)	(0.08)
Observations	162,779	162,779	162,779	162,779	162,779	162,779
Pseudo R^2	0.558	0.233	0.558	0.233	0.558	0.233
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.11: **Mechanisms: Effect of buyouts and acquisitions on political contribution behavior**

This table shows stacked DiD regression results on the effect of merger acquisition on merged firm political contribution behavior.

The dependent variables are a dummy indicator that equals one if the firm is making a positive political contribution in a given year in columns (1) and (4), logarithmic of political contribution dollar amount plus one in columns (2) and (5), and political contribution dollar amount in columns (3) and (6).

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	D_PolContrib	ln(Contrib)	Contrib($)	D_PolContrib	ln(Contrib)	Contrib($)
Model Type	*OLS*	*OLS*	*PPML*	*OLS*	*OLS*	*PPML*
Post_ma × Treat_ma	-0.339***	-2.619***	-0.463***	-0.342***	-2.654***	-0.462***
	(-12.12)	(-10.83)	(-3.58)	(-12.24)	(-11.03)	(-3.57)
ln(Asset)				0.010***	0.124***	
				(4.85)	(8.17)	
Assets						0.000
						(0.39)
Observations	235181	235181	186467	234127	234127	185556
R^2	0.681	0.822	0.937	0.682	0.823	0.937
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.12: **Mechanisms: politically connected firms**

This table shows triple DiD regression results on the subsample of politically connected firms, which are firms that have made political contributions both before and after the deal.

The dependent variables are subsidy dollar amount scaled by total assets in column (1), logarithmic of total assets in column (2), subsidy dollar amount scaled by total revenue in column (3), and logarithmic of total revenue in column (4). A linear regression model with fixed effects is used in columns (2) and (4), while columns (1) and (3) use a Poisson regression model with fixed effects.

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma × Treat_ma	-1.605***	-133506.005***	-0.863**	-1.129***	-0.131***
	(-5.92)	(-2.65)	(-2.53)	(-4.90)	(-6.05)
Post_ma × Treat_ma × D_always_contrib	1.199**	76125.164	0.719	1.060***	0.102***
	(2.49)	(0.74)	(1.62)	(4.02)	(2.67)
Observations	235181	235181	162779	162779	235181
R^2 / Pseudo R^2	0.405	0.396	0.558	0.233	0.403
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.13: **Mechanisms: Political connections pre-deal**

This table shows triple DiD regression results on the effect of political connections of buyers and targets before the deal. Panel A shows the triple difference on the subsample of merged firms that have buyers with no political connections prior to the merger, and Panel B shows the subsample on merged firms with targets that have no political connections prior to the merger.

The dependent variables are subsidy dollar amount scaled by total assets in column (1), logarithmic of total assets in column (2), subsidy dollar amount scaled by total revenue in column (3), and logarithmic of total revenue in column (4). A linear regression model with fixed effects is used in columns (2) and (4), while columns (1) and (3) use a Poisson regression model with fixed effects.

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Buyer never contributed before deal

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma × Treat_ma	1.617***	78335.633	0.100	0.393**	0.157***
	(3.30)	(0.92)	(0.34)	(2.04)	(4.17)
Post_ma × Treat_ma × D_nocontrib_pre_buyer	-2.885***	-190473.197**	-0.628*	-1.006***	-0.259***
	(-5.37)	(-1.98)	(-1.75)	(-4.30)	(-6.23)
Observations	235181	235181	162779	162779	235181
R^2 / Pseudo R^2	0.405	0.396	0.558	0.233	0.403
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Panel B: Target never contributed before deal

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma × Treat_ma	-1.265***	-92023.076*	-0.491**	-0.606***	-0.103***
	(-4.49)	(-1.74)	(-1.97)	(-4.10)	(-4.58)
Post_ma × Treat_ma × D_nocontrib_pre_target	0.030	-79624.505	-0.171	0.003	0.009
	(0.07)	(-0.83)	(-0.31)	(0.01)	(0.25)
Observations	235181	235181	162779	162779	235181
R^2 / Pseudo R^2	0.405	0.396	0.558	0.233	0.403
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.14: **Mechanisms: Cross Border versus Domestic Mergers**

This table shows stacked triple DiD regression on the subsample of cross-border mergers.

