Three Essays in Empirical Finance

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Dedicated to my family and friends

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Introduction

Governance and regulatory factors are critical drivers of firm performance and productivity. External factors, such as government regulations, establish the framework within which firms operate, shaping the broader economic environment. Meanwhile, internal factors, such as corporate governance, are essential for optimizing firm operations and maximizing shareholder value. Together, these elements influence the strategic decisions, such as innovation, and the overall effectiveness of firms in the economy.

Additionally, with the recent emphasis on the role of firms and investors in mitigating climate change and its impacts, new climate and environmental regulations are being enacted in nearly every major industrial economy. Consequently, firms are now expected to address shareholder concerns while also operating in an environmentally and socially responsible manner. However, there is limited understanding of how firms manage their strategic decisions to simultaneously maximize shareholder value and pursue sustainability goals.

In this thesis, I empirically investigate three fundamental questions in this regard: First, do government regulations significantly impact firm value and their economic performance? Second, how is the innovation output of firms affected when key employees invest their personal wealth in early-stage firms? Third, how do internal strategic decisions regarding corporate resource allocation influence the emission behavior of firms participating in the European Union Emissions Trading System?

More specifically, in the first chapter, using the election of Donald Trump as President of the United States as an unexpected event signaling potential future deregulation, I found that firms in the most regulated industries gained about $25 million more than firms in the least regulated industries in the 10 trading days following the election. This suggests that the market expected deregulation to bring approximately $27 billion more benefits to highly regulated firms compared to less regulated ones. I further explore the economic mechanisms driving this stock price reaction, providing evidence that more regulations particularly hamper high-growth firms. Additionally, consistent with regulatory capture, I find that more regulations benefit incumbent firms as well as firms that are politically connected.

In the second chapter, we create a novel dataset linking angel investors to their corporate employers, driven by the observation that many angel investors are simultaneously employed by for-profit firms. We refer to these individuals as "angel employees." Using this novel data, we find that angel employees negatively impact the innovation output of their corporate employers, while positively influencing start-up success. The negative effect is more pronounced when the incentives for angel investments are higher. Our findings suggest that angel employees trade off time and effort between their employer and their personal start-up investments.

In the third chapter, I investigate the internal carbon markets operated by firms within the European Union's Emissions Trading System (EU ETS). First, I establish the active use of these internal markets by demonstrating that the volume of carbon permits transacted internally is ten times higher than what would be expected from random matching of traders in the market. I provide additional evidence that firms use internal transactions strategically. Furthermore, I find that firms with internal carbon markets become more carbon-intensive following a policy change that makes carbon permits scarcer and more valuable, indicating cross-subsidization within internal markets. My analysis reveals that internal resource decisions can undermine the effectiveness of market-based climate policies, even without considering carbon leakage. These findings underscore the importance of reporting internal carbon permit pricing and suggest that aligning internal transfer prices with market prices could mitigate this issue.

Overall, my thesis delves into the significant impacts of environmental regulations and governance issues on firms across diverse settings. I demonstrate that stringent regulations tend to disadvantage high-growth firms while favoring incumbent firms by reducing competitive pressures. Concurrently, market-based environmental regulations, such as emissions trading systems, prove effective in reducing firms' emission levels. However, their effectiveness is compromised by strategic internal resource reallocation within firms. Additionally, I highlight how employees' conflicts of interest can undermine firms' innovation output. These findings collectively underscore the complex interplay between regulatory frameworks, internal strategic decisions, and employee incentives in shaping firms' environmental and innovative outcomes.

2

Impact of Regulations on Firm Value: Evidence from the 2016 U.S. Presidential Election

Santanu Kundu [1]

Abstract: Using the 2016 US presidential election result as a shock to the expectations about the future regulatory environment, I find that most regulated firms earned approximately 4% higher cumulative abnormal stock returns than least regulated firms during the first 10 trading days after the election. Exploring economic mechanisms, I find evidence consistent with the explanation that more regulations disproportionately harm high-growth firms and allow incumbent firms to extract rents through lower competition and political favoritism. Stock returns are also followed by a shift in firm fundamentals over the three years after 2016, consistent with the economic mechanisms.[2]

Keywords: Regulation, Event Studies, Growth Opportunities, Competition, Political Economy

[*This chapter is based on a previously published paper (see footnote 2)*]

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[2]An adapted version of this chapter has been published. See Kundu. (2023). Impact of Regulations on Firm Value: Evidence from the 2016 U.S. Presidential Election. *Journal of Financial and Quantitative Analysis*, pp. 1-33, In Press. When citing the chapter, please cite the published paper.

2.1 Introduction

It is widely believed that there has been a significant increase in government regulations over the past decades, both in the United States (US) and around the world (Shleifer (2005), Davis (2017)). The increasing pervasiveness of the regulatory state in the US is also reflected in the page count of the Code of Federal Regulations (CFR), which has increased from 20,000 in 1950 to almost 180,000 in 2016 (Davis (2017)). Presumably, the regulatory environment also changes when different political parties assume power at the White House, with Republican presidents likely to be less in favor of government regulations than Democratic presidents. Unsurprisingly, companies spend billions of dollars developing political connections (such as by lobbying policymakers, making campaign contributions, etc.) to relax regulatory burdens on them and thereby maximize shareholder value. However, it is not clear whether changes in regulations themselves impact firm value. For example, it is not clear whether positive returns to political connections and lobbying are due to changes in regulations or less enforcement of regulations (Yu and Yu (2011)), or due to government support in other forms (Duchin and Sosyura (2012)). In this study, I attempt to fill this gap by providing empirical evidence of the effect of expected regulatory changes on firm value.

A priori, it is not clear how regulatory changes impact firm value. On the one hand, an expansive regulatory environment can benefit incumbent firms by increasing their innovativeness over the long term (Porter (1991)), shielding incumbent firms from competition (Djankov et al. (2002), Gutiérrez and Philippon (2019)), and by enabling incumbent firms to capture the regulators (Stigler (1971), Posner (1974)). In this case, deregulation in the future could harm the value of incumbent firms today. On the other hand, an increase in regulations can impose an additional burden on firms by increasing their compliance costs, limiting their growth (Djankov et al. (2006), Dawson and Seater (2013)), and reducing their innovativeness compared with other less regulated firms (Jaffe et al. (1995)). Such a line of argument suggests that future deregulation benefits the shareholders of incumbent firms.

Various empirical challenges have inhibited scholars from exploring the above possibilities. First, regulation is an abstract concept that is difficult to quantify. Additionally, the incidence of regulatory burdens is largely unobserved for the vast majority of firms in the economy. Many studies consider that certain industries, such as the utility and finance industries, are regulated. However, firms in other other industries may be equally or more strictly regulated, such as firms in the pharmaceutical industry or manufacturing related industries. Jaffe et al. (1995) and Berman and Bui (2001) highlight some of these issues. Second, and perhaps most importantly, regulatory changes take effect over long periods involving public debates and votes. Thus, it becomes difficult to identify when economic agents incorporate expected changes in regulations into their actions (Binder (1985)).

In this study, I use an empirical setting that addresses these empirical challenges. First, in line with studies such as Snowberg et al. (2007), assuming stock markets are efficient in processing forward-looking information, I use the short-term abnormal price reaction of a firm's listed common equity to measure the expected economic benefits of future lower regulations that investors expect to accrue to the firm. However, this approach only allows for valid inference if the date of changes in expectations of investors about the future cash flows is known reasonably accurately. Hence, I use the 2016 US presidential election result as an exogenous shock to the expectation about the future federal regulatory policy in the US. As noted in Wagner et al. (2018a), Wagner et al. (2018b), and Child et al. (2020), Trump's election was largely unexpected. Such an unexpected nature of the event helps us to capture a more precise time for the change in the market's expectations. Additionally, deregulation was one of the top priorities of President Trump's election campaign; during his campaign, he published his "Contract with the American Voter", which specifically mentions rolling back two regulations for every new federal regulation introduced and reducing the role of government in the economy by imposing hiring freezes and budget cuts (Belton et al. (2017)). Finally, I use a novel measure of industry-specific regulation from the RegData database, as developed by Al-Ubaydli and McLaughlin (2017), to assign firms to different groups depending on the level of regulatory restrictions of their three-digit North American Industry Classification System (NAICS) based industries.

Firms in the most regulated industries earned approximately 4% higher cumulative abnormal stock returns (adjusted by the Fama−French five factors and the momentum factor) during the first 10 trading days after the election results were declared than firms in the least regulated industries. This was equivalent to $25 million for an average firm (roughly $27 billion on aggregate) in the most regulated industries. These results are obtained after controlling for various firm characteristics (i.e., market capitalization, illiquidity, capital structure, expected tax rates, import dependency of an industry, political connections, and size), which account for potential omitted variables that could also shape investors' expectations around Trump's election. For example, Wagner et al. (2018a) document that investors expected lower tax burdens for firms after Trump's election. Following similar logic, controls such as import dependence and exposure to government spending in an industry account for a possible shift in investors' expectations related to Trump's foreign trade policy and infrastructure spending, respectively. Additionally, the specifications take into account state fixed effects and broader industry fixed effects. Hence, I account for any state-level or broader industry-wide unobserved factors that may potentially bias my estimates. To understand how the fixed effects might be useful in accounting for potential omitted variable bias, let us consider the case of the construction industry (with the two-digit NAICS code of 23). It is plausible that Trump's ambitious plans for infrastructure spending could have been good news for investors in the construction sector. But, I employ a NAICS two-digit industry fixed effect that compares firms within the construction sector that are allocated to differently regulated granular industries.

Although, I control for several firm characteristics, its is still possible that Trump's election changed investors' expectations about unobserved factors other than regulations. For example, the current specifications cannot account for *within*-industry heterogeneity that is also correlated with the measure of regulation and a shift in investors' expectation after Trump's election. Let us consider the utilities sector (having the three-digit NAICS code of 221). Within this sector, Trump's election may have been a positive shock to more polluting (fossil fuel-based) power producers and a negative shock to less polluting (renewable) power producers. I show below that the results remain unchanged after accounting for environmental regulations, firm-specific pollution or climate responsibility (Ramelli et al. (2019)). Furthermore, I also document that the results are robust to alternative specifications accounting for event-time clustering. While I acknowledge that it is difficult to rule out all possible alternative explanations, the robustness of the results alleviates such concerns to a large extent. Additionally, the results are an intention-to-treatment effect, i.e., the abnormal reaction of the stock price is in expectation of deregulation in the future and not of deregulation itself.

I provide three potential economic channels through which investors might have expected most regulated firms to benefit. First, existing empirical evidence suggests that regulation affects economic growth by imposing various costs on doing business. For example, Alesina et al. (2005), Djankov et al. (2006), Dawson and Seater (2013), Pizzola (2018) and Gutiérrez and Philippon (2019) and many others show that increased government regulation impedes economic growth by imposing costs on firms capitalizing on their growth opportunities. Hence, deregulation can be beneficial for high-growth firms, *especially* firms that are strictly regulated. I find evidence confirming the existence of this channel. That is, the positive returns of firms in the most regulated industries are driven by firms with ex-ante higher growth opportunities, as measured by Tobin's Q. For example, a one standard deviation higher Tobin's Q was associated with an approximately 1.5% increase in abnormal stock returns by the end of the 10-day event window for firms in the most regulated industries, compared with those in the least regulated industries.

Second, it is well established that deregulation leads to an increase in competition. Early theories of regulation (Stigler (1971), Posner (1974)) suggest that one of the purposes of regulation is to thwart competition within each industry by "capturing" the regulators. As such, regulation serves the purpose of incumbents in an economy. Hence, significant deregulation can increase competition and thus affect the economic rents gathered by regulated industry incumbents. Thus, if deregulation is expected, firms facing low competition ex-ante would likely lose the most in future. By using the text-based product market concentration measure developed by Hoberg and Phillips (2016), I find that the valuation of firms in the most regulated industries *and* with ex-ante high product market concentration is negatively affected during the event window. That is, during the 10-day event window, for the most regulated firms, a one standard deviation higher industry concentration was associated with an approximately 2.3% decrease in firm valuation relative to that of the least regulated firms.

Third, I find evidence consistent with political favoritism existing in regulatory policies. Political scientists have long noted that economic regulation is often moderated by the political environment of the regulated entities (Short (2019)). For example, a Republican federal government might be more inclined to enact less stringent regulations and/or impose laxer regulatory standards for firms located in Republican states than for firms located in Democrat states (Asher and Novosad (2017)). A decrease in overall regulations would make such favoritism less valuable for firms in Republican states. Consistent with this literature, I find that firms incorporated in states with more Republican Members of Congress gained value after the election. In contrast, in line with the intuition that political favoritism is less valuable when deregulation is expected, firms that were in the most regulated industries and incorporated in Republican states experienced a negative abnormal stock return of approximately 4.1% during the 10-day event window compared with firms in the least regulated industries. These results show that political favoritism of regulators is one of the major mechanisms enabling incumbent firms to derive benefit for their shareholders.

In the last set of analyses, I study whether the documented initial stock-price reactions were followed by changes in firm fundamentals over the next few years. I perform a difference-in-differences type analysis over the sample period 2014−2019. I investigate whether firms in the most regulated industries, and firms with ex-ante higher growth opportunities, lower competitive threat, and in Republican states in the most regulated industries experienced any changes in their profitability, cash flow, sales, and sales growth during 2017−2019 compared with 2014−2016 *and* compared with other firms. I do find some evidence consistent with the initial stock price reaction. Although not invariably statistically significant, the evidence suggests there was a shift in the firm fundamentals of the most regulated firms during the Trump presidency, which lends additional support to the economic mechanisms documented above.

The primary contribution of this paper is its documenting of evidence suggesting that changes in the federal regulatory environment have a first-order impact on firm value for firms in the most regulated industries. There is a literature on political connections and how they impact firm valuation (e.g. Faccio (2006), Ferguson and Voth (2008), De Figueiredo and Richter (2014), Akey (2015), Borisov et al. (2016), Child et al. (2020), and many others). However, most of these studies do not explicitly examine how regulated firms react to expected regulatory changes conditional on being politically connected. For example, Akey (2015) discusses anecdotal evidence suggesting there are regulatory benefits to firms but does not examine the impact of changes in regulations themselves. In a similar vein, Child et al. (2020) provide evidence on how S&P500 firms connected to Trump benefited from lower regulatory actions and penalties against them but do not examine how changes in overall regulations affected firm valuations. I complement this strand of literature by documenting direct evidence that the expectation of a more lenient federal regulatory environment had a positive impact on firm valuation, particularly that of the most regulated firms.

Second, many recent studies document the impact of regulation on broader macro-economic outcomes. For example, Dawson and Seater (2013) examine the trend of US regulations and GDP growth, Gutiérrez and Philippon (2019) examine the sensitivity of firm entry based on the growth rate of its industries and how regulations moderate such sensitivities, and Simkovic and Zhang (2020) investigate innovativeness of new entrants based on regulations. I complement these studies and the earlier studies on regulatory changes by Schwert (1981) and Binder (1985) by providing direct firm-level evidence on how the expectation of a more lenient regulatory federal environment impacts incumbent firms.

The remainder of this paper is organized as follows. Section 2.2 describes the institutional setting and data used in this study, section 2.3 provides the main empirical results; section 2.4 provides evidence on various economic channels; section 5 provides evidence on real effects based on firm fundamentals; and section 6 concludes.

2.2 Institutional Setting, Data and Methodology

2.2.1 Institutional Setting

One of the reasons why this study focuses on Trump's election is that lowering federal regulations was a key platform of his election campaign. Consistent with his election campaign promise, immediately after assuming office, President Trump signed Executive Order 13771, entitled "Reducing Regulation and Controlling Regulatory Costs" which directed agencies to implement his campaign promise, as published in his "contract". According to the Office of Information and Regulatory Affairs and data collected by the Regulatory Studies Center of George Washington University, the number of "major" rules[3] published during the first three years of Trump's presidency was one of the lowest in the past twenty-five years. Figure 2.1(a) demonstrates this point. It plots the number of major rules per year from 1994 to 2019. The black bars indicate the years of the Trump administration. Additionally, in Table A2, I formally document that the number of such rules passed during the Trump presidency between 2017 and 2019 was 60% $(= exp(0.48) - 1)$ - 80% $(= exp(0.60) - 1)$ lower than in other years from 1981 to 2019. This finding is consistent with the report from Belton and Graham (2019), which shows that there was slow but somewhat effective progress on deregulation under the Trump administration by the end of 2018 with 514 deregulatory rulemakings outstanding across various agencies. Moreover, the Trump administration kept a vast majority of the administrative positions empty as an indirect way to reduce the burden of regulations (Heidari-Robinson (2017)).

Was Trump's focus on lower regulations unique? It is believed that Republicans are much more against regulation than Democrats. For example, in a December 2016 Pew Survey, 71% of Republican-oriented respondents agreed that government regulation of business is harmful, compared with 31% of Democrats.[4] Hence, regulation might have been an equally important topic of discussion during other Republican presidential campaigns. However, I document below that this was not the case.

First, although columns (2) and (4) of Table A2 suggest that compared with Democratic administrations, Republican administrations are associated with approximately 20%$(= exp(0.192)-1)$ fewer major rules being passed, this is not statistically significantly different from the number of major rules passed by the Democratic administrations from 1981 to 2019. However, the number of major rules that were passed during the Trump administration was 60% lower than during other administrations, even after controlling for Republican presidencies in general. Second, to further establish this point, I collect English-language news articles from Factiva mentioning the word "regulation" related to the subject of national "Presidential Elections" in the United States during the Bush (2000) and the Trump (2016) presidential campaigns. I focus on the 2000 election as this election was also closely contested and resulted in a Republican

[3] A major rule is defined as "one that has resulted in or is likely to result in (1) an annual effect on the economy of $100 million or more; (2) a major increase in costs or prices for consumers, individual industries, federal, state, or local government agencies, or geographic regions; or (3) significant adverse effects on competition, employment, investment, productivity, or innovation, or on the ability of the United States-based enterprises to compete with foreign-based enterprises in domestic and export markets."

[4] See: https://www.pewresearch.org/politics/2016/12/08/3-political-values-government-regulation-environment-immigration-race-views-of-islam/

president taking office. On average, there were 150 more articles in a given month mentioning the word "regulation" during the months leading up to the general election of 2016 than during the months leading up to the general election of 2000. Figure 2.1(b) shows the difference in the number of articles on a logarithmically-transformed scale in the months leading up to the election in each election year (i.e., a comparison of January to October in 2000 with this period in 2016). As can be seen, on average, during this period, in 2016 there were three times as many news articles mentioning the word "regulation" than in 2000. In unreported results, I find that these estimates are statistically significant at the 1% level even after taking into account a month-of-the-year fixed effect.

Hence, the Trump administration indeed followed a more lenient policy on enactment and enforcement of regulations than other Republican and Democratic administrations. These results indicate that decreasing regulatory restrictions was one of the main platforms of the Trump election campaign and was a greater focus of discussion than during another closely contested presidential election campaign. Hence, the Trump election is analyzed as the cleanest setting in which a credible signal about possible deregulation in the future was sent to investors.

2.2.2 Regulation Data and Measurement

Measurement of regulation is in itself a complicated exercise. Some research in this field measures regulatory penetration using page counts of the Code of Federal Regulations (hereinafter, CFR), as in Dawson and Seater (2013).[5] Similarly, Mulligan and Shleifer (2005) use file size data of legislation in the US as a proxy for regulatory intensity. Becker and Mulligan (1999) use different measures of regulation such as Congressional committee size and regulatory costs of various federal agencies as a percentage of GNP. However, for my cross-sectional test, I need a measure of regulation that differentiates between groups of firms based on their regulatory intensity. Hence, I use a recently devised NAICS three-digit industry−specific measure of regulation that is based on textual analysis of the CFR (Al-Ubaydli and McLaughlin (2017)). The data are provided by the RegData project of Mercatus Center of George Mason University.[6]. For an elaborate discussion of the measure, I refer the reader to Al-Ubaydli and McLaughlin (2017). I group industries in quartiles based on their regulatory restrictions. Table 1 provides examples of industries in each quartile.

[Insert Table 1 here]

The above table shows that the Al-Ubaydli and McLaughlin (2017) method largely aligns the industries in a way that one would expect ex-ante. For example, most of the industries in the top quartile are likely to have higher regulations than the industries in other quartiles. For example, utilities, petroleum and coal products manufacturing, and chemical manufacturing are classified as some of the most regulated industries. This distribution of industries gives sufficient confidence that the method can be applied for further analysis.

[5] CFR contains the most recent relevant regulations and is updated each year in four waves.

[6] I thank the Mercatus Center at the George Mason University and Omar Al-Ubaydli and Patrick A. McLaughlin for making the data available. Several recent studies use these data. For example, Gutiérrez and Philippon (2019), Coffey et al. (2020)

2.2.3 Other Financial Data

Daily stock returns data are obtained from the Center for Research in Security Prices (CRSP) and the accounting variables are taken from Compustat. The Fama and French (2015) factors and the momentum factors are obtained from Kenneth French's website. The industry definitions of the NAICS are taken from the United States Census Bureau website. As control variables, I use variables that have been previously found to predict stock returns. Amihud (2002) shows that the illiquidity measure is related to stock returns through an illiquidity premium. The daily Amihud (2002) illiquidity measure is calculated using the absolute return scaled by the total dollar volume of shares traded on that day. I also control for firm size by taking the logarithm of total assets for the fiscal year ending 2015 and the market capitalization of a firm as of at the beginning of the event window. I obtain the data for the fiscal year ending 2015 from Compustat. Furthermore, as Bhandari (1988) and Welch (2004) find that a firm's stock returns are related to capital structure reflected by its debt−equity ratio, I also control for each firm's debt−equity ratio in the empirical set-up. In addition, I control for expected cash tax rates as these are shown to have been an important driver of stock returns around the 2016 US presidential election (Wagner et al. (2018a)). I calculate one-year expected (effective) cash tax rates as given by equation (1).

$$EXPECTED\_CASH\_TAX\_RATE_i = CASH\_TAX\_PAID_i/(PRETAX\_INCOME_i - SPECIAL\_ITEMS_i) \tag{2.1}$$

I use the one-year effective cash tax rate as a proxy for future tax rates as it is shown to predict future tax rates better than a ten-year tax rate (Wagner et al. (2018a)). However, in unreported results, I find that the main conclusions of the study remain unaffected if I use the long-term (ten year) average cash tax rates as a proxy for expected effective tax rates.

Another source of omitted variable bias could be import dependence if firms with high import dependence also have higher regulatory restrictions. It is difficult to find firm-level data on imports. However, I collect import data for three-digit NAICS industries from the United States International Trade Commission (USITC). Additionally, I control for whether an industry has government exposure, based on Belo et al. (2013). This allows me to account for other possible Trump policies such as infrastructure and defense, which might confound with the regulatory burden of firms.

RegData provides regulatory restriction scores for 54 NAICS three-digit industries. Hence, I discard firms that are not in these 54 industries. I retain the daily returns data from CRSP for the common shares of the firms (i.e., stock returns belonging to share codes 10 and 11). I also drop firms that are incorporated outside the US. Additionally, I exclude firms allocated NAICS code 525 (funds, trusts, and other financial vehicles) and 35 firms that changed their NAICS code during the study. This procedure leads to a final sample of 2,413 firms, which are used in this study.

2.2.4 Other Non-Financial Data

Together with the presidential election, 435 congressional districts for the House of Representatives and 34 out of 100 senate seat elections were held on the same day in 2016. Hence, I collect the US members of Congress from each state as of the election date and their respective party affiliation by using election results data from MIT Election Lab.[7] Additionally, I control for political connections in all of my regression specifications. I measure political connection based on donations to the Trump campaign from firms via their Political Action Committees (PACs), or through individual contributions, or based on the lobbying expenditure of firms during the three years before 2016. I collect these data from the Center for Responsive Politics. Political connections are particularly important as compared with non-highly regulated industries, highly regulated industries could be expected to have more political connections to lower their regulatory burden. To explicitly control for such omitted variables that may affect the results, I control for political connections for each firm *within* each regulatory quartile. Finally, I obtain product market competition data from Hoberg and Phillips (2016).[8]

Table 2 presents a brief overview of how the main variables used in this study are distributed across different regulation quartiles. As can be seen, the regulated firms in the fourth quartile tend to have lower liquidity than those in other quartiles. However, there is no apparent difference in the size, debt−equity ratio, and cash tax rates of firms across quartiles. In addition, the average gross return of firms during the 10-day event window is higher for firms in the most regulated industries than for firms in other industries. This gives an initial indication that the most regulated firms had a more positive price reaction to their firm value than the other firms.

[Insert Table 2 here]

For some of my analyses, I use the climate responsibility of firms by using data from the MSCI KLD database. I use the scores related to the environmental strengths and concerns as covered in this database following Ramelli et al. (2019). Climate responsibility for a firm is calculated by subtracting environmental concerns from environmental strengths, as reported by the MSCI KLD database.[9]

2.2.5 Empirical Strategy

I employ an event study methodology for this analysis. I take November 9, 2016, as the first day after the event (the election results were declared on November 8) when the market starts as the beginning of the event window. For each firm in my sample, I use a 252-day estimation window that ends 30 days before the event. I calculate the Fama-French five factors and momentum-adjusted cumulative abnormal returns (CARs) for each firm over the event window. I use multiple definitions of the event window: 1, 5, and 10 trading days after the event. I examine a short term (maximum of 10 trading days) period for two reasons. First, as mentioned in Fama (1998), the "bad-model problem" is limited over shorter horizons. Second, as noted in Wagner et al. (2018a), the reaction of stock prices to policy changes becomes increasingly

[7]See: https://electionlab.mit.edu/
[8] I thank Gerard Hoberg and Gordon Phillips for making these data available.
[9] I thank Stefano Ramelli and Alexander Wagner for sharing their data

difficult to measure over longer periods due to the inherent difficulties in distinguishing changes in economic agents' expectation from a policy change (Schwert (1981)). Some industries may be affected differently from others due to having had different expectations about policies, besides deregulation, that would be enacted by the Trump administration. For example, investors could have expected that firms in the construction industry or the defense industry would benefit from the Trump administration. Hence, I employ broader two-digit NAICS based industry fixed effects to absorb such unobserved industry-specific heterogeneity that might have driven the stock returns during this short period. I also employ state fixed effects, as states might vary with respect to their local economic conditions, industry characteristics, political economy features, and other economic policies. Moreover, as I only consider expected changes in federal regulations, employing state fixed effects helps to account for unobserved heterogeneity due to variation in expectations of the state-level regulatory environment. In all of the specifications, the standard errors are clustered at the two-digit NAICS industry level. For each event window specification, I calculate each firm's CAR and regress it on the dummy variable identifying the quartile of regulation that the firm is in (controlling for other firm characteristics). The empirical specification is formulated as below:

$$\mathrm{CAR}_{id} = \alpha + \sum_{i=2}^{4} \beta \times \mathrm{QUTILE\_}i + \gamma \times \mathbf{X}_i + \varepsilon_i + \vartheta_i + \nu_i \tag{2.2}$$

In the above equation, α is the intercept and QUTILE_i is a dummy variable that takes a value of 1 if the firm is in the i^{th} least regulated industry quartile and 0 otherwise. In the basic empirical setup, QUTILE_1 is taken as the reference category, as it indicates firms that are in the least regulated industries. I use quartiles to group firms in terms of their level of regulatory restrictions to account for any potential non-linearity of the stock−price reaction to expected deregulation. $\boldsymbol{X}$ is the vector of control variables (as discussed previously) for each firm. ε_i is the industry fixed effect, ϑ_i is the state fixed effect, and ν_i is the error term. $\mathrm{CAR_{id}}$ denotes the cumulative abnormal return for firm i until day d after the event. In this case, d takes the values 1, 5, and 10, as mentioned above.

2.3 Empirical Results

2.3.1 Regulation and Stock Returns

In this section, I first present graphical evidence of the effect of Trump's win in the US presidential election of 2016 on the stock−price reaction of firms in the most regulated industries.

[Insert Figure 2.2 here]

Figure 2.2 presents the difference in value-weighted CARs of the firms in quartile four (most regulated) and quartile one (least regulated) around the event date, i.e., November 8, 2016. As is apparent from the figure, there is a distinct increase in abnormal returns on November 9, 2016, for this "long-short" portfolio: firms in the most regulated industries on average gained approximately 3% in market value on the first day after the election outcome was announced.

This effect persisted over the following days. Hence, Figure 2.2 provides some initial evidence of the effect of expected deregulation on stock prices.

Next, I analyze this relationship more formally. Table 2.3 presents the results of estimating Equation (2.2), where I regress individual firm's CARs on QUTILE (representing which quartile of regulated industries the firm belongs to, with 2 being the second lowest and 4 being the highest) and other control variables.

[Insert Table 2.3 here]

Columns (1), (2), and (3) show the result of regressing 1-day, 5-day, and 10-day CARs of the firms on a dummy variable QUTILE_N (where N = 2,3, and 4) representing the respective regulatory quartile in which the firm is placed, based on its industry and other control variables. The asymmetric price reaction is evident from the results. Compared with firms in lower quartiles, higher quartiles have higher CARs during the 10-day event window after the election. For example, the point estimate on QUTILE_3 in column (3) suggests that firms in the third quartile had 2.2% higher CARs during the event window than the firms in the lowest quartile. CARs are highest for stocks in the fourth quartile. As suggested by the point estimate on QUTILE_4 in column (3), firms in this most regulated quartile had 3.9% higher CAR, on average, than firms in the least regulated quartile. In line with the findings of Wagner et al. (2018a), I also find that the coefficient on EXPECTED_CASH_TAX_RATE is positive and statistically significant. In columns (4) to (6), I control for political connections for firms within each regulatory quartile. This specification aids interpretation of the point estimates on the regulatory quartile dummies after accounting for political connections within each quartile. As is evident from the results, the point estimates remain almost unchanged after applying these controls. Hence, the baseline results cannot be explained by political connections of regulated firms that could potentially be jointly correlated with regulations and stock returns around the event date. I also find that industries that were import−dependent experienced lower returns than industries that were not import−dependent, as denoted by the coefficient on IMPORT_DEPENDENT. This suggests that the "America First" policy of the Trump campaign led to an increased trade−policy uncertainty.

The specification can explain approximately 9% to 12% of the variation in CARs. A 3.9% increase in abnormal returns is equivalent to approximately \$25 million (=3.9% $\times\, exp(13.35)$) for an average firm in the most regulated industries. Overall, these results show that investors expected a significant economic benefit to accrue to the firms in the most regulated industries due to expected deregulation during the Trump presidency. This result is unlikely to be driven by broader industry−level shocks as they are obtained after including two-digit NAICS fixed effects. Additionally, unobserved state-level factors that might change simultaneously on election day and are correlated with lower expected federal regulations are unlikely to drive the results, as state fixed effects are likely to account for this.

Based on a back-of-the-envelope calculation, one can back-out the true effect of deregulation based on Trump's election and his pre-election winning odds. For example, if the probability of Trump's win was p before the election and that of Hillary Clinton was $(1-p)$, then the observed firm value immediately before the election (V_1) can be written as: $V_1 = p \times DR + (1-p) \times R$, where, DR is the firm value associated with the new regulatory regime if Trump wins, and R is the firm value associated with continued regulation if Hillary Clinton wins. After the election, the market became certain about DR, i.e., p became equal to 1. Hence, after the election, the observed firm value, V_2, can be written as $V_2 = DR$. Therefore, the change in firm value, $\Delta V = V_2 - V_1$, is $(DR - R)/(1 - p)$. Hence, it follows that $DR - R$, ΔR, is the true change in firm value due to expected changes in regulation after Trump's win and that the actual firm value change associated with deregulation due to Trump's win, ΔR, is $\Delta V/(1 - p)$. As per FiveThirtyEight,[10] the probability of Donald Trump's win immediately before the election day was approximately 28%, i.e., p was equal to 0.28. ΔV can be proxied by stock returns. Hence, for the most regulated firms, the true effect on firm value (using the point estimates from Table 3) due to Trump's deregulation plan would have been approximately 5.55% (= 4%/0.72).

To interpret the result causally, one would ideally require the election win of Trump to be only associated with changes in future regulations. However, we know that this was not the case. In addition to deregulation, Trump's presidency was also associated with lower expected tax rates (Wagner et al. (2018a)), possibly more political uncertainty given his relatively unconventional nature, more lenient views on environmental and climate change policies, disproportionately larger benefits to businesses tied to him (and not to the Republican party (Child et al. (2020))), larger fiscal spendings on infrastructure and military, and more trade uncertainty because of his "America First" stance, among other things. Even though, in all of my specifications, I control for some of these confounding factors (e.g., expected tax rates, dependence on government spending of industry to account for the effect of larger fiscal spend, import dependence to account for more trade policy uncertainty, political connection to the Republican Party as well as to Donald Trump through individual and PAC donations within each regulatory quartile, a broader industry fixed effects to take into account within industry heterogeneity), I cannot rule out that an unobservable omitted variable is still biasing my estimates. Hence, in the following sub-sections, I perform a series of robustness tests to demonstrate the stability of the main findings and alleviate such concerns as much as possible.

2.3.2 Robustness Tests

2.3.2.1 Accounting for Event Date Clustering & Alternate Specifications

In the main specification, the standard errors are clustered at the two-digit NAICS industry level. This implicitly assumes that if stock returns are contemporaneously correlated in any other way, the test statistics might be biased upward. To account for such a bias, I follow Karpoff and Malatesta (1995) and estimate a seemingly unrelated regression (SUR) model. To implement this model, I calculate the value-weighted abnormal returns for a portfolio of firms belonging to each regulatory restriction quartile over event windows and analyze four time−series of returns. The event windows are to [-22,1], [-22,5] and [-22,10] corresponding to the 1-, 5- and 10-day comparisons of abnormal returns. For each regression specification, I incorporate a dummy variable that takes the value of 1 for days after the election, and 0 otherwise. I compare the coefficient on this dummy variable across the four regressions to obtain statistical inference for the SUR model.

[10]Link: https://projects.fivethirtyeight.com/2016-election-forecast/

The results of the analysis are shown in Panel A of Table 2.4. The coefficient estimate of 0.005 on QUTILE_4 in row (1) and column (1) of the table shows that firms in the fourth quartile had 0.5% higher (value-weighted) abnormal returns on the first day after the election result than on the 22 days before the election. This result is statistically significant at the 1% level. Next, the coefficient in row (1) and column (5) shows that firms in the most regulated industries earned on average 1.5% higher abnormal returns than firms in the least regulated industries. This is essentially the difference between the coefficients in column (1) and column (4) of row (1), where the test statistic is the Wald test of equality between the two coefficients based on chi-square distribution. Other coefficients in columns (1)−(4) and columns (5)−(7) can be interpreted in a similar way. In terms of economic magnitude, 1.5% is close to the 2% difference in CAR obtained in the baseline specifications for firms in the most regulated industries. Similarly, the coefficients over the 10-day event window suggests that the firms in the most regulated industries experienced 0.3% more abnormal returns *per day* than those firms in the least regulated industries.

[Insert Table 2.4 here]

Although the SUR model takes into account contemporaneous correlation of outcomes across different observation units, it cannot incorporate cross-sectional time−invariant variables. Thus, using this model, it is not possible to control for important confounding factors, such as expected tax rates, and political connections.[11]

Another important concern is the lack of a proper control group; the baseline specification invariably compares the abnormal returns of firms in highly regulated quartiles to those of firms in the least regulated quartile. Hence, if some confounding factors affect firms in different regulation quartiles asymmetrically, the point estimates in the main specification could be biased. One way of dealing with this is to compute (abnormal) returns using other world indices (excluding the US), as done in Zhang (2007). Hence, I calculate abnormal returns using a one-factor model that represents the return of global markets, aside from the US. For this purpose, I use two types of implementation. First, I calculate value-weighted total returns in dollars from the Compustat Global universe of common stocks.[12] I use these value-weighted returns as a single factor in a one-factor model to predict stock returns. I use the abnormal returns calculated from this one-factor model as the dependent variable to estimate the specification of Equation (2.2). The first two columns in Panel B of Table 2.4 present the results obtained from using this methodology. For brevity, I do not report the coefficients on the control variables. Essentially, I replicate the specifications in Table 2.3. The inference remains largely the same; firms in the most regulated quartile gained 4.3% more market value, on average, than those in the least regulated quartile during the 5 days following Trump's

[11] Additionally, the SUR model is not completely equivalent to the specification in Equation (2.2), as the SUR model compares a time-series change in CARs for a given portfolio, whereas the specification in Equation (2.2) is a cross-sectional comparison of CARs.

[12] I download all the exchange rates from Refinitiv to convert local currency to US dollars. I then apply the formula: Total Return = $((PRCCD_t/AJEXDI_t) * TRFD_t))/(((PRCCD_{t-1}/AJEXDI_{t-1}) * TRFD_{t-1})) - 1$

election.[13] In the second implementation, I use the return of the iShares All Country World Index Exchange Trade Fund (ACWX) as the factor predicting stock returns in a one-factor model. The ACWX comprises large and mid-cap stocks across the world, excluding the US. As of March 2022, the ACWX had 4.4 trillion US dollars under management. Once I calculate the abnormal returns from this model, I proceed in the same way as before, by estimating the baseline specification of Equation (2.2). Columns (3) and (4) of Panel B in Table 2.4 present the results obtained from using this specification. As can be seen, the same inference is obtained.