The dependent variables are subsidy dollar amount scaled by total assets in column (1), logarithmic of total assets in column (2), subsidy dollar amount scaled by total revenue in column (3), and logarithmic of total revenue in column (4). A linear regression model with fixed effects is used in columns (2) and (4), while columns (1) and (3) use a Poisson regression model with fixed effects.

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma × Treat_ma	-1.096***	-91674.692*	-0.413*	-0.540***	-0.089***
	(-4.32)	(-1.92)	(-1.77)	(-3.75)	(-4.37)
Post_ma × Treat_ma × crossborder	-1.293**	-158127.840	-0.836	-0.703	-0.095*
	(-1.97)	(-0.97)	(-1.07)	(-1.28)	(-1.96)
Observations	235181	235181	162779	162779	235181
R^2 / Pseudo R^2	0.405	0.396	0.558	0.233	0.403
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table A.15: **By issuing agency: State versus Federal Subsidies**

This table shows stacked DiD regression on subsidy sub-types, with the sample split into federal subsidies and state subsidies.
The dependent variables are subsidy dollar amount in columns (1) and (2), and count of subsidies transactions in columns (3) and (4). All regressions use a Poisson regression model with fixed effects.

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	State Subsidy($)	Federal Subsidy($)	State Count	Federal Count
Model Type	*PPML*	*PPML*	*PPML*	*PPML*
Post_ma × Treat_ma	-0.682***	-0.936**	-0.686***	-1.161***
	(-3.60)	(-2.39)	(-4.68)	(-4.01)
Post_lbo × Treat_lbo	-1.883***	0.346	-1.272**	0.142
	(-3.12)	(0.44)	(-2.48)	(0.18)
Observations	138,076	64,785	138,076	64,785
Pseudo R^2	0.471	0.533	0.189	0.224
IndustryYearCohortFE	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes
F-test: Post_ma = Post_lbo	3.63*	2.13	1.21	2.44
p-value	0.057	0.145	0.271	0.119

SE adjusted for clustering at firm level

Table A.16: **Treatment Effects by Subsidy Program Type**

This table reports stacked DiD regression results separately by subsidy program type. The dependent variables are subsidy dollar amounts for crisis-related subsidies, subsidized loans, grants, job-related subsidies, and tax subsidies. All specifications are estimated using PPML.

The event window is 11 years, from $t-5$ to $t+5$, with $t=0$ defined as the year of acquisition or buyout. $Post_ma$ and $Post_lbo$ are dummy variables equal to one for post-treatment years for M&A and buyout events, respectively. $Treat_ma$ and $Treat_lbo$ are treatment indicators equal to one for treated firms and zero for control firms. The reported coefficients compare post-treatment changes for M&A targets and buyout targets relative to their respective control firms.

All specifications include firm-by-cohort and industry-year-by-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the target firm level, and t-statistics are reported in parentheses. The row "F-test: $Post_ma = Post_lbo$" reports tests of equality between the M&A and buyout treatment effects, with corresponding p-values reported below. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	Crisis($)	Loans($)	Grant($)	Jobs($)	Tax Subsidy($)
Model Type	*PPML*	*PPML*	*PPML*	*PPML*	*PPML*
Post_ma × Treat_ma	-0.464**	-0.353	-0.762***	-0.846***	-0.578***
	(-2.01)	(-0.87)	(-2.82)	(-3.33)	(-3.12)
Post_lbo × Treat_lbo	-0.510	-0.249	1.407	-0.979	-2.872***
	(-0.48)	(-0.20)	(1.24)	(-1.25)	(-2.96)
Observations	107,322	44,277	51,267	62,627	101,085
Pseudo R^2	0.540	0.550	0.387	0.294	0.468
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes
F-test: Post_ma = Post_lbo	0.002	0.007	3.479*	0.027	5.411**
p-value	0.967	0.935	0.062	0.870	0.020

SE adjusted for clustering at target firm level

Table A.17: **Heterogeneity in the Effect of M&A on Subsidies by Program Type**

This table reports stacked triple-difference regression results examining heterogeneity in the effect of M&A on government subsidies across subsidy program types. The dependent variables are subsidy dollar amounts for crisis-related subsidies, investment subsidies, grants, job-related subsidies, and property tax subsidies. All specifications are estimated using PPML.