Yet another way of dealing with the problem of event-time clustering is to calculate the standard errors of the estimate in a different way. To this end, I follow Cohn et al. (2016). I compute 1-, 5-, and, 10-day CARs over a "nonevent" period (from October 1, 2015, to September 30, 2016). To compute the CARs, I use the same factor model with an estimation period dating back to October 1, 2014.[14] After calculating the CARs, I run the same specification as in Equation (2.2) and obtain the coefficients on the three regulatory quartile dummies. I then calculate the t-statistics by subtracting the mean point estimate over the nonevent period from the event-day point estimate and dividing the resulting difference by the standard deviation of the point estimates over the nonevent period. In the last two columns of Panel B in Table 2.4, I report the same specification as in Table 2.3 but t-statistics are calculated based on the methodology of Cohn et al. (2016). As can be seen, the results remain unaltered. Additionally, in unreported results, I double cluster standard errors across two-digit NAICS and the state of incorporation of firms. The results remain unchanged.

Finally, I use a continuous measure of regulatory restrictions instead of using quartiles. As can be seen in Table A3, the inference remains unaltered. In this regard, I note that the use of quartiles allows me to demonstrate the possible non-linear impact on firms of an expected decrease in regulations, i.e., only the most regulated firms are affected when regulations are expected to decrease. For example, in Table 3, point estimates are significant only for QUTILE_3 and QUTILE_4. Additionally, the economic significance of the point estimate for QUTILE_4 is considerably larger than that for the others. Hence, I use quartiles as my preferred specification.

In a nutshell, the results in this section suggest that unaccounted for cross-correlation of stock returns across different firms due to event−date clustering is unlikely to drive the main result. The results remain robust to using alternate (continuous) measure of regulations at the industry level.

2.3.2.2 Other Robustness Tests

First, I account for the foreign operation of firms. The focus of the Trump campaign was "Make America Great Again" which emphasized giving advantages to firms that are more

[13] However, one important caveat is worth mentioning in this approach. This approach will lead to valid inference if global stock markets are unaffected by US macroeconomic news. Given the extant literature documenting how information, especially macroeconomic news, in the US spills over to other markets (e.g., Eun and Shim (1989), Becker et al. (1990), Bongaerts et al. (2022)), the implicit assumption that other markets are unaffected by the outcome of the US election is likely to be violated.

[14] Hence, for each of the 252 days (from October 1, 2015, to September 30, 2016) I calculate CARs using a rolling event window of one year

domestically oriented. This might have affected the valuation of more domestically oriented firms differently to those of less domestically oriented firms. To perform this analysis, I collect data on firms' foreign operations from Compustat (namely, foreign sales from the segment files) and calculate the average percentage of foreign sales for each firm over the three years before 2016. When I control for foreign sales of a firm, the results remain unchanged, as shown in Table A4. To some extent, this accounts for the firm-level impact of trade policy uncertainty under the Trump administration.

Second, in Table A5, I further investigate if the results are robust after accounting for firm-specific policies aiming to become climate responsible in the future. Ramelli et al. (2019) show that firms with more responsible climate policies gained in market value after the 2016 election compared with firms having less responsible climate policies. Thus, my results might wrongly attribute the positive valuation effect of firms to expected deregulation if investors' expectations regarding climate change−related policies account for stock−price reactions. To examine this possibility, I use the climate responsibility data from Ramelli et al. (2019). As can be seen, the main results remain unaffected after accounting for such climate responsibility in my sample. Hence, the results do not appear to be biased by the possibly laxer climate change−related policies of the Trump administration.

Additionally, I do a falsification test. As this study is based on one event, it might be possible that the firms in the top quartile might have been most likely to generate such abnormal positive returns on any random date due to some unobserved characteristics that are not absorbed by industry fixed effects. In order to address these concerns, I employ a falsification test. First, during the three-year period before the election result was declared, I randomly select 252 trading days and run a similar event study around each day. If there is any bias in the regression analysis, I should find a large fraction of these randomly selected 252 days also display a statistically significant and economically similar positive reaction.

[Insert Figure 2.3 here]

Panel A of Figure 2.3 shows the distribution of regression t-statistics and Panel B shows the distribution of coefficients obtained by executing the specification of Table 2.3 pertaining to the point estimate on QUTILE_4 for 252 randomly generated dates for 1-day CARs. As can be seen, the likelihood of observing positive and statistically significant returns for an average firm in the top quartile of regulation is zero. None of the regression coefficients are significant at the 5% level and the economic magnitudes are also very small compared with those reported in Table 2.3. While the actual estimate from Table 2.3 is approximately 2.0%, the maximum estimate obtained in the falsification tests is 0.4%. That is, on any given year (consisting of approximately 252 trading days), the likelihood of any random day generating an abnormal return for the firms in the topmost quartile of regulation is zero.

Overall, this section documents that explanations other than the expectation of deregulation post−election day do not seem to alter the inference of the main result. Thus, while the election is not a perfect instrument for expected deregulation, the extensive robustness tests described above alleviate concerns that other omitted variables drive both the election result and the abnormal returns for the most regulated firms and provide suggestive evidence of a causal impact of expected deregulation after Trump's 2016 election win.

2.3.3 Event Study Around Shift in Trump's Odds of Winning

In this section, I investigate whether the stock price of firms in the most regulated industries behaved in the same way when Trump's probabilities of winning changed during the months prior to the election. I conduct an event study in the same way as before around July 30, 2016, when, according to the analytics website FiveThirtyEight, Donald Trump had 50.1% probability of winning compared with 49.9% for Hillary Clinton.[15] According to FiveThirtyEight, this was the only date on which Trump had a higher probability of winning than Hillary Clinton leading up to the presidential election. While the gap between Clinton and Trump closed gradually leading up to July 30, there was a sudden widening of their winning probabilities immediately after July 30. Thus, the sudden *decrease* in Trump's probability of winning allows me to investigate whether and how stock prices of firms in the most regulated industries react to investors' expectations about future *increase* in regulations.

To analyze abnormal returns around July 30, I implement the same event study framework as before. However, in this case, the event date is considered to be July 30. The first day of trading after the event day is August 1, 2016, as July 30, 2016 was a Saturday. In this case, one would expect that firms in the most regulated industries would react negatively compared with firms in the least regulated industries as Trump's probability of winning suddenly decreased.

[Insert Table 2.5 here]

The results of the analysis are shown in Table 2.5. As can be seen from the point estimate on QUTILE_4, firms in the most regulated industries reacted negatively during the 1−3 day event window after July 30. The point estimate on QUTILE_4 in column (1) of the table implies that firms in the most regulated industries earned 0.7% lower abnormal returns than firms in the least regulated industries on August 1, 2016. However, in unreported results, I find that such negative reactions did not persist over the medium-term (e.g. 10-day).

Aside from documenting the robustness of the main results, this analysis sheds light on two issues. First, whether one should expect the most regulated firms to also react when there is an expected *increase* in regulations (rather than a decrease due to the election event itself). Second, whether such a reaction would be negative. One line of argument is that the most regulated firms benefit from any change in regulations, irrespective of whether the change is an increase or decrease in regulations. This can be due to many reasons, including but not limited to regulators adjusting pre-existing regulations to reflect more contemporary developments in an industry, the ability of regulated firms to comply with changes in regulation, and implementation of regulations through lobbying that could only benefit already regulated firms. If so, one might expect regulated firms to always benefit from a change in regulations. However, the results of this analysis indicate that a possible increase in expected regulations is considered negative by investors in most regulated firms in this study.

[15] Link: https://projects.fivethirtyeight.com/2016-election-forecast/

2.4 How Deregulation Benefits Shareholders

In this section, I explore the specific economic channels through which investors expected the firms to benefit from deregulation.

2.4.1 Regulation and Growth Opportunities

One of the ways through which regulation impacts economic growth is by imposing additional constraints on firms. Thus, firms maximize their profits subject to an additional constraint, which leads to inefficient allocation of resources and decreases firm profitability (Jaffe and Palmer (1997)). Such constraints can be particularly harmful to high-growth firms. For example, Arnold et al. (2011) show that European product market regulations impact firms with above-average productivity growth more than other firms. Furthermore, regulations impose costs that firms must meet by using otherwise productive resource (such costs can be both administrative costs and policy costs (Crafts (2006))), which harms the profitability of firms. Thus, one would expect that a reduction in regulations benefit high-growth firms in regulated industries more than low-growth firms, such that shareholders adjust their valuations of firms accordingly. Two challenges in testing this hypothesis are that regulations or firms' compliance costs and/or innovation decisions do not change randomly and it is difficult to observe regulatory burdens at the individual firm level. Thus, in the following analysis, I focus on an ex-ante measure of expected firm growth.

Empirically, I test the hypothesis that high−growth firms are affected more than low−growth firms by employing the baseline specification of Equation (2.2) and introducing interaction terms between each regulatory quartile and Tobin's Q (as a measure of expected future growth of firms). I measure Tobin's Q (TOBINS_Q) as the ratio of the sum of market capitalization and total assets minus book equity minus deferred tax liability to total assets. However, as noted in Laeven and Levine (2007), this conventional way of calculating Tobin's Q may be unsuitable for banks. Hence, I follow Laeven and Levine (2007) for the financial firms in the sample, by measuring Tobin's Q as the ratio of operating income (earnings before interest and taxes) to total assets. As with other accounting variables, I take the data for calculating Tobin's Q for the 2015 fiscal year.

[Insert Table 2.6 here]

The results of the analysis are presented in Table 2.6. Not all firms have all of the data for computing Tobin's Q. Hence, the sample size in these regressions differs from those in the earlier tables. Columns (1) to (3) implement an interaction term between each quartile of regulated industries and Tobin's Q. It is evident from the results that firms having higher growth opportunities *and* present in the most regulated industry quartile reacted more positively after the event than those firms in the least regulated industry quartile. In terms of the economic magnitude of the abnormal returns, a one standard deviation increase in Tobin's Q for firms in the most regulated industries resulted in approximately 1.5% (= 0.8% $\times$ 1.87) more positive abnormal returns during the 10-day event window, as compared to other firms in the least regulated industries. This is equivalent to a \$9.40 million relative gain in market value for an average firm in the most regulated industries. These results imply that higher regulations impact firms with higher growth opportunities more than firms with lower growth opportunities and, thus can stymie economic growth.

2.4.2 Regulation and Competition

In the economic theory of regulation of Stigler (1971), the demand for regulation comes from industry and "is designed and operated primarily for its benefit". This theory is often referred to as the "capture" theory of regulation. Posner (1974) suggests that industry incumbents seek regulation to protect themselves from future competition from new entrants and extract rents from consumers. After these seminal works of Stigler (1971) and Posner (1974), many other studies show that deregulation benefits consumers by introducing competition into various sectors of the economy. For example, Djankov et al. (2002) show that countries with higher entry regulations have lower per-capita GDP than countries with lower entry regulations. Bertrand and Kramarz (2002) analyze the French retail industry and find that regulation decreased competition. Besley and Burgess (2004) reach similar conclusions investigating labor regulations in India. Similarly, the removal of interstate branch banking restrictions introduced credit competition in the US (Rice and Strahan (2010)).

[Insert Table 2.7 here]

Accordingly, I explore whether Trump's election impacted firms facing more competition differently from firms facing less competition for a given level of regulatory intensity. I use the product market concentration measure developed by Hoberg and Phillips (2016) for measuring the industry concentration of the firms' product market. As with all other variables, I use product market concentration (TEXT_BASED_HHI) data as of 2015. If investors expect that firms having higher product market concentration due to deregulation will experience a threat to their future profitability, one would expect to see firms in more regulated industries *and* having ex-ante lower competitive threat, as indicated by the higher HHI, to have lower cumulative abnormal returns than firms operating in less regulated industries. Table 2.7 presents the results of this analysis. Columns (1) to (3) of the table show that firms in more regulated industries facing lower competition lost significant market value during the 10-day event window. At the end of the 10-day event window (column (3)), a one standard deviation (= 0.27) increase in TEXT_BASED_HHI of a firm was associated with an approximately 2.35% (= 8.8% $\times$ 0.27) decrease in abnormal returns for a firm in the most regulated industries compared with those for firms in the least regulated industries. This translates to an average relative decline of $14.75 million in firm value for an average firm in the most regulated industry compared with a firm facing similar competition in the least regulated industry. These results show that investors lowered their expectation of future profitability for firms having ex-ante higher market power in more regulated industries. This implies that higher regulations help incumbent firms derive monopoly rents by shielding them from competition.

2.4.3 Regulation and Political Favoritism

In this section, I explore to what extent links to powerful politicians can explain the abnormal returns observed. The extensive literature on political connections shows that politically connected firms tend to gain market value after their connections win elections. For example, using data from 47 countries, Faccio (2006) documents positive abnormal returns of 1.5% when a political connection of a firm becomes active. Ferguson and Voth (2008) study the value of political connections in Nazi Germany and find that connected firms gained significant value immediately after the Nazis came to power in 1933. Akey (2015) documents the positive impact of political networks by examining the political contribution of firms in close elections in the US. More recently, Child et al. (2020) document that firms connected to Trump had positive CARs following the 2016 election.

One of the possible reasons for these patterns mentioned in this strand of the literature is that politically connected firms benefit from favorable regulatory treatment because of their connections. However, there is no direct evidence supporting this claim. For example, although Child et al. (2020) show that firms politically connected to Trump gained market value in the 20 days after the election and some of these firms received favorable regulatory treatment, it is not clear how much of the positive reaction was due to the expectation of such favorable regulatory treatment. That is, compared with when deregulation is not expected, when deregulation is expected political connections might become less valuable, as an imminent decrease in the overall cost of regulations means that more favorable regulatory treatment due to political connections is of less value to firms. Conversely, favorable regulatory treatment might be so firm-specific that an overall decrease in regulations does not change the value of political connections, or firms benefit from political connections other than by getting regulatory benefits.

Short (2019) reviews the large literature in political science that documents the role of politicians in enforcing regulations depending on their political affiliations and that of their constituents. There is some (albeit limited) evidence that firms may benefit through such political favoritism. For example, Asher and Novosad (2017), Albouy (2013), Ansolabehere and Snyder (2006) present evidence of political favoritism exhibited by ruling parties in the US and other developing countries for regions represented by elected members belonging to the ruling party. Furthermore, Khwaja and Mian (2005) present evidence that politicians' strength in their local constituencies affects firms' ability to be favored by lenders. More closely related to the current study is that of Asher and Novosad (2017), which documents that private firms in India receive favorable regulatory treatment in regions represented by the members of the ruling party.

If firms do benefit from political favoritism through the leniency of regulators, then such benefits might decrease when overall regulations are reduced. To test whether this is the case, I follow the literature on corporate law and define firms as politically connected to the Republican Party if they are incorporated in a state with a greater than median proportion of Republican Members of Congress. Such a definition is motivated by two reasons. First, corporate law scholars note that the US Congress, based on its authority over interstate commerce via the Commerce Clause, has discretion over various aspects of corporate law (Kahan and Rock (2005), Winship (2013)). Thus, defining political connection in such a way helps to capture possible political favoritism in the enactment of federal regulations that can impose additional costs on shareholders via corporate laws. This reflects the fact that while corporate laws might appear to impact only the relationship between the shareholders and the managers of a firm, they can also impact the overall regulatory compliance of a firm, as the costs and the benefits of which are borne by the shareholders of a firm (Wallace (2009)). For example, if compliance with environmental regulations reduces the litigation cost of a firm, then, under corporate law, such compliance is part of the fiduciary duty of the managers of a firm toward their shareholders. Second, one could use the headquarter state of a firm as a proxy for the probability of firms being favored by regulators. However, such a definition would likely mismeasure the economically relevant regions for a firm and thus introduce noise.

[Insert Table 2.8 here]

The results of the analysis are shown in Table 2.8. As with the previous analyses, I interact a dummy variable (REPUBLICAN) indicating states with a greater than median proportion of Republican Members of Congress with each regulatory quartile to tease out the economic channel. As one would expect, columns (1) to (3) show that the firms in states having more Republican than Democrat Members of Congress gained significant market value during the event window; this is demonstrated by the positive and statistically significant point estimate on the REPUBLICAN variable. This finding is consistent with the literature on political connections, which documents positive abnormal returns for politically connected firms after an election win by the politician to whom they are connected. The results also complement previous studies in the corporate law literature documenting federal intervention in state corporate laws in the US (Kahan and Rock (2005), Bebchuk and Hamdani (2006), Winship (2013)). Moreover, the coefficients of interaction with the regulation quartile dummies have a negative sign. The point estimates imply that a firm in the most regulated industry and from a state with a greater than the median number of Republican Members of Congress are associated with a 4% decrease in firm value by the end of the 10-day event window after the election, compared with the least regulated firms. Thus, investors considered political connections less valuable for firms in the most regulated industries than for those in the least regulated industries, consistent with the hypothesis that deregulation decreases opportunities for firms to seek political favors and capture regulators.

2.4.4 Robustness of Mechanisms

I also validate the robustness of the results by calculating the abnormal returns, as in section 3.2.1, using a one-factor model that represents the return of global markets (except the US), as measured by the returns of the ACWX. I re-run the specifications stated in sections 4.1, 4.2, and 4.3 by replacing the dependent variable with the abnormal returns calculated by adjusting the returns by ACWX. For brevity, I show the results for 5-day CAR in Table A6. As can be seen, the results remain qualitatively similar to those in Tables 6, 7 and 8. For example, column (1) shows that firms in the most regulated industries *and* having higher ex-ante growth opportunities experienced higher abnormal returns than other firms. Column (2) shows that firms in the most regulated industries *and* with lower ex-ante competitive threat, experienced lower abnormal returns than other firms. Finally, column (3) documents that firms that could possibly gain from connection to Republican Members of Congress *and* were in the most regulated industries also experienced lower abnormal stock returns than other firms.

Overall, the results presented in Table A6 demonstrate that the economic mechanisms documented above are not sensitive to the selection of the model for calculating abnormal returns.

2.5 Real Effects

Thus far, the results document how investors expected possible deregulation to impact firms in the most regulated industries. However, it remains unclear whether firms' fundamentals over the few years following the 2016 election were consistent with investors' reactions around the election date, especially those of firms in the most regulated industries. Therefore, I explore whether firms in the most regulated industries experienced changes in their profitability, cash flow, or sales after 2016. I measure profitability by earnings before interest and taxes (EBIT) and cash flows (CASH_FLOW) by adding back depreciation and amortization to EBIT. As measures of sales, I use the natural logarithm of total sales ($Log(SALES)$), and sales growth, measured as the yearly percentage change in sales (SALES_GROWTH). I estimate the following empirical specification:

$$\mathrm{Y}_{i,j,t} = \alpha + \sum_{N=1}^{4} \beta_{1N} \times \mathrm{QUTILE\_N}_j \times \mathrm{POST}_{i,t} + \ \beta_3 \times \mathrm{C}_{i,j,t} \ + \ \gamma_{j,t} \ + \ \nu_i \ + \ \epsilon_{i,j,t} \tag{2.3}$$

In the above equation, $Y_{i,j,t}$ is the profitability, sales or sales growth for firm i in a NAICS two-digit industry j in year t. QUTILE_N is a dummy variable indicating the four regulatory quartiles, as before. This equation is estimated for the sample period 2014−2019. The variable POST takes the value of 1 for the years 2017−2019, and 0 otherwise. $C_{i,j,t}$ is a vector of firm-level time-varying control variables, which are the logarithm of assets, debt-to-equity ratio, and tax rates. I also employ firm fixed effects (ν_i) and NAICS two-digit industry times year fixed effects ($\gamma_{j,t}$). Unlike the regressions in the previous sections, I cannot take into account unobserved time−invariant state−specific effects by employing state fixed effects along with a firm fixed effect (as none of the firms changed its location between 2014 and 2019). Hence, to account for the possible cross-correlation of firm fundamentals for firms within a state, I cluster the standard errors at the firm and the state level.

[Insert Table 2.9 here]

The results of the estimation are shown in Table 2.9. Most regulated firms had higher sales and sales growth during the years under the Trump administration than in the previous three years. The point estimates imply that firms in the most regulated industries (QUTILE_4) had 9.5% higher sales and 5.7% higher sales growth than firms in the least regulated industries during 2017−2019 than during 2014−2016. This is consistent with the abnormal stock returns for firms in the most regulated industries around the election day. However, as documented in the first two columns of the table, there were no significant differences in firm profitability or cash flows.

Furthermore, in Table A7 I document that firm fundamentals also changed consistent with the economic mechanisms as implied by the stock price reactions. For example, the point estimates in Panel A imply that a one standard deviation increase in Tobin's Q was associated with a 1.9% ($= 1.56 \times 0.012$) increase in profitability or a 10% increase in sales or 2.6% increase in sales growth for firms in the most regulated industries compared with firms in the least regulated industries during 2017−2019 than during 2014−2016. We find similar results for competition and political favoritism in panels B and C, respectively.

While the point estimates are not always statistically significant, these results are suggestive evidence consistent with the initial stock price reaction. Ex-ante, there are at least three reasons that might bias against finding any significant effect on firm fundamentals. First, it might take many years for deregulation to impact firm fundamentals. Given that the Trump presidency lasted for four years, the time horizon may be insufficient to observe such effects. Second, the previous results on stock returns capture an intention-to-treat effect rather than a treatment effect. Hence, it is still possible that there was insufficient deregulation to affect firm fundamentals. Finally, it is possible that the Trump administration was not able to carry out effective deregulation due to political and bureaucratic hurdles, as noted by Belton and Graham (2019) and Coglianese et al. (2021). Hence, the evidence, although weak, provides additional credibility to the economic mechanisms driving the main set of results.

As noted in Hassan et al. (2019), a significant portion of firm-level political risk, as measured from the quarterly earnings conference calls, is driven by regulation and legislation. If the Trump administration was lenient on the enactment and enforcement of regulations then market participants might have expected lower political risk for firms in the most regulated industries during the Trump administration years than before. I further examine this possibility in Table 2.10. For this analysis, I collect firm-level quarterly political risk scores from Hassan et al. (2019) and analyze them over the period 2014−2019, as before. The dependent variable in the regression is the natural logarithm of one plus the firm-level political risk score (PRISK). The coefficients of interest are the coefficients on the interaction term of each regulatory quartile dummy and POST. As before, POST is a dummy variable that takes a value of 1 for the years 2017−2019 and 0 otherwise. I additionally employ firm fixed effects and year fixed effects. As can be seen in columns (1)−(3) of the Table 2.10, firm-level political risk decreased by 9% to 18% for the most regulated firms during the Trump administration years of 2017−2019 than in 2014−2016. In column (3), I further employ a NAICS two-digit industry and year fixed effects. The results remain statistically significant and economic magnitude also increases.

[Insert Table 2.10 here]

Overall, the results in this section document evidence that firm fundamentals (particularly those of firms in the most regulated industries) during the Trump administration years reacted in line with what investors had expected around election day in 2016.

2.6 Conclusion

Using the election of Donald Trump as the President of the United States as an unexpected exogenous shock to the expectation of future deregulation in the US, I find that firms in the most regulated industries gained approximately $25 million more than firms in the least regulated industries during the 10 trading days following the election. This indicates that the markets expected that due to deregulation, approximately, $27 billion more benefits would accrue to firms in the most regulated industries than for firms in the least regulated industries. This price reaction is significant after controlling for a host of possible confounding factors and alternative specifications. The positive effect on the valuation of the most regulated firms is driven by high-growth firms. Firms with less competitive threats lost market value in the most regulated industries, implying that increased regulations benefit incumbent firms by shielding them from competition. Additionally, I provide novel evidence that political favoritism becomes less valuable for more regulated firms under the expectation of deregulation.

However, there are limitations to the study. While the results provide suggestive evidence of a causal impact of the Trump election on expected deregulation, they do not confirm a causal relationship, as it is challenging to do so based on a single event. Furthermore, this study does not speak to the overall welfare effects of changes in the regulatory environment. Additionally, the study does not explore the effect on private firms. Furthermore, changes in the scale and complexity of regulations and their effects on firms are not explored. These topics are left for future research.

Figures and Tables

Figure 2.1: Comparison of the Emphasis on Regulations by Donald Trump

Figure 1(a) plots the number of major rules published by each presidential year (February, 1 to January, 31) from 1981 to 2018 as published by the Office of Information and Regulatory Affairs. The vertical axis represents the number of major rules and the presidential year is plotted along the horizontal axis. The red bars belong to the years under the Trump administration. Figure 1(b) plots the logarithm of the number of news articles pertaining to national "Presidential Elections" in the Factiva database that mentions the word "regulation" during the first ten months (January to October) leading up to the elections in 2000 and 2016. The red (blue) line is for the election in 2016 (2000) leading to the victory of Donald Trump (George W. Bush).

Figure 1(a): Major rules by presidency years

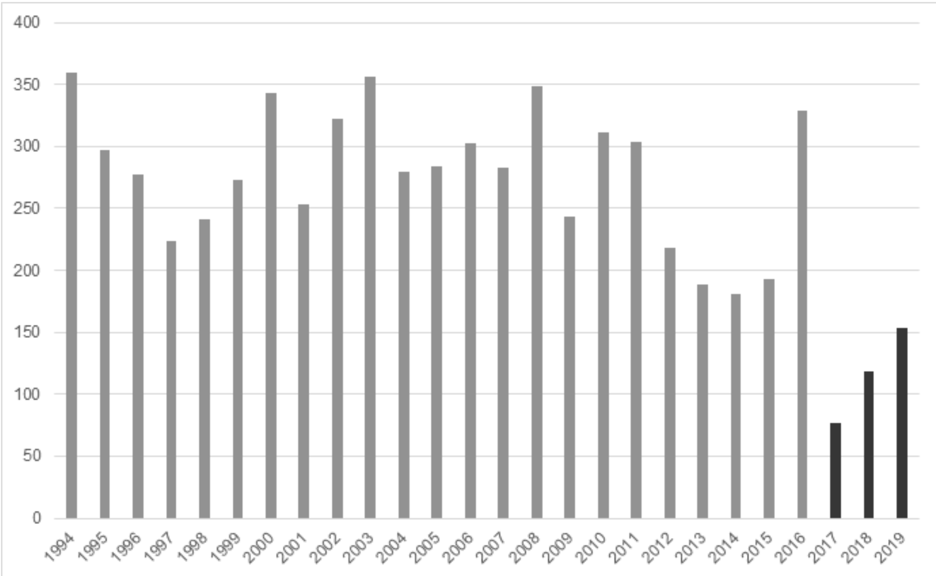

Figure 1(b): Bush v/s Trump articles on regulation

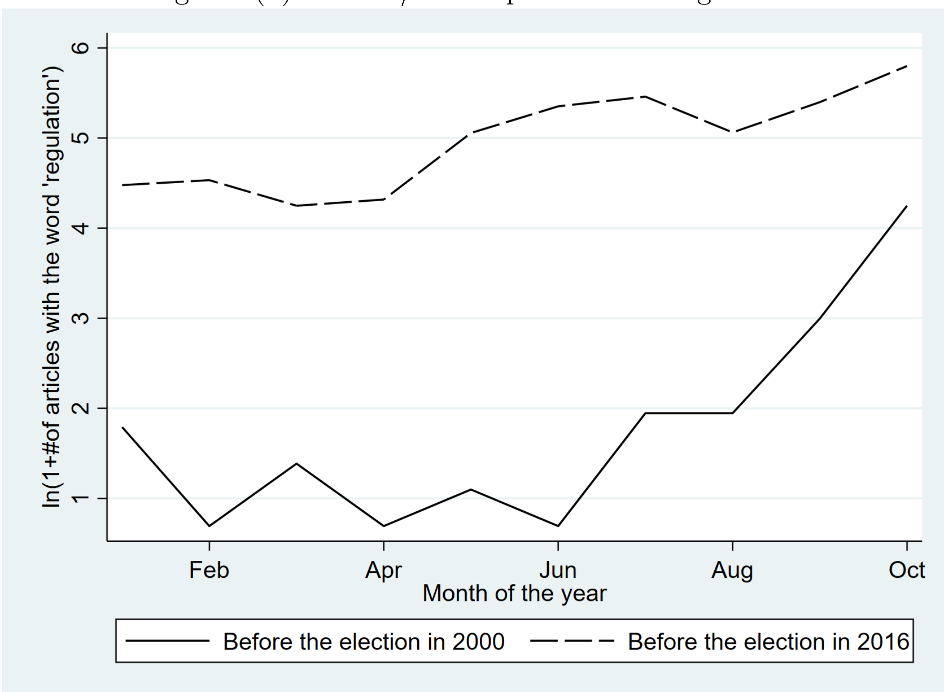

Figure 2.2: Cumulative Abnormal Returns around the Election Day

This figure plots the difference between the value-weighted cumulative abnormal returns of firms in the highest quartile of regulated industries (QUTILE_4) to those for firms in the lowest (QUTILE_1) quartile around November 8, 2016, the event−date (i.e., QUTILE_4 - QUTILE_1) in the vertical axis in basis points. The horizontal axis plots the trading day with respect to the event−date marked by the vertical line in the graph at time $T = 0$.

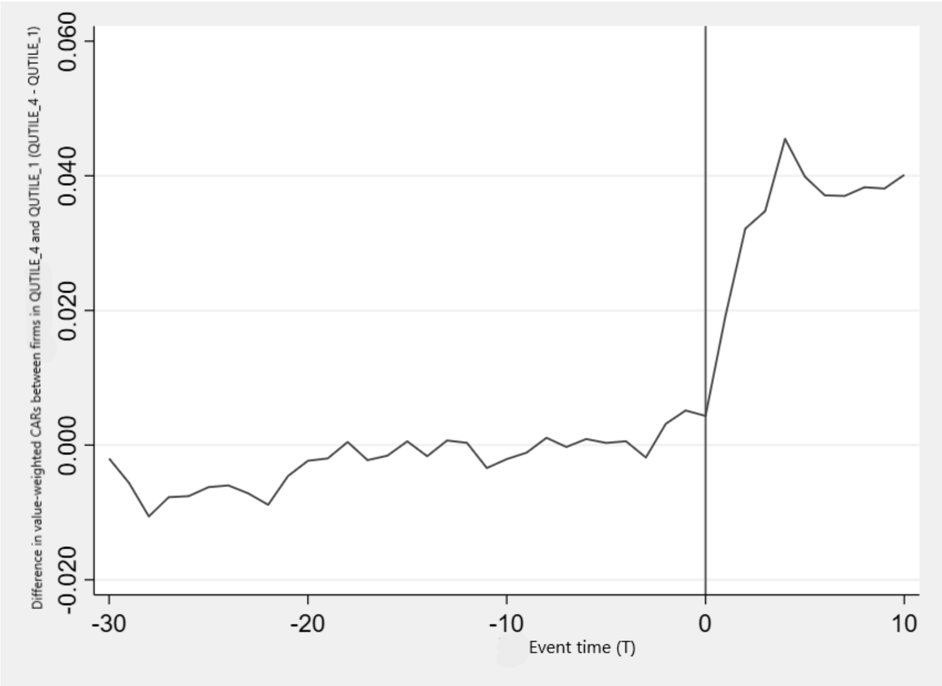

Figure 2.3: Falsification Tests

Panel A of this figure plots the frequency distribution of t-statistics on the coefficient estimate of QUTILE_4 while running the main regression specification presented in Eq. (2) across 252 randomly generated dates between January 1, 2013 and November 8, 2016. Panel B of the figure plots the frequency distribution corresponding to the point estimates of QUTILE_4 across these 252 regressions.

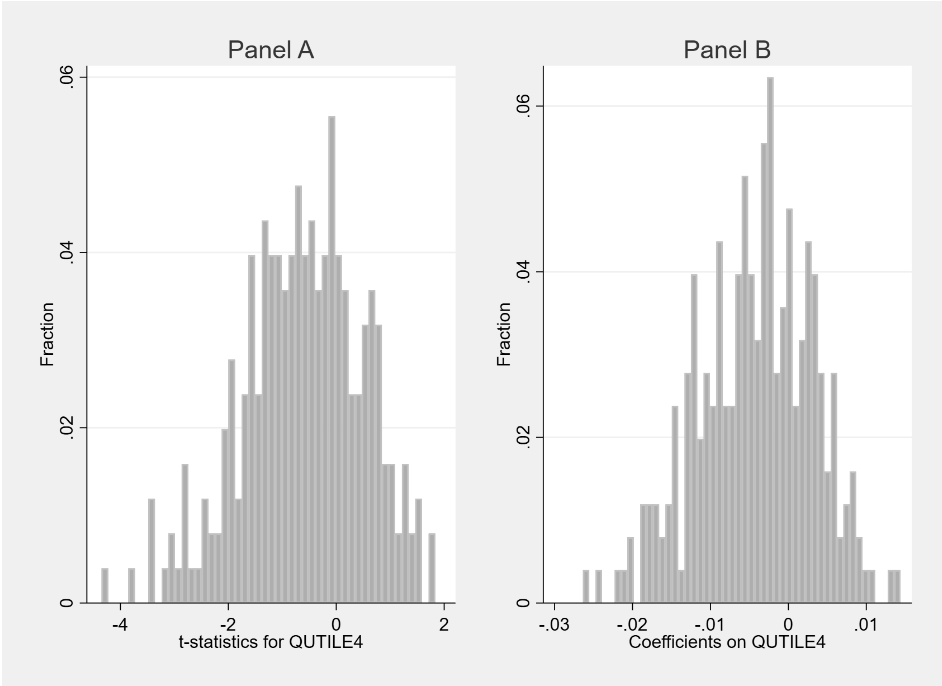

Table 2.1: Example of Industries in each Regulatory Quartile

This table presents a list of three digit NAICS industries that form the top 10 in each regulated industry quartile. The industry definitions are taken from United States Census Bureau website. Column (4) of the table lists the 10-day value-weighted cumulative abnormal returns (CARs) associated with each industry for the study. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively, based on a t-test comparing the value-weighted CARs to zero.

Quartile	NAICS	Name of the Industries	10-day CAR
4	325	Chemical Manufacturing	4.83%***
	324	Petroleum and Coal Products Manufacturing	6.72%***
	515	Broadcasting (except Internet)	3.18%***
	522	Credit Intermediation and Related Activities	3.25%***
	611	Educational Services	6.14%***
	562	Waste Management and Remediation Services	4.24%*
	481	Air Transportation	2.22%
	221	Utilities	-2.58%**
	541	Professional, Scientific, and Technical Services	2.15%**
	523	Securities, Commodity Contracts, and Other Activities	1.99%**
3	336	Transportation Equipment Manufacturing	-1.12%
	112	Animal Production and Aquaculture	3.95%
	624	Social Assistance	-3.14%
	327	Nonmetallic Mineral Product Manufacturing	2.35%
	524	Insurance Carriers and Related Activities	1.74%
	621	Ambulatory Health Care Services	2.38%
	111	Crop Production	-6.77%**
	312	Beverage and Tobacco Product Manufacturing	-3.54%***
	512	Motion Picture and Sound Recording Industries	-2.89%
	212	Mining (except Oil and Gas)	2.84%
2	331	Primary Metal Manufacturing	5.83%***
	445	Food and Beverage Stores	-1.61%
	517	Telecommunications	-2.17%
	483	Water Transportation	4.11%
	486	Pipeline Transportation	1.12%*
	211	Oil and Gas Extraction	-4.44%***
	311	Food Manufacturing	-4.90%***
	424	Merchant Wholesalers, Nondurable Goods	-2.16%
	454	Nonstore Retailers	-1.52%
	423	Merchant Wholesalers, Durable Goods	-1.85%
1	335	Electrical Equipment, Appliance, & Comp. Mfg	0.35%
	213	Support Activities for Mining	-4.19%**
	425	Wholesale Electronic Markets, Agents & Brokers	3.79%**
	236	Construction of Buildings	-2.93%*
	519	Other Information Services	-3.38%**
	333	Machinery Manufacturing	-0.98%
	334	Computer and Electronic Product Manufacturing	-1.27%**
	115	Support Activities for Agriculture and Forestry	0.89%
	326	Plastics and Rubber Products Manufacturing	-2.89%
	551	Management of Companies & Enterprises	2.96%***

Table 2.2: Summary Statistics

This table reports descriptive statistics for the variables used in the main analyses of the paper. The main sample includes 2,413 firms around November 8, 2016. QUTILE_1-QUTILE_4 are the four groups of firms based on the quartiles of regulatory restrictions of the NAICS 3-digit industry that the firm belongs to. QUTILE_1 is for least regulated and QUTILE_4 is for most regulated firms. The variables are: ILLIQUIDITY is a firm-specific illiquidity measure based on Amihud (2002), Log(MARKET_CAP) is the natural logarithm of market capitalization for each firm, Log(TOTAL_ASSETS) is the natural logarithm of total assets for each firm, DEBT_TO_EQUITY is the debt-equity ratio based on the book value of debt and equity of a firm, EXPECTED_CASH_TAX_RATE is expected tax rate calculated according to Wagner et al. (2018a), POLITICAL_CONNECTION is a dummy variable if a firm has donated to Republican PAC and/or any individuals from the firm has donated to Donald Trump's campaign during the 2016 election cycle, GOVT_EXPOSURE is a dummy variable identifying industries that have higher than median level of exposure to government spending calculated as per Belo et al. (2013), IMPORT_DEPENDENT is a dummy variable indicating industries that are import dependent based on data from United States International Trade Commission (USITC). TOBINS_Q is the firm level measure of growth opportunities measured as in Akey (2015), TEXT_BASED_HHI is the firm level measure of competition from Hoberg and Phillips (2016), REPUBLICAN is a dummy variable indicating states with more Republican Members of Congress and 10-DAY CAR is the cumulative abnormal returns for each stock over ten days after the election. All variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables have been winsorized at 1 and 99 percentiles.