Each panel reports a separate heterogeneity test. Panel A examines whether the M&A treatment effect differs for firms that consistently make political contributions. Panel B examines whether the effect differs for firms that consistently make state-level political contributions. Panel C examines whether the effect differs for acquisitions involving large buyers. Panel D examines whether the effect differs for firms with improved cash flow growth. Panel E examines whether the effect differs for firms with improved employment growth. The triple-interaction coefficients report the differential treatment effect for the corresponding subgroup.

The event window is 11 years, from $t-5$ to $t+5$, with $t=0$ defined as the year of acquisition. $Post_ma$ is a dummy variable equal to one for post-acquisition years and zero otherwise. $Treat_ma$ is a treatment indicator equal to one for M&A target firms and zero for control firms. All specifications include firm-by-cohort and industry-year-by-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	Crisis	Investment	Grant	Jobs	Property Tax
Panel A: Always Contributes					
Post_ma × Treat_ma	-1.320***	-1.281**	-0.694*	-1.826***	-0.981
	(-3.39)	(-2.54)	(-1.78)	(-3.40)	(-1.17)
Post_ma × Treat_ma × D_always_contrib	1.811***	0.936	0.095	1.531**	0.577
	(3.95)	(1.50)	(0.16)	(2.48)	(0.61)
Observations	84,368	20,495	40,246	49,360	20,501
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes
Panel B: State-Level Always Contributes					
Post_ma × Treat_ma	-0.860***	-1.071**	-0.704*	-1.378***	-0.568
	(-2.71)	(-2.53)	(-1.92)	(-3.46)	(-1.33)
Post_ma × Treat_ma × D_state_always	0.974*	0.679	0.136	0.931*	-0.394
	(1.91)	(1.09)	(0.23)	(1.72)	(-0.33)
Observations	84,368	20,495	40,246	49,360	20,501
Pseudo R^2	0.547	0.382	0.380	0.288	0.291
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes
Panel C: Large Buyer					
Post_ma × Treat_ma	-2.879***	-1.777***	-0.913*	-1.203**	-2.256***
	(-5.24)	(-3.43)	(-1.65)	(-2.24)	(-4.35)
Post_ma × Treat_ma × D_large_buyer	2.893***	1.233**	0.379	0.405	1.668**
	(4.79)	(2.04)	(0.60)	(0.65)	(2.18)
Observations	84,348	20,495	40,226	49,350	20,493
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

t statistics in parentheses.
Standard errors are clustered at the firm level.
All specifications absorb firm-by-cohort and industry-year-by-cohort fixed effects.
* $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.17: **Heterogeneity in the Effect of M&A on Subsidies by Program Type, Continued**

This table reports stacked triple-difference regression results examining heterogeneity in the effect of M&A on government subsidies across subsidy program types. The dependent variables are subsidy dollar amounts for crisis-related subsidies, investment subsidies, grants, job-related subsidies, and property tax subsidies. All specifications are estimated using PPML.

Each panel reports a separate heterogeneity test. Panel A examines whether the M&A treatment effect differs for firms that consistently make political contributions. Panel B examines whether the effect differs for firms that consistently make state-level political contributions. Panel C examines whether the effect differs for acquisitions involving large buyers. Panel D examines whether the effect differs for firms with improved cash flow growth. Panel E examines whether the effect differs for firms with improved employment growth. The triple-interaction coefficients report the differential treatment effect for the corresponding subgroup.