	QUTILE_1			QUTILE_2			QUTILE_3			QUTILE_4		
Variables	N	Mean	Std.Dev	*N*	Mean	Std.Dev	N	Mean	Std.Dev	N	Mean	Std.Dev
ILLIQUIDITY	596	0.00	0.01	354	0.00	0.00	312	0.00	0.01	1158	0.00	0.01
Log(MARKET_CAP)	603	13.15	2.28	356	13.75	2.24	316	13.84	2.33	1166	13.36	2.15
Log(TOTAL_ASSETS)	603	6.43	2.20	356	7.13	2.09	316	7.33	2.41	1166	6.91	2.30
DEBT_TO_EQUITY	603	0.36	2.08	356	0.65	2.97	316	0.67	2.41	1166	0.52	2.41
EXPECTED_CASH_TAX_RATE	603	0.13	0.16	356	0.12	0.16	316	0.17	0.16	1166	0.13	0.16
POLITICAL_CONNECTION	603	0.41	0.49	356	0.48	0.50	316	0.51	0.50	1166	0.40	0.49
GOVT_EXPOSURE	603	0.60	0.49	356	0.61	0.49	316	0.68	0.47	1166	0.89	0.31
IMPORT_DEPENDENCE	603	0.66	0.47	356	0.52	0.50	316	0.52	0.50	1166	0.28	0.45
TOBINS_Q	549	1.82	1.31	339	1.59	0.91	298	1.45	1.39	1112	1.64	1.87
TEXT_BASED_HHI	575	0.32	0.29	336	0.30	0.27	294	0.32	0.31	1125	0.21	0.24
REPUBLICAN	602	0.11	0.31	356	0.12	0.32	316	0.12	0.33	1165	0.13	0.33
10-DAY CAR	603	-0.01	0.10	356	-0.02	0.11	316	0.00	0.11	1166	0.03	0.10

Table 2.3: Baseline Results – Regulation and Stock Returns

The dependent variable is CAR_K, where $K = \{1, 5, 10\}$, is the K-day cumulative abnormal returns after November 8, 2016. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions QUTILE_1 is the reference category. ILLIQUIDITY is a firm-specific illiquidity measure based on Amihud (2002), Log(MARKET_CAP) is the natural logarithm of market capitalization for each firm, Log(TOTAL_ASSETS) is the natural logarithm of total assets for each firm, DEBT_TO_EQUITY is the debt-equity ratio based on the book value of debt and equity of a firm, EXPECTED_CASH_TAX_RATE is expected tax rate calculated following Wagner et al. (2018a), MISS_TAX_RATE is a dummy variable indicating firms whose tax rates could not be computed as of 2015, POLITICAL_CONNECTION is a dummy variable if a firm has donated to Republican PAC and/or any individuals from the firm has donated to Donald Trump's campaign during the 2016 election cycle, GOVT_EXPOSURE is a dummy variable identifying industries that have higher than median level of exposure to government spending calculated as per Belo et al. (2013), IMPORT_DEPENDENT is a dummy variable indicating industries that are import dependent based on data from the United States International Trade Commission (USITC). Columns (4) - (6) additionally control for firm-level political connection within each regulatory quartile. Variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables are winsorized at 1 and 99 percentiles. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	CAR_01	CAR_05	CAR_10	CAR_01	CAR_05	CAR_10
QUTILE_2	0.012*	0.014	0.014	0.014**	0.016	0.015
	(1.88)	(1.02)	(1.07)	(2.61)	(1.21)	(1.25)
QUTILE_3	0.013***	0.027***	0.022***	0.018***	0.031***	0.025**
	(2.89)	(4.05)	(3.61)	(3.10)	(3.04)	(2.29)
QUTILE_4	0.021***	0.042***	0.039***	0.022***	0.045***	0.040**
	(4.89)	(3.41)	(2.86)	(4.60)	(3.18)	(2.83)
ILLIQUIDITY	-0.223**	-0.232	-0.233	-0.224**	-0.232	-0.234
	(-2.37)	(-1.30)	(-0.92)	(-2.38)	(-1.28)	(-0.93)
Log(MARKET_CAP)	0.004	0.003	-0.002	0.004	0.003	-0.002
	(1.37)	(0.79)	(-0.72)	(1.36)	(0.78)	(-0.73)
Log(TOTAL_ASSETS)	-0.003	-0.006	-0.003	-0.003	-0.006	-0.003
	(-1.20)	(-1.18)	(-0.65)	(-1.19)	(-1.17)	(-0.65)
DEBT_TO_EQUITY	-0.000	-0.000	-0.001	-0.000	-0.000	-0.001
	(-0.22)	(-0.41)	(-0.75)	(-0.20)	(-0.39)	(-0.75)
EXPECTED_CASH_TAX_RATE	0.015*	0.021**	0.014	0.015*	0.022**	0.014
	(1.85)	(2.27)	(1.37)	(1.84)	(2.33)	(1.35)
MISS_TAX_RATE	0.002	0.004	0.014	0.002	0.004	0.014
	(0.45)	(0.49)	(1.39)	(0.48)	(0.51)	(1.40)
POLITICAL_CONNECTION	-0.000	-0.001	-0.003	0.003	0.005	-0.000
	(-0.02)	(-0.18)	(-0.45)	(0.94)	(0.81)	(-0.07)
GOVT_EXPOSURE	-0.000	-0.005	-0.007	-0.000	-0.005	-0.007
	(-0.04)	(-0.52)	(-1.03)	(-0.03)	(-0.52)	(-1.01)
IMPORT_DEPENDENT	-0.015***	-0.021**	-0.015	-0.016***	-0.021**	-0.016*
	(-4.36)	(-2.32)	(-1.65)	(-4.65)	(-2.40)	(-1.76)
QUTILE_2×POLITICAL_CONNECTION				-0.004	-0.006	-0.002
				(-0.92)	(-0.79)	(-0.23)
QUTILE_3×POLITICAL_CONNECTION				-0.010	-0.007	-0.006
				(-1.48)	(-0.81)	(-0.52)
QUTILE_4×POLITICAL_CONNECTION				-0.002	-0.009	-0.002
				(-0.64)	(-1.03)	(-0.34)
R^2	0.11	0.12	0.09	0.11	0.12	0.09
N	2,413	2,413	2,413	2,413	2,413	2,413
2-digit NAICS FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.4: Alternate Specifications – Regulation and Stock Returns

Panel A of the table implements a seemingly unrelated regression (SUR) to account for event-time clustering. For each regulatory quartile, I calculate a value-weighted portfolio returns over the period of [-22,10] days around the election. Columns (1)-(4) of Panel A denote the coefficient of the SUR model on the dummy variable indicating whether a given day is after the election day of not for the most regulated (QUTILE_4) to the least regulated (QUTILE_1) industries, respectively. Columns (5)-(7) compare the coefficients of QUTILE_1 with those of other quartiles using a Wald test. In Panel B, columns (1)-(4) implements a one-factor model where the factor is either the index return of the iShares All Country World Index which excludes the US (in columns (1) and (2)) or it is the value-weighted returns of the universe of Compustat firms excluding the US (in columns (3) and (4)). In the columns (5)-(6), I calculate t-statistics of the regression output in Table 3 following Cohn et al. (2016). The dependent variable is CAR_K, where $K = \{1, 5\}$, is the K-day cumulative abnormal returns after November 8, 2016 based on the Fama-French five factors and the momentum factor model. As before, QUTILE_1 indicates the least and QUTILE_4 indicates the most regulated industries with QUTILE_1 being the reference category. For brevity, point estimates on other control variables are not reported. Variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables are winsorized at 1 and 99 percentiles. N presents number of firms in the regression. The regressions in Panel B are with 2-digit NAICS industry fixed effects and state fixed effects. Standard errors are clustered at 2-digit NAICS level in columns (1)-(4). t-statistics are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A – Adjusting for event-day clustering using Seemingly Unrelated Regressions

	Coefficients on:				Difference with QUTILE_1		
	QUTILE_4	QUTILE_3	QUTILE_2	QUTILE_1	$Q4-Q1$	$Q3-Q1$	$Q2-Q1$
1-Day	0.005***	-0.006***	-0.013***	-0.010***	0.015***	0.004*	-0.003
5-Day	0.003***	-0.003**	-0.004	-0.004	0.007***	0.001	0.000
10-Day	0.002***	-0.002	0.000	-0.001	0.003**	-0.001	0.000

Panel B – Alternative Methods to Account for Event-day Clustering

	1-Factor Model using:				*t-stats computed using:*	
	Compustat		*ACWX*		*Cohn et al. (2016)*	
	CAR_01	CAR_05	CAR_01	CAR_05	CAR_01	CAR_05
QUTILE_2	0.016*	0.028	0.029	0.012**	0.014	0.016
	(2.02)	(1.58)	(2.09)	(1.66)	(0.37)	(1.51)
QUTILE_3	0.012**	0.018	0.012	0.012**	0.018***	0.031***
	(2.76)	(1.38)	(2.68)	(1.18)	(4.34)	(3.50)
QUTILE_4	0.024***	0.043***	0.030*	0.024***	0.022***	0.045***
	(3.56)	(2.93)	(3.53)	(2.81)	(4.98)	(4.68)
R^2	0.15	0.15	0.12	0.16	0.15	0.15
N	2,413	2,413	2,413	2,413	2,413	2,413
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
2-digit NAICS FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.5: Other Trump Event – Regulation and Stock Returns

This table presents the results of implementing the baseline specification of Eq. (2) after July 30, 2016. Donald Trump's probability of winning was 50.1% on July 30, 2016. According to the data from FiveThirtyEight, this was the only day when Donald Trump had a higher probability of winning than Hillary Clinton leading up to the election day. The dependent variable is $\mathrm{CAR_K}$, where $K = \{1, 2, 3\}$, is the K-day cumulative abnormal returns after July 30, 2016. As before, the abnormal returns are calculated from a five factor model including momentum over the last year one month from the event date. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions QUTILE_1 is the reference category. ILLIQUIDITY is a firm-specific illiquidity measure based on Amihud (2002), Log(MARKET_CAP) is the natural logarithm of market capitalization for each firm, Log(TOTAL_ASSETS) is the natural logarithm of total assets for each firm, DEBT_TO_EQUITY is the debt-equity ratio based on the book value of debt and equity of a firm, EXPECTED_CASH_TAX_RATE is expected tax rate calculated following Wagner et al. (2018a), MISS_TAX_RATE is a dummy variable indicating firms whose tax rates could not be computed as of 2015, POLITICAL_CONNECTION is a dummy variable if a firm has donated to Republican PAC and/or any individuals from the firm has donated to Trump's campaign during the 2016 election cycle, GOVT_EXPOSURE is a dummy variable identifying industries that have higher than median level of exposure to government spending calculated as per Belo et al. (2013), IMPORT_DEPENDENT is a dummy variable indicating industries that are import dependent based on data from the United States International Trade Commission (USITC). Variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables are winsorized at 1 and 99 percentiles. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	CAR_01	CAR_02	CAR_03
QUTILE_2	-0.002	-0.001	-0.005
	(-0.69)	(-0.27)	(-1.42)
QUTILE_3	0.001	-0.004	-0.008
	(0.47)	(-1.24)	(-1.36)
QUTILE_4	-0.007**	-0.008**	-0.010**
	(-2.64)	(-2.79)	(-2.37)
ILLIQUIDITY	-0.028*	-0.024	-0.011
	(-1.89)	(-1.29)	(-0.99)
Log(MARKET_CAP)	-0.000	-0.001	-0.002***
	(-0.57)	(-0.45)	(-3.24)
DEBT_TO_EQUITY	-0.000	-0.000	-0.000
	(-1.01)	(-1.20)	(-0.96)
EXPECTED_CASH_TAX_RATE	0.011**	0.013*	0.016*
	(2.28)	(1.87)	(1.81)
MISS_TAX_RATE	0.002	0.001	0.008**
	(1.07)	(0.54)	(2.71)
GOVT_EXPOSURE	-0.005*	-0.007**	-0.011**
	(-1.84)	(-2.46)	(-2.54)
IMPORT_DEPENDENT	0.008***	0.006***	0.015***
	(5.06)	(3.10)	(6.46)
POLITICAL_CONNECTION	0.004*	0.002	0.001
	(1.79)	(0.94)	(0.49)
QUTILE_2×POLITICAL_CONNECTION	0.002	0.002	0.002
	(0.77)	(0.40)	(0.30)
QUTILE_3×POLITICAL_CONNECTION	-0.006**	0.004	0.004
	(-2.74)	(1.23)	(1.33)
QUTILE_4×POLITICAL_CONNECTION	-0.000	0.001	0.005
	(-0.04)	(0.56)	(1.54)
R^2	0.05	0.04	0.07
N	2,409	2,409	2,408
2-digit NAICS FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

Table 2.6: Mechanism – Regulation and Growth Opportunities

This table investigates whether growth opportunities are related to stock returns for firms in the most regulated industries. The dependent variable is CAR_{K}, where $K = \{1{,}5,\ 10\}$, is the K-day cumulative abnormal returns after November 8, 2016. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions QUTILE_1 is the reference category. Growth Opportunities is measured by TOBINS_Q as in Akey (2015), i.e., the ratio of the total assets plus market value of equity minus common equity minus deferred tax liability to total assets. Variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables are winsorized at 1 and 99 percentiles. Point estimates on the control variables are not reported for brevity. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	CAR_01	CAR_05	CAR_10
QUTILE_2	0.008	-0.005	0.005
	(0.99)	(-0.40)	(0.60)
QUTILE_3	0.011	0.026**	0.025
	(1.05)	(2.15)	(1.54)
QUTILE_4	0.009	0.024**	0.026**
	(1.71)	(2.64)	(2.60)
TOBINS_Q	-0.002	0.004**	0.006**
	(-1.71)	(2.28)	(2.52)
QUTILE_2×TOBINS_Q	0.002	0.013**	0.008
	(0.91)	(2.22)	(1.67)
QUTILE_3×TOBINS_Q	0.003	0.000	-0.001
	(0.77)	(0.07)	(-0.17)
QUTILE_4×TOBINS_Q	0.008***	0.011**	0.007**
	(3.36)	(2.77)	(2.18)
R^2	0.13	0.15	0.11
N	2,272	2,272	2,272
Other Controls	Yes	Yes	Yes
2-digit NAICS FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

Table 2.7: Mechanism – Regulation and Competition

This table investigates if competition is related to stock returns for firms in more regulated industries. The dependent variable is CAR_K, where $K = \{1,5, 10\}$, is the K-day cumulative abnormal returns after November 8, 2016. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions QUTILE_1 is the reference category. Competition is the text based measure of Hoberg and Phillips (2016) as of 2015. Variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables are winsorized at 1 and 99 percentiles. Point estimates on the control variables are not reported for brevity. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	CAR_01	CAR_05	CAR_10
QUTILE_2	0.013	0.018	0.025**
	(1.69)	(1.69)	(2.27)
QUTILE_3	0.033***	0.051***	0.039*
	(3.88)	(3.37)	(1.94)
QUTILE_4	0.036***	0.071***	0.062***
	(4.54)	(3.66)	(3.15)
TEXT_BASED_HHI	0.011*	0.022	0.002
	(1.77)	(1.65)	(0.16)
QUTILE_2×TEXT_BASED_HHI	-0.003	0.002	-0.009
	(-0.30)	(0.04)	(-0.22)
QUTILE_3×TEXT_BASED_HHI	-0.039***	-0.045*	-0.023
	(-3.64)	(-2.03)	(-0.87)
QUTILE_4×TEXT_BASED_HHI	-0.047***	-0.099***	-0.088**
	(-3.61)	(-3.62)	(-2.84)
R^2	0.13	0.15	0.11
N	2,303	2,303	2,303
Other Controls	Yes	Yes	Yes
2-digit NAICS FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

Table 2.8: Mechanism – Regulation and Political Favoritism

This table investigates if political favoritism to states with more Republican Members of Congress is related to stock returns for firms in more regulated industries. The dependent variable is CAR_K, where $K = \{1,5, 10\}$, is the K-day cumulative abnormal returns after November 8, 2016. *QUTILE_N* is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions QUTILE_1 is the reference category. REPUBLICAN is a dummy variable indicating the US states with greater than median number of Republican Members of Congress. All variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables are winsorized at 1 and 99 percentiles. Point estimates on the control variables are not reported for brevity. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry fixed effects and state fixed effects. Standard errors are clustered at 2-digit NAICS level. t-statistics are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	CAR_01	CAR_05	CAR_10
QUTILE_2	0.030**	0.029	0.035
	(2.27)	(1.23)	(1.45)
QUTILE_3	0.024***	0.055***	0.059**
	(4.02)	(2.89)	(2.69)
QUTILE_4	0.042***	0.083***	0.085**
	(3.92)	(2.73)	(2.49)
REPUBLICAN	0.019***	0.033***	0.040***
	(3.07)	(4.03)	(5.35)
QUTILE_2×REPUBLICAN	-0.016*	-0.014	-0.020
	(-1.85)	(-1.01)	(-1.51)
QUTILE_3×REPUBLICAN	-0.006	-0.024*	-0.033*
	(-0.14)	(-1.74)	(-1.82)
QUTILE_4×REPUBLICAN	-0.018**	-0.036**	-0.040*
	(-2.67)	(-2.19)	(-2.09)
R^2	0.10	0.12	0.08
N	2,418	2,418	2,418
Other Controls	Yes	Yes	Yes
2-digit NAICS FE	Yes	Yes	Yes
State FE	No	No	No

Table 2.9: Real Effects – Firm Fundamentals

This table investigates if firm fundamentals changed over the three years after 2016 in accordance with the stock price reaction around the election day compared with three years before the election. The dependent variables in columns (1) - (4) are EBIT, CASH_FLOW, natural logarithm of sales (Log(SALES)) and SALES_GROWTH, respectively. EBIT is earnings before interest and taxes, CASH_FLOW is EBIT with depreciation and amortization added back and *SALES* is the revenue of a firm. QUTILE_N is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions QUTILE_1 is the reference category. The regressions also control for logarithm of firm assets, expected tax rates and debt-equity ratio as time-varying firm-level controls. The regressions are estimated on a sample period between 2014 and 2019. POST is a dummy variable taking the value of 1 for the years 2017−2019 and zero, otherwise. All variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables are winsorized at 1 and 99 percentiles. N presents number of firms in the regression. The regressions are with 2-digit NAICS industry × year fixed effects and firm fixed effects. Standard errors are clustered at the firm and state level. t-statistics are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	EBIT	CASH_FLOW	Log(SALES)	SALES_GROWTH
QUTILE_2×POST	0.022	-0.109	0.037	0.008
	(1.12)	(-0.39)	(0.95)	(0.36)
QUTILE_3×POST	-0.020*	-0.678	-0.014	0.002
	(-1.96)	(-1.43)	(-0.48)	(0.11)
QUTILE_4×POST	-0.001	0.178	0.092***	0.057***
	(-0.04)	(0.46)	(3.22)	(3.32)
N	10,576	10,392	11,122	11,590
Other Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
2-digit NAICS × Year FE	Yes	Yes	Yes	Yes

Table 2.10: Real Effects – Political Risk

This table investigates if firms in the most regulated industries experienced lower political risk during the Trump presidency. The dependent variable is natural logarithm of the text based firm-level political risk (PRISK) as measured by Hassan et al. (2019) from the quarterly earnings conference calls. *QUTILE_N* is a dummy variable indicating the quartile of regulated industry for each firm, with quartile one (QUTILE_1) indicating the least and quartile four (QUTILE_4) indicating the most regulated industries. In the regressions QUTILE_1 is the reference category. The regressions are estimated on a sample period between 2014 and 2019. POST is a dummy variable taking the value of 1 for the years 2017-2019 and zero, otherwise. The regressions control for logarithm of assets, expected tax rates and debt-equity ratio measured at a firm-year level. All variables are defined in Table A.1 in the Appendix. All non-logarithmic continuous variables are winsorized at 1 and 99 percentiles. *N* presents number of firms in the regression. Standard errors are clustered at the firm and state level. *t*-statistics are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	PRISK	PRISK	PRISK
QUTILE_2×POST	0.014	0.042	0.076
	(0.15)	(0.45)	(0.95)
QUTILE_3×POST	-0.039	-0.041	-0.030
	(-0.49)	(-0.49)	(-0.24)
QUTILE_4×POST	-0.088*	-0.120**	-0.182***
	(-1.86)	(-2.05)	(-2.88)
Log(TOTAL_ASSETS)		0.146***	0.108***
		(4.20)	(3.50)
EXPECTED_CASH_TAX_RATE		-0.022	-0.023
		(-0.32)	(-0.33)
DEBT_TO_EQUITY		-0.003	-0.003
		(-0.11)	(-0.11)
	(214.65)	(4.98)	(5.84)
R^2	0.31	0.31	0.31
N	39,010	31,116	31,116
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	No
2-digit NAICS FE× Year FE	No	No	Yes

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3

Angels and Demons: The Negative Effect of Employees' Angel Investments on Corporate Innovation

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Abstract: We link data on angel investors in the US to their employment histories. Employers' innovation output decreases when their employees personally invest in early-stage firms. We contribute two novel channels to the literature: agency conflicts and loss of highly skilled human capital. On the other hand, early-stage firms benefit from financing by angel investors employed at public firms. Angel investors divert time and effort from their employer to their personal investments. We highlight a trade-off between the costs of angel investors for their employers and the benefit for their start-ups.

Keywords: Angel Investors, Agency Conflicts, Distraction, Innovation

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3.1 Introduction

Angel investors, individuals who personally invest in start-ups, are an important driver of innovation and success of their portfolio start-ups (Kerr et al., 2014; Lerner et al., 2018). In this paper, we investigate the broader role of such angel investors. We define so-called *angel employees* as angel investors who are simultaneously employed at a publicly listed corporation. More specifically, we ask the following question: Do angel employees help or hinder the innovation output of their employer?

Ex-ante, it is unclear how angel employees impact their employer's innovation. On the one hand, angel employees could help their employer innovate. Angel employees might use their personal investments to acquire knowledge about start-ups' existing and future innovative activities[3] and guide innovative activities at their employer. On the other hand, angel employees could have a detrimental impact on corporate innovation. This is rooted in a standard principal-agent or multitask framework (Holmstrom and Milgrom, 1991; Jensen and Meckling, 1976). Angel employees must trade off exerting effort in the innovative activities of their employer or their personal investments. This

To test these conflicting hypotheses, we exploit novel data, which link angel investments of individuals to their employment history. We collect personal equity investments in US early-stage firms between 2001-2018 from Crunchbase which totals more than $21 billion of early stage capital. We then obtain employment histories of angel investors in our sample from LinkedIn and manual searches. We match 1,845 angel employees who have 5,379 investments and are employed at 792 unique publicly listed firms in the US. The company with the most angel employees in our sample is Alphabet, with a total of 196 employees who personally invested in 433 start-ups between 2001 and 2018.

We start with descriptive statistics. The average size of a funding round with angel employee participation is large at 5.7 million USD at the mean and 1.7 at the median. Angel employees are closely related to innovation and many employees have functions such as "tech", "engineer", "innovation" or "product". They are also senior and are thus important decision makers at their employers. 43% are directors and 14% are executives with the remainder managers, or vice presidents. The early-stage firm is usually closely located near the headquarter of the employer. Also, the industry of the start-up is closely related to the industry of the employer.

In our baseline specification, we analyze correlations between the presence of angel employees and future innovation output. Our preferred dependent variable of interest is the economic value of patents, based on stock market reactions to patent grants (Kogan et al., 2017). Our second measure is (forward) citations received. The independent variable of interest is either defined as a dummy when angel employees are present or not or the natural logarithm plus the total number of angel employees. In our baseline regression, the presence of angel employees is associated with 4% lower economic value of patents.

This baseline result is purely correlational and likely suffers from omitted variable bias. For example, a life-cycle based hypothesis implies that employees invest in innovative start-ups if

[3]Indeed, previous research has shown that many angel investors obtain a board seat or act in an advisory role (Wallmeroth et al., 2018).

innovation output of their employer is declining. To address this, we exploit within firm-year variation. We use the fact that angel employees and innovation output are geographically dispersed across states within a firm. We aggregate innovation across research departments of a firm in a firm–state–year panel. This allows us to control for observed and unobserved firm–year, firm–state and state–year fixed effects. These fixed effects account for firm-year explanations such as the life cycle stage. We analyze whether innovation declines in a research department where angel employees are co-located. Indeed, innovation output declines by $47 million after four years when angel employees are co-located.

Next, we analyze dynamic effects in a staggered differences-in-differences event time framework. Within a firm, we compare a firm-state with angel employees (treated) to a firm-state without (never treated). Reassuringly, we do not see any evidence of pre-trends. The negative effect is confined to the time *after* employees personally invest in start-ups. This alleviates the concern that reverse causality explains our results.

We complement our results with an instrumental variable regression. To some extent, venture capital competes with angel employees. We use the staggered implementation of the PIR, the so-called "prudent man rule" as a competition shock which crowds out angel employees. We provide support for this as state pension funds suddenly provide much more VC financing and because they tend to invest locally. The staggered state-wise implementation of the PIR is a strong negative predictor of angel employee activity. The second stage confirms our baseline and difference-in-differences results. We argue that the exclusion restriction is plausible as previous research has showed that if anything, venture capital *positively* affects innovation output Kortum and Lerner (2000). This would bias us against finding a negative effect.

We provide evidence on two novel channels which explain the negative baseline effects. First, agency conflicts and second, employee exit and thus loss of highly skilled human capital. Angel employees trade-off whether to exert effort at their employer or their personal investments. This trade-off is exacerbated by the long-term nature of angel investments and the potential to earn extra-ordinary returns. There are two ways how a agency conflict can manifest: selection and/or treatment. Careful selection of investments might involve time intensive deal scouting. Angel employees might also actively monitor their portfolio start-ups. In doing so, angel employees might help the start-ups in their day-to-day operations and provide advice and expertise. They could also be involved in intensive board meetings for follow-on financing rounds, an acquisition, or going public. We proxy for active monitoring and selection with ex-post startups success, and expect lower innovation for ex-post relatively more successful start-ups. We find this to be the case. The negative effect is more pronounced if the linked start-ups were ex-post relatively successful.

To further analyze agency conflicts, we explore the effect of the passage of the Small Business and Jobs Act (SBJA), which made angel investments tax exempt after 2010. We use the passage of the law as a quasi-exogenous shock that affects the *incentives* of angel employees to select and monitor their start-up investments. Thus, angel employees would be incentivized to spend more time and effort in their invested start-ups rather than at their employer. This allows us to tease out the effect of agency conflicts inherent in angel investments. The negative effect is much more pronounced for angel employees who invested in start-ups eligible for tax exemption after 2010.

As a second channel, we look at exit rates and the loss of human capital. Active angel employees are, during the time of their investment, 2% more likely to exit their firm. This is costly for employers due to high replacement costs especially for highly skilled employees.

We turn the focus to the point of view of the start-ups. We ask the question whether angel employees are beneficial for their portfolio start-ups. In difference-in-differences regressions, we compare start-ups financed by angel employees to start-ups financed by other angel investors. We see that angel employee participation in a funding round sharply increases follow-on venture capital participation, a higher M&A probability as well as higher innovation output of the early-stage firms. Angel employees are thus either skilled in selecting and/or treating their investments, which our analysis does not allow us to disentangle. This evidence is inconsistent with the hypothesis that angel employees are bad employees. It is rather consistent with the hypothesis that innovation output of employers particularly suffers when its skilled employees divert their time and effort.

Lastly, we perform some plausibility and robustness exercises. We first show that our baseline effects are primarily due to angel employees who are *directly* related to innovation. We next generalize our findings also to non patent based innovation outcomes such as new product announcements, trademarks, and scientific publications. Our results also hold when generalizing to only private employers as well as excluding recently IPO firms.

Our analysis is nuanced as we show negative effects for employers and positive effects for early-stage firms. We perform a back-of-the-envelope welfare analysis and compare the innovation output lost on the employer side with the innovation gained on the start-up side. We calculate an aggregate yearly loss of 1,506 patents for public employers and a gain of 1,993 patents for start-ups. This crude calculation indicates that the welfare effects are likely positive.

We primarily contribute to the literature which explores the role of angel investors in the economy. To the best of our knowledge, we are the first to combine employment and investment information of angel investors. 20% of angel investors are angel employees and we show that they can be important corporate decision makers. Previous research has shown that angel investors are beneficial for early-stage firms (e.g., Kerr et al., 2014; Lerner et al., 2018; Sudek et al., 2008) We add to the literature an important source of heterogeneity of angel investors. It seems to be highly skilled employees with industry expertise who are beneficial for early-stage firms.

The remainder of this paper is structured as follows. Chapter section 3.2 provides a description of how we obtain data on angel employees and descriptive statistics of our sample. Chapter section 3.3 provides empirical results. Chapter section 3.4 shows channels. Chapter section 3.5 analyzes the point of view of early-stage firms. Chapter section 3.6 looks at robustness. Chapter section 3.7 concludes.

3.2 Data

3.2.1 Angel Investments

Our data on angel investments comes from Crunchbase. Crunchbase contains information on more than 450,000 funding rounds across 173 countries. The company gathers information initially through crowd sourcing and validates accuracy through its dedicated data science team. The vast majority of data is collected through its partnerships with more than 3,500 investment firms, an active community of users, and staff who continuously update data.[4] We focus on angel investments and thus only retain participation in funding rounds of individuals. In total, there are 25,999 unique angel investors in the sample, of which 14,772 have investments in early-stage firms headquartered in the US.

3.2.2 Employment Histories

Key to our data collection is matching angel investors to their employer. We obtain biographies either through LinkedIn or through manual searches. We obtain historical employment data from public LinkedIn profiles. Crunchbase provides individual profile links for the majority of angel investors in our sample. We verify these and collect missing links through manual searches. We match employer names from LinkedIn to publicly listed firms using a fuzzy name matching algorithm. For this purpose, we obtain historical names from CRSP. We standardize names and remove legal pre- and suffixes. Then we compute a Levenshtein distance which measures the distance between strings. We manually verify close strings. Second, we complement this data with manual data collection. Well known individuals such as Mark Zuckerberg might be less likely to have a LinkedIn profile page. Omitting such angel employees might introduce sample selection. We mitigate this problem by manually obtaining the employment history of all angel employees with at least three investments in our data. Of the 10,723 unique angel investors, we obtain full employment history (LinkedIn and manual searches) of 9,006 angel investors, a coverage of 84%.

We note that angel investments do not need to be disclosed, so we are likely to capture a lower bound of the angel investor universe. A concern is whether the public disclosure of angel investments suffers from selection bias. Start-ups might have an incentive to strategically disclose prominent investors as they can serve as a credible signal to the market. Strategic disclosure by angel employees on the other hand might be more problematic. If employees of more innovative corporations are less likely to disclose their angel investments, we would overestimate the negative effect of angel employees. However, this is unlikely for two reasons. First, we look at the total number of angel employees of the whole corporation, so strategic disclosure needs to be correlated on a firm level. Second, in order for this to be a problem, there needs to be a correlation between angel investment disclosure and *future* declining corporate innovation. We do not think that this is likely, but this remains a potential concern for our analysis.

[4]Crunchbase has been compared to traditional datasets and is the most extensive database for early-stage start-up funding round information (Dalle et al., 2017; Ling, 2015; Retterath and Braun, 2020) A number of recent papers rely on Crunchbase for data on early-stage private financing rounds (Dimmock et al., 2019; Edwards and Todtenhaupt, 2020; Kaplan and Lerner, 2017).

3.2.3 Sample Construction and Independent Variables

We display filter steps in Table 3.1. In order to restrict ourselves to angel investments, we only keep data on equity or equity-like investments that are tied to individuals. We remove investments tied to venture capital partners and individuals employed in a corporate venture capital unit. We restrict the sample to US early-stage firms in the years 2001 to 2018 due to low data coverage before 2001. After matching angel investors to corporations, our final data set of angel employees is comprised of 1,845 unique angel employees, which work for 792 unique corporate employers. Since angel employees have multiple investments and the size of the funding rounds are large, the total of all unique funding rounds in the final sample covers more than $21 billion early stage financing. This includes many well-known startups and angel employees. More detail on data collection and background information is available in Section A.3.2 in the Appendix.

We make use of the standardized nature of LinkedIn profile information and collect information on location and the individual's role within the organization for all angel employees in our sample. Following evidence from business angel surveys, we assume an average angel investment holding period of five years.[5] Our variable of interest is either defined as a dummy variable equal to one if there is at least one angel employee on a firm level. Alternatively we take the natural logarithm plus one of the total number of angel employees. The variation in this variable comes from two sources: 1) existing employees of a firm start to invest in early-stage firms and effectively become angel employees and 2) existing angel employees move across firms. We retain both sources of variation, however the vast majority comes from the first source.

3.2.4 Innovation Output

Our main measure of firm innovation is the economic value of patents obtained from Kogan et al. (2017), henceforth referred to as KPSS. The provided data links patent numbers to publicly listed firms and includes the economic value of patents from 2001 to 2018, which in total includes 1,283,974 patents granted to 5,321 firms. The economic value of patents is based on stock market reactions to patent grants. We are primarily interested in understanding whether angel employees provide value to shareholders. As noted in Kline et al. (2019), the KPSS measure is particularly suitable for this purpose as opposed to other measures of innovation. We aggregate our preferred innovation variable on a yearly level and scale by total assets following Kogan et al. (2017). Our second measure of firm innovation are citation-weighted patents. Since younger patents naturally have less time to be cited, we perform a truncation-adjustment and control for year and technology class fixed effects (Lerner and Seru 2021, and Dass et al. 2017). In order to limit a possible truncation bias, we follow the suggestions by Lerner and Seru (2021). We obtain citations received until December 31, 2021 directly from the United States Patent Office (USPTO) accessed through Patentsview. Our main regressions only use patents granted until the year 2018, such that each patent has at least three years to be cited. In order to identify innovation creation more timely, we

[5] The American Angel (2017) among others say that the target mean and median duration of a typical angel investment is five years. The results are quantitatively and qualitatively similar when assuming that angel employees keep their investments for shorter time periods or forever.

use the application year of the patent. We use three alternative measures of innovation that are non-patent based: the number of trademarks and new product launches. We also look at science publications, obtained from Arora et al. (2021).

Some of our regressions make extensive use of the precise location where innovation is generated. Specifically, we aggregate innovation on a more granular firm-state-year level. In the following, we use IBM as an example to highlight the potential usefulness of such a panel. Patent data includes detailed information on which city where each inventor works. Over our timeframe, we observe patent filings of IBM inventors in 45 different states (plus Washington D.C. and Virgin Islands). Based on this data, IBM only lacks research departments in Mississippi, Montana, North Dakota, South Dakota, and West Virginia, but otherwise they have a presence in all other states. This indicates that innovation creation at a large firm can be geographically quite dispersed. IBM is officially incorporated and headquartered in Armonk, New York. In the year 2016, we analyze the distribution of innovation generation across US states for IBM. New York, as the headquarter location, is the state with the largest share of innovation generation. As a percentage of citations generated, New York however, only makes up around 10% of the total citations generated that year for IBM. California follows with 7.2%, and Texas with 6.8%. IBM, as a large and research-intensive firm, is arguably an extreme example, so we repeat this exercise systematically. The average firm in our sample generates 64% at the mean and 76% at the median of innovation in the headquarter state. The average non headquarter state generates 6% of innovation at the mean and 0% at the median. The previous literature often attributes all innovation to the headquarter state. In later analyses, we will make use of where the innovation output is created.

We also match patents to startups in our sample. We perform a fuzzy name matching algorithm and exclude punctuation, capitalization and legal pre- and suffixes. We only keep matched firms in the two databases if they are located in the same state. In total, we match 418,973 patents to 12,236 start-ups. From this data, we compute truncation-adjusted citation-weighted patents on a startup-year level.

3.2.5 Other Control Variables

We obtain additional firm level control variables from CRSP and Compustat. We follow Fang et al. (2014) and control for the following 15 variables: log of market capitalization, research and development expenses, Tobin's Q, profitability, asset tangibility, the log of firm age, the Herfindahl index defined over yearly sales in the 4-digit SIC code, Herfindahl index squared to capture non-linear effects, stock liquidity proxied by the daily mean bid-ask spread, capital expenditures, leverage, financial constraints, past patent stock, and the log of the number of employees. We also control for the presence of a corporate venture capital program following Ma (2020)[6]. All variables and sources are listed and described in Appendix A.3.1. To mitigate the impact of outliers, we winsorize all continuous variables at the 1% level.

[6]More corporations have active angels than an active corporate venture capital program. There is hardly any overlap between the two ways of investing in startups within the corporation. It is very rare that an employee invests in a startup and the corporate venture capital program of the employer invests in the same startup.

3.2.6 Descriptive Statistics

Table 3.2, Panel C presents the descriptive statistics of the variables used in our study. The economic value of patents refers to the innovation output of a firm as measured by stock market reactions to patent grants applied in the next year. Our sample statistics are quantitatively similar to previous studies (Fang et al., 2014). The patent distribution is highly skewed. The mean economic value of patents in our sample is 3% of the book value of a firm. Our main variable of interest, the number of angel employees, is also highly skewed. The vast majority of firms do not have angel employees. We directly address econometric concerns due to the skewed distribution in the upcoming sections, e.g. in later analyses we confine the analysis to within-firm with angel employees.

[Insert Table 3.2 here]

Our setting involves personal angel investments of individuals who are simultaneously employed at a public corporation. We are not aware of existing research that has collected this data, therefore, we first provide some descriptive statistics on several dimensions of our sample, to make the presented evidence more accessible to interested readers, encourage more research, and also to motivate the choice of some of our specifications.

Angel employees are not a rare occurrence. 42% of angel investors with observable employment history are or were employed at a listed corporation. This is consistent with survey evidence such as The American Angel (2017). In this survey, 55% of angel investors are or were previously executives at for-profit companies and 46% are or were members of the board. When we look at a narrow time around the angel investment (up to 5 years after), 20% of angel investors are active angel employees. In our data, 792 publicly listed firms have angel employees. Firms in information technology and related industries have the most (see Panel A in Table 3.2). The company with most angel employees in our sample is Alphabet, to which we can link a total of 196 employees who personally invested across 433 start-ups between 2001 and 2018.

To provide a sense of what role angel employees play at their employer, we visualize their function in Figure 3.1. 43% of angel employees are members of the board of directors. 14% are executives (of which 35% are CEOs) and we classify the remainder as others. When we look more closely into the third category, almost all belong to upper management. Most angel employees report their title as: presidents, vice president, group lead, and other senior managerial roles. Another observation is that many angel employees are in innovation related roles. We often see titles such as product manager, developer, researcher, etc. on the self-provided LinkedIn profile. In a later analysis we separate angel employees in whether they are in a innovation related and non-innovation position. We use the profile information obtained from LinkedIn and based on the title of each employee, we tag employees with the words "product", "innov", "research", "engineer", "tech", among other keywords as innovation-related and angel employees with titles such as "finance", "legal", "accounting", "audit", "operation", "banking", among others as non-innovation related angels. We classify 61% as innovation related, 6% as non-innovation related. The remainder group is classified as neither and is largely composed of directors and executives. This highlights that angel employees are either senior level employees or are also closely related to innovation.

[Insert Figure 3.1 here]

We next look at the size of funding rounds in our sample. As shown in the first row of Panel B of Table 3.2, the median (mean) funding round in our final data set is \$1.7M (\$5.7M). This amount is significantly larger than a typical angel financing round as previously reported by surveys The American Angel (2017). Unfortunately, we do not observe the individual amount each angel employee invests, but only the total amount of each financing round. Many rounds include both angel investors and venture capital investors. The large amounts however make it unlikely that these investments are only token investments, but rather that there are large potential returns, and thus incentives to help the portfolio early-stage firm succeed.

In later analyses we exploit the fact that most investments are local. For this purpose, we compute the distance by using the headquarter location of the employer and the location of the start-ups, when available. We infer the distance from the city level and use the latitude and longitude of the city midpoint. As shown in the second row of Panel B of Table 3.2, the median (mean) distance of an angel employee investment is 59 (1,185) miles. We compute a dummy equal to one if the angel and startup are located in the same state. The dummy is equal to one for 59% of angel employees' investments.

We also look at the industry similarity between employers and start-ups in which angel employees invest. Early-stage investments are characterized by high information uncertainty. Angel employees have industry expertise and can leverage this to select high-quality early-stage firms. Crunchbase does not provide standard industry classifications such as SIC or NAICS codes. We therefore compute a textual product market closeness variable between start-ups and corporations similar in spirit to Hoberg and Phillips (2010). We obtain a textual description of corporations from section 1 or 1A from the 10K of corporations from EDGAR. Crunchbase provides a textual description of most start-ups in our sample. We weigh unique words in both vectors by their occurrence and compute a cosine similarity. Linked start-ups have an average cosine similarity of 3.5%. To interpret this number, we compare it to the similarity of randomly matched pairs. We draw 3,000 random startup-corporation matches and receive a cosine similarity of 1.1%. Actual matches are therefore more than three times closer than a randomly matched pair.

To sum up, a large portion of angel investors are angel employees. Angel employees are often board members, executives or other senior employees and are closely related to innovation. They tend to make investments that are large in value, local and the business of the early-stage firms are closely related to the industry of their employers.

3.3 Empirical Results

3.3.1 Baseline Panel Regression

To investigate the effect of angel employees on innovation output, we estimate the following panel regression:

$$Innovation_{i,t+1} = \beta \times AngelEmployeeDummy_{i,t} + \gamma \times \mathbf{X}_{i,t} + \theta_i + \phi_t + \epsilon_{i,t} \tag{3.1}$$

where i represents firm i in year t. We measure *Innovation* as the innovation output of patents filed in the next year. We use two main measures of innovation output: The total yearly economic value of patents scaled by total assets following Kogan et al. (2017), and truncation-adjusted citation-weighted patents. In our baseline specification, we define our independent variable of interest, *Angel Employee Dummy*, as equal to one if a firm employs at least one angel employee in a year.[7] Alternatively, we define the independent variable as the natural logarithm plus one of the total number of angel employees. The vector $\boldsymbol{X}$ represents 15 standard control variables, as previously described. The variables θ and ϕ are firm and year fixed-effects, respectively. Year fixed-effects account for year-specific shocks to innovation. Firm fixed-effects control for non time-varying unobserved factors on the firm level. We cluster standard errors on a firm level to correct for auto-correlation of innovation at a given firm over time following Fang et al. (2014).

[Insert Table 3.3 here]

The results are presented in Table 3.3. In Panel A, column (1) of the first row, the presence of at least one angel employee is associated with a decrease in the economic value of patents by 3% of the book value. In column (2) we alternatively use the natural logarithm of one plus the total number of angel employees. Due to the skewed nature of our variables, in columns (3) and (4) we repeat the previous regressions, however we replace the dependent variable with an inverse hyperbolic sine transformation.[8] The results are unchanged. In column (5), we restrict the sample to only those firms that patented during our sample period. The results remain qualitatively similar. Panel B of the table repeats the analysis but for a different dependent variable: truncation-adjusted citation-weighted patents.

3.3.2 Within Firm-year: Angel Employees and Innovation Output Across States

One reason that prevents us from drawing causal conclusions from our baseline results is a firm life-cycle based explanation. If a firm matures and faces lower future growth opportunities, employees might personally invest in start-ups to diversify themselves. This would lead to a spurious correlation of angel employees with lower future innovation. In order to alleviate such concerns, we introduce firm-year fixed effects to the regression. This controls for observed and unobserved firm-year level omitted variables such as firm life-cycle stage, yearly capital expenses, or the annual research budget. Adding this stringent set of fixed effects is only possible if we use *within* firm-year variation of angel employees and innovation output. For this, we use the location of angel employees as provided by LinkedIn data. For innovation output, we use USPTO data which provides the precise location of each inventor listed on a patent. Similar to Foley and Kerr (2013), we assign the economic value (or forward citations received) of each patent proportionally to the U.S. states the inventors are located in. We thus aggregate innovation output across each firm per state per year. We ask the following

[7]The results are robust to using the raw number, as well as scaled versions (e.g., by the board size) of the total number of angel employees at a firm.

[8]The inverse hyperbolic sine transformation is defined as $log(y_i + (y_i^2 + 1)^{1/2})$ and has the additional desirable property to allow zeros to be included without adding a constant term.

question: Does innovation for a firm decline precisely in the state where an angel employee is co-located? In order to answer this question, we run the following regression:

$$Innovation_{i,s,t+k} = \beta \times AngelEmployeeDummy_{i,s,t} + \theta_{i,t} + \phi_{i,s} + \psi_{s,t} + \epsilon_{i,s,t} \tag{3.2}$$

The unit of observation in this regression is the innovation output aggregated on a firm-state-year level. $\theta_{i,t}$ captures firm-year fixed effects as discussed previously. $\phi_{i,s}$ captures firm-state fixed effects, and $\psi_{s,t}$ captures state-year fixed effects. We include firm-state dummies to control for the fact that a given firm might have an R&D department in Washington because of local technology expertise in this state. State-year dummies capture state-wide economic shocks. $AngelEmployeeDummy_{i,s,t}$ now is a dummy variable equal to one if there is at least one angel employee in a given firm-year-state observation and zero otherwise. The dependent variable is either the total economic value of patents or total truncation adjusted citation weighted patents, again on a firm-state-year level. The coefficient of interest in this regression is β. We cluster standard errors on a firm-state level. Given this strict set of fixed effects, the relevant variation to identify the effect on innovation output comes from the time varying presence of angel employees for a firm in a given state.

[Insert Table 3.4 here]

Results are shown in Table 3.4. Since the total assets of a firm is absorbed by firm-year fixed effects, the economic value of patents can be directly interpreted in million USD. In Panel A, we see that angel employee presence does not immediately lead to a decline in innovation output. However, the presence of at least one angel employee is associated with -32 million USD lower economic value of patents after three years. The effect persists over time.

To put the economic effect into context, we relate the $62 million figure after 5 years to the sample mean. Angel employees are most prevalent among large firms that are highly innovative. The average economic value of patents in a firm-state observation conditional on angel employee and patenting activity is $400 million per year. Relative to this number, the economic effect is thus a decrease of around 15% when angel employees are co-located.

3.3.3 Within Firm-year: Event Study and Dynamic Effects

An important concern for a causal interpretation of our results is reverse causality. Reverse causality makes intuitive sense if innovation output is already on a downwards path and this causes employees to invest in early-stage firms. To address this concern, we estimate the following difference-in-differences regression in an event time framework:

$$Innovation_{i,s,c} = \phi \sum_{c=-10}^{c=+9} D_c \times \sum_{c=-10}^{c=+9} \beta_c D_c \times AngelEmployeeDummy_{i,s} + \phi_{i,s} + \epsilon_{i,s,c} \tag{3.3}$$

for this specification, *Angel Employee Dummy* is a dummy variable equal to one if there is at least one angel employee at the firm in the state. This dummy variable is interacted with time dummies relative to the first angel employee in the firm. Control observations are the remaining firm-state observations that do not have a co-located angel employee. This specification automatically controls for firm-year fixed effects as we directly compare states with angel employees to those without in the same firm-year. We additionally include Firm-State fixed effects, as well as time fixed effects. We again cluster standard errors on a firm-state level.

The regression is essentially a staggered difference-in-differences where all events are normalized relative to event time. We thus compare treated firm-states, those with an angel employee presence, with never-treated firm-states. We do so in order to mitigate a negative weighting problem common to many staggered difference-in-differences settings (Baker et al. 2022).

[Insert Figure 3.2 here]

Figure 3.2 visually presents the results of the difference-in-differences event study. The presence of at least one angel employee in a state decreases the economic value of innovation. There is a small effect in the treatment year, however the coefficient turns negative and statistically significant afterwards, increasing over time. There are no pre-trends. The negative effect is prominent only *after* the presence of angel employees. To some extent this reduces concerns that reverse causality drives the effect. We replicate the event study using citation weighted patents in Panel B with similar results.

One concern here is that the results are driven purely by the headquarter state of the firm. We do not know the headquarter state for all firms in the sample, however we repeat the analysis and drop the most important state for each firm as measured by innovation output. Indeed, and as expected for senior level employees, the majority of angel employees are located in the biggest state. We visualize the result of the analysis in Figure A.3.1. The results are similar however the economic significance is cut in half. The negative effect of angel employees is thus not purely driven by senior employees in the headquarter state.

3.3.4 Instrumental Variable Regression: Competition from Venture Capital

Our previous results address some endogeneity concerns, however we are unable to rule out omitted variable bias for an individual choice like conducting angel investments. In an ideal experiment, we would want to randomize angel employees across US firms. As this is not feasible, we use an instrumental variable approach to complement our previous analyses. We base the instrument on competition in the early-stage financing market. Recent literature has shown, theoretically (Hellmann and Thiele 2015) and empirically (Hellmann et al. 2021), that venture capital and angel financing can be substitutes. Our descriptive statistics also show that investments by angel employees tend to be large. Thus, to some extent, they might compete with traditional venture capital funds. Our instrumental variable regressions exploit an arguably exogenous variation provided by increased competition in the early-stage financing market.

We exploit the staggered passage of the so-called PIR, the "prudent man rule" (González-Uribe 2020). These regulatory-induced changes lead to an inflow into venture capital of state pension funds. Specifically, state pension funds were mandated to apply modern portfolio theory and invest as a prudent investor should, and thus include local private equity in a diversified portfolio. González-Uribe (2020) shows an economically significant 54% increase in capital commitment after a state passes the PIR. We use this staggered adoption in a 2SLS regression, where the inflow of venture capital serves as a plausibly exogenous instrument which crowds out angel employees. There are two necessary conditions: relevance and the exclusion restriction.

The relevance condition is likely satisfied for several reasons. First, due to their large size, state pension funds are one of the largest investors in private equity (González-Uribe 2020). Second, the literature and summary statistics show that angel investments tend to be local. There is also a strong local bias for pension funds (Hochberg and Rauh 2013). Lastly, when more venture capital enters the market and provides financing to early-stage firms, it is unlikely that future financing rounds will involve angel investors. We test the relevance condition formally in a first stage regression. Consistent with a crowding-out hypothesis, the staggered passage of the PIR negatively predicts angel employees. The F-statistic is equal to 52 and decreases for longer time periods. The F statistic is thus higher than the conventional threshold of 10, but short of recently proposed thresholds such as Lee et al. (2022).

The exclusion restriction is a necessary assumption that our instrument does not directly affect future innovation of the employers of angel employees. We argue that this assumption is plausible for several reasons: First, the source of funds in the PE market comes from pension funds which are often constrained or have a home bias to invest locally (Cumming and Dai, 2010; González-Uribe, 2020; Hochberg and Rauh, 2013). This variation in fund raising is likely to be unrelated to corporate innovation. Second, Hirukawa and Ueda (2011) show that VC has no effect on innovation, whereas Kortum and Lerner (2000) show that VC investments create significant *positive* spillovers. More recently, Howell et al. (2020) show that VC investments tend to be pro-cyclical rather than counter-cyclical. Such evidence biases us against finding a negative effect of our instrument on corporate innovation.

[Insert Table 3.5 here]

Table 3.5 presents the results of the regression. The point estimates are statistically significant and negative across the specifications and panels. The evidence largely confirms our previous results.

The economic magnitudes of our instrumental variable analyses are large. There are several reasons for this. As discussed before, angel investment activity is primarily unobserved and hence measured with error. Given the data available to researchers, we are likely to capture a lower bound of angel employee activity. It is likely that angel investments within the firm are followed by even more investments due to internal spillover effects of investment decisions similar to Ouimet and Tate (2020). The instrumental variable regressions may additionally pick up this unobserved angel investment activity and account for measurement error. We also note that, assuming unbiasedness, the OLS regression estimates an average treatment effect, whereas the IV estimates a *local* average treatment effect. For our setting, it is plausible that the subset of employees that are dis-incentivized to invest in early-stage firms (compliers) due to competition from VC are the ones that are likely to have a higher negative impact. The subset of employees that are likely to invest regardless of competition in the early-stage financing market (never-takers) might have a weaker localized effect.

Our magnitudes are of economic significance but smaller compared to the effect of CEO characteristics on innovation. One example is the importance of independent boards in Balsmeier et al. (2017). Another example is the effect of inventor CEOs in Islam and Zein (2020), who find that firms with inventor CEOs have 66% more patent applications, 117% more citations, and 122% higher economic value of patents. Given that angel employees are likely not as influential as CEOs, it is not surprising that our estimates are smaller in magnitude.

3.4 Channels

We present evidence on two novel channels. First, we provide a string of evidence that suggests agency conflicts are a channel for the negative relationship between angel employees and future firm innovation. The second channel we highlight is that angel employees are more likely to leave the firm and the loss of highly skilled human capital is costly for employers.

3.4.1 Agency conflicts

Angel employees' attention is a limited resource and agents strategically allocate time and effort to their tasks. Essentially, an angel employee faces the trade-off between exerting effort at her corporate employer and her personal investments.

3.4.1.1 Ex-Post Successful Start-ups

Angel investments are characterized by high risk and potentially high reward. Angel investors often receive so-called *homerun* returns of more than 100% of their initial investment (Wiltbank et al. 2009). Such a risky endeavor might incentivize angel employees to spend significant time selecting and/or monitoring their own investments rather than exerting effort at their corporate employer. In the following, we hypothesize that investments that are relatively successful, i.e., which did not fail, should lead to a stronger agency conflict. This can be due to several reasons. The investment duration in a non-failed start-up is likely longer compared to a start-up that fails. For ongoing investments, angels might be engaged with their start-ups to help them succeed. Additionally, if some angels obtain board seats, there will be time intensive board meetings for follow-on financing rounds, or if the start-up is ultimately acquired, or goes public. Finally, for relatively more successful start-ups, angel employees might be particularly engaged and exert effort to select such investment opportunities. Overall, we expect the negative relationship to be stronger for firms associated with relatively more successful (non failed) start-ups.

We incorporate ex-post information about a start-up's status to test whether results are driven by ex-post non failed start-ups. We mark start-ups as failed if they are flagged as defunct or did not receive additional funding in the last 5 years. We take the number of failed and non-failed start-ups of all employees' investments for each corporation in a given year. We then run the baseline specification of equation 3.1. We replace the *Angel Employees* variable with *Non-Failed Start-ups* and *Failed Start-ups* which is the natural logarithm of the number of non-failed and failed start-ups per firm-state per year, respectively.

[Insert Table 3.6 here]

Table 3.6 documents our results. As before in Panel A, we document the results using the economic value of patents as our dependent variable. In Panel B, we use truncation adjusted citation weighted patents as our dependent variable. In both panels, we consistently observe that links to non-failed start-ups are associated with lower future firm innovation. The effect for non-failed start-ups is more pronounced and is statistically different from the coefficient on failed start-ups. The coefficient for failed start-ups is still negative.

Hence, the results show that the negative effects are most pronounced when the early-stage firms of angel employees are relatively successful. The results are also consistent with the explanation that highly skilled senior employees are affected by agency conflicts.

3.4.1.2 Incentives to Invest: Evidence from the Small Business and Jobs Act 2010

To identify the effect of angel employees' *incentives* to engage with their portfolio start-ups, we exploit the passage of The Small Business and Jobs Act 2010 (SBJA). This regulatory change presents a plausibly exogenous shock to angel employees' incentives to be more involved with their invested start-ups. The SBJA allows investors to exclude 100% of the eligible gain from qualified small business stock (QSBS) upon sale or exchange from September 27, 2010 onwards (Edwards and Todtenhaupt 2020). To qualify as a QSBS, the firm must be listed as a domestic C-corporation and have less than $50M in total assets. Exemption from capital gains taxes is granted if an angel investor holds her investment for at least 5 years. Some industries are excluded, however, almost all start-ups in our sample are in treated industries.[9]

This regulatory change provides us with a unique setting to test some predictions using our data. In principle, if angel investments are tax-exempt, conditional on being an angel investor, there are more incentives to divert time and effort as future capital gains are higher. We argue that if agency conflicts indeed drive the observed negative effect, capital gains tax exemption of angel investments should lead to a stronger agency conflict and hence a more negative effect. We note that our objective is not to randomly allocate angel employees across firms. Rather, we attempt to disentangle the *incentives* of angel employees to engage with their portfolio firms and study the effect of higher angel incentives on employer innovation output.

$$Innovation_{i,t+k} = \beta_1 \times Treated_{i,t} + \beta_2 \times Treated_{i,t} \times Post_t + \gamma \times \mathbf{X}_{i,t}\phi_i + \psi_t + \epsilon_{i,t} \quad (3.4)$$

In order to test our hypothesis, we run a difference-in-difference regression (as shown in Equation 3.4). In our preferred specification, we identify treated firm-years as any firm-year with at least one angel employee. $Treated_{i,t}$ takes the value of 1 if there is at least one angel employee in a firm-year and zero, otherwise. $Post$ is a dummy equal to one in the years after 2010. $X_{i,t}$ is a vector of firm-year level control variables as before.

[Insert Table 3.7 here]

[9] Explicitly excluded are, for example, financial services, accounting, law, farming, hotels, among others.

The results of the analysis are shown in Table 3.7. Panel A presents the results with the economic value of patents as the dependent variable. Our results are driven purely by angel employees after the SBJA 2010 came into effect. In panel B, we repeat the same analysis with citation-weighted patents and reach similar conclusions.

Defining treated and control firms in this analysis is not trivial. We therefore complement the previous analysis with another treatment definition. To do so, we try to differentiate QSBS-eligible and non-eligible angel employees. A QSBS-angel employee is one who invests in at least one QSBS-eligible start-up, which represents the vast majority (87%) of angel employees in our sample. We follow Edwards and Todtenhaupt (2020) to identify start-ups qualified for the capital gains tax exemption without considering the start-ups' industries from Crunchbase. We perform a similar difference-in-difference type of analysis as before with the following specification:

$$\begin{aligned} Innovation_{i,t+k} = \beta_1 \times QSBS_{i,t} + \beta_2 \times QSBS_{i,t} \times Post_t + \beta_4 \times NonQSBS_{i,t} \\ + \beta_5 \times NonQSBS_{i,t} \times Post_t + \theta_i + \psi_t + \epsilon_{i,t} \end{aligned} \tag{3.5}$$

In Equation 3.5, $QSBS_{i,t}$ is a dummy variable equal to one if there is at least one angel employee who invests in QSBS eligible start-ups during our sample period in firm i in year t. The coefficients of interest are β_2 and β_5. If agency conflicts through incentives to engage with their portfolio start-ups are driving the negative effect of angel employees and the future economic value of patents, then we would expect the coefficient β_2 to be significantly more negative than β_5. In Table A.3.1 we document the results. As before, in panel A, the dependent variable is the economic value of patents. We see that the point estimates are higher for β_2 compared to β_5, especially over the shorter horizon. This is line with the expectation that firm-years with angel employees invested in QSBS start-ups are the main drivers. We note that the difference between β_2 and β_5 is not statistically significant at conventional levels. This is to be expected as the point estimates are estimated with noise given the challenges discussed above. In panel B, where the dependent variable is citation weighted patents, we see a clearer pattern. The point estimates β_2 and β_5 are statistically different from each other only in the first two years at the 5% level.

The results in this section are consistent with an agency conflict based explanation behind our main results. Angel employees seem to divert more effort from their employers when there are regulatory-induced higher ex-ante *incentives* to benefit from their investments.

3.4.2 Angel Employee Exit: Loss of Human Capital

In the following we provide evidence that angel employees are more likely to leave their employer. The loss of these individuals might be costly for their employers.

$$Exit_{i,t} = \beta \times ActiveAngelEmployee_{i,t} + \theta_i + \phi_{ft} + \epsilon_{i,t} \tag{3.6}$$

where $Exit_{i,t}$ is a dummy variable equal to one if the employee leaves the firm in a given year. *Active Angel Employee* is a dummy variable equal to one if the employee is around this time an active angel investor. This specification allows to analyze an within-individual comparison as we can compare an angel employee during the time of investment to before or after. Thus, θ is an employee fixed effect. ϕ is a firm-year fixed effect.

We thus assess exit rates of (angel) employees over time in Table 3.8. Active angel employees are significantly more likely to exit their firm. We see a 2% increased exit rate. Due to high employee replacement costs especially of highly skilled employees, this loss of human capital is likely detrimental for employers.

3.5 Angel Employees are Beneficial for Start-ups

We now turn to the start-up perspective and ask the following question: Is it beneficial for an early stage firm to be financed by an angel employee? Angel employees might be very skilled individuals and might possess valuable industry expertise and networks. They might also either carefully select or treat their investment. Selection would be before the investment and means that angel employees contact entrepreneurs, attend presentations and analyze pitch documents. Treatment would be after the investment and means that angel employees attend meetings, establish connections and give advice to the early-stage firm. To analyze this, we compare the future success of start-ups financed by angel employees to those financed by other angel investors in the Crunchbase sample.

We regress the presence of angel employees on the probability of start-up success and future innovation of the startup. We run a difference-in-differences regression to look at the effect of angel employees' presence. We measure start-up success with four separate variables: 1) a dummy equal to one if the early-stage firm receives venture capital financing, 2) a dummy variable equal to one if the early-stage firm was ultimately acquired (M&A), 3) a dummy variable equal to one if the early-stage firm went public (IPO), and 4) future innovation output of the early-stage firm as measured by citation-weighted patents. We look at the effect in an event time framework normalized to one year before investment. We include start-up and time fixed effects and cluster standard errors on a start-up level.

[Insert Figure 3.3 here]

Compared to the universe of angel-backed start-ups, the presence of angel employees increases future success of start-ups. We first note that receiving financing from angel employees significantly increases the chances of receiving venture capital financing. We also see an increased probability that the start-up will be acquired. We see significantly positive effects until 7 years after angel employee investment and the probability is increased by between 1% and 2% annually. For IPOs, there does not seem to be any economically sizable effect. Lastly, we see that angel employee participation is related to higher future innovation output. The economic effects are equal to around 0.4 more citation-weighted patents, an effect that prevails in the long run.

These results suggest that angel employees have an economically meaningful positive effect on their portfolio firms. This highlights an important source of heterogeneity among angel investors: Those angel investors with industry expertise tend to be the ones who have a positive effect on future success. We stress that we are unable to separate between treatment and selection in this analysis. Angel employees might be able to carefully select promising early-stage firms on (unobservable) characteristics. They might also provide valuable advice to help them succeed.

Additionally, these results also shed some light on the underlying mechanism behind our main results. These results are inconsistent with a "bad employee" hypothesis, specifically, that the negative effects on innovation output of the employer is due to low quality employees. These results are rather consistent with the view that a firm's future innovation suffers particularly because its highly skilled workforce is diverting time and effort from their corporate employer to their personal investments.

Overall, we present both sides of the coin. On the one hand, angel employees are detrimental for corporate employers' innovation. On the other hand, start-ups seem to benefit from angel employees' participation. This raises the question of welfare implication, which we address in the next sub-section of the paper.

3.6 Robustness

3.6.1 Evidence from Angel Roles: Innovation-related Angels

In the following, we introduce one source of heterogeneity: the role of angel employees at their employers. We analyze whether the observed negative effect on innovation output is more pronounced when the angel employees are closely related to innovation. To do so, we split the total number of angel employees into those that are likely to work in innovation-related functions and those that are not. As described earlier, based on the textual title information of each employee, we tag employees with the words "product", "innov", "research", among other keywords as innovation-related and angel employees with titles such as "finance" or "legal" as non-innovation related angels. We then run the baseline regression with the key independent variable split into two parts: two indicator variables measuring innovation-related and non-related angel employees separately.

Panel A of Table 3.9 shows that the results are largely driven by the sub-sample of angel employees that are innovation related. The economic magnitudes for innovation-related angel employees are higher compared to the baseline specification. Non-innovation related angel employees do not have an effect. There is, however limited power to detect significant effects for non-innovation related angel employees. The majority of employees in the sample have titles such as "engineer", "tech", "product" which we classify as innovation related. The remainder of the sample with titles such as "legal" or "finance" only makes up a small fraction of angel employees.

[Insert Table 3.9 here]

3.6.2 Non-Patent-Based Measures of Innovation

We address a number of additional aspects not considered previously in the Appendix. One possible concern is the use of patents as a measure for firm innovation. After successful innovation, a corporation faces the challenge to either patent or keep the invention secret (trade secret). Since our dependent variable only captures disclosed patents, if the most valuable inventions are not disclosed and protected due to low imitation costs, then this would lead to a measurement error in our estimates. It can also be the case that firms do not file patents, but are still innovative (Koh et al., 2021). Therefore, we obtain data on three non-patent based outcome variables: trademarks, new product launches, and scientific publications. Firms have strong incentives to file trademarks and launch new products. Compared to patents, there is less substitution with trade secrets.

[Insert Table A.3.2 here]

If innovation output is reduced, then one would expect to find fewer trademarks, new product launches, and scientific publications. Indeed, that is what we find. Angel employees are associated with fewer new product launches over the subsequent 1 to 5 years in Panel A of Table A.3.2. The point estimates suggest that a firm-year with at least one angel employee is associated with approximately 10% fewer product launches after three years ($= exp^{-0.11}$). Similar conclusions can be drawn from trademarks in Panel B, and scientific publications in Panel C of Table A.3.2.

3.6.3 Private Firms

Our analysis so far has focused on the effect of angel employees on publicly listed corporations. We generalize the negative relationship and alternatively only consider private employers. We match patent data to angel employees and to their private employers.

[Insert Table A.3.3 here]

The results are shown in Table A.3.3. The effect of angel employees is negative and statistically significant after year 2. By and large, the results are comparable to the results for publicly listed firms. Our observed negative effect thus generalizes to private firms.

3.6.4 Excluding Recent IPO Years

Recent IPOs can provide employees with more liquidity to conduct angel investments. At the same time, going public could have a detrimental impact on the innovation of firms (Bernstein, 2015). Hence, such a mechanical association could bias our inferences. To address such concerns, in Table A.3.4, we re–run our baseline regressions by excluding firm–years that are immediately within two years of the IPO–year for a firm.

The results do not change. In fact, some of the point estimates become larger compared with those in Table 3.3 suggesting that an endogenous association between IPOs and employee liquidity is unlikely to explain our results.

3.6.5 Outsourcing

We look at interactions between the employer and the start-ups themselves. An alternative interpretation to our findings is that corporations might actively push their employees to invest in early-stage firms. We call this interpretation *outsourcing*. It would be problematic if some innovation is outsourced and not situated within the boundary of the employer. A string of evidence shows that this hypothesis is unlikely. First, there are few common investments between angel employees and corporate venture capital units. We do, however for future financing rounds of the startups, observe participation by other venture capital funds. This indicates that the linked start-ups have financing needs that are not met by the employees' corporations. Second, we analyze exchanges between corporations and angel employees' start-ups. We find very few citations from the corporation to the start-up and the other way around. Angel employees' investments seem to be mostly personal and no knowledge (as measured by citations) flows between start-up and corporation. We do observe a small number of M&A transactions and see negative announcement effects on average. This could potentially be consistent with our evidence if employees are cashing out and the acquisition is the manifestation of an agency conflict similar to Benson and Ziedonis (2010). However the sample size and the number of transactions is too small to make substantiated claims.

3.6.6 Welfare Analysis

Our analysis indicates that angel employees have a negative effect on the innovation output of their employers, but a positive one on their portfolio startups. This raises the question of welfare effects. We perform a back-of-the-envelope calculation to assess overall welfare of these two effects. We attempt an apples-to-apples comparison by only considering citation-weighted patents, a measure available on both sides. Ultimately, we compare the reduced innovation output on the employer level to the increased innovation output on the start-up level.

From column (4) in Table 3.4, i.e., four years after angel employee presence, we obtain a coefficient of -3. Thus the presence of angel employees reduces citation-weighted patents by -3 truncation-adjusted citation-weighted patents. In total, we observe 502 *first–time* presence of angel employees in the firm-state panel. Hence, taking the 3 reduction in citation-weighted patents per angel employee, we compute a total loss of around 1,506 truncation-adjusted citation-weighted patents. This is the yearly aggregate innovation loss for the economy due to angel employees.

We perform a similar exercise at the start-up level. From the regressions underlying the Figure 3.3 we obtain a coefficient of around 0.4 in the long run for citation-weighted patents. This is the effect of angel employee participation on citation-weighted patents on a startup level. The start-ups in our sample unsurprisingly patent at a much lower rate compared to publicly listed firms. A benefit from the startup point of view is scalability: We have 1,845 angel employees in our sample and an angel employee on average invests in 2.7 startups. We thus compute an increase of $(0.4 \times 1,845 \times 2.7 =)$ 1992.6 citation-weighted patents. This number is a bit higher compared to the innovation lost at publicly traded firms.

This back-of-the-envelope estimate suggests that the welfare effects when only considering citation-weighted patents are positive. However, we emphasize the crude nature of these calculations. We do not consider any other negative or positive extant effects of angel employees in these calculations. Furthermore, we only consider innovation while leaving out aspects such as IPOs, M&As, employment, industry competition, product pricing, and, other non-patent based measures of innovation. If anything, our analysis highlights the need for more research in this direction to better understand the welfare implications of actors in the economy involved in many different tasks.

3.7 Conclusion

Using novel data linking angel investors to their corporate employers, we find that what we refer to as - angel employees - negatively impact the innovation output of their corporate employer. On the positive side, angel employees seem to have a positive impact on start-up success. The negative effect is stronger when incentives of angel investments are higher. We shed light on two novel channels: agency conflicts and the loss of human capital. Taken together, angel employees trade off time and effort between their employer and their personal start-up investments.

Figures and Tables

Table 3.1: Sample Selection Steps

This table shows the filters applied and the corresponding number of angel investors.

No.	Sample Selection Step	No. of Individuals
(1)	All investments tied to individuals (so-called angel investors) in Crunchbase at the point of data collection (January 2022)	25,299
(2)	Only angel investors with investments into start-ups headquartered in the US	14,772
(3)	Remove the following investment types: product crowdfunding, grants, ICOs, non-equity assistance, post-IPO funding, secondary market, debt financing, corporate rounds	14,624
(4)	Only investments between 2001-2018	10,723
(5)	Only angel investors with employment histories (LinkedIn or manual)	9,006
(6)	Only investments by angel investors employed at publicly listed corporations at any point in time (42% of step 5)	3,812
(7)	Angel investors are employed at a publicly listed corporation around time of investment (until 5 years post investment), angel employees, 20% of step 5	1,845

Figure 3.1: Role of Angel Employees

This figure visualizes the roles of angel employees in our data. Roles are defined as the position the angel employees list in their LinkedIn profile. The size of the font is weighted by counts, i.e. more frequently mentioned roles are displayed more prominently.

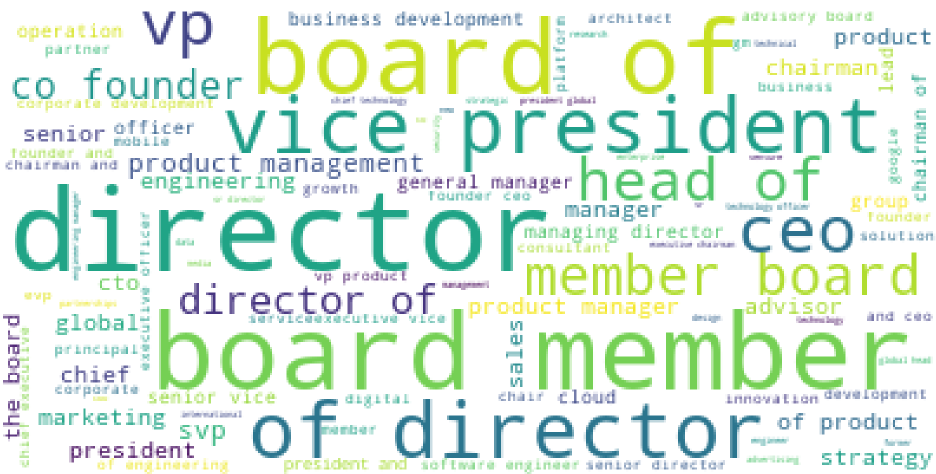


Table 3.2: Industries with Angel Employees and Summary Statistics

Panel A shows the top five SIC industries that employ the most angel employees. For each industry we list two example firms. Panel B and C shows summary statistics on the startup and the firm level, respectively. Variable definitions are provided in Section A of the Appendix.