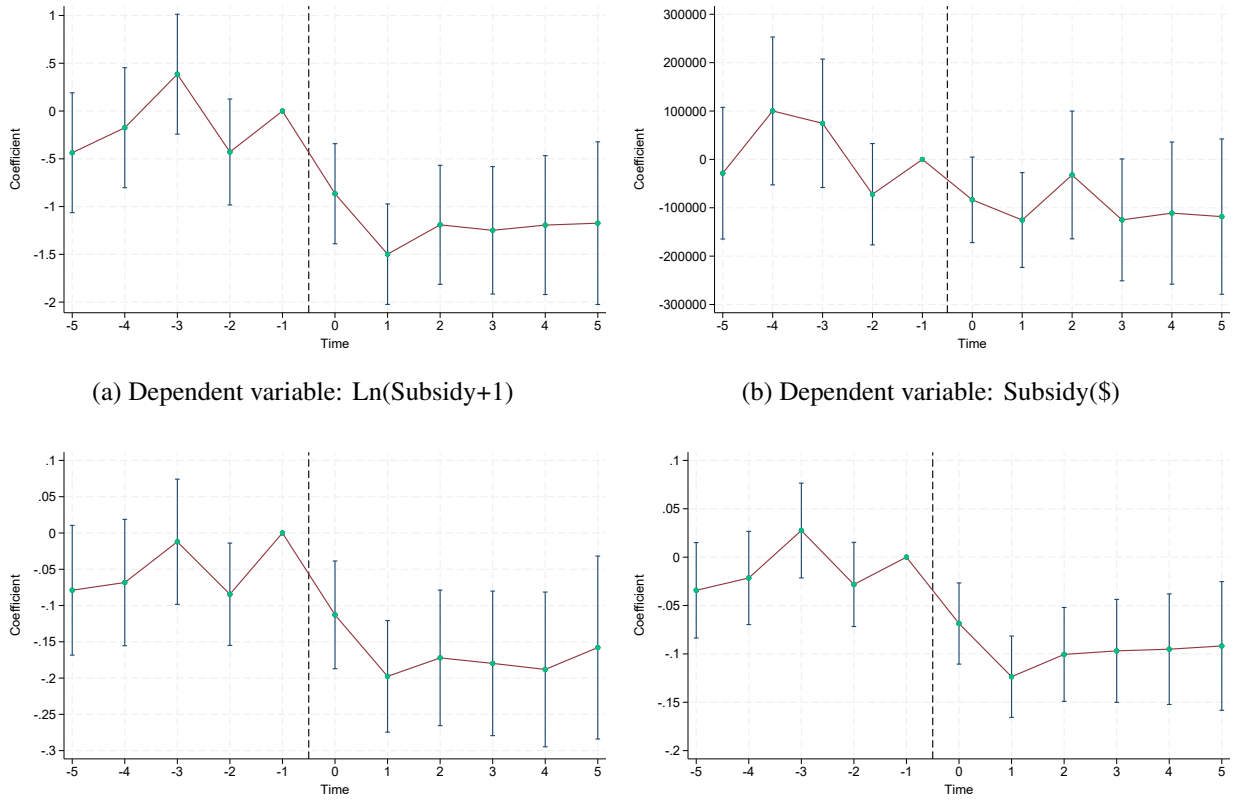
The event window is 11 years, from $t-5$ to $t+5$, with $t=0$ defined as the year of acquisition. $Post_ma$ is a dummy variable equal to one for post-acquisition years and zero otherwise. $Treat_ma$ is a treatment indicator equal to one for M&A target firms and zero for control firms. All specifications include firm-by-cohort and industry-year-by-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	Crisis	Investment	Grant	Jobs	Property Tax
Panel D: Improvement in Cash Flow Growth					
Post_ma × Treat_ma	-0.559**	-1.027***	-0.851***	-0.674**	-1.436**
	(-2.16)	(-3.20)	(-2.67)	(-2.22)	(-2.07)
Post_ma × Treat_ma × improve_cf_growth	0.560	1.313*	1.391*	-1.140	1.613*
	(0.61)	(1.75)	(1.95)	(-1.58)	(1.67)
Observations	84,368	20,495	40,246	49,360	20,501
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes
Panel E: Improvement in Employment Growth					
Post_ma × Treat_ma	-0.579**	-0.979***	-0.692**	-0.970***	-0.461
	(-2.26)	(-2.84)	(-2.33)	(-3.19)	(-0.81)
Post_ma × Treat_ma × improve_emply_growth	1.143*	1.607***	0.915	0.736	-1.781**
	(1.89)	(2.73)	(0.73)	(1.29)	(-2.45)
Observations	84,368	20,495	40,246	49,360	20,501
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

t statistics in parentheses.
Standard errors are clustered at the firm level.
All specifications absorb firm-by-cohort and industry-year-by-cohort fixed effects.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

APPENDIX B

ROBUSTNESS TESTS



(a) Dependent variable: Ln(Subsidy+1)

(b) Dependent variable: Subsidy($)

(c) Dependent variable: Count

(d) Dependent variable: D_sub

Figure B.1: **Coefficient plot for local projection DiD**

This figure shows the point estimates and 95% confidence intervals of the coefficients on the local projection DiD robustness test, where the x-axis numbers represent the year relative to the buyout year (year 0). The dependent variables are a logarithmic of subsidy dollar amount plus one, winsorized dollar amount of subsidies, Count of number of subsidy transactions, and a dummy indicator that equals one for non-zero subsidies, respectively, in panels A to D.