Panel A: Industries and Employers with Most Angel Employees

Rank	SIC	Description	Example firms
1	7370	Services-Computer Programming, Etc.	Alphabet, Facebook
2	7372	Services-Prepackaged Software	Microsoft, Adobe
3	5961	Retail-Catalog & Mail-Order Houses	Amazon, Wayfair
4	2836	Biological Products	Moderna, Unity Biotech
5	7374	Services-Computer Processing, Data Preparation	Square, Paypal Holdings

Panel B: Summary Statistics on the Angel Employee - Startup level

Variable	N	Mean	SD	Min	25%	50%	75%	Max
Funding Round Size (USD M)	5,379	5,70	29.01	0.01	0.67	1.70	4.00	120.00
Startup-Corporation Distance (miles)	3,491	1,185	1,567.11	0.00	31.54	59.41	2,419.80	4,388.74
Same State Dummy	3,491	0.59	0.49	0.00	0.00	1.00	1.00	1.00
Startup-Corporation Industry Similarity	3,551	0.04	0.04	0.00	0.01	0.02	0.04	0.50
Board of directors dummy	5,379	0.43	0.50	0.00	0.00	0.00	1.00	1.00
Executive dummy	5,379	0.14	0.35	0.00	0.00	0.00	0.00	1.00
Innovation-related Angel dummy	5,379	0.61	0.49	0.00	0.00	1.00	1.00	1.00
Non-Innovation-related Angel dummy	5,379	0.06	0.49	0.00	0.00	0.00	1.00	1.00

Panel C: Summary Statistics on the Employer level

Variable	N	Mean	SD	Min	25%	50%	75%	Max
Firm-Year Panel								
Economic Value of $Patents_{t+1}$	70,408	0.03	0.12	0.00	0.00	0.00	0.00	1.2
Citation-Weighted $Patents_{t+1}$	70,408	0.42	1.15	0.00	0.00	0.00	0.00	9.21
Angel Employee Dummy	70,408	0.03	0.18	0.00	0.00	0.00	0.00	1.00
Firm-State-Year Panel								
$EconomicValueofPatents_{t+1}$	330,956	27.63	142.17	0.00	0.00	0.00	0.18	1,522.29
$Citation-WeightedPatents_{t+1}$	330,956	0.26	0.76	0.00	0.00	0.00	0.00	9.11
Angel Employee Dummy	330,956	0.01	0.08	0.00	0.00	0.00	0.00	1.00

Table 3.3: Baseline Regression: Angel Employees and Innovation Output

This table reports the fixed effect regression of equation 3.1. The unit of observation is on a firm-year level. The dependent variable in Panel A is the economic value of patents scaled by assets (Kogan et al. (2017)) in the next year ($Innovation_{t+1}$). In Panel B, we alternatively use truncation-adjusted citation-weighted patents. The independent variable, *Angel Employee Dummy*, is equal to one if there is at least one angel employee at the firm. Alternatively it is the natural logarithm of the number of angel employees on a firm level. Columns (3) and (4) use a inverse hyperbolic sine transformation of the dependent variable. Column (5) only looks at firms which patent. The regression includes 15 standard control variables. Variable definitions are provided in section A of the Appendix. The regression includes Firm and Year fixed effects. Standard errors are clustered by firm. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Economic Value of Patents					
	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$
Angel Employee Dummy	-0.03***		-0.03***		-0.03***
	(-4.43)		(-4.45)		(-3.03)
ln(1+Angel Employees)		-0.05***		-0.05***	
		(-5.44)		(-5.52)	
Panel B – Dependent variable: Citation-Weighted Patents					
	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}
Angel Employee Dummy	-0.19***		-0.09***		-0.15**
	(-4.23)		(-3.93)		(-2.57)
ln(1+Angel Employees)		-0.27***		-0.10***	
		(-5.22)		(-4.42)	
For Both Panels					
Observations	70,408	70,408	70,408	70,408	31,915
Controls	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table 3.4: Within Firm-Year: Angel Employees and Innovation Output Across States

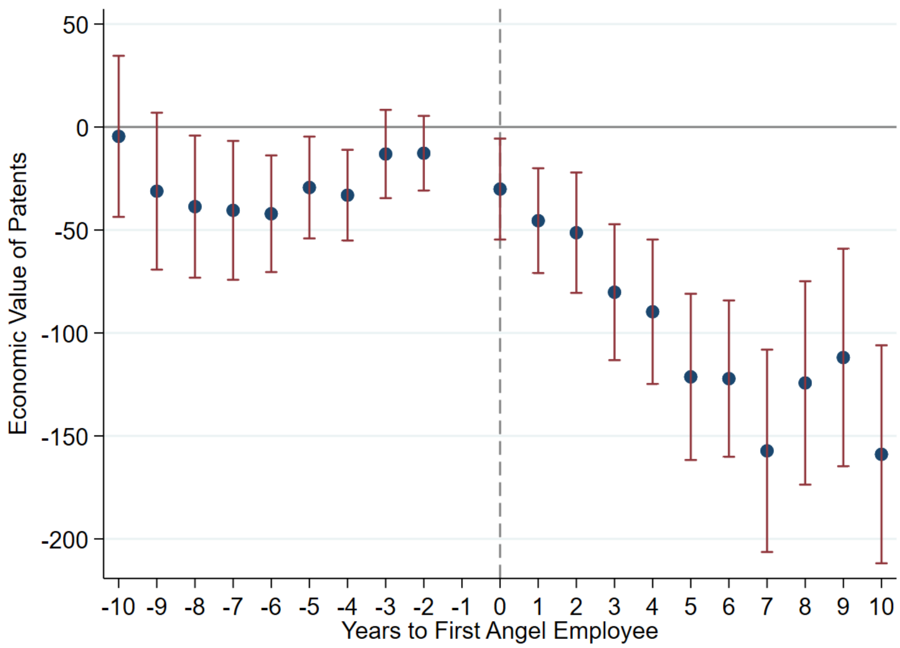
This table reports the result of the fixed effect regression of equation 3.2. The unit of observation is on a firm-state-year level. The dependent variable in Panel A is the economic value of patents (Kogan et al. (2017)) over the subsequent k years ($Innovation_{t+k}$), where $k = [1, 5]$, respectively. In Panel B, we alternatively use truncation-adjusted citation-weighted patents. The independent variable, *Angel Employee Dummy*, is equal to one if the firm employs at least one angel employee in a given year in a given state in the US. Variable definitions are provided in section A of the Appendix. The regression includes Firm-Year, Firm-State, and State-Year fixed effects. Standard errors are clustered by Firm-State. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Economic Value of Patents ($KPSS$)					
	$KPSS_{t+1}$	$KPSS_{t+2}$	$KPSS_{t+3}$	$KPSS_{t+4}$	$KPSS_{t+5}$
Angel Employee Dummy	9.63	-12.54	-31.88***	-46.52***	-61.87***
	(1.03)	(-1.32)	(-3.23)	(-4.71)	(-5.82)
Panel B – Dependent variable: Citation-Weighted Patents (CIT)					
	CIT_{t+1}	CIT_{t+2}	CIT_{t+3}	CIT_{t+4}	CIT_{t+5}
Angel Employee Dummy	-0.12	-1.20***	-2.26***	-3.07***	-3.45***
	(-0.27)	(-2.67)	(-4.60)	(-5.99)	(-6.48)
For Both Panels					
Observations	330,956	311,488	292,020	272,552	253,084
Firm-Year FE	YES	YES	YES	YES	YES
Firm-State FE	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES

Figure 3.2: Event Study: Effect of Angel Employees on Corporate Innovation

This figure reports the result of the fixed effect panel regression of equation 3.3 in a firm-state-year event time panel. The dependent variable in Panel A is the economic value of patents (Kogan et al. (2017)). In Panel B, we alternatively use truncation-adjusted citation-weighted patents. We visualize the coefficient on time dummies interacted with the dummy variable, *Angel Employee Dummy*, which is equal to one if there is at least one angel employee at the firm in the state. Time is relative to the first angel employee on a firm level. The coefficients are normalized to the year before the first angel employee. Variable definitions are provided in section A in the Appendix. The regression includes Firm-State and Firm-Year fixed effects. Standard errors are clustered by Firm-State. Confidence intervals are at the 5% level.

Panel A: Economic Value of Patents



Panel B: Citation-Weighted Patents

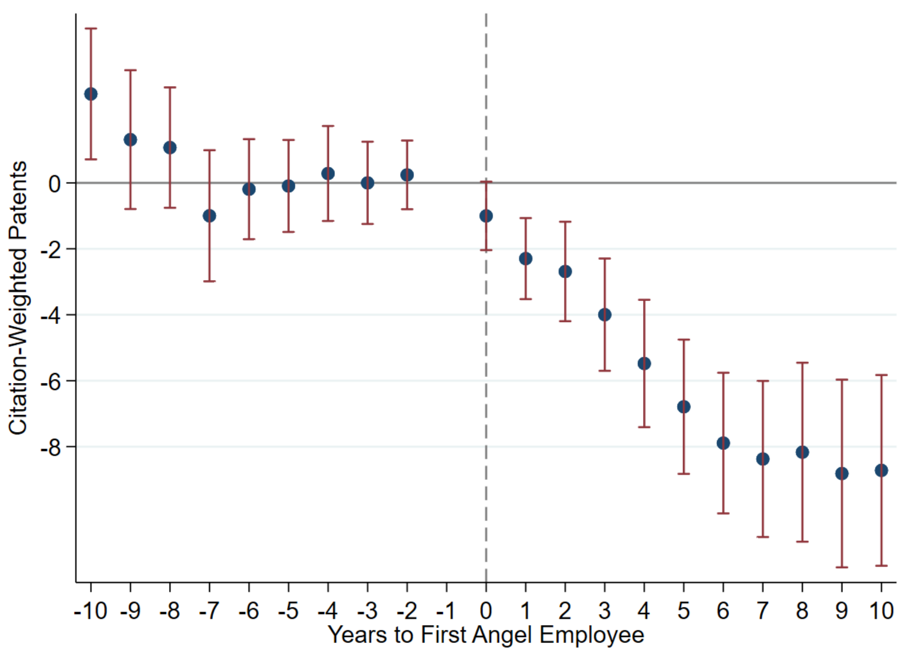


Table 3.5: Instrumental Variable Regression

This table reports the result of a instrumental variable regression similar to equation 3.2. The dependent variable is innovation output over the subsequent k years ($Innovation_{t+k}$), where $k = [1, 5]$, respectively. In Panel A it is the economic value of patents (Kogan et al. (2017)). In Panel B, we alternatively use truncation-adjusted citation-weighted patents. The independent variable, *Angel Employee Dummy*, is equal to one if there is at least one angel employee at the firm in the state. Alternatively it is the natural logarithm of the number of angel employees on a firm level. Variable definitions are provided section A in the Appendix. The first stage F statistic is reported. The regression includes Firm-Year and Firm-State fixed effects. Standard errors are clustered by Firm-State. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Economic Value of Patents ($KPSS$)					
	$KPSS_{t+1}$	$KPSS_{t+2}$	$KPSS_{t+3}$	$KPSS_{t+4}$	$KPSS_{t+5}$
Angel Employee Dummy	-500.05**	-464.63*	-570.16**	-780.35**	-734.76*
	(-2.12)	(-1.89)	(-2.08)	(-2.24)	(-1.66)
ln(1+Angel Employees)	-465.88**	-431.43*	-527.67**	-715.42**	-675.18*
	(-2.11)	(-1.89)	(-2.10)	(-2.29)	(-1.72)
Panel B – Dependent variable: Citation-Weighted Patents (CIT)					
	CIT_{t+1}	CIT_{t+2}	CIT_{t+3}	CIT_{t+4}	CIT_{t+5}
Angel Employee Dummy	-47.24***	-48.06***	-47.67***	-65.56***	-80.12***
	(-3.57)	(-3.46)	(-3.14)	(-3.21)	(-2.76)
ln(1+Angel Employees)	-44.01***	-44.62***	-44.12***	-60.11***	-73.63***
	(-3.58)	(-3.49)	(-3.20)	(-3.36)	(-2.97)
For Both Panels					
Observations	330,956	311,488	292,020	272,552	253,084
Firm-State FE	YES	YES	YES	YES	YES
Firm-Year FE	YES	YES	YES	YES	YES
1st Stage F-Stat	52.1	46.9	40.4	30.5	20.2

Table 3.6: Effect of Relatively Successful Start-ups

This table reports the result of the fixed effect regression of equation 3.2. The unit of observation is on a firm-state-year level. The dependent variable in Panel A is the economic value of patents scaled by assets (Kogan et al. (2017)) over the subsequent k years ($Innovation_{t+k}$), where $k = [1, 5]$, respectively. In Panel B, we alternatively use truncation-adjusted citation-weighted patents. The independent variable of interest is split into two parts, depending on whether an angel employee is linked to a failed or non-failed startup. We categorize a start-up as failed when the startup is either defunct of did not receive financing in the last 5 years. Variable definitions are provided in section A of the Appendix. The regression includes Firm-Year, Firm-State, and State-Year fixed effects. Standard errors are clustered by Firm-State. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Economic Value of Patents ($KPSS$)					
	$KPSS_{t+1}$	$KPSS_{t+2}$	$KPSS_{t+3}$	$KPSS_{t+4}$	$KPSS_{t+5}$
Non-Failed Start-ups	-10.57	-42.09***	-62.26***	-95.27***	-108.08***
	(-0.77)	(-2.67)	(-3.84)	(-5.01)	(-4.78)
Failed Start-ups	42.68***	15.20	-23.90**	-40.07***	-69.92***
	(4.09)	(1.40)	(-2.13)	(-3.58)	(-5.53)
Panel B – Dependent variable: Citation-Weighted Patents (CIT)					
	CIT_{t+1}	CIT_{t+2}	CIT_{t+3}	CIT_{t+4}	CIT_{t+5}
Non-Failed Start-ups	-1.57**	-2.93***	-4.73***	-5.87***	-5.97***
	(-2.44)	(-4.12)	(-6.12)	(-6.88)	(-5.71)
Failed Start-ups	1.84***	0.07	-1.58***	-2.97***	-4.25***
	(3.76)	(0.14)	(-3.18)	(-5.59)	(-7.60)
For Both Panels					
Observations	330,956	311,488	292,020	272,552	253,084
Firm-Year FE	YES	YES	YES	YES	YES
Firm-State FE	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES

Table 3.7: Evidence from the SBJA Capital Gains Exemption

This table reports the result of the fixed effect regression of equation 3.4. The unit of observation is on a firm-state-year level. The dependent variable in Panel A is the economic value of patents scaled by assets (Kogan et al. (2017)) over the subsequent k years ($Innovation_{t+k}$), where $k = [1, 5]$, respectively. In Panel B, we alternatively use truncation-adjusted citation-weighted patents. We interact the variable, *Angel Employee Dummy*, with Post, which is equal to one for all years after the passage of the SBJA. Variable definitions are provided in section A of the Appendix. The regression includes Firm-Year, Firm-State, and State-Year fixed effects. Standard errors are clustered by Firm-State. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Economic Value of Patents ($KPSS$)					
	$KPSS_{t+1}$	$KPSS_{t+2}$	$KPSS_{t+3}$	$KPSS_{t+4}$	$KPSS_{t+5}$
Angel Employee Dummy	51.20***	48.79***	42.28***	32.75***	16.24*
	(3.80)	(4.93)	(4.48)	(3.30)	(1.80)
Angel Employee Dummy × Post	-53.15***	-81.27***	-103.75***	-121.17***	-138.01***
	(-4.35)	(-6.27)	(-8.52)	(-9.22)	(-8.01)
Panel B – Dependent variable: Citation-Weighted Patents (CIT)					
	CIT_{t+1}	CIT_{t+2}	CIT_{t+3}	CIT_{t+4}	CIT_{t+5}
Angel Employee Dummy	3.29***	2.72***	1.85***	0.80	0.33
	(6.68)	(6.93)	(3.15)	(1.37)	(0.87)
Angel Employee Dummy × Post	-4.35***	-5.19***	-5.75***	-5.91***	-6.68***
	(-7.91)	(-8.39)	(-6.07)	(-4.32)	(-4.72)
For Both Panels					
Observations	330,956	311,488	292,020	272,552	253,084
Firm-Year FE	YES	YES	YES	YES	YES
Firm-State FE	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES

Table 3.8: Loss of Human Capital: Exit of Angel Employees

This table reports the fixed effect regression of equation 3.6. The unit of observation is on a firm-year level. The independent variable, $Exit_{t+1}$ is a variable equal to one if the employee has exited the firm. The dependent variable *Active Angel Employee* is equal to one if the employee is currently an active angel employee, defined as investing in early-stage firms in the last 5 years. Variable definitions are provided in the Appendix. The regression includes Firm-Year and Employee fixed effects. Standard errors are clustered by Firm-Year. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	(1)
Dependent variable:	$Exit_{t+1}$
Active Angel Employee	0.02***
	(6.07)
Observations	245,304
Firm-Year FE	YES
Employee FE	YES

Figure 3.3: Effect of Angel Employees on Start-up Success

These figures report the result of difference-in-difference regressions on the relationship between angel employee investment and start-up success. The dependent variables, from top left to bottom right, are: a VC financing dummy, an M&A dummy, an IPO dummy, and citation-weighted patents on a startup-year level. We visualize the coefficient on time dummies interacted with a treatment dummy, defined as equal to one if the startup is financed by an angel employee from our sample. Control startups are those financed by other angel investors from the Crunchbase universe. The coefficients are normalized to the year before the first angel investment. All regressions include *Startup* as well as *Time* fixed effects. Standard errors are clustered by start-up. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. *t*-statistics are displayed in parenthesis.

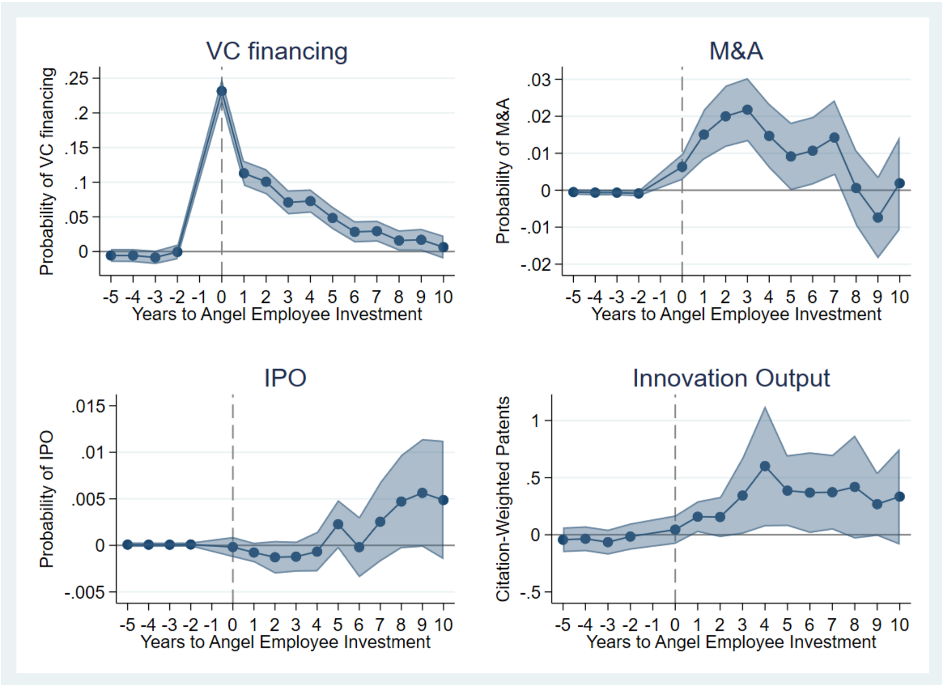


Table 3.9: Innovation-related Angel Employees

This table reports the result of the fixed effect regression of equation 3.2. The unit of observation is on a firm-state-year level. The dependent variable in Panel A is the economic value of patents scaled by assets (Kogan et al. (2017)) over the subsequent k years ($Innovation_{t+k}$), where $k = [1, 5]$, respectively. In Panel B, we alternatively use truncation-adjusted citation-weighted patents. The dependent variable of interest is split into two parts, depending on whether the angel employee is working in an innovation-related role or not depending on some keywords such as "product manager", "technology", "researcher", etc. Variable definitions are provided in section A of the Appendix. The regression includes Firm-Year, Firm-State, and State-Year fixed effects. Standard errors are clustered by Firm-State. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Economic Value of Patents ($KPSS$)					
	$KPSS_{t+1}$	$KPSS_{t+2}$	$KPSS_{t+3}$	$KPSS_{t+4}$	$KPSS_{t+5}$
Innovation related Angels	9.32	-19.61	-58.82***	-79.59***	-97.05***
	(0.79)	(-1.60)	(-4.55)	(-6.04)	(-6.19)
Non-Innovation related Angels	63.21*	72.11*	51.89	48.92	30.02
	(1.68)	(1.95)	(1.38)	(1.39)	(0.67)
Panel B – Dependent variable: Citation-Weighted Patents (CIT)					
	CIT_{t+1}	CIT_{t+2}	CIT_{t+3}	CIT_{t+4}	CIT_{t+5}
Innovation related Angels	0.48	-1.95***	-3.71***	-4.09***	-4.87***
	(0.88)	(-3.56)	(-6.31)	(-7.12)	(-6.90)
Non-Innovation related Angels	0.41	0.24	0.41	-1.10	-0.02
	(0.24)	(0.13)	(0.25)	(-0.73)	(-0.01)
For Both Panels					
Observations	330,956	311,488	292,020	272,552	253,084
Firm-Year FE	YES	YES	YES	YES	YES
Firm-State FE	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES

APPENDIX

A.3.1 Variable Definitions

This section provides the variable definitions. All variables are measured at an annual frequency. All continuous variables are winsorized at the 1% and 99% level.

1. *Angel Employee* – Either a dummy equal to one if there is at least one angel employee. Alternatively the natural logarithm plus one of the total number of angel employees on a firm level. "Angel Employee" is an individual who is an angel investor and around the time of investment employed at a publicly traded corporation. We assume a holding period of 5 years and aggregate the number of unique individuals on a firm basis. We obtain this variable by combining information on angel financing from Crunchbase and employment information from LinkedIn (plus some manual searches).

2. *Innovation Output* – Either the economic value of patents aggregated on a firm-year level or on a firm-state-year level. On a firm-year level, the variable is scaled by total assets following Kogan et al. (2017). Alternatively, we use truncation-adjusted citation weighted patents as in Hall et al. (2005). Patents linked to firms is obtained from the website of Noah Stoffman. All other patent data is directly from the United States Patent and Trademark Office (USPTO).

3. *Size* – Natural logarithm of the market value of the firm. The information is obtained from Compustat.

4. *R&D Expenditures* – Total R&D expense scaled by book value of assets. The information is obtained from Compustat.

5. *Tobin's Q* – Book value of assets (AT) + market capitalization (MC) - common equity value (CEQ) - balance sheet deferred taxes, if available (TXDB) / total assets (AT). The information is obtained from Compustat.

6. *Profitability* – Operating income scaled by book value of assets. The information is obtained from Compustat.

7. *Tangibility* – Property, plant and equipment scaled by book value of assets. The information is obtained from Compustat.

8. *Age* – Natural logarithm of the number of years the firm appears in Compustat.

9. *Herfindahl-Index (Squared)* – Industry competition as measured by the Herfindahl index (squared) defined over yearly sales in a 4-digit SIC code. The information is obtained from Compustat.

10. *Liquidity* – Stock liquidity measured as the daily mean bid-ask spread. The information is obtained from CRSP.

11. *Capital Expenditures* – Capital Expenditure scaled by the book value of the firm. The information is obtained from Compustat.

12. *Leverage* – Leverage ratio of the firm's total debt scaled by book value of assets. The information is obtained from Compustat.

13. *Financial Constraints* – Dummy variable indicating Financial Constraints if a firm is flagged as falling in the top tercile of the distribution of financial constraints every year by either of the measures proposed by Kaplan and Zingales (1997), Whited and Wu (2006) and Hadlock and Pierce (2010). The information is obtained from Compustat.

14. *Patent Stock* – Total number of patents assigned to a firm in the last 20 years (equivalent to patent expiry period). The information is obtained from the website of Noah Stoffman.

15. *Number of Employees* – Natural logarithm of the total number of employees. The information is obtained from Compustat.

16. *Corporate Venture Capital* – A dummy variable equal to one if the firm has an active corporate venture capital program. The variable was constructed following Ma (2020). The information is obtained from Refinitiv (formerly VentureXperts by Thomson Reuters).

17. *PIR* – The staggered PIR, the so-called "prudent man rule". We obtain this data from González-Uribe (2020).

18. *Failed Start-ups* – The number of Start-ups that are either defunct or did not receive any financing in the last 5 years. The information is obtained from Crunchbase.

19. *Board/Executive Angel Dummy* – We tag angel employees as board members if they mention "director" or "board member" in their title. We tag them as executives if they mention "executive" or any C-suite abbreviation. The information is based on textual information from the job title on LinkedIn.

20. *Innovation /Non-innovation related Angels* – We tag employees with the words: "product", "innov", "research", "tech", "engineer", among other keywords as innovation-related and angel employees with titles such as "finance", "legal", "accounting", "audit", "operation", "banking", among others into non-innovation related angels. The information is based on textual information from the job title on LinkedIn.

21. *Trademarks* – The log of one plus the total amount of trademarks applied in a given year. We obtain trademarks linked to gvkeys from Heath and Mace (2020).

22. *Product Announcements* – The log of one plus the total amount of new product launches in a given year. We follow the methodology of Chu et al. (2020) and proxy for new product launches by screening the key developments (Compustat) database for the following keywords: “unveil”, “launch”, and “new product”. We obtain the data from Compustat.

23. *Scientific Publications* – The log of one plus the total amount of scientific publications. We obtain the data from Arora et al. (2020). We use version 7 (December 2020) available here: https://zenodo.org/record/4320782

24. *Funding Round Size* – This variable is equal to the size of the funding round in million USD. We obtain this data from Crunchbase.

25. *Startup Corporation Distance* – This is the We compute this data by combining Crunchbase location information (when available) with headquarter location info from firms 10-Ks.

26. *Startup-Corporation Industry Similarity* – This is the cosine similarity of textual descriptions of startups with that of their employers similar to Hoberg and Phillips (2010). We compute this data by combining Crunchbase textual descriptions (when available) with product descriptions from firms 10-Ks.

27. *VC financing/M&A/IPO* – These are indicator variables equal to one if the firm raises venture capital, is acquired, or goes public. We obtain this data from Crunchbase.

28. *Exit* – This variable is equal to one if the employee leaves the firm. We obtain this data from LinkedIn.

A.3.2 Data Description

In the parts below, we provide more details on how we obtained the data used in this paper. We start with a more detailed description of the Crunchbase dataset and then explain how we obtained the employment histories from LinkedIn.

A.3.2.1 Crunchbase

Crunchbase was the starting point for our data collection. We obtained the data through a private API and used a bulk download on January 1st 2022. The relational database provides information on staged funding rounds, e.g. which company raised how many funds, who participated, and when the investment took place. We first merge the funding round data with information on investments, e.g. which investors participated in which funding round. This provides an overview of who invested in which funding round. Most of the investments are venture capital investments, so the next step is to obtain personal (angel) investments. We do this by merging the dataset with the people database. The people database covers more than 870,000 individuals that have connection to the start-up world. Most individuals in the database are founders, so they are not material to our research. We only keep investments that are tied to individuals. The next step is to limit the data set to individuals investing in US firms. Additionally, we manually verified our angel investors. E.g. we eliminated individuals tied to venture capital firms and individuals tied to a corporate venture capital arm of a firm.

Crunchbase also provides information on employment histories in the so-called jobs database. We can therefore see which individual worked in which firm. We initially used this data for preliminary results, but decided that the coverage was not sufficient. We therefore looked for an alternate database which provides more comprehensive employment histories.

A.3.2.2 Employment History

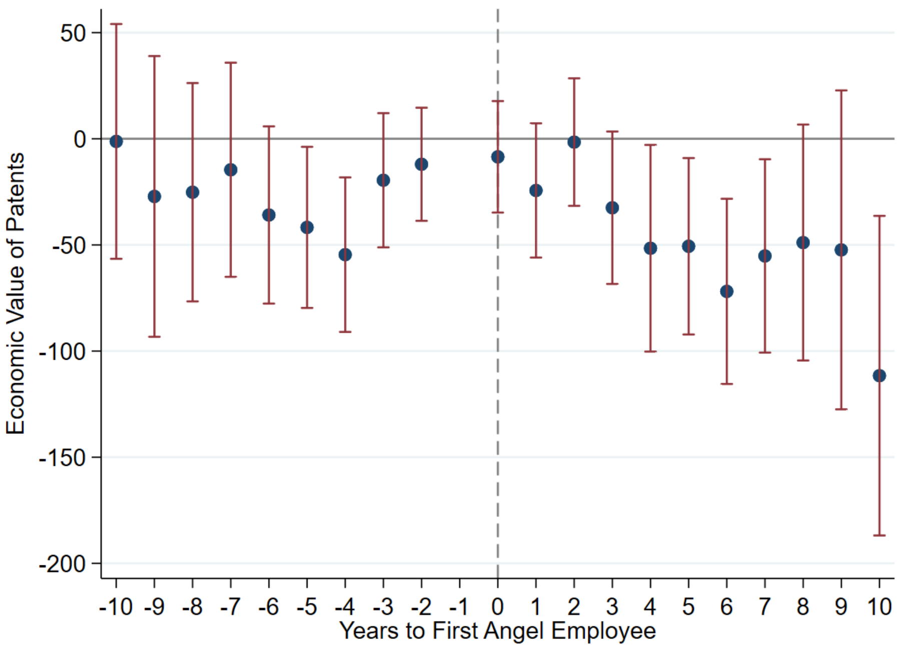
Crunchbase already provided links to individual LinkedIn profiles to the vast majority of investors in our sample. We manually verified whether these links were in fact correct and compared the employment history listed in Crunchbase with the history listed in LinkedIn. For the subset of individuals with missing LinkedIn links, we were able to collect the link for roughly 66% of the remaining subsample. We again verified whether we map the correct individual by comparing employment histories. As mentioned in the paper, we were left with a small set of individuals (who sometimes had many investments) without a LinkedIn profile. This could result in a substantial selection bias if high-level employees are less likely to have a LinkedIn profile. We thus ranked the sample by number of investments and manually obtained employment histories for all individuals with at least 3 angel investments. We were able to find employment information for 98% of all angel investments in our sample.

We also performed a number of cleaning exercises. One can in principle provide any information on LinkedIn. The information is self reported and not independently audited. We remove jobs when the job title refers to being an investor in the firm. For instance, many individuals claim to work for Tesla and state their position as "investor" or "shareholder". We remove these jobs from our data, as it is unlikely that these individuals are decision makers at that firm. Also, many start-up founders are stating their firm name as follows: FIRM NAME (acquired by ACQUIRER). We cleaned the employer name such to make sure that we do not falsely match an investor to a listed ACQUIRER. We performed a string search to look for instances of "M&A", "acquired", "acquisition", etc. to eliminate these instances.

Figure A.3.1: Event Study: Effect of Angel Employees on Corporate Innovation - Without Biggest State

This figure reports the result of the fixed effect panel regression of equation 3.3 in a firm-state-year event time panel. The dependent variable in Panel A is the economic value of patents (Kogan et al. (2017)) over time. In Panel B, we alternatively use truncation-adjusted citation-weighted patents. We visualize the coefficient on time dummies interacted with the dummy variable, *Angel Employee Dummy*, which is equal to one if there is at least one angel employee at the firm in the state. Time is relative to the first angel employee on a firm level. The coefficients are normalized to the year before the first angel employee. The sample here excludes the headquarter state which is proxied by the omission of the biggest state as measured by innovation output. Variable definitions are provided in section A of the Appendix. The regression includes Firm-State and Firm-Year fixed effects. Standard errors are clustered by Firm-State. Confidence intervals are at the 5% level.

Panel A: Economic Value of Patents



Panel B: Citation-Weighted Patents

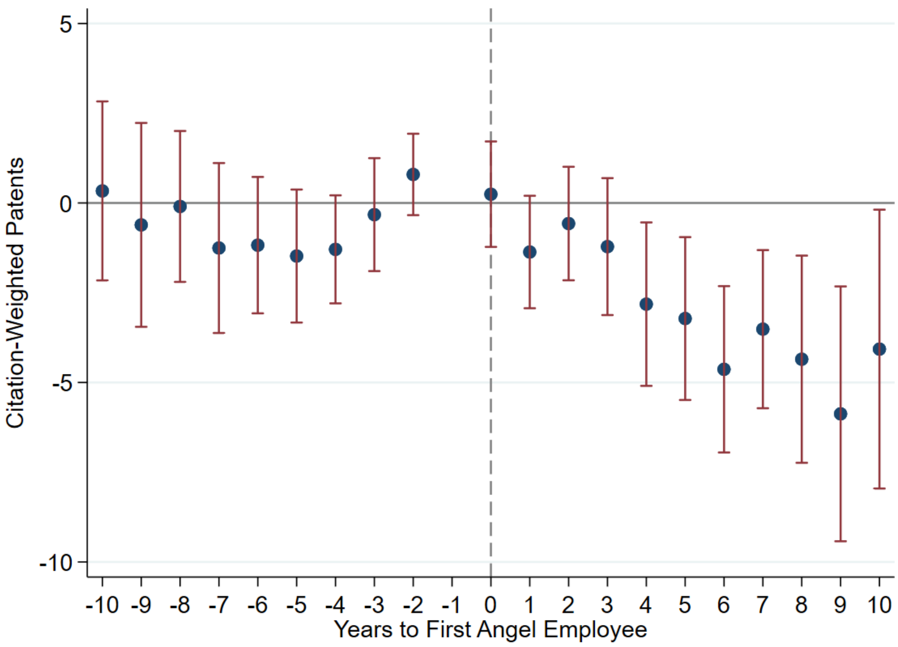


Table A.3.1: Evidence from the SBJA Capital Gains Exemption: QSBS- and Non-QSBS Angels

This table reports the result of the fixed effect regression of equation 3.5. The unit of observation is on a firm-state-year level. The dependent variable in Panel A is the economic value of patents scaled by assets (Kogan et al. (2017)) over the subsequent k years ($Innovation_{t+k}$), where $k = [1, 5]$, respectively. In Panel B, we alternatively use truncation-adjusted citation-weighted patents. We split the variable Angel Employee Dummy into two parts and interact them with Post, which is equal to one after the SBJA. We split Angel Employees into two groups based on whether they invest in start-ups likely to be qualified for tax exemption provided by the SBJA. Variable definitions are provided in the Appendix. The regression includes Firm-Year, Firm-State, and State-Year fixed effects. Standard errors are clustered by Firm-State. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Economic Value of Patents					
	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$
QSBS Angels Dummy	46.26**	47.44**	54.78**	31.22	2.11
	(2.35)	(2.28)	(2.39)	(1.48)	(0.10)
QSBS Angels Dummy × Post	-49.85**	-83.08***	-119.44***	-122.37***	-125.53***
	(-2.42)	(-3.75)	(-4.82)	(-5.08)	(-5.00)
Non-QSBS Angels Dummy	53.28***	50.22***	39.34***	33.82***	20.88**
	(4.66)	(4.68)	(3.79)	(3.12)	(1.99)
Non-QSBS Angels Dummy × Post	-33.01	-43.44**	-65.46***	-101.62***	-134.98***
	(-1.62)	(-2.08)	(-3.01)	(-4.02)	(-4.49)

Panel B – Dependent variable: Citation-Weighted Patents					
	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}
QSBS Angels Dummy	2.05**	2.07**	2.12**	1.37	1.04
	(2.27)	(2.09)	(2.06)	(1.44)	(1.11)
QSBS Angels Dummy × Post	-3.20***	-4.59***	-6.05***	-6.41***	-7.33***
	(-3.39)	(-4.38)	(-5.41)	(-5.93)	(-6.55)
Non-QSBS Angels Dummy	3.70***	2.94***	1.77***	0.61	0.11
	(5.78)	(4.88)	(3.16)	(1.12)	(0.20)
Non-QSBS Angels Dummy × Post	-3.85***	-4.90***	-5.35***	-6.19***	-6.66***
	(-3.19)	(-4.11)	(-4.75)	(-4.80)	(-4.57)
For Both Panels					
Observations	330,956	311,488	292,020	272,552	253,084
Controls	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table A.3.2: Non-Patent Based Innovation Output

This table reports the fixed effect regression of equation 3.1 in a firm-year panel. The dependent variable in Panel A, (*NPA*) is the natural logarithm of one plus the number of new product announcements of the firm. In Panel B, the dependent variable is the natural logarithm of one plus the total number of trademarks (*TM*) of the firm. In Panel C, the dependent variable is the natural logarithm of one plus the total number of scientific publications (*PUBS*) of the firm. The dependent variable Angel Employee Dummy is equal to one if the firm employs at least one angel employee. The regression includes 15 standard control variables. Variable definitions are provided in the Appendix. The regression includes Firm and Year fixed effects. Standard errors are clustered by firm. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are in parenthesis.