Table B.1: **Matching Balance**

Panel A:Covariate Balance for CEM Matched Samples

Variable	Treated Mean	Control Mean	Std. Diff.
Panel A: M&A CEM Matched Sample			
Pre-deal subsidy amount	221,000	211,000	0.017
Pre-deal subsidy count	0.274	0.270	0.007
Any pre-deal subsidy	0.393	0.393	-0.000
Pre-deal subsidy trend	-20,600	-8,984	-0.020
Assets	5,252	440,000	-0.046
Panel B: PE CEM Matched Sample			
Pre-deal subsidy amount	193,000	218,000	-0.044
Pre-deal subsidy count	0.238	0.295	-0.138
Any pre-deal subsidy	0.429	0.429	0.000
Pre-deal subsidy trend	-70,700	19,953	-0.165
Assets	1,326	140,000	-0.028

Notes: This table reports covariate balance for the CEM matched samples used in the robustness tests. Panel A reports balance for the M&A matched sample, and Panel B reports balance for the PE matched sample. Matching is performed within industry and baseline year. Control firms are matched to treated firms using coarsened bins for pre-deal subsidy amount, pre-deal subsidy count, pre-deal subsidy trend, and firm size. The table reports weighted means for treated and matched control firms, using CEM weights for control observations. Standardized differences are calculated as the difference in treated and control means divided by the pooled standard deviation.

Panel B: Covariate Balance for Mahalanobis Nearest-Neighbor Matched Samples

Variable	Treated Mean	Matched Control Mean	Std. Diff. Before	Std. Diff. After
Panel A: M&A Mahalanobis NN Matched Sample				
Pre-deal subsidy amount	220,000	221,000	-0.032	-0.001
Pre-deal subsidy count	0.276	0.276	-0.027	0.000
Any pre-deal subsidy	0.396	0.396	0.039	0.000
Pre-deal subsidy trend	-21,800	-21,900	-0.014	0.000
Log assets	19.813	19.833	0.117	-0.008
Panel B: PE Mahalanobis NN Matched Sample				
Pre-deal subsidy amount	194,000	192,000	-0.077	0.004
Pre-deal subsidy count	0.256	0.256	-0.073	0.000
Any pre-deal subsidy	0.438	0.438	0.125	0.000
Pre-deal subsidy trend	-81,600	-80,600	-0.118	-0.002
Log assets	19.057	19.057	-0.136	0.000

Notes: This table reports covariate balance for the Mahalanobis nearest-neighbor matched samples used in the robustness tests. Panel A reports balance for the M&A matched sample, and Panel B reports balance for the PE matched sample. Treated firms are matched to three nearest-neighbor control firms using Mahalanobis distance based on pre-deal subsidy amount, pre-deal subsidy count, an indicator for any pre-deal subsidy, pre-deal subsidy trend, and log assets. Matched control means are weighted using the matching weights generated by `psmatch2`. Standardized differences are calculated as the difference in treated and control means divided by the pooled standard deviation.

Table B.2: **Effect of Buyouts and Acquisitions on Firm Subsidies: CEM Robustness**

This table reports stacked DiD regression results using coarsened exact matching (CEM) weights. Panel A reports the baseline treatment effect using a combined $Post \times Treat$ indicator that pools M&A and buyout events. Panel B estimates separate treatment effects for M&A events and private equity buyouts.

The dependent variables are the logarithm of subsidy dollar amount plus one in column (1), subsidy dollar amount in columns (2) and (3), subsidy transaction count in column (4), and an indicator equal to one for positive subsidy receipt in column (5). Columns (1), (2), and (5) are estimated using OLS with fixed effects, while columns (3) and (4) are estimated using PPML.

The event window is 11 years, from $t-5$ to $t+5$, with $t=0$ defined as the year of acquisition or buyout. $Post$ is a dummy variable equal to one for post-treatment years and zero otherwise. $Treat$ is a dummy variable equal to one for treated firms and zero for matched control firms. In Panel B, $Post_ma \times Treat_ma$ captures the M&A treatment effect, while $Post_lbo \times Treat_lbo$ captures the buyout treatment effect. All specifications include firm-by-cohort and industry-year-by-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Combined firm subsidies

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post×Treat	-0.981***	-111407.700***	-0.607***	-0.909***	-0.078***
	(-7.14)	(-4.12)	(-2.71)	(-7.52)	(-7.04)
Observations	253,087	253,087	99,402	99,402	253,087
R^2 / Pseudo R^2	0.558	0.535	0.712	0.291	0.559
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Panel B: Combined firm subsidies: Separate dealtypes

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma×Treat_ma	-1.012***	-117455.200***	-0.596***	-0.919***	-0.081***
	(-7.13)	(-4.23)	(-2.65)	(-7.39)	(-7.11)
Post_lbo×Treat_lbo	-0.434	-2750.302	-0.914	-0.761**	-0.023
	(-0.96)	(-0.04)	(-0.75)	(-2.01)	(-0.58)
Observations	253,087	253,087	99,402	99,402	253,087
R^2 / Pseudo R^2	0.558	0.535	0.712	0.291	0.559
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes
F-test: Post_ma = Post_lbo	1.53	2.03	0.07	0.17	2.01
p-value	0.216	0.154	0.795	0.683	0.157

SE adjusted for clustering at firm level

Table B.3: **Effect of Buyouts and Acquisitions on Firm Subsidies: Mahalanobis Nearest-Neighbor Robustness**

This table reports stacked DiD regression results using a Mahalanobis nearest-neighbor matched sample. Panel A reports the baseline treatment effect using a combined $Post \times Treat$ indicator that pools M&A and buyout events. Panel B estimates separate treatment effects for M&A events and private equity buyouts.

The dependent variables are the logarithm of subsidy dollar amount plus one in column (1), subsidy dollar amount in columns (2) and (3), subsidy transaction count in column (4), and an indicator equal to one for positive subsidy receipt in column (5). Columns (1), (2), and (5) are estimated using OLS with fixed effects, while columns (3) and (4) are estimated using PPML.

The event window is 11 years, from $t-5$ to $t+5$, with $t=0$ defined as the year of acquisition or buyout. $Post$ is a dummy variable equal to one for post-treatment years and zero otherwise. $Treat$ is a dummy variable equal to one for treated firms and zero for matched control firms. In Panel B, $Post_ma \times Treat_ma$ captures the M&A treatment effect, while $Post_lbo \times Treat_lbo$ captures the buyout treatment effect. All specifications include firm-by-cohort and industry-year-by-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Combined firm subsidies

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post×Treat	-1.353***	-207797.500***	-2.055***	-0.800***	-0.108***
	(-4.12)	(-2.96)	(-3.25)	(-3.15)	(-4.18)
Observations	4,438	4,438	856	856	4,438
R^2 / Pseudo R^2	0.710	0.635	0.860	0.256	0.710
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Panel B: Combined firm subsidies: Separate dealtypes

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma×Treat_ma	-1.370***	-203786.800***	-2.092***	-0.821***	-0.110***
	(-4.04)	(-2.74)	(-3.21)	(-3.16)	(-4.15)
Post_lbo×Treat_lbo	-1.109	-265583.500**	0.190	-0.230	-0.089
	(-1.03)	(-2.00)	(0.12)	(-0.20)	(-0.91)
Observations	4,438	4,438	856	856	4,438
R^2 / Pseudo R^2	0.710	0.635	0.860	0.256	0.710
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes
F-test: Post_ma = Post_lbo	0.06	0.17	1.77	0.25	0.04
p-value	0.815	0.682	0.183	0.619	0.838

SE adjusted for clustering at firm level

Table B.4: **Effect of buyout and acquisition on firm subsidies: stacked robustness regressions, 7-year window**

This table shows stacked DiD regression results using merged target and buyer firms as the treatment group. Panel A shows the test using a $Post \times Treat$ variable that does not distinguish between the types of acquisition event, and Panel B shows the test with merger acquisitions and private equity leverage buyouts separated as two event types.