Panel A: New Product Announcements					
	NPA_{t+1}	NPA_{t+2}	NPA_{t+3}	NPA_{t+4}	NPA_{t+5}
Angel Employee Dummy	-0.05	-0.08**	-0.11***	-0.07	-0.10*
	(-1.56)	(-2.23)	(-4.05)	(-1.67)	(-1.94)
Panel B: Trademarks					
	TM_{t+1}	TM_{t+2}	TM_{t+3}	TM_{t+4}	TM_{t+5}
Angel Employee Dummy	-0.07**	-0.11**	-0.17***	-0.12**	-0.11***
	(-2.23)	(-2.85)	(-3.97)	(-2.31)	(-2.00)
Panel C: Scientific Publications					
	$PUBS_{t+1}$	$PUBS_{t+2}$	$PUBS_{t+3}$	$PUBS_{t+4}$	$PUBS_{t+5}$
Angel Employee Dummy	-0.02	-0.18***	-0.32***	-0.33***	-0.41***
	(-1.31)	(-2.64)	(-3.64)	(-3.12)	(-3.19)
For All Panels					
Other Controls	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table A.3.3: Angel Employees at Private Firms

This table reports the fixed effect regression of equation 3.1 in a firm-year panel. The dependent variable is the economic value of patents scaled by assets (Kogan et al. (2017)) over the subsequent k years ($Innovation_{t+k}$), where $k = [1, 5]$, respectively. The dependent variable Angel Employee Dummy is equal to one if the firm employs at least one angel employee. The sample is composed of all private firms in the US obtained from ORBIS. We limit ourselves to firms with turnover of at least 10M$. Due to limited data availability of private firms, the regression does not include control variables. Variable definitions are provided in the Appendix. The regression includes Firm and Year fixed effects. Standard errors are clustered by firm. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	CIT_{t+1}	CIT_{t+2}	CIT_{t+3}	CIT_{t+4}	CIT_{t+5}
Angel Employee Dummy	0.01	-0.04*	-0.08***	-0.08***	-0.13***
	(0.23)	(-1.95)	(-3.57)	(-3.55)	(-3.84)
Observations	2,349,209	2,338,687	2,323,400	2,146,491	1,970,683
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table A.3.4: Baseline Regressions excluding recent IPOs: Angel Employees and Innovation Output

This table reports the fixed effect regression of equation 3.1. The unit of observation is on a firm-year level. We drop the first two years of observations after a firm went public. The dependent variable in Panel A is the economic value of patents scaled by assets (Kogan et al. (2017)) in the next year ($Innovation_{t+1}$). In Panel B, we alternatively use truncation-adjusted citation-weighted patents. The independent variable, *Angel Employee Dummy*, is equal to one if there is at least one angel employee at the firm. Alternatively it is the natural logarithm of the number of angel employees on a firm level. Columns (3) and (4) use a inverse hyperbolic sine transformation of the dependent variable. Column (5) only looks at firms which patent. The regression includes 15 standard control variables. Variable definitions are provided in section A of the Appendix. The regression includes Firm and Year fixed effects. Standard errors are clustered by firm. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. *t*-statistics are displayed in parenthesis.

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Economic Value of Patents					
	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$	$KPSS_{t+1}$
Angel Employee Dummy	-0.04***		-0.04***		-0.04***
	(-4.61)		(-4.62)		(-3.59)
ln(1+Angel Employees)		-0.07***		-0.07***	
		(-5.77)		(-5.83)	
Panel B – Dependent variable: Citation-Weighted Patents					
	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}	CIT_{t+1}
Angel Employee Dummy	-0.28***		-0.11***		-0.25***
	(-5.05)		(-4.39)		(-3.59)
ln(1+Angel Employees)		-0.45***		-0.16***	
		(-6.72)		(-5.56)	
For Both Panels					
Observations	56,823	56,823	56,823	56,823	26,283
Controls	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

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4

Internal Carbon Markets

Santanu Kundu [1]

Abstract: I document the existence of a novel market for internal resource reallocation within firms, specifically internal carbon markets. Firms participating in the European Union Emissions Trading System (EU ETS) reallocate carbon permits between subsidiaries. They transfer carbon permits to subsidiaries that have received insufficient free permits from the regulator to help offset their additional emissions. In response to a policy change that restricts the supply of free permits, subsidiaries of firms with internal carbon markets emit more carbon. The increase in carbon emissions is consistent with an undervaluation of these permits relative to the market when carbon permits are transferred internally. Overall, the paper highlights a novel mechanism that can limit the effectiveness of market–based climate policies.

Keywords: Internal carbon markets, EU ETS, carbon trading, internal capital markets, climate policy

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4.1 Introduction

Carbon emissions trading systems (ETSs) are considered as the most efficient policy instrument to reduce carbon emissions (Montgomery, 1972; Nordhaus, 1993; Tietenberg, 1990, among others). Acknowledging their flexibility and efficiency, several countries and regions have implemented ETSs. As shown in Figure (4.1), such systems are already in place in Canada, China, the EU, the UK, the US (California cap-and-trade system and the Regional Greenhouse Gas Initiative), Switzerland while many more are under consideration. Moreover, the global prevalence of such systems is on the rise, with around 20% of worldwide emissions falling under the purview of an ETS as of 2023, a substantial increase compared to the 5% reported in 2010, as illustrated in Figure (4.2). ETSs are typically implemented as a cap-and-trade system where the participants trade carbon permits based on their cap (or free permits) and their actual emissions.

One unique and little studied feature of such a market is that the regulator allocates free permits to the installations (as opposed to a firm). Hence, if installations are owned by the same parent firm or Global Ultimate Owner (GUO), it can operate an internal market by reallocating permits within its firm boundary as shown in Figure 4.3.[2] This is the case for over half of the firms that own an installation operating in the European Union Emissions Trading System (EU ETS), the world's largest carbon trading system. They have access to an internal market for trading carbon permits, i.e., an internal carbon market (ICO_2M, hereafter). However, little is known about the importance of ICO$_2$Ms and their role in emissions reduction. In this paper, I study this unique feature of such ETSs and how it can influence the carbon emissions of firms operating in the ETS.

To begin, I provide evidence of the prevalence and active use of ICO$_2$Ms in the EU ETS. On average, 30% of carbon permits that are traded internally, i.e., firms affiliated with an ICO$_2$M (or, simply ICO$_2$M firms) trades carbon permits with a counterparty that belongs to the same parent firm operating another installation within the EU ETS. I document that if counterparties in this market were to trade at random, we would expect the percentage of internal transaction to be less than 1% on average. In a more systematic analysis, I document that during the sample period, if any ICO$_2$M firm is in deficit, i.e., would require to purchase additional carbon permits to cover its emissions for a given year, then the volume of internal transactions increases by 73% and is received by the ICO$_2$M firm that is in deficit. This evidence is consistent with at least two explanations: first, that internal markets are preferred over transacting in external markets, and second, the pattern of transaction is important as it helps the ICO$_2$M firm in deficit in avoiding paying penalties of non-compliance, which can be as high as five times the market price of carbon, in the most efficient way.

Next, I examine whether ICO$_2$Ms have any impact on the emission behavior of firms in the EU ETS. But why should ICO$_2$Ms even matter in the context of carbon emissions? The rationale stems from the well-established body of research within the realms of internal capital markets (Stein, 1997) and internal labor markets (Baker and Holmstrom, 1995). These studies emphasize

[2]For example, row 3 of the figure demonstrates transaction between two subsidiaries belonging to the energy firm Iberdrola. Similarly, the last row in the figure demonstrates transaction between two subsidiaries of ExxonMobil located in different EU countries.

the pivotal role played by internal resource allocation practices within firms. Simultaneously, it's noteworthy that internal markets are prone to cross-subsidization of resources when compared with market prices, as evidenced by prior research (e.g., Cristea and Nguyen, 2016; Gopalan et al., 2007; Pfeiffer et al., 2011).

Within an emissions trading system (ETS), where carbon prices play a pivotal role in shaping firms' decisions regarding emissions abatement, cross-subsidization in carbon permits between installations in ICO$_2$M firms can create varying incentives for emissions reduction between ICO$_2$M firms and those belonging to firms operating a single installation, ultimately resulting in differences in emissions output. These cross-subsidization can manifest in either direction, either distorting prices to be higher or lower than the market price of carbon. When prices are distorted lower (higher), ICO$_2$M firms will have reduced (elevated) incentives to engage in emissions abatement, subsequently leading to increase (decreases) in their emissions. In contrast, without such price distortions, emissions from ICO$_2$M firms and non-ICO$_2$M firms should not display significant discrepancies.

I leverage the transition from Phase II to Phase III in the EU ETS to tease out the effect of ICO$_2$Ms. During this transition, the proportion of allocated free permits decreased from over 90% in Phase II to just 43% in Phase III. Additionally, regulators made it clear that they would design additional mechanisms to avoid extremely low carbon prices in the future driven by oversupply of carbon permits. This shift coincided with a significant increase in carbon price, amplifying the significance of firms' carbon permit reallocation during Phase III compared to Phase II.

In a difference-in-differences framework that compares ICO$_2$M firms to non-ICO$_2$M firms before and after the implementation of Phase III, my analysis reveals that ICO$_2$M firms exhibit a notable increase in their emissions intensities, with percentages ranging from 16% to 27% (depending on the specific model) during Phase III, in contrast to other firms. These findings remain robust even after factoring in various potential confounding variables and incorporating fixed effects for subsidiaries, parent firms, and years. To explain these findings, I also offer a simple model of cross-subsidization in resource reallocation, where prices are distorted to be lower than the market price of carbon, a concept supported by prior research (e.g., Matvos and Seru, 2014; Rajan et al., 2000; Scharfstein and Stein, 2000) to motivate my empirical analysis and explain my findings.

It's worth noting that ICO$_2$M firms may differ in size from other firms. To address concerns that these results may be influenced by firm size, I conduct a further analysis using propensity-scored matched samples based on firm size within the same NACE 2-digit industry, ensuring comparability in size within a given industry. This additional analysis reaffirms the initial findings and I obtain similar results. Additionally, the main findings continue to hold when using a reduced sample of ICO$_2$M firms that have multiple facilities within a single firm and compare them to non-ICO$_2$M firms.

Next, I turn to the economic mechanisms behind the results. I attempt to discern the specific motivation driving carbon permit reallocation by HQs in ICO$_2$M firms. One prominent explanation is profit maximization. ICO$_2$M firms could face unique profitable opportunities.

Hence, they could increase their emissions but at the same time, also become more profitable. Alternatively, they can use increase their emissions particularly in those EU countries with lower tax rates so that they can increase their profitability in these countries. I do not find evidence consistent with for either of these explanations.

However, I obtain four evidence consistent with the reduction of disparity in divisional profitability across a firm as proposed by Rajan et al. (2000). Within this framework disparity in profitability across the divisions of a firm is costly as it disincentivizes the managers to take the most efficient investment. This is possible as HQs hold property rights over carbon permits, while divisional managers have the discretion to make operational decisions within their divisions. Thus, to minimize ex-post large disparity in divisional profitability, HQs would cross-subsidize ex-ante less profitable divisions at the expense of more profitable divisions.

Consistent with this line of reasoning I obtain the following evidence. First, ICO_2Ms characterized by greater dispersion in profitability and emissions ex-ante, i.e., in 2012 (one year prior to the transition from Phase II to Phase III) demonstrate increased carbon emissions and a heightened carbon-intensity during Phase III compared to Phase II. Second, ICO_2Ms with higher dispersion in subsidiaries' profitability initially also reduce their dispersion ex-post. Suggesting that HQs of ICO_2M firms can successfully reduce disparity in divisional profitability through cross-subsidization of carbon permits. Third, ICO_2M firms with higher profitability as of 2012 *and* characterized by greater profitability (ex-ante), also decrease their profitability during Phase III consistent with HQs cross-subsidizing (ex-ante) less profitable divisions at the expense of the better ones. Finally, particularly those ICO_2M firms that have lower profitability with the ICO_2M ex-ante *and* belong to a ICO_2M that is disparate in its profitability ex-ante, also receive more volume carbon permits internally during Phase III as compared to Phase II. These results collectively suggest that HQs of ICO_2M firms used the carbon permits in the EU ETS to foster greater equality among divisions in terms of their profitability.

In essence, this paper underscores a crucial aspect in the design of market-based climate policies, like an ETS. Neglecting the dynamics of resource sharing within firms may pose limitations on the efficacy of such policies. The evidence suggests that internal transfers of carbon permits within a firm may result in undervaluation of these permits. Consequently, managers respond to this undervaluation by exceeding their emissions compared to similar firms, emphasizing the need to consider internal resource-sharing incentives in the formulation of climate policies. One simple solution would be to ensure that the carbon permits are always transacted at the market price.

This paper is related to three strands of the literature. First, I add to the growing literature on climate finance, especially in corporate finance. As noted in Dai et al. (2021) a vast majority of the current literature in climate finance focuses on asset pricing and financial market implications. Most related to this paper is Bartram et al. (2021) where the authors document that firms in the US relocate emissions to their facilities outside California in response to the California cap-and-trade emissions trading system. The role of firm boundaries has also been studied in Akey and Appel (2021), especially, how parents' limited liability protection impacts toxic chemical releases from subsidiaries. In a related theory paper, Heider and Inderst (2021) models how financial constraints can have implications on optimal environmental policy.

However, none of these papers highlight the role of ICO_2Ms. I complement this growing literature in corporate finance by studying the internal carbon markets of firms.

Second, I complement the extensive literature studying the EU ETS. Perino et al. (2021), Cludius et al. (2021), Duscha et al. (2021), Perino et al. (2019), Perino and Willner (2017), Perino and Willner (2016), Ellerman et al. (2015) and Böhringer (2014) discuss various aspects of the EU ETS design. Trading behaviour in the EU ETS have been studied by Abrell et al. (2021), Schleich et al. (2020), Naegele and Zaklan (2016), Fan et al. (2016), Betz and Schmidt (2015), Jaraitė-Kažukauskė and Kažukauskas (2014), Martin et al. (2014b), and Zaklan (2013) among many others. A vast majority of these studies do not investigate the role of firm boundaries, except, Schleich et al. (2020), Betz and Schmidt (2015) and Zaklan (2013). These papers document that firms with more permits transfer more permits to other firms belonging to the same parent firm. Overall, I complement this strand of literature by not only demonstrating strategic transfer of carbon permits across subsidiaries, but I also document the effects of such possibility of ex-post permit transfer on firm's carbon emissions in the EU ETS.

Finally, this paper builds on the large literature in internal capital markets (e.g., Maksimovic and Phillips, 2002; Rajan et al., 2000; Stein, 1997) reviewed in Maksimovic and Phillips (2013). The majority of the literature on internal capital markets studies the flow of capital between business units. For example, Buchuk et al. (2020), Almeida et al. (2015), Gopalan et al. (2007) study the internal monetary transactions of business groups among group affiliated firms in various emerging economies. Similarly, Glaser et al. (2013) study capital allocation in a large multinational conglomerate. I complement the empirical literature on internal capital markets, first, by establishing a novel observation: firms operating within the EU ETS strategically employ ICO_2Ms, aligning with the existing body of literature on internal capital markets. Secondly, I also document the impact of having such an internal market for carbon emissions of firms operating in the EU ETS.

4.2 Overview of the EU ETS

The EU ETS is the flagship climate policy tool of the EU. It was setup in 2005 as the world's first international carbon emissions trading system and covers approximately 45% of the carbon emissions of the EU from 11,000 installations across 31 countries. The EU ETS is currently in its fourth trading phase (2021–2030) with the first three trading phases covering the years 2005–2007, 2008–2012 and 2013-2020, respectively. The EU ETS operates as a cap-and-trade system. Phase I, 2005–2007, was a three year pilot of the ETS in order to prepare for the Phase II (2008–2012). It started with 28 EU member countries. As a pilot project, Phase I covered only CO_2 emissions from power generators and other energy intensive industries. Additionally, nearly all permits were given out for free during this time. However, it provided policymakers valuable experience and it succeeded in establishing a robust infrastructure for monitoring, reporting and verification of carbon emissions across the EU countries. During the Phase II (2008–2012), three additional countries (Iceland, Lichtenstein and Norway) joined the ETS. 90% of the permits were still allocated for free.

In Phases I and II, the allocation of permits was decentralized, with each country allocating permits (according to its National Allocation Plan) to installations in its country based on "grandfathering", i.e., installations were allocated free permits as per their historical emissions. However, this changed during trading Phase III. Beginning 2013, the EU made a transition towards auctioning as the default mode of allocation and the allocation was centralized according to the Benchmarking Decision of the EU.[3]

The Benchmarking Decision makes free allocation available to installations based on their four digit NACE based industry or product benchmarks. The benchmark is defined as the 10% of the best performing installation for a given sector or product based on average CO_2 emissions per unit of output during the period 2007–2008. The allocation is determined based on the following formula:

$$Q_{i,j,t} = B_j \times HAL_{i,j} \times LRF_{j,t} \times CSCF_t \tag{4.1}$$

where, $Q_{i,j,t}$ are the free permit received by the installation i, in industry j in year t. B is the benchmark in sector j, HAL is the historical activity level measured as the median activity level during 2005–2008 (or from 2009 until 2010, if larger) for the installation i in sector j. LRF is the linear reduction factor that linearly decreases from 0.8 in 2013 to 0.3 in 2020. Finally, CSCF is the cross-sectional correction factor applied uniformly across all installations to align the total free allocation to the EU wide cap on emissions. As noted in Martin et al. (2014a), an important feature of this allocation methodology is that the free allocation is based on production capacity prior to the trading phase and annual updates occur automatically via the LRF. The allocation is not tied to the actual production levels.

Importantly, the LRF takes the value of 1 for all installations belonging to the carbon leakage (CL) list. This essentially means that all such installations receive 100% of their benchmarked emission permits for free. A sector is categorized at the risk of carbon leakage based on their carbon intensity (CI, measured as the ratio between total cost[4] and the gross value added of the sector) and/or trade intensity (TI, calculated as the ratio of total value of imports and exports with third countries to the total market size for the commodity (annual turnover plus total imports from third countries)). If CI is greater than 5 percent and TI is greater than 10 percent, or either CI or TI are greater than 30 percent, then a sector is added CL list. There are 154 granular NACE-based industries that are part of this list. The CL list is reviewed regularly and the criteria for inclusion under the CL list became stricter during Phase IV (2021–2030) of the EU ETS. Furthermore, starting 2013, electricity producers had to buy all carbon permits from the market. Thus, for electricity producers LRF in equation (4.1) takes on the value zero.

4.3 Data description

The data for the study comes from three primary sources. First, I get the carbon permit trading data from the European Union Transaction Log (EUTL) database. The database

[3]See: https://eur-lex.europa.eu/legal-content/EN/TXT/HTML/?uri=CELEX:32011D0278&from=EN

[4]Both direct and indirect costs are included. Direct costs are calculated as the value of CO_2 emissions using a proxy price of 30 euros per ton of carbon emissions (Martin et al., 2014a).

provides information for each permit that has been transacted in the EU ETS, either through an exchange or through over-the-counter transactions. It records the name of the organization that transfers the permit along with the GUO of the organization, the receiving organization, the GUO of the receiving organization, date and time of the transaction, country of origin and destination, the type of transaction and the quantity of permits traded.[5] While the data is granular and rich with information, it has two limitations. First, the transaction prices are not disclosed. In my communication with the European Commission and also with many national authorities, it was made clear that prices for transactions are not recorded in the EUTL database. Second, the data is published with a three year delay. At the time of writing this draft, trading data was available until 2018.

The second database that I use for the analysis is the Monitoring, Reporting and Verification (MRV) database. This database reports installation wise yearly verified emissions, amount of free allocation that the installation received, and the total amount of permits the installation provided for its verified emissions. This database also provides the name of each installation, its location, the name of the firm owning the installation along with an unique national identifier of the firm.

The third database is the Orbis financials and ownership database that is offered by Bureau van Dijk, a Moody's company. Orbis is the largest cross-country firm-level database covering both public and private firms. It provides balance sheet and income statements as well as detailed information on firms' location, industry and crucially for this study, their ownership structures. In the following sub-section, I describe how I match the three datasets.

4.3.1 Linking Orbis to EU ETS databases

The first set of matching is done between the MRV database and Orbis. The process is made relatively less cumbersome due to the availability of the national identifiers. In many cases, the national identifier could be easily mapped uniquely to a particular firm. For some countries, like the United Kingdom, appending the two-letter country code, *GB* in this case, provides the corresponding Bureau van Dijk Identifier (BvDID). BvDID uniquely identifies a firm in the Orbis database. However for many other countries, for example Germany and Italy, this is not the case. Hence, for these countries, I manually go through possible national identifiers in the database and assign the company for the corresponding identifier. In those cases where the national identifiers could not be found in the database, I do a manual name search to find out the BvDIDs. Additionally, there are some cases where national identifiers seems to be wrongly entered. This happens for simple formatting issues such as leaving out the leading zeros from the ID or an additional special character. I manually correct them to get their BvDIDs. By this process, I could match nearly all of the installations to Orbis through their BvDIDs.[6]

The second set of matching is done between the EUTL database and Orbis. This is more time consuming as the EUTL database does not always contain the national identifier of the firm. Hence, the matching between the EUTL database and Orbis is purely based on name matching. In order to ensure accuracy, I manually match each individual organization to Orbis taking into

[5]See: https://ec.europa.eu/clima/ets/transaction.do

[6]A small number of Greek installations could not be matched due to their name ambiguity.

account their location in order to avoid ambiguities. Even though some organizations could not be mapped, a vast majority of transactions during my sample period could be mapped to firm listed in Orbis. One limitation of the EUTL database is that it does not track the full path of permit flow, i.e., even though I could know that a firm is receiving or transferring certain number of permits, many times it is often unknown from which installation (if at all) these permits are coming from or going to. Hence, the analyses investigating trading decisions within the EU ETS can only be done at a subsidiary or firm level.

4.3.2 Mapping Ownership

Once I get the BvDIDs for the owners of the installations in the MRV database and the counterparties involved in a transaction from the EUTL database, I map each BvDID, in each year, to the corresponding Global Ultimate Owner (GUO) in that year. A GUO is defined as a firm having more than 50% ownership of a subsidiary. I consider 50-50 joint ventures as independent firms. In the most recent data revisions, Bureau van Dijk provides vintage files for ownership links for each year for each BvDID. In the first step, I use this vintage ownership link files to map the BvDID for the GUO. However, I found that in many cases the ownership links were not complete. For example, PORSCHE HOLDING SE was mapped to a different identifier as compared to VOLKSWAGEN AG. In order to avoid such issues, I manually go through the mapping and check for all possible such cases and correct them. Furthermore, many cases, especially when installations are owned by private equity funds, are difficult to trace back to the same GUO as often times, they use different names or its abbreviations. For these cases, I use manual google searches to avoid any inconsistencies as much as possible.

For each firm, its subsidiaries and installations, I get their locations at the country level based on the information available in the EUTL database and the Orbis database. I also get industry information from Orbis based on the second revision of the statistical classification of economic activities (NACE Rev.2) as followed by the EU.

Overall, the database constructed in this way is a relatively clean dataset mapping installations and trading counterparties in the EU ETS to their corporate parents. The unavoidable inaccuracies due to the procedure described should be minor and should not bias results in a systematic way.

4.3.3 Summary Statistics

The unit of observation of my analysis is at the subsidiary-year level. This means that for ICO$_2$M firms these firms are the subsidiaries belonging to the same GUO. For each subsidiary (or GUO, in case of independent firms) in a given year, I calculate the total emissions by aggregating the verified emissions for all stationary installations covered under the EU ETS under the control of the given subsidiary or the firm. In a similar way, I calculate the total amount of free permits allocated across all installations for a given subsidiary/firm by aggregating the installation specific free allocations. The MRV database for Phase II and Phase III additionally comes with four-digit NACE Rev.2 codes for a vast majority of installations. Additionally, I collect firm-year level annual financials from Orbis as mentioned above.

Insert Table 4.1 here

Table 4.1 provides summary statistics of the variables used in the study for the period 2008-2019. CO_2INT is the emissions intensity measured as emissions in tons from a given firm in a given year scaled by the firm's revenue (in thousand euros). The average of 1.53 suggests that the average firm has emissions that is roughly 1.5 times its revenues (in thousand euros). The variable ICO_2M suggests that roughly 52% of firm-year level observations are associated with an internal carbon market. In a similar vein, mean of $ICO_2M_{Diverse,EBIT}$, $ICO_2M_{Diverse,EBITDA}$, $ICO_2M_{Diverse,PBT}$ suggests that roughly 20% of the observations belong to ICO_2Ms that have greater than median standard deviation in EBIT, EBITDA, or PBT across its subsidiaries as of 2012. The variable ROA is return on assets defined as revenues over total assets of firms. *Cash Flow* is the total cash holding of firms scaled by its revenues. *Allocation* is a dummy variable indicating subsidiaries that received greater than the median number of free permits among the cross-section of firms in a given year.

4.4 Hypotheses Development

How could ICO$_2$Ms matter for corporate carbon emissions within the EU ETS? Ex-ante, it is not clear if they should matter at all.

To start with, it's essential to note that managers in ICO$_2$M firms have the flexibility to engage in trading within both the internal and external markets, whereas managers in non-ICO$_2$M firms are restricted to the external market. This distinction means that in the case of an excess or shortfall of permits, managers in non-ICO$_2$M firms must exclusively trade with external market participants, i.e., counterparties who are not part of the same parent–firm. In contrast, managers in ICO$_2$M firms could trade with either external or internal market counterparts.

Second, precisely because there is a possibility to trade permits internally within the firm, the role of the HQs become important. It is well recognized in the literature in internal capital markets that HQs are important in decision making and resource reallocation within firms (e.g., Matvos and Seru, 2014; Rajan et al., 2000; Stein, 1997, among others). One crucial factor underscoring the importance of HQs is their control rights over the firm's resources (Gertner et al., 1994). This authority empowers HQs to redistribute resources among their divisions as they deem most appropriate.

To understand how ICO$_2$Ms might influence firms' emissions behavior, let us consider two scenarios. In the first scenario, ICO$_2$Ms does not matter. This can happen in two ways. First, it's plausible that HQs within ICO$_2$M firms opt not to participate in the reallocation of carbon permits across their installations and subsidiaries. Second, HQs could choose to reallocate carbon permits, but they do so at the prevailing market price. In both these cases, ICO$_2$M and non-ICO$_2$M firms would engage in carbon market trading at the market price, i.e., ICO$_2$M and non-ICO$_2$M firms face the same carbon price, implying *no* price distortion compared to the market price. As the carbon price predominantly guides emissions abatement efforts in an emissions trading system, there would be no discernible difference in emissions reduction between these two groups of firms, resulting in similar emission levels. This leads us to our initial hypotheses:

H1: *If there is no price distortion in the internal reallocation of carbon permits, or if internal carbon markets are irrelevant, emissions will not differ between between internal carbon market firms and other firms operating in the EU ETS.*

In the second scenario, when HQs engage in the redistribution of carbon permits, they must also determine an internal carbon price. HQs have the option to set this price either higher or lower than the prevailing market price. Existing evidence from the literature suggests that HQs are more inclined to choose an internal price below the current market rate. A compelling and direct evidence of this tendency can be found in Gopalan et al. (2007), where the authors examined related party loans within Indian business groups. Their findings revealed that a significant majority of intra-group loans were disbursed at rates below the market interest rate. More recent, although indirect evidence of such cross-subsidization can be found in Kisgen and Kong (2022), Cline et al. (2014) among others.

In the context of redistributing carbon permits, if HQs distort price to cross-subsidize some subsidiaries and installations, this would result in a lower carbon price faced by ICO_2M firms compared to the market price. A lower carbon price (or an *expected* lower carbon price) would consequently lead to reduced incentives for these firms to pursue emissions abatement, resulting in higher emissions compared to non-ICO_2M firms. This leads us to the second hypothesis:

H2: *In the presence of price distortion in internal carbon markets, emissions internal carbon market firms will be higher than other firms operating in the EU ETS.*

In order to further motivate my empirical analysis, I provide a simple conceptual framework in Section A1 in the Appendix to demonstrate the theoretical viability of the hypotheses.

Before delving into the empirical analysis, it's essential to underscore some key points concerning the hypotheses discussed earlier.

First, the key implication of the model is primarily rooted in the distorted internal pricing of carbon permits by HQs. In response to a reduced carbon price, divisional managers of ICO_2M firms tend to curtail their abatement efforts, consequently leading to an increase in emissions. As in Stein (1997), it's crucial to recognize that ex-post under-pricing of emission permits is not a prerequisite for *H2* to be applicable. If managers in ICO_2M firms anticipate that any surplus permits may be redistributed by HQs at a lower price, this expectation would prompt them to raise their emissions, consequently decreasing the supply of permits available for HQs to redistribute.

Second, it's important to underscore that HQs may not consistently distort the carbon price. Instead, such distortions may occur on specific occasions. Nevertheless, even in such instances, the expected price is reduced, factoring in the probability of these distortions happening.

Finally, while there isn't direct evidence of distorted transfer prices in internal carbon permit reallocation, a recent World Bank report raises the possibility of such price distortion. This report, detailed in "State and Trends of Carbon Pricing 2023," analyzes internal pricing within approximately 1,200 firms, with roughly half of them subject to a carbon price as of 2022. It's worth noting that during this period, the carbon price in the EU ETS stood at around 100 euros per ton of CO_2. Nonetheless, as shown in Figure (4.5), a significant majority of firms applied a carbon price lower than 50 euros per ton of CO_2. Hence, it is highly probable that some firms, including those within the EU ETS, priced their carbon permits below the prevailing market rate.

4.5 Identification Strategy & Empirical Results

4.5.1 Identification Strategy

I use the transition from Phase II to Phase III of the EU ETS in 2013 and the subsequent pronounced carbon price increase as a shock to the importance of reallocation of permits within firms' boundaries. This is primarily because of two reasons. First, as noted by the European Commission (EC),[7] the number of permits that were allocated for free during Phase III dropped dramatically to 43% from more than 90% during Phase II. This drop in the supply of free permits creates a potential scarcity of carbon permits for firms operating in Phase III of the EU ETS relative to Phase II. Second, the EC also indicated a further tightening of the permit supply rules in the future. Specifically, the EC introduced the Market Stability Reserve (MSR) which was formally announced in 2017. The MSR was launched to further tighten the supply of permits in the EU ETS and to avoid any excess supply in the future (Borghesi et al., 2023). This resulted in a significant increase in carbon prices (De Jonghe et al., 2020).

Collectively, these policy changes substantially increase the importance of permit reallocation within firm boundaries as it makes carbon permits a critical factor of production that is expected to become scarcer and more expensive for firms operating installations in the EU ETS. Hence, any benefit or cost of such reallocation within firm boundaries should become more prominent during Phase III as compared to Phase II.

For most of my analysis, I use a difference-in-differences methodology and compare firms that are part of an ICO_2M to firms that are not. In what follows, I first demonstrate the relevance of ICO_2Ms and then examine their effect on the emission behavior of firms in a difference-in-differences setting.

4.5.2 Active Usage of Internal Carbon Markets

First, I perform a counterfactual analysis motivated by the fact that more than 30% of the carbon emissions are transacted by ICO_2M firms through internal markets in a given year. If internal markets are not relevant, then one should expect that a similar amount of carbon permits should be transacted internally if the buyer and the seller trade randomly in this market.

Therefore, in order to quantitatively assess the proportion of carbon permits transacted through ICO_2M if traders transact randomly, I introduce randomness to the seller side of the transaction data. Subsequently, I compute the percentage of carbon permits that a firm trades through their ICO_2M within a specific year. This process is repeated for 1,000 iterations.

Insert Figure 4.4 here

[7] See: https://climate.ec.europa.eu/eu-action/eu-emissions-trading-system-eu-ets/development-eu-ets-2005-2020_en

The results are depicted in Figure (4.4). As is evident from figure, a substantial majority (exceeding 90%) of the observations are below 0.01. This suggests that in a scenario where traders transact randomly, internal transactions account for 1% of carbon permits or more in over 90% of instances in a given year. Additionally, there is a zero probability that we observe volume as high as 30% as we do observe in the actual trading data.[8] Hence, the analysis implies that ICO_2Ms actively used, and the observed volumes being transacted internally are not merely a result of chance but rather indicative of intentional and strategic actions.

Next, I examine the trading patterns within the EU ETS from 2008 to 2019 to determine if ICO_2M firms use internal markets when needed. For this analysis, I determine the "net position" of counterparties in a trade using the verified emissions data. For a given transaction, the buy-side counterparty is labeled as "Deficit" if they have more emissions than their initially endowed carbon permits by the regulator, meaning they *need* to buy additional permits to cover the excess emissions. Conversely, a sell-side counterparty is labeled as "Surplus" if their yearly emissions are less than their allotted carbon permits, allowing them to sell any extra permits. It is important to note that installations with excess permits are *not required* to sell them; they can also keep them for future use or sale. Finally, a transaction is identified as "internal" if both counterparties belong to the same parent firm in a given year. The results of the analysis is shown in Table 4.2.

Insert Table 4.2 here

In the first column of Table 4.2, the negative point estimate on $Internal$ suggests that internal transactions typically involve lower volumes. In column (2), we examine how the volume of internal transactions changes when the counterparty needs to buy additional allowances to cover emissions that exceed their allotted permits. This means the counterparty is in "deficit" and needs to purchase more permits. The main effect of "Net Position" is not statistically significant. However, I find a positive and statistically significant coefficient for the interaction term $Internal \times Net\ Position$. Specifically, the volume of carbon permits transferred internally increases by about 70% ($= exp(0.53) - 1$). This result indicates that internal markets are important and preferred over external markets, especially when a counterparty needs to comply with regulations. If external markets were as important as internal markets, we wouldn't see a positive and statistically significant coefficient for the interaction term. Results in column (3) suggest that when the counterparty has a surplus of permits, they do not necessarily trade more. In fact, the estimates are negative, indicating that the counterparty trades less internally, which is expected since surplus permits can be carried over to future years.

Is there a difference in trading behavior during Phase III compared to Phase II? As discussed earlier, the transition to Phase III makes carbon permits scarcer and more valuable. If cross-subsidization incentives are strong enough, one might expect an increase in internal trading volume. Columns (4) and (5) of the table explore this possibility. Column (4) looks at counterparties in deficit (buy-side), while column (5) looks at those in surplus (sell-side).

[8] I find similar results if I also randomize the amount of carbon permits transacted in a given transaction. The results also remain unchanged if I also remove transactions conducted via an exchange.

The statistically insignificant estimate for the interaction term $Internal \times Net\ Position \times POST$ in column (4) suggests no change for counterparties needing permits to offset additional emissions. However, there is a major change in column (5). The positive estimate for $Internal \times Net\ Position \times POST$ indicates a 150% ($= exp(0.94) - 1$) increase in the volume of internal transactions. This suggests that while ICO$_2$M firms needing more carbon permits continue to receive support through the internal market, firms with surplus permits significantly increase internal transactions during Phase III compared to Phase II. This supports the idea that cross-subsidization of carbon permits becomes more important for ICO$_2$M firms in Phase III.

Overall, the results of this section demonstrates that internal carbon markets in EU ETS are active and firms use their internal markets for reallocating permits strategically within their firm boundaries.

4.5.3 Effects of Internal Carbon Markets: Transition to Phase III

Having established the relevance of ICO$_2$Ms in the EU ETS, I explore its effect on carbon emissions. As demonstrated in Section (4.4), theoretically, the effect of ICO$_2$Ms is ambiguous on the emission behavior of firms.

To empirically investigate its effect, I employ the following specification:

$$\begin{aligned} CO_2INT_{i,j,t} = \beta_1 \times ICO_2M_{i,j} \ + \ \beta_2 \times POST_t \ + \ \beta_3 \times POST_t \times ICO_2M_{i,j} \ + \\ \gamma \times X_{i,t} \ + \ \delta_i \ + \ \eta_j \ + \ \theta_t \ + \ \epsilon_{i,j,t} \end{aligned} \tag{4.2}$$

ICO_2M is a dummy variable that takes the value of one for subsidiaries belonging to the same parent firm with at least two installations (or a single independent firm operating more than one installation) covered by the EU ETS in a given year during the sample period, and zero, otherwise. The main dependent variable, CO_2INT, is a measure of carbon intensity as calculated by total carbon emissions scaled by the revenue of the subsidiary directly owning the installation in a given year. I also use other measures of carbon footprint such as carbon emissions scaled by total assets and the natural logarithm of total emissions for robustness check. $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012, X is a vector of subsidiary/firm level time–varying control variables. I employ subsidiary/firm fixed effects (δ_i), parent firm fixed effects (η_j), and year fixed effect (θ_t).

Table 4.3 presents the results of estimating equation (4.2). The coefficient of interest is the coefficient on the double interaction term $ICO_2M \times POST$. If agency issue in the form of price distortion is at play, we would expect to see a positive coefficient on the double interaction term as is hypothesized in H2. This is indeed what we find across various specifications in Table 4.3.

Insert Table 4.3 here

Column (1) includes only year fixed effects. The main variable of interest is the coefficient on the double interaction term $ICO_2M \times POST$. It is positive and statistically significant suggesting that ICO$_2$M firms increased their emissions intensities during Phase III as compared to before by 18% ($= exp(0.165) - 1$). In Column (2), we further include subsidiary fixed effects (or firm fixed effects for non-ICO_2M firms). The economic magnitude if double interaction term almost doubles to 31% (= $exp(0.271) - 1$) or 16% of the sample mean (= $0.27 \div 1.53$) and the point estimate remains statistically significant. The results do not change if we include additional time–varying controls (natural logarithm of assets, cash holdings, return on assets, and an indicator variable *Allocation* that identifies firms that received greater than the median number of free permits among the cross-section of firms in a given year) in column (3) and control for additional parent–firm fixed effects in column (4).

Additionally, Figure 4.6 plots the results dynamically. Specifically, it shows the dynamic treatment effects pertaining to the specification of column (4) in Table 4.3. Except for the time period between $t = -4$ and $t = -3$, Figure 4.6, does not seem to suggest any obvious pre-trend, and the effect is solely concentrated during the treatment period, i.e., after 2013. This further strengthens a causal interpretation of the results. Finally, column (5) of Table (4.3) demonstrates the robustness of the main result by using the natural logarithm of total emissions as the dependent variable. The economic magnitude implied by the point estimate suggests that firms associated with ICO_2Ms also increase their overall emissions by approximately 13%.

One concern could be that my results are driven by comparing dissimilar firms with each other. After all, firms with two or more installations within the EU ETS could be larger than other firms. This is similar to the concern noted in the internal capital markets literature in Hund et al. (2012). Hence, to alleviate such concerns, I re-run my analyses using a matched sample difference-in-difference estimation. For the transition to Phase III, I predict the likelihood of a given firm being included in the internal carbon market or not, based on firm size as of 2012 and the same 2-digit NACE industries. I employ a three nearest neighbor matching based on propensity scores from a probit model. I keep a firm only once in the final matched sample.

Insert Table 4.4 here

The results of the analysis are shown in Table 4.4. The results are qualitatively the same as those documented in Table 4.3. Column (4) in the table suggests that firms with ICO_2Ms increase their carbon intensities by approximately 18% in Phase III as compared to Phase II. This magnitude is very similar to the one reported in column (4) of Table (4.3). As before, I plot the dynamics of the treatment effect in Figure 4.7. Here also we do not see a visible pre-trend and we could see that emissions intensities start to increase after 2013.

Figure (4.8) further plots the dynamics of emissions intensities for the two groups of firms separately. The blue bars represent non-ICO_2M firms, and the red bars represent ICO_2M firms. The main takeaway from this figure is that emissions intensities are decreasing for both groups, but they are decreasing less for ICO_2M firms. This indicates that the EU ETS is effective in incentivizing emissions reduction for all firms, though it is less effective for ICO_2M firms. This finding is important because it does not suggest that the EU ETS is ineffective overall; instead, it highlights potential inefficiencies due to internal resource allocation decisions within firms, which make some firms less effective within the EU ETS.

Overall, the results in Table 4.3 and Table 4.4 are consistent with the adverse effects of ICO_2Ms on carbon emissions. Consistent with the theory, such adverse effects could be a manifestation of agency issues within the firm boundaries with respect to resource allocation.

4.5.4 Discussion of the Economic Magnitude

It is important to understand what is the extent of cross-subsidization that would result in the higher emissions that we obtain from Table (4.3). This is particularly important as we do not observe the prices for individual transactions in the dataset. In order infer the extent of cross-subsidization, I rely on a simple conceptual framework outlined in section A1 in the Appendix.

It is fairly straightforward to show that in the extent of cross-subsidization induced price distortion, carbon abatement for ICO_2M firms would be lower than in the baseline where the internal transfer price of carbon permit would be equal to the market price.

Insert Figure 4.9 here

In Figure (4.9), I plot the relation of equation (18) from the section A1 in the Appendix. The y-axis in the figure represents the extent of carbon emissions that are abated less in ICO_2M firms compared to non-ICO_2M firms. Similarly, the x-axis represents the extent to which the carbon permits are priced lower in ICO_2M firms compared to the market price. The additional parameters required to estimate the relation has been calibrated from the data as of 2012. As shown in the figure, internal price which is roughly 25% lower than the market price would result in the observed magnitude of higher emissions in ICO_2M firms.

Gopalan et al. (2007) is one of the few studies that document the extent of subsidies provided through the internal transfer of resources. The authors find that, for Indian business groups, more than 80% of intra-group loans are provided for free, representing a 100% discount. Of the loans that do carry some interest, a significant fraction are provided at a below-market rate, typically at a discount of more than 50%. Thus, the magnitudes of carbon emissions abatement obtained in Table (4.3) seem consistent with a significant, though lower, subsidization of carbon permits in the internal markets.

4.6 Economic Mechanisms

What drives managers of ICO_2M firms to expect a lower transfer price of carbon permits in the EU ETS? I explore two mutually non-exclusive explanations, namely, HQ's preference to avoid more diversity in divisional profitability and HQs motive to favor connected divisional managers.

4.6.1 Financially Efficient Carbon Emissions

One motivation for the internal reallocation of carbon permits could be to maximize financial profitability in specific subsidiaries while increasing their emissions. According to Stein (1997), if the headquarters of ICO_2M firms cross-subsidize the most profitable firms ex-ante, these firms would have higher emissions and higher profitability during Phase III compared to other firms. I investigate whether this is the case in Table 4.5.

Insert Table 4.5 here

I use three standard measures of profitability as my dependent variables in columns (1), (2), and (3), namely, earnings before interest and taxes (EBIT), earnings before interest, taxes, depreciation (EBITDA), and profit before tax (PBT) (all scaled by revenues) respectively. If cross-subsidizing the most profitable subsidiaries within an ICO_2M firm leads to increased profitability, the point estimate on the triple interaction term $ICO_2M \times Emissions\ Intensity \times POST$ should be positive. However, the point estimates are consistently negative across all three specifications and are statistically significant in columns (1) and (3). Therefore, it does not appear that firms are increasing their profitability by increasing their emissions.

Another motivation for trading carbon permits internally could be for shifting profits to firms within an ICO_2M that are located in low tax jurisdictions within the EU compared to other firms within the same ICO_2M. Thus, these firms might increase their emissions particularly in low tax countries within the EU where they should also have higher profitability at the same time. I investigate whether this is the case in Table 4.6. Since, I am comparing firms within the same ICO_2M, the regression is estimated on the sample of ICO_2M firms only.

Insert Table 4.6 here

As before, I use EBIT, EBITDA, and PBT as my dependent variables in columns (1) to (3) respectively. If profit shifting were driving the increase in emissions, we would expect the triple interaction term $High_{Tax} \times Emissions\ Intensity \times POST$ to have a positive coefficient. Indeed, the point estimate on this interaction term is consistently positive across the three columns. However, these estimates are far from statistically significant. Therefore, there is limited evidence to suggest that profit shifting is driving the higher emissions of ICO_2M firms during Phase III.

4.6.2 Reduction of Disparity Across Subsidiaries

The first prediction from this strand of literature that is applicable in the present context is that firms that are part of an ICO_2M that have more variability in their ability to utilize resources efficiently would be more prone to the negative effects of inefficient resource reallocation, thereby, increasing their emissions.

To investigate if the pattern of emissions would align with such predictions, for each GUO operating an ICO_2M, I calculate the diversity in their EBIT (or EBITDA, emissions, and emissions intensity) across its subsidiaries (that are active in the EU ETS) as of 2012. I then categorize ICO_2Ms as having high diversity ($ICO_2M_{Diverse}$) if they have greater than median variability in their EBIT (or EBITDA, PBT, and emissions intensity) across its subsidiaries and low diversity ($ICO_2M_{NonDiverse}$) if they are below the median. After classifying the firms with ICO_2Ms in such a way, I re-run the baseline difference-in-differences specification by incorporating a dummy variable for each of these categories.

Insert Table 4.7 here

The results of the estimation are shown in Table 4.7. Column (1) presents the results using EBIT to classify ICO_2Ms. Columns (2), (3), and (4) classify ICO_2Ms based on EBITDA, PBT, and emissions intensity, respectively. Across all columns, it is clear that the increase in emissions intensity is primarily driven by more diverse ICO_2Ms. The point estimates for

$ICO_2M_{Diverse} \times POST$ are larger than those for $ICO_2M_{NonDiverse} \times POST$. For example, in column (1), the point estimates indicate that firms within diverse ICO_2Ms become 25.6% more carbon intensive, while non-diverse ICO_2M firms become 15.1% more carbon intensive. Additionally, the increase in emission intensity is statistically significant between these groups. Except for the second column, the point estimates are also largely statistically different from each other, as indicated by the p-values.

One question that naturally arises is why HQs cannot step in and correct the inefficiencies. Such problems would not arise if HQs could design and enforce contracts to share surplus ex-post. However, as in Rajan et al. (2000), it is not possible to contract on ex-post surplus. Second, it can also be possible that such inefficient emissions makes ICO_2M firms less divergent in profitability across their divisions.

I compute the yearly standard deviation in subsidiary profitability, measured by EBIT or EBITDA, or PBT for each ICO_2M. I then explore whether ICO_2Ms that demonstrated variability in their EBIT, EBITDA or PBT in 2012 also undergo a decline in diversity during Phase III, as compared to Phase II.

Insert Table 4.8 here

The results are presented in Table (4.8). The regressions are run at the parent–firm and year level and only includes ICO_2Ms (as the standard deviations can only be computed for these sub-sample of firms). The first two columns measure diversity using EBIT, while the middle two columns use EBITDA, and the last two columns use PBT. The coefficient estimate on $ICO_2M_{Diverse} \times POST$ is consistently negative and statistically significant, suggesting that the variability in profitability across subsidiaries decreases, especially for those ICO_2Ms that initially exhibited more disparity across its subsidiaries.

Finally, I document that the convergence in the profitability of ICO_2M firms is driven by firms that were either more profitable before the policy change or are better at managing their carbon emissions with respect to their free allocation levels. This is consistent with the explanation that the managers of more profitable and/or efficient divisions expect the HQs to penalize them and cross-subsidize worse divisions.

Insert Table 4.9 here

The results of our analysis are presented in Table (4.9). The regression is run on the sample of ICO_2M firms. In both panels, the dependent variable for the first two columns is EBIT, while the middle two columns use EBITDA, and the last two columns use PBT. In Panel A, the independent variable $High_{Profit}$ is a binary variable indicating firms within an ICO_2M that exceed the median in terms of their EBIT, EBITDA, or PBT as of 2012 in columns (1) to (3) respectively. Notably, it is evident that firms within an ICO_2M that have EBIT, EBITDA or PBT above the median are experiencing a decline in profitability during Phase III in comparison to Phase II. This is consistent with the motive of resource allocation as discussed in Rajan et al. (2000).

Shifting our focus to Panel B, I further obtain that these results are driven by ex-ante more profitable firms that are part of ex-ante more diverse ICO_2Ms. This is implied by the negative and statistically significant point estimate on the triple interaction term $ICO_2M_{Diverse} \times High_{Profit} \times POST$.

Finally, I obtain confirming results from the trading behavior. In Table 4.10 I investigate if an ICO_2M firm requires to offset its excess emissions in a given year, are they likely to receive more carbon permits internally if they are part of an ex-ante more diverse ICO_2M and it has lower than median ex-ante profitability among the subsidiaries of the ICO_2M.

Insert Table 4.10 here

In the table, the dependent variable is the same across the three columns, namely, the volume of carbon permits transacted in a given transaction. For brevity, the table only shows the main effects and the interaction effects that are significant in at least two of the three columns. The double interaction term $ICO_2M_{Diverse} \times POST$ is positive and statistically significant suggesting that ICO_2M firms with greater than median disparity among its subsidiaries increase the volume of transaction during Phase III as compared to Phase II. Likewise, the tripe interaction term $Internal \times Deficit \times ICO_2M_{Diverse}$ is generally positive and statistically significant implying that the ICO_2M firms that need to mandatorily offset their additional emissions receive more carbon permits. However, as suggested by the coefficient on $Internal \times ICO_2M_{Diverse} \times Deficit \times POST$ this volume dramatically decreased during Phase III but only is more than compensated by the increase in the transaction volume during Phase III to ICO_2M firms that are also less profitable ex-ante as indicated by the interaction term $Internal \times ICO_2M_{Diverse} \times Low_{Profit} \times Deficit \times POST$.

In summary, the findings in this section underscore that the adverse impact of ICO_2Ms on firms' emissions can largely be attributed to the strategic actions of HQs. HQs primary objective is to reduce disparity in divisional profitability, achieved through cross-subsidization where carbon permits are reallocated to support relatively under-performing divisions.

4.6.3 Alternative Mechanisms

4.6.3.1 Results Confounded by Overall Monitoring Difficulty in Conglomerates

One can argue that the results may naturally arise due to the likelihood that ICO_2M firms are often affiliated with conglomerates. This association suggests that the results stem from the inherent inefficiencies in resource allocation within conglomerates, rather than being specific to the operation of ICO_2Ms.

In order to examine this possibility, I categorize ICO_2Ms as follows: ICO_2Ms that are diverse (either in EBIT, EBITDA, log of emissions, and emissions intensity) and are difficult to monitor as is indicated if all of the three indicators of monitoring difficulty as discussed in column (1) of Table (**??**), $ICO_2M_{Diff,Diverse}$. Similarly, ICO_2Ms that are diverse and are not difficult to monitor ($ICO_2M_{NoDiff,Diverse}$), ICO_2Ms that are not diverse and are difficult to monitor ($ICO_2M_{Diff,NonDiverse}$), and ICO_2Ms that are not diverse and are not difficult to monitor ($ICO_2M_{NoDiff,NonDiverse}$). I then compare how the emissions intensities of firms that belong to these ICO_2Ms change during Phase III as compared to other firms using the same difference-in-difference setting.

Insert Table 4.11 here

Results are shown in Table (4.11). If the results are purely driven by the likelihood that ICO_2M firms are part of a conglomerate and hence are inefficient, we should observe that results are driven by ICO_2M firms that are difficult to monitor as shown in Ozbas and Selvili (2009). However, as the results show, the point estimates on the interaction terms $ICO2M_{Diff,Diverse} \times POST$ and $ICO2M_{NoDiff,Diverse} \times POST$ as well as the estimates $ICO2M_{Diff,NonDiverse} \times POST$ and $ICO2M_{NoDiff,NonDiverse} \times POST$ are not statistically different from each other. Additionally, the point estimates of $ICO2M_{Diff,Diverse} \times POST$ and $ICO2M_{Diff,NonDiverse} \times POST$ are statistically different from each other in these specifications, thus, re-enforcing the point that results are driven by ex-ante variability of their efficiency of resource utilization within ICO_2M firms.[9]

In conclusion, the findings from this analysis align with the notion that the primary results are unlikely to be a byproduct of the potential affiliation of ICO_2M firms with conglomerates. The observed rise in carbon intensities does not appear to be a reflection of broader inefficiencies in resource allocation within conglomerates.

4.7 Conclusion

In this paper, I investigate the internal carbon markets operated by firms within the European Union's Emissions Trading System (EU ETS). First, I establish the active use of these internal markets by showing that the volume of carbon permits transacted internally is ten times higher than what would be expected from random matching of traders in the market. I provide additional evidence that firms use internal transactions strategically.

Second, I document that firms with internal carbon markets become more carbon-intensive in response to a policy change that makes carbon permits more scarce and valuable. This behavior is consistent with the cross-subsidization of carbon permits within internal markets.

Third, I provide evidence in line with Rajan et al. (2000) regarding the economic motivation behind such cross-subsidization in internal carbon markets. Additionally, I demonstrate that these results are unlikely to be biased by carbon leakage or general economic inefficiencies documented for multi-divisional firms or business groups.

My analysis suggests that internal resource decisions within a firm can make market-based climate policies, such as an emissions trading system, less effective than they otherwise would be. Interestingly, this occurs even without considering carbon leakage. My results also highlight the importance of reporting the internal pricing of carbon permits, as proposed by the United States Securities and Exchange Commission under new climate-related reporting requirements. My analysis offers a simple solution: to regulate that the internal transfer price of carbon permits is equal to the market price.

Overall, I highlight a potentially important friction in designing market-based climate policies such as an emissions trading system.

[9] In unreported results, I find similar results for the corporate favoritism based explanation, i.e., the results are not driven solely by monitoring difficulty.

Figures and Tables

Figure 4.1: Coverage of Emissions Trading System across the World

This figure is obtained from the International Carbon Alliance Partnership and reflects data as of October 2023. The regions or countries shaded in blue have already established an Emissions Trading System (ETS). The green regions or countries are in the process of implementing an ETS, and the yellow regions or countries are currently contemplating the possibility of implementing such a system.

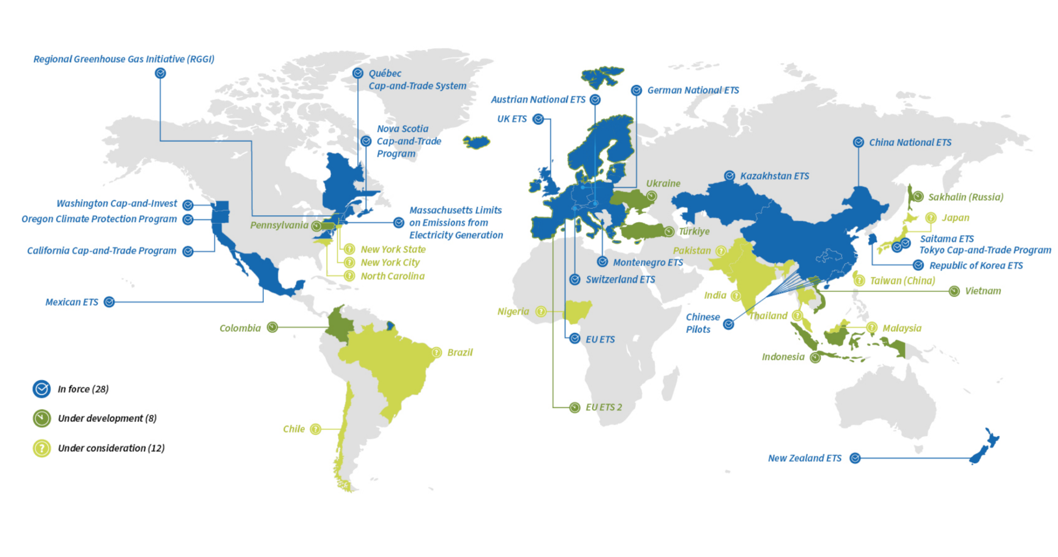

Figure 4.2: Share of Global Emissions covered by Carbon Pricing

The figure is taken from the World Bank report titled "State and Trends of Carbon Pricing 2023". On the horizontal axis of the figure, time is represented in calendar years, while the vertical axis illustrates the percentage of global emissions covered by either an Emissions Trading System (ETS) or a carbon tax. As of 2023, around 18.4% of global emissions are regulated by ETS, and approximately 6% are subject to a carbon tax. The data provided is current as of April 1, 2023.

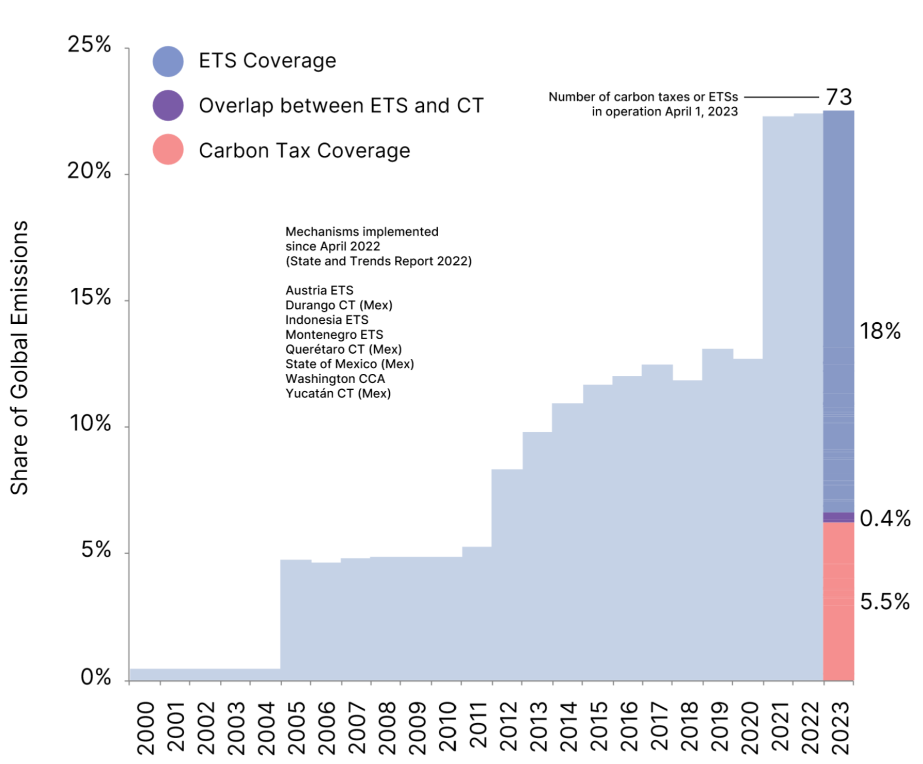

Figure 4.3: Example of Transactions in Internal Carbon Market

The picture below provides some examples of Internal Carbon Markets (ICO_2Ms). For example, the third row shows a transaction between two subsidiaries of Iberdola in Spain transacting in carbon allowances worth 1.2 million tonnes of CO_2. This picture is a snapshot of the carbon trading registry as provided by the European Commission. In this picture, rows number 3 and rows 5 to 10 are examples of transactions in an ICO_2M.

Transferring Registry	Transferring Account Name	Transferring Account Holder	Acquiring Registry	Acquiring Account Name	Acquiring Account Holder	Nb of units
Spain	Iberdrola Clientes España, S.A.U.	Iberdrola Clientes España, S.A.U.	Spain	Energyworks Vitvall, S.L.	ENERGYWORKS VIT-VALL, S.L.	136766
Italy	INDUSTRIA LATERIZI VOGHERESE S.p.A.	ILV INDUSTRIA LATERIZI VOGHERESE S.r.l.	Italy	iCASCO - Trading	i.CA.S.CO. S.p.A	14000
Spain	Iberdrola Clientes España, S.A.U.	Iberdrola Clientes España, S.A.U.	Spain	Iberdrola Generación Térmica, S.L.U. – Central Térmica de Velilla, grupos 1 y 2	Iberdrola Generación Térmica, S.L.	1117083
Estonia	Renergypro account	Strolia Arturas	Slovenia	BELEKTRON EKOTRADING d.o.o	BELEKTRON EKOTRADING d.o.o.	5000
Spain	Iberdrola Clientes España, S.A.U.	Iberdrola Clientes España, S.A.U.	Spain	Iberdrola Generación Térmica, S.L.U. – Ciclo Combinado de Aceca, grupo 3	Iberdrola Generación Térmica, S.L.	196066
Spain	Iberdrola Clientes España, S.A.U.	Iberdrola Clientes España, S.A.U.	Spain	Iberdrola Generación Térmica, S.L.U. – Central de Ciclo Combinado de Arcos	Iberdrola Generación Térmica, S.L.	347602
Spain	Iberdrola Clientes España, S.A.U.	Iberdrola Clientes España, S.A.U.	Spain	Iberdrola Generación Térmica, S.L.U. – Ciclo Combinado de Castellón, grupos 3 y 4	Iberdrola Generación Térmica, S.L.	584118
Spain	Iberdrola Clientes España, S.A.U.	Iberdrola Clientes España, S.A.U.	Spain	Energyworks Monzón, S.L.	Energyworks Monzón, S.L.	56126
Spain	Iberdrola Clientes España, S.A.U.	Iberdrola Clientes España, S.A.U.	Spain	Iberdrola Generación Térmica, S.L.U. – Ciclo Combinado Escombreras, grupo 6	Iberdrola Generación Térmica, S.L.	141282
Belgium	176 Esso Raffinaderij	Exxonmobil Petroleum & Chemical	France	ERSAS PJG	ESSO RAFFINAGE	88000

Figure 4.4: Placebo Test: Transactions through Internal Carbon Markets if Traders meet Randomly

I perform 1,000 randomization of the seller side of the EU Emissions Transactions Log containing the trading data. For each randomization, I calculate the percentage of carbon allowance a firm transacts internally in a given year. The plot is the frequency distribution of this percentage over the 1,000 iterations.

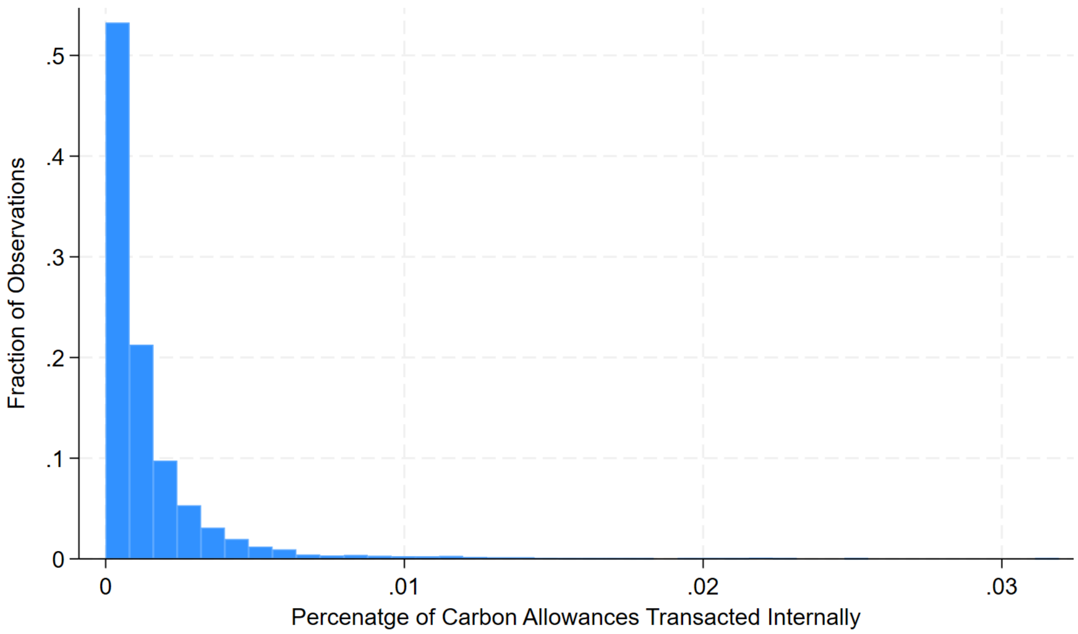

Figure 4.5: Internal Carbon Pricing of Firms

The figure is taken from the World Bank report titled "State and Trends of Carbon Pricing 2023". The horizontal axis in the Figure plots the different internal carbon prices that firms use as disclosed to the Carbon Disclosure Project (CDP) across different pricing bins. The vertical axis represents the number of firms in each of pricing bins. The CDP questionnaire is from 2022.

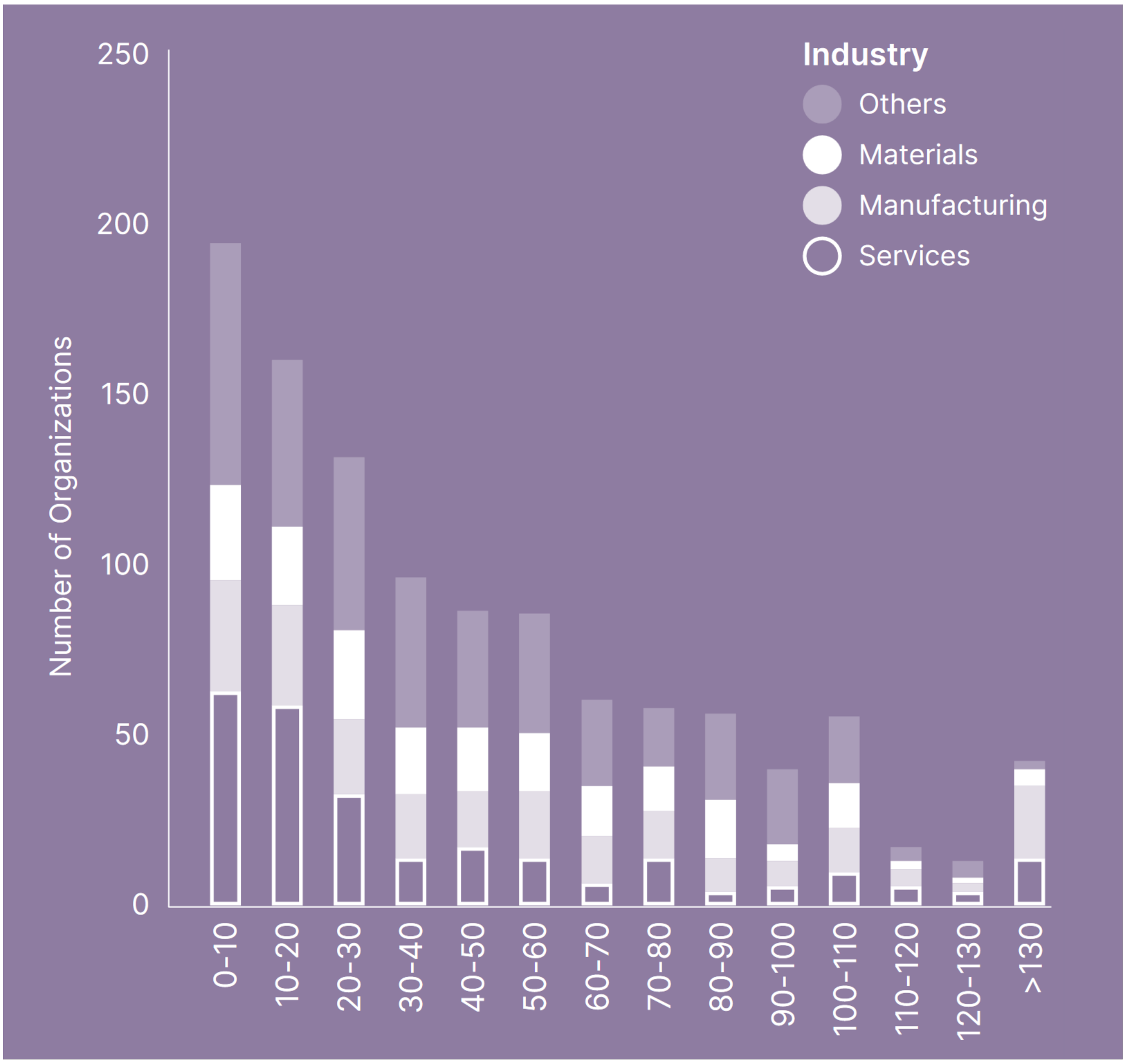

Figure 4.6: Plotting Dynamic Effects

This table plots the dynamic treatment effect of having an internal carbon market on the emissions intensity ($= Total\ Emissions/Revenue$ during Phase III of the EU ETS as compared to the Phase II. The $x - axis$ plots the yearly time period relative to year 2013 ($t = 0$). The y-axis plots the coefficient on the interaction term $POST \times ICO_2M$ in Equation 4.2 for each year with the year 2012 as the reference year. Standard errors are clustered by the parent firm. Confidence intervals are at the top/bottom 2.5%.

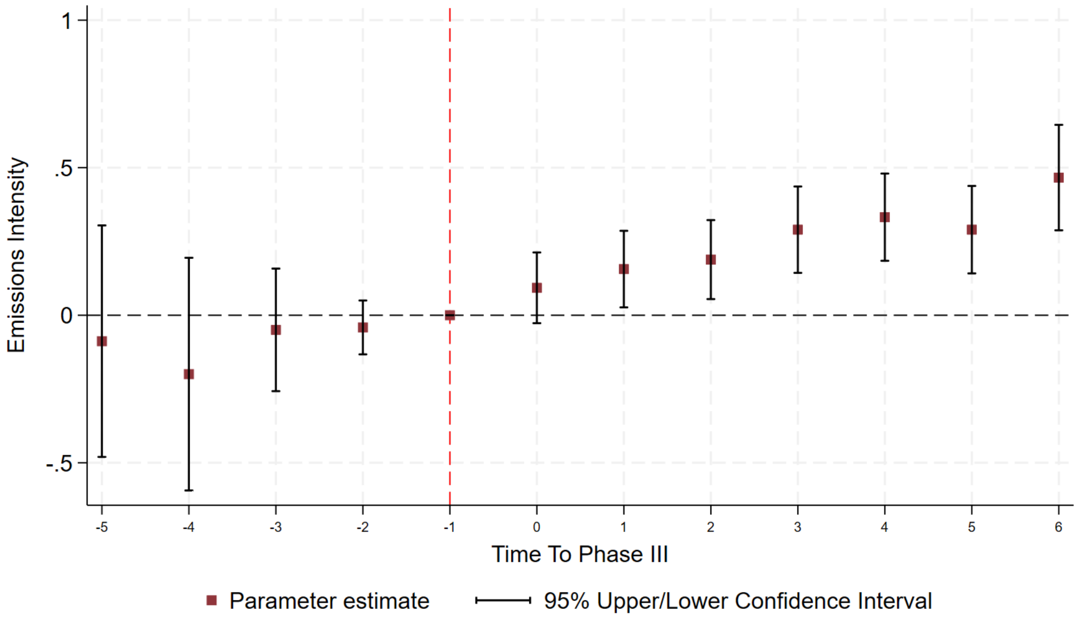

Figure 4.7: Plotting Dynamic Effects: Propensity Score Matching Statistics

This table plots the dynamic treatment effect of having an internal carbon market on the emissions intensity ($Total\ Emissions/Revenue$ during Phase III of the EU ETS as compared to the Phase II based on a propensity score matched sample. The $x - axis$ plots the yearly time period relative to year 2013 ($t = 0$). The y-axis plots the coefficient on the interaction term $POST \times ICO_2M$ in Equation 4.2 for each year with the year 2012 as the reference year. Standard errors are clustered by the parent firm. Confidence intervals are at the top/bottom 2.5%.

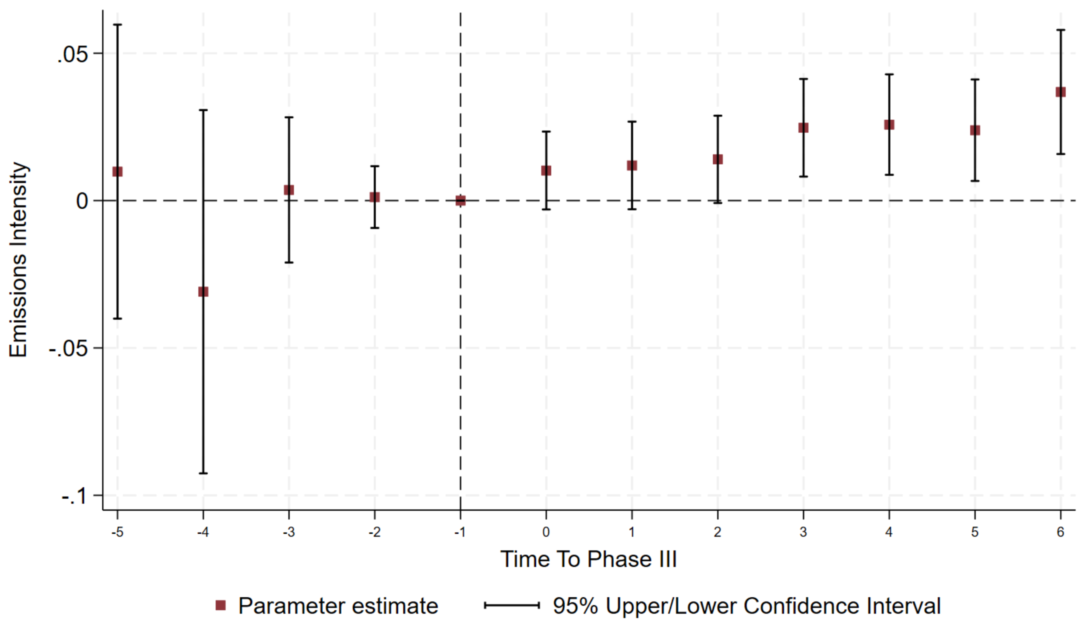

Figure 4.8: Plotting Dynamic Effects by Sub Groups

This table plots the dynamic treatment effect of having an internal carbon market on the emissions intensity ($Total\ Emissions/Revenue$ during Phase III of the EU ETS as compared to the Phase II based on a propensity score matched sample. The $x - axis$ plots the yearly time period relative to year 2013 ($t = 0$). The y-axis plots the coefficient on the interaction term $POST \times ICO_2M$ in Equation 4.2 for each year with the year 2012 as the reference year. Standard errors are clustered by the parent firm. Confidence intervals are at the top/bottom 2.5%.