The dependent variables are a logarithmic of subsidy dollar amount plus one in column (1), winsorized dollar amount of subsidies in columns (2) and (3), Count of number of subsidy transactions in column (4), and a dummy indicator that equals one for non-zero subsidies in column (5). A linear regression model with fixed effects is used in columns (1), (2), and (5), while columns (3) and (4) use a Poisson regression model with fixed effects.

The event window is 7 years, from t-3 to t+3, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Combined firm subsidies

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post×Treat	-1.306***	-94723.102**	-0.473***	-0.658***	-0.108***
	(-6.69)	(-2.50)	(-2.60)	(-5.34)	(-6.91)
Observations	159946	159946	89197	89197	159946
$R^2/PseudoR^2$	0.471	0.438			0.474
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Panel B: Combined firm subsidies: Separate dealtypes

	(1)	(2)	(3)	(4)	(5)
	Ln(Sub+1)	Subsidy($)	Subsidy($)	Count	D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma×Treat_ma	-1.499***	-113102.671***	-0.504***	-0.676***	-0.123***
	(-6.91)	(-2.70)	(-2.68)	(-5.28)	(-7.06)
Post_lbo×Treat_lbo	-0.204	10165.347	-0.138	-0.377	-0.022
	(-0.52)	(0.12)	(-0.21)	(-0.83)	(-0.74)
Observations	159946	159946	89197	89197	159946
$R^2/PseudoR^2$	0.471	0.438			0.474
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table B.5: **Effect of buyout and acquisition on firm subsidies: stacked robustness regressions, relaxed target and buyer size difference: 1% to 100 times size of target**

This table shows stacked DiD regression results using merged target and buyer firms as the treatment group. Panel A shows the test using a $Post \times Treat$ variable that does not distinguish between the types of acquisition event, and Panel B shows the test with merger acquisitions and private equity leverage buyouts separated as two event types.

The dependent variables are a logarithmic of subsidy dollar amount plus one in column (1), winsorized dollar amount of subsidies in columns (2) and (3), Count of number of subsidy transactions in column (4), and a dummy indicator that equals one for non-zero subsidies in column (5). A linear regression model with fixed effects is used in columns (1), (2), and (5), while columns (3) and (4) use a Poisson regression model with fixed effects.

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Combined firm subsidies

	(1) Ln(Sub+1)	(2) Subsidy($)	(3) Subsidy($)	(4) Count	(5) D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post×Treat	-1.105***	-62689.511**	-0.310**	-0.520***	-0.093***
	(-7.35)	(-1.96)	(-2.44)	(-5.93)	(-7.78)
Observations	248368	248368	167288	167288	248368
R^2	0.411	0.389			0.412
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Combined firm subsidies: Separate dealtypes

	(1) Ln(Sub+1)	(2) Subsidy($)	(3) Subsidy($)	(4) Count	(5) D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma×Treat_ma	-1.201***	-73050.764**	-0.327**	-0.528***	-0.100***
	(-7.58)	(-2.15)	(-2.52)	(-5.91)	(-7.96)
Post_lbo×Treat_lbo	-0.073	49586.323	0.090	-0.275	-0.013
	(-0.18)	(0.62)	(0.15)	(-0.65)	(-0.41)
Observations	248368	248368	167288	167288	248368
R^2	0.411	0.389			0.412
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Table B.6: **Effect of Buyouts and Acquisitions on Firm Subsidies: Excluding Post-COVID Years**

This table reports stacked DiD regression results after excluding observations from 2020 through 2022. Panel A reports the baseline treatment effect using a combined $Post \times Treat$ indicator that pools M&A and buyout events. Panel B estimates separate treatment effects for M&A events and private equity buyouts.

The dependent variables are the logarithm of subsidy dollar amount plus one in column (1), subsidy dollar amount in columns (2) and (3), subsidy transaction count in column (4), and an indicator equal to one for positive subsidy receipt in column (5). Columns (1), (2), and (5) are estimated using OLS with fixed effects, while columns (3) and (4) are estimated using PPML.