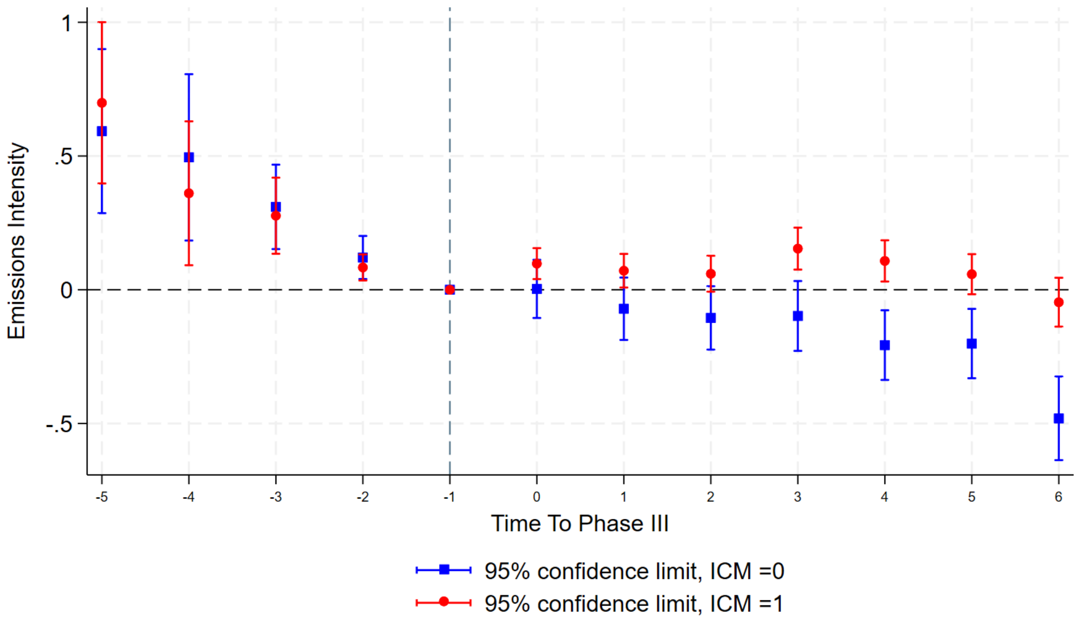

Figure 4.9: Extent of Cross-Subsidization

This table plots the extent to which carbon emissions abatement are lower in internal carbon market (ICO_2M) firms compared to other firms. The y-axis plots the extent of lower carbon emissions abatement. The x-axis plots the extent to which carbon permits are subsidized with respect to the market price. The relation between carbon emissions abatement and the internal price of carbon permits come from the equation (18) developed in the conceptual framework in section A1 in the Appendix.

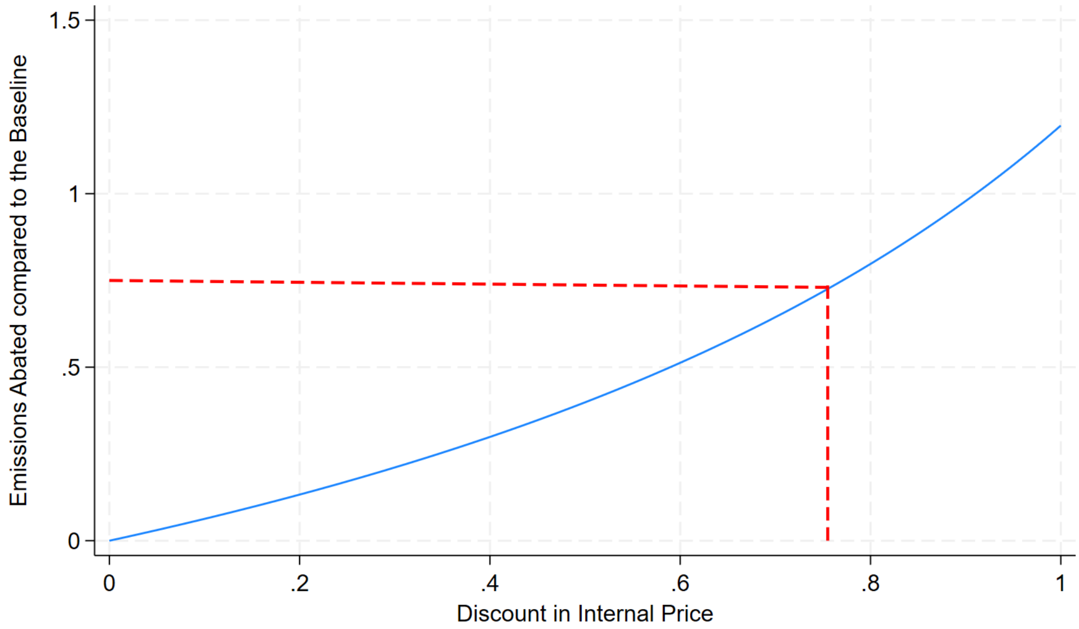

Table 4.1: Summary Statistics

This table provides the descriptive statistics for the variables used in the study. CO_2INT is total verified emissions for a given firm/subsidiary in a given year scaled by revenues. ICO_2M is a dummy variable taking the value of 1 if the subsidiary is part of a parent firm owning at least two installations covered by the EU ETS, i.e., they can operate an internal carbon market. $ICO_2M_{Diverse,EBIT}$, $ICO_2M_{Diverse,EBITDA}$, and $ICO_2M_{Diverse,PBT}$ are dummy variables taking the value of 1 if the ICO_2M–parent firm has greater than median standard deviation in $EBIT$, $EBITDA$, or PBT respectively across all the subsidiaries as of 2012 and zero, otherwise. $Cash\ Flow$ is defined as the total profit plus depreciation of a firm (or the subsidiary of a ICO_2M–parent firm) scaled by its revenue in a given year. Similarly, ROA is return on assets, *Log of Assets* is the the natural logarithm of total assets of a firm (or the subsidiary of a ICO_2M–parent firm), *Allocation* is a dummy variable indicating subsidiaries that received greater than the median number of free allowances among the cross-section of firms in a given year. $High_{EBIT}$, $High_{EBITDA}$, and $High_{PBT}$ are dummy variables indicating subsidiaries having greater than median $EBIT$, $EBITDA$, and PBT respectively within a ICO_2M–parent firm as of 2012. The sample period is from 2008-2019. All non-logarithmic continuous variables are winsorized at 2.5% and 97.5%-ile.

Variables	#Observations	Mean	Std.Dev	Min	Max
CO_2INT	32,049	1.53	2.70	0.00	11.43
ICO_2M	38,209	0.52	0.50	0.00	1.00
$ICO_2M_{Diverse,EBIT}$	38,209	0.22	0.41	0.00	1.00
$ICO_2M_{Diverse,EBITDA}$	38,209	0.22	0.41	0.00	1.00
$ICO_2M_{Diverse,PBT}$	38,209	0.21	0.41	0.00	1.00
$Cash\ Flow$	30,090	0.07	0.09	-0.21	0.30
ROA	32,078	0.99	0.74	0.02	3.26
$Log\ of\ Assets$	36,365	10.95	2.24	-6.54	22.22
$Allocation$	38,209	0.51	0.50	0.00	1.00
$EBIT$	38,209	0.51	0.50	0.00	1.00
$EBITDA$	38,209	0.51	0.50	0.00	1.00
PBT	38,209	0.51	0.50	0.00	1.00
$High_{EBIT}$	15,723	0.60	0.49	0.00	1.00
$High_{EBITDA}$	15,723	0.61	0.49	0.00	1.00
$High_{PBT}$	14,831	0.58	0.49	0.00	1.00

Table 4.2: Trading in Internal Carbon Markets

This table presents the evidence of active internal carbon markets (ICO_2M). The regression is estimated on a sample of all transaction in the EU ETS from 2008 to 2019. The dependent variable is the natural logarithm of the amount of carbon emissions being traded using carbon permits in a given transaction. $Internal$ is a dummy variable indicating if the counterparties in a transaction belong to the same parent firm. $Net\ Position$ is a dummy variable indicating if the buyer side in a transaction requires to buy additional carbon permits in a given year to offset its overall emissions in a year in column (2) and it indicates if the seller side has surplus carbon permits to sell in a given year in column (3). The standard errors are clustered at the $Transferring\ Country \times Acquiring\ Country$ level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dep. Variable:	$ln(Transaction\ Amount)$				
Net Position:		Deficit	Surplus	Deficit	Surplus
Internal	-0.161*	-0.281	-0.147	-0.146	0.311*
	(-1.73)	(-1.41)	(-0.81)	(-0.66)	(1.67)
Net Position		0.181	-0.294***	0.212	-0.274**
		(1.15)	(-3.05)	(1.34)	(-2.14)
$Internal \times Net\ Position$		0.533***	-0.217	0.717***	-0.741***
		(4.55)	(-1.64)	(4.20)	(-7.16)
$Internal \times POST$				-0.267	-0.734***
				(-1.42)	(-4.83)
$Net\ Position \times POST$				-0.145	0.053
				(-0.91)	(0.41)
$Internal \times Net\ Position \times POST$				-0.206	0.944***
				(-0.79)	(4.49)
Observations	1,543,011	121,752	128,686	121,752	128,686
Transferring Country FE	Yes	Yes	Yes	Yes	Yes
Acquiring Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Table 4.3: Effect of Internal Carbon Markets: Baseline

This table presents the baseline results showing the effects of internal carbon markets on the carbon emissions of firms. ICO_2M is a dummy variable indicating subsidiaries belonging to an internal carbon market firm. The dependent variable is total emissions scaled by revenue of a firm or a subsidiary of a ICO_2M–parent firm in a given year in columns (1) - (4) and it is the natural logarithm of emissions in column (5). $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. *Cash Flow* is defined as the total profit plus depreciation of a subsidiary in a given year. ROA is return on assets defined as revenue scaled by total assets of a subsidiary in a given year, *Log of Assets* is the the natural logarithm of total assets of a firm or the subsidiary of a ICO_2M–parent firm, *Allocation* is a dummy variable indicating subsidiaries that received greater than the median number of free allowances among the cross-section of firms in a given year. Regressions include subsidiary, parent firm, and year fixed effects depending on the specification. The standard errors are clustered at the subsidiary–level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. *t*-statistics are displayed in parenthesis.

Dep. Variable	*Emissions/Revenues*				$ln(Emissions)$
ICO_2M	0.243***				
	(2.58)				
$ICO_2M \times POST$	0.165**	0.271***	0.274***	0.262***	0.074*
	(2.40)	(5.27)	(5.50)	(4.47)	(1.89)
ROA			-0.235***	-0.286***	0.302***
			(-4.04)	(-3.98)	(8.60)
Log of Assets			-0.074**	-0.079*	0.145***
			(-1.97)	(-1.84)	(5.67)
Cash Flow			-0.777***	-0.742***	0.151
			(-5.01)	(-4.09)	(1.53)
Allocation			0.720***	0.619***	0.268***
			(14.08)	(11.37)	(7.17)
Observations	32,049	31,969	29,282	27,727	23,697
Subsidiary FEs	No	Yes	Yes	Yes	Yes
Parent Firm FEs	No	No	No	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes

Table 4.4: Robustness – Propensity Score Matched Diff-in-Diff

This table presents the results of estimating the difference-in-difference specifications of Table (4.3) using a size and industry based propensity score matched 3–nearest neighbors sample. The matching is done as of 2012. ICO_2M is a dummy variable indicating subsidiaries belonging to an internal carbon market firm. The dependent variable is total emissions scaled by revenue of a firm or a subsidiary of a ICO_2M–parent firm in a given year in columns (1) - (4) and it is the natural logarithm of emissions in column (5). $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. *Cash Flow* is defined as the total profit plus depreciation of a subsidiary in a given year. ROA is return on assets defined as revenue scaled by total assets of a subsidiary in a given year, *Log of Assets* is the the natural logarithm of total assets of a firm or the subsidiary of a ICO_2M–parent firm, *Allocation* is a dummy variable indicating subsidiaries that received greater than the median number of free allowances among the cross-section of firms in a given year. Regressions include subsidiary, parent firm, and year fixed effects depending on the specification. The standard errors are clustered at the subsidiary–level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dep. Variable			*Emissions/Revenues*		*ln(Emissions)*
ICO_2M	0.446***				
	(4.51)				
$ICO_2M \times POST$	0.028	0.124***	0.162***	0.165***	0.356***
	(0.51)	(2.60)	(3.44)	(3.16)	(3.36)
ROA			-0.340***	-0.412***	0.771***
			(-4.14)	(-4.07)	(6.40)
Log of Assets			-0.232**	-0.249**	1.024***
			(-2.56)	(-2.18)	(6.51)
Cash Flow			-0.576***	-0.508**	0.075
			(-3.08)	(-2.33)	(0.22)
Allocation			0.632***	0.537***	3.143***
			(11.56)	(9.42)	(19.46)
Observations	25,515	25,500	23,471	22,354	22,370
Subsidiary FEs	No	Yes	Yes	Yes	Yes
Parent Firm FEs	No	No	No	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes

Table 4.5: Financially Efficient Carbon Emissions

This table reports the result of analyzing whether emission intensities are associated with profitability for internal carbon market firms. The dependent variable is EBIT, EBITDA, and PBT in columns (1), (2), and (3) respectively. *Emissions Intensity* is defined as emissions scaled by revenue. ICO_2M is a dummy variable indicating subsidiaries belonging to an internal carbon market firm. $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. Regressions include all control variables including firm, parent–firm, and year fixed effects. The standard errors are clustered at the subsidiary–level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dep. Variable:	EBIT	EBITDA	PBT
Emissions Intensity	-0.002	-0.007**	-0.003
	(-0.45)	(-1.98)	(-0.51)
$ICO_2M \times$ *Emissions Intensity*	-0.011	0.001	-0.007
	(-1.40)	(0.19)	(-1.05)
ICO_2M × POST	0.014	0.009	0.021**
	(1.44)	(1.25)	(2.03)
Emissions Intensity $\times$ $POST$	0.003	0.002	0.004
	(1.04)	(0.80)	(1.30)
$ICO_2M \times$ *Emissions Intensity* $\times$ $POST$	-0.008**	-0.005	-0.010**
	(-2.17)	(-1.61)	(-2.46)
Observations	27,724	25,533	27,721
Controls	Yes	Yes	Yes
Subsidiary FEs	Yes	Yes	Yes
Parent Firm FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Table 4.6: Profit Shifting

This table reports the result of analyzing whether emission intensities are associated with profitability for internal carbon market firms if they are located in high tax jurisdictions within the EU. The regression is estimated on the sample of internal carbon market firms. The dependent variable is EBIT, EBITDA, and PBT in columns (1), (2), and (3) respectively. *Emissions Intensity* is defined as emissions scaled by revenue. $High_{Tax}$ is a dummy variable indicating subsidiaries belonging to an internal carbon market firm in countries that have greater than median corporate tax rate as of 2012. *POST* takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. Regressions include all control variables including firm, parent–firm, and year fixed effects. The standard errors are clustered at the subsidiary–level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. *t*-statistics are displayed in parenthesis.

Dep. Variable:	EBIT	EBITDA	PBT
Emissions Intensity	0.001	0.003	0.002
	(0.20)	(0.92)	(0.67)
$High_{Tax}$ × *Emissions Intensity*	-0.009*	-0.007	-0.007
	(-1.78)	(-1.45)	(-1.53)
Emissions Intensity × *POST*	-0.003**	-0.001	-0.004**
	(-2.22)	(-0.88)	(-2.35)
$High_{Tax}$ × *Emissions Intensity* × *POST*	0.003	0.002	0.004
	(1.30)	(0.89)	(1.39)
Observations	14,523	13,581	14,521
Controls	Yes	Yes	Yes
Subsidiary FEs	Yes	Yes	Yes
Parent Firm FEs	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 4.7: Diversity in Profitability and Emissions from ICO_2M Firms

This table reports the result of analyzing whether internal carbon market (ICO_M) firms having more variability in profits ex-ante (i.e., as of 2012) are becoming more carbon intensive during Phase III. $ICO2M_{Diverse}$ ($ICO2M_{NonDiverse}$) is a dummy variable indicating ICO_2Ms that are above (below) the median standard deviation in EBIT, EBITDA, PBT, and emissions intensity ($Emissions/Revenu$) across all the subsidiary of a parent firm as of 2012 in columns (1), (2), (3), and (4) respectively. $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. The dependent variable is emissions intensity ($Emissions/Revenue$)). Regressions include all control variables including subsidiary, parent–firm, and year fixed effects. The standard errors are clustered at the subsidiary–level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	ICO_2Ms *Sorted on* :			
	$EBIT$	$EBITDA$	PBT	$(Emissions/Revenue)$
$ICO_2M_{Diverse}\times$ POST	0.256***	0.233***	0.289***	0.258***
	(3.99)	(3.68)	(4.57)	(3.82)
$ICO_2M_{NonDiverse}\times$ POST	0.151**	0.197***	0.098*	0.163***
	(2.85)	(3.44)	(1.66)	(3.62)
Wald-test p-values:	0.08	0.57	0.00	0.11
Observations	27,727	27,727	27,727	27,727
Controls	Yes	Yes	Yes	Yes
Subsidiary FEs	Yes	Yes	Yes	Yes
Parent Firm FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

Table 4.8: Ex-Ante and Ex-Post Diversity in Profitability

This table investigates whether internal carbon markets (ICO_2Ms) that are initially diverse ($ICO_2M_{Diverse}$) in profitability ex-ante (i.e., as of 2012) also reduce their diversity during Phase III as compared to Phase II. The regression is estimated on a sample of parent firm–year panel operating an ICO_2M. $ICO_2M_{Diverse}$ is a dummy variable indicating ICO_2Ms that are above (below) the median standard deviation in EBIT as of 2012 in columns (1) and (2), in EBITDA in columns (3) and (4), and in PBT in columns (5) and (6). The dependent variable in columns (1) and (2) is the standard deviation of EBIT across the subsidiaries in an ICO_2M in a given year. Similarly, in columns (3) and (4) the dependent variable is the standard deviation in EBITDA, and in PBT in columns (5) and (6) in a particular year. $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. Regressions include parent–firm, HQ country of the parent–firm, and year fixed effects. The standard errors are clustered at the parent–firm level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

	Diversity in EBIT		Diversity in EBITDA		Diversity in PBT	
$ICO_2M_{Diverse}$	0.150***		0.133***		0.232***	
	(7.63)		(9.46)		(6.67)	
$ICO_2M_{Diverse} \times$ POST	-0.046**	-0.037*	-0.035***	-0.026*	-0.079**	-0.051
	(-2.44)	(-1.93)	(-2.59)	(-1.95)	(-2.20)	(-1.37)
Observations	3008	2901	2710	2608	3006	2899
Parent Firm FEs	N	Y	N	Y	N	Y
Country FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y

Table 4.9: Ex-Post Profitability in Ex-Ante Better Subsidiaries

This table investigates whether subsidiaries of an internal carbon market (ICO_2M) firm that are initially having greater than median EBIT, EBITDA, or PBT ex-ante (i.e., as of 2012) also reduce their profitability during Phase III as compared to Phase II. The regression is estimated on a sample of ICO_2M firms only. $High_{Profit}$ is a dummy variable indicating ICO_2M firms that are above the median in EBIT, EBITDA, and PBT as of 2012 in columns (1), (2), and (3) respectively. The dependent variable is EBIT, EBITDA, and PBT in a given year in columns (1), (2) and (3) respectively. In Panel B, $ICO_2M_{Diverse}$ is a dummy variable indicating ICO_2M firms that are above the median standard deviation in EBIT, EBITDA, and PBT across its subsidiaries as of 2012 in columns (1), (2), and (3) respectively. $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. Regressions include all control variables including parent–firm, subsidiary, and year fixed effects. The standard errors are clustered at the subsidiary level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dep. Variable:	EBIT	EBITDA	PBT
Panel A: Decrease in Profitability for Profitable Firms			
$High_{Profit}$ × POST	-0.028***	-0.016***	-0.026***
	(-5.32)	(-3.45)	(-4.74)
Observations	13064	12284	12833
Panel B: Decrease in Profitability for Profitable Firms			
$ICO_2M_{Diverse} \times POST$	0.016**	0.016**	0.023***
	(2.17)	(2.47)	(2.85)
$ICO_2M_{Diverse} \times High_{Profit}$	0.026***	0.021*	0.049***
	(1.91)	(1.70)	(2.92)
$High_{Profit} \times POST$	-0.012**	-0.002	-0.001
	(-2.18)	(-0.40)	(-0.24)
$ICO_2M_{Diverse} \times High_{Profit} \times POST$	-0.027***	0.024***	-0.040***
	(-2.78)	(-2.78)	(-3.89)
Observations	13064	12284	12833
For Both Panels			
Other Controls	Y	Y	Y
Subsidiary FEs	Y	Y	Y
Parent Firm FEs	Y	Y	Y
Year FEs	Y	Y	Y

Table 4.10: Evidence from Trading in Carbon Permits

This table provides evidence from trading in the European Union Emissions Trading System (EU ETS) that is consistent with the cross-subsidization in internal resource allocation to reduce diversity in divisional profitability in an internal carbon market (ICO_2M). The regression is estimated on a sample of all transaction in the EU ETS from 2008 to 2019. The dependent variable is the natural logarithm of the amount of carbon emissions being traded using carbon permits in a given transaction. $Internal$ is a dummy variable indicating if the counterparties in a transaction belong to the same parent firm. $Deficit$ is a dummy variable indicating if the buyer-side of a transaction is in deficit of carbon permits, i.e., it requires to buy additional carbon permits in a given year to offset its overall emissions in a year. $ICO_2M_{Diverse}$ is a dummy variable indicating ICO$_2$M firms that are above the median standard deviation in EBIT, EBITDA, and PBT across its subsidiaries as of 2012 in columns (1), (2), and (3) respectively. Low_{Profit} is a dummy variable indicating if the buyer-side of a transaction is a subsidiary in an ICO_2M that is below the the median in EBIT, EBITDA, and PBT across its subsidiaries as of 2012 in columns (1), (2), and (3) respectively. $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. The regression includes fixed effects for the transferring country, acquiring country, and the year of transaction. For brevity, the table only shows the main effects and the interaction effects that are significant in at least two of the three columns. The standard errors are clustered at the $Transferring\ Country \times Acquiring\ Country$ level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dep. Variable:	$ln(Transaction\ Amount)$		
Profitability Measure:	EBIT	EBITDA	PBT
$ICO_2M_{Diverse} \times POST$	0.596**	0.484	0.591**
	(1.98)	(1.52)	(2.19)
$Internal \times Deficit \times ICO_2M_{Diverse}$	0.722	0.885**	1.127***
	(1.56)	(2.10)	(3.33)
$Internal \times ICO_2M_{Diverse} \times Deficit \times POST$	-1.445**	-1.401***	-2.279***
	(-2.46)	(-3.00)	(-4.63)
$Internal \times ICO_2M_{Diverse} \times Low_{Profit} \times Deficit \times POST$	1.809***	2.164**	2.701**
	(2.59)	(2.26)	(2.42)
Transferring Country FE	Y	Y	Y
Acquiring Country FE	Y	Y	Y
Year FE	Y	Y	Y

Table 4.11: Alternative Explanation: Inefficiencies in Conglomerates

This table investigates whether results are driven purely by the monitoring difficulty of ICO_2M firms. ICO_2Ms that are diverse (either in EBIT, EBITDA, log of emissions, and emissions intensity in columns (1), (2), (3), and (4)) and are difficult to monitor as is indicated if all of the three indicators of monitoring difficulty as discussed in column (1) of Table (**??**), $ICO_2M_{Diff,Diverse}$. Similarly, ICO_2Ms that are diverse and are not difficult to monitor ($ICO_2M_{NoDiff,Diverse}$), ICO_2Ms that are not diverse and are difficult to monitor ($ICO_2M_{Diff,NonDiverse}$), and ICO_2Ms that are not diverse and are not difficult to monitor ($ICO_2M_{NoDiff,NonDiverse}$). $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. Regressions include all control variables including firm, parent–firm (GUO), and year fixed effects. The standard errors are clustered at the firm level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dep. Variable:	*Emissions/Revenue*			
$ICO_2M_{Diff,Diverse}$	0.400*	0.440**	0.431**	0.521**
	(1.87)	(1.99)	(2.01)	(2.23)
$ICO_2M_{Diff,Diverse}$ × POST	-0.047	-0.077	0.134	-0.061
	(-0.29)	(-0.49)	(0.86)	(-0.34)
$ICO_2M_{NoDiff,Diverse}$ × POST	0.330***	0.322***	0.343***	0.316***
	(4.62)	(4.37)	(4.79)	(4.24)
$ICO_2M_{Diff,NonDiverse}$	-0.064	-0.115	0.106	0.053
	(-0.46)	(-0.92)	(0.62)	(0.66)
$ICO_2M_{Diff,NonDiverse}$ × POST	0.097	0.166*	-0.107	0.123**
	(0.84)	(1.71)	(-0.66)	(2.08)
$ICO_2M_{NoDiff,NonDiverse}$ × POST	0.140**	0.181***	0.135**	0.221***
	(2.21)	(2.92)	(2.12)	(4.18)
Wald test p-values, row(3), row(5):	0.06	0.14	0.01	0.01
Observations	23,581	23,581	23,581	23,581
Controls	Yes	Yes	Yes	Yes
Subsidiary FEs	Yes	Yes	Yes	Yes
Parent Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 4.12: Can the Results be Biased by Carbon Leakage?

This table investigates whether results are driven purely by the monitoring difficulty of ICO_2M firms. ICO_2Ms that are diverse (either in EBIT, EBITDA, log of emissions, and emissions intensity in columns (1), (2), (3), and (4)) and are difficult to monitor as is indicated if all of the three indicators of monitoring difficulty as discussed in column (1) of Table (**??**), $ICO_2M_{Diff,Diverse}$. Similarly, ICO_2Ms that are diverse and are not difficult to monitor ($ICO_2M_{NoDiff,Diverse}$), ICO_2Ms that are not diverse and are difficult to monitor ($ICO_2M_{Diff,NonDiverse}$), and ICO_2Ms that are not diverse and are not difficult to monitor ($ICO_2M_{NoDiff,NonDiverse}$). $POST$ takes the value of 1 for the years 2013-2019 and zero for the years 2008-2012. Regressions include all control variables including firm, parent–firm (GUO), and year fixed effects. The standard errors are clustered at the firm level. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dep. Variable:	*Emissions/Revenue*			
ICO_2M	0.627***			
	(4.87)			
$ICO_2M \times POST$	0.149	0.203***	0.263***	0.265***
	(1.58)	(2.99)	(3.84)	(3.32)
$CLEAK$	0.534***	1.607***	1.516***	1.587***
	(4.33)	(13.21)	(12.26)	(11.67)
$ICO_2M \times CLEAK$	-0.972***	-1.059***	-1.169***	-1.194***
	(-5.34)	(-6.91)	(-7.59)	(-6.92)
$CLEAK \times POST$	-0.099	0.315***	0.189***	0.133*
	(-0.95)	(4.41)	(2.66)	(1.65)
$CLEAK \times ICO_2M \times POST$	0.098	-0.032	-0.049	-0.062
	(0.71)	(-0.35)	(-0.55)	(-0.62)
Observations	32,049	31,969	29,282	27,727
Controls	No	No	Yes	Yes
Subsidiary FEs	No	Yes	Yes	Yes
Parent Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

APPENDIX

A.4.1 Conceptual Framework

In order to formulate the hypotheses, I rely on a simple conceptual framework of internal resource reallocation within firms.

In my model I consider a continuum of firms of two types, multi-division firms and single-division firms in the same industry and producing the same product. The multi-division firms have two divisions and a headquarter (HQ). Firms operate in competitive input and output markets and are polluting, i.e., they emit carbon dioxide (CO_2) as a result of their production. They are regulated by a cap-and-trade system like the EU ETS. Hence, the divisions are initially allocated with a certain number of CO_2 allowances by the regulator for free. If they emit less, they can sell the excess allowances at the current market price, whereas, if the divisions emit more CO_2 than the initially allocated allowances, they have to compensate the shortfall by purchasing allowances from the other participants in the cap-and-trade system. The divisions of the multi-division firms are part of internal carbon markets (ICO_2M) where they can alternatively also buy and/or sell allowances from the other division within the same firm.

Each division is allocated K units of capital. Capital can be used for two purposes, for normal operational activities and for the abatement of CO_2 emissions. q is the fraction of the capital that is used for the abatement of CO_2 emissions. $c()$ is the abatement cost function. Following Heider and Inderst (2021), $c()$ is assumed to be quadratic such that, when capital qK is deployed for abatement, the cost of abatement becomes:

$$c(qK) = \frac{\alpha_c}{2} q^2 K^2, \tag{4.3}$$

where α_c is a constant in the above equation.

Following Maksimovic and Phillips (2002) and Matvos and Seru (2014), I assume that a division's profitability can be expressed as an outcome of a Leontief–style production function $zMin\{(1-q)K, \frac{C}{\alpha_a}\}$. C is the carbon footprint of production of each division and α_a coverts each unit of capital to its equivalent carbon emissions given the production technology. As the firms operate within the EU ETS, C is also equal to the allowances consumed by each division and is the constraining factor of production. Thus, we can write $C = \alpha_a(1-q)K$. The variable z is the profitability per unit of capital employed in normal operating activities. Thus, $z(1-q)K$ are the profits of a division from its operating activities. Each division receives the same free allocation of emission allowances (E) from the regulator. The utility of each divisional manager (U) is assumed to be increasing with the profitability of the division. Hence, the utility function of a divisional manager can be formulated as:

$$U = (1-q)Kz \; - \; c(qK) \; + \; (E - C)P_M, \tag{4.4}$$

where the market price for each allowance is P_M.

A.4.1.1 Characteristics of Single–Division Firms

The single-division firm has a manager who is also the CEO of the firm. Thus, the manager has complete control over the resources of the division. The manager can choose to invest a fraction $q_s < 1$ of the total capital K in abatement technology. The abatement cost for the firm can be written as $c(q_sK) = \frac{\alpha_c}{2} q_s^2 K^2$, where the total capital invested in abatement costs is q_sK.

A.4.1.2 Characteristics of Firms with an Internal Carbon Market

Of the two divisions of the multi-division firm, one is a "donor" division and the other is a "recipient" division. Each division is endowed with the same number of free allowances (E) by the regulator. The manager at the donor division has access to the abatement technology. Using the abatement technology, the donor divisional manager can save allowances from the initially allocated free allowances (by the regulator) so that it can cover part of the the shortfall of the recipient division. The manager at the recipient division doesn't have access to any abatement technology and hence, cannot implement any emissions reduction at the division.[10] The donor division is thus similar to the single-division firm except that, in this case, the donor division is controlled by a HQ, which is the ultimate decision maker for the allocation of emission allowances across its divisions. Importantly, the HQ also decides on the price at which this internal allocation of resources takes place. The donor divisional manager has the discretion to allocate capital between normal operating activities and emissions abatement activities given the transfer price set by the HQ.

I model agency issues within multi-division firms as the HQ's preferences for distorting transfer prices, i.e., the HQ undervalues cash flows from the donor division and overvalues cash flows from the recipient division (Matvos and Seru, 2014). In what follows I will demonstrate if and how the absence or the presence of price distortion predict emissions from firms that are part of an ICO_2M compared to the ones that are not.

A.4.1.3 Case I: Without Price Distortion

A.4.1.3.1 Single-Division Firms

Let the utility of the manager be U_s. The manager decides to put a fraction q_s of capital K into abatement. The emissions of the firm thus becomes C_s. If $C_s < E$, then the manager sells the allowances to the emissions trading market at the current price P_M per unit of allowance. Thus, the utility function of the manager can be expressed as:

$$U_s = (1 - q_s)Kz \; - \; c(q_sK) \; + \; (E - C_s)P_M. \tag{4.5}$$

The utility of the manager, U_s, is maximized for an optimum q_s (q_s^*) given by the solution of the first–order condition that We can derive by taking the derivative of equation (4.5)

[10] I make this assumption for simplicity. One can also assume that the bad division has worse abatement technology.

with respect to q_s (after substituting $C_s = \alpha_a(1 - q_s)K$. By doing so, gives us the following expression for the optimum q_s:

$$q_s^* = \frac{\alpha_a P_M \; - \; z}{\alpha_c K}. \tag{4.6}$$

A.4.1.3.2 Firms with an Internal Carbon Market

Let the utility of the divisional managers be U_D and U_R for the donor and the recipient (of excess carbon allowances) divisions, respectively. The utility of the HQ is represented by U_{HQ}.

Theoretically, there can be two sub-cases. One, when the HQ engages in internal transfers and, one when it does not. However, when the HQ does not engage in internal reallocation of resources, the (donor) divisional manager's decision on abatement would not be different from a single division firm, as the manager can then sell any excess allowances to the secondary market just like the manager of the single-division firm for the same price. Hence, I only consider the sub-case with internal transfer for divisions that belong to an ICO_2M.

A.4.1.3.3 With Internal Transfers

In the case when the HQ reallocates allowances across divisions, the HQ must decide on a transfer price to do so. Let this price be P_L. Given this price, the manager of the donor division decides to use a fraction q_I^w of the capital K for abatement and as a result, emits $C_{D,I}^w$ of carbon over the period. The utility of the manager, $U_{D,I}^w$ can then be expressed as:

$$U_{D,I}^w = (1 - q_I^w)Kz \; - \; c(q_I^w K) \; + \; (E - C_{D,I}^w)P_L. \tag{4.7}$$

For the recipient division, the HQ transfers $(E - C_{D,I}^w)$ from the donor division at the internal price, P_L. Hence, the utility of the manager, $U_{R,I}^w$ can then be expressed as:

$$U_{R,I}^w = Kz \; - \; (E - C_{D,I}^w)P_L \; - \; (C_R - E + C_{D,I}^w)P_M, \tag{4.8}$$

where, C_R is the carbon emissions of the recipient division.

The utility of the HQ, $U_{D,I}^w \; + \; U_{R,I}^w$, can be expressed as:

$$U_{HQ,I}^w = (2 \; - \; q_I^w)zK \; - \; c(q_I^w K) \; - (C_R - E + C_{D,I}^w)P_M \tag{4.9}$$

A.4.1.3.4 Solving through Backward Induction

Let q_I^{w*} be the fraction of capital K that the manager of the donor division optimally decides to allocate given the internal price P_L. Proceeding as before, we consider the first-order condition by differentiating $U_{D,I}^w$ with respect to q_I^w. This gives us q_I^{w*} as:

$$q_I^{w*} = \frac{P_L \alpha_a \; - \; z}{\alpha_c K}. \tag{4.10}$$

For an equilibrium to exist, at q_I^{w*}, the HQ should also be able to choose a P_L^* that would also maximize its utility. To check if this is the case, I substitute the expression of q_I^{w*} in equation (4.9) and differentiate the resulting expression with respect to P_L. The resulting first-order condition gives us the optimal P_L^* as:

$$P_L^* = P_M. \tag{4.11}$$

Substituting this P_L^* for P_L in equation (4.10) we get the optimum q_I^w as:

$$q_I^{w*} = \frac{\alpha_a P_M - \ z}{\alpha_c K}. \tag{4.12}$$

Comparing the expression of q_s^* from equation (4.6) and q_I^{w*} from equation (4.12), we observe that $q_I^{w*} = q_s^*$. Hence, based on this analysis, I formulate the following hypothesis: **H1:** *If there is no price distortion in the internal reallocation of carbon allowances, or if internal carbon markets are irrelevant, emissions will not differ between between internal carbon market firms and other firms operating in the EU ETS.*

A.4.1.4 Case II: With Price Distortion

Following Matvos and Seru (2014), I model price distortion by assuming that the HQ undervalues the cash flows from allowance trading of the donor division and overvalues the cash flows from the recipient division by an exogenously determined fraction $\theta(\in (0,1))$.

By design the manager of the single-division firm doesn't face the cost of price distortion. Hence, nothing changes for the single-division firm. As a result, the optimal fraction of capital that the firm employs can still be expressed as in equation (4.6) by q_s^*. For simplicity, I assume that the price distortion parameter θ captures both the relative undervaluation of the cash flows from the donor division and the overvaluation of the cash flows from the recipient division.

As in the case without price distortion, when the HQ is *not* reallocating allowances internally, even in the presence of price distortion, the donor division can sell its allowances to the secondary market at a price P_M. Hence, the utility function of the donor division can be expressed exactly as in equation (4.5) and the optimal fraction of capital that the donor division will employ can be represented by equation (4.6) as $\frac{P_M \alpha_a \ - \ z}{\alpha_c K}$. By design, this is also the first-best solution that maximizes the value of the utility of the HQ.[11] Hence, I formally consider only the case with internal transfer in the presence of price distortion.

A.4.1.4.1 With Internal Transfers

When the HQ reallocates allowances across divisions, the divisional manager of the donor (recipient) division is not able to sell (buy) the excess (shortfall) of allowances from the secondary market. Let the utility of the donor and the recipient divisions be U_{D2}^w and U_{R2}^w. The HQ now values the cash flows of the donor division as θU_{D2}^w. Hence, instead of realizing

[11]Even though the HQ does not reallocate allowances internally, it will still undervalue (overvalue) the cash flows

a utility of $U^w_{D2} + U^w_{R2}$, the HQ realizes a utility of $\theta U^w_{D2} + U^w_{R2}$. Given this utility, the HQ determines a new price P_{L2} for each unit of allowance that it reallocates in the internal market that would maximize its utility.

Given the price P_{L2}, the manager of the donor division allocates a fraction q^w_{II} of the capital K for abatement which results in $C^w_{D,II}$ of carbon emissions. utility function of the manager for the donor division can then be written as:

$$U^w_{D2} = (1 - q^w_{II})Kz \ - \ c(q^w_{II}K) \ + \ (E - C^w_{D,II})P_{L2} \ . \tag{4.13}$$

Similarly the utility for the manager of the recipient division can be expressed as:

$$U^w_{R2} = Kz \ - \ (E - C^w_{D,II})P_{L2} \ - \