The event window is 11 years, from $t-5$ to $t+5$, with $t=0$ defined as the year of acquisition or buyout. $Post$ is a dummy variable equal to one for post-treatment years and zero otherwise. $Treat$ is a dummy variable equal to one for treated firms and zero for control firms. In Panel B, $Post_ma \times Treat_ma$ captures the M&A treatment effect, while $Post_lbo \times Treat_lbo$ captures the buyout treatment effect. All specifications include firm-by-cohort and industry-year-by-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Combined firm subsidies

	(1) Ln(Sub+1)	(2) Subsidy($)	(3) Subsidy($)	(4) Count	(5) D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post×Treat	-1.348***	-98139.079**	-0.517**	-0.761***	-0.112***
	(-6.42)	(-2.50)	(-2.50)	(-5.38)	(-6.72)
Observations	278,801	278,801	182,526	182,526	278,801
R^2 / Pseudo R^2	0.427	0.415	0.575	0.233	0.426
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Panel B: Combined firm subsidies: Separate dealtypes

	(1) Ln(Sub+1)	(2) Subsidy($)	(3) Subsidy($)	(4) Count	(5) D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma×Treat_ma	-1.458***	-113277.923***	-0.554**	-0.759***	-0.120***
	(-6.26)	(-2.67)	(-2.57)	(-5.17)	(-6.47)
Post_lbo×Treat_lbo	-0.632*	341.358	-0.146	-0.797*	-0.058**
	(-1.66)	(0.00)	(-0.22)	(-1.69)	(-2.09)
Observations	278,801	278,801	182,526	182,526	278,801
R^2 / Pseudo R^2	0.427	0.415	0.575	0.233	0.426
IndustryYearCohortFE	Yes	Yes	Yes	Yes	Yes
FirmCohortFE	Yes	Yes	Yes	Yes	Yes
F-test: Post_ma = Post_lbo	3.46*	1.12	0.34	0.01	3.55*
p-value	0.063	0.290	0.561	0.939	0.060

SE adjusted for clustering at firm level

Table B.7: **Effect of buyout and acquisition on firm subsidies: Baseline TWFE robustness regressions**

This table shows TWFE DiD regression results using merged target and buyer firms as the treatment group. Panel A shows the test using a $Post \times Treat$ variable that does not distinguish between the types of acquisition event, and Panel B shows the test with merger acquisitions and private equity leverage buyouts separated as two event types.

The dependent variables are a logarithmic of subsidy dollar amount plus one in column (1), winsorized dollar amount of subsidies in columns (2) and (3), Count of number of subsidy transactions in column (4), and a dummy indicator that equals one for non-zero subsidies in column (5). A linear regression model with fixed effects is used in columns (1), (2), and (5), while columns (3) and (4) use a Poisson regression model with fixed effects.

The event window is 11 years, from t-5 to t+5, with t=0 being the year of acquisition or buyout. Post is a dummy variable equal to one for the post-treatment years and zero for other years. Treat is a dummy variable equal to one for acquired firms and zero for control firms. All models include firm-cohort and year-industry-cohort fixed effects. Regression constants are not reported. Standard errors are adjusted for clustering at the firm level, and t-statistics are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Combined firm subsidies

	(1) Ln(Sub+1)	(2) Subsidy($)	(3) Subsidy($)	(4) Count	(5) D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post×Treat	-1.190***	-120146.931***	-0.559***	-0.601***	-0.096***
	(-6.55)	(-3.40)	(-3.17)	(-5.00)	(-6.71)
Observations	56203	56203	42970	42970	56203
$R^2/PseudoR^2$	0.373	0.363	0.539	0.239	0.371
IndustryYearFE	Yes	Yes	Yes	Yes	Yes
FirmFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

Panel B: Combined firm subsidies: Separate dealtypes

	(1) Ln(Sub+1)	(2) Subsidy($)	(3) Subsidy($)	(4) Count	(5) D_sub
Model Type	*OLS*	*OLS*	*PPML*	*PPML*	*OLS*
Post_ma×Treat_ma	-1.345***	-142730.033***	-0.596***	-0.619***	-0.107***
	(-6.66)	(-3.60)	(-3.22)	(-4.89)	(-6.75)
Post_lbo×Treat_lbo	-0.331	5254.134	-0.154	-0.368	-0.031
	(-0.90)	(0.08)	(-0.28)	(-1.02)	(-1.11)
Observations	56203	56203	42970	42970	56203
$R^2/PseudoR^2$	0.373	0.363	0.539	0.239	0.371
IndustryYearFE	Yes	Yes	Yes	Yes	Yes
FirmtFE	Yes	Yes	Yes	Yes	Yes

SE adjusted for clustering at firm level

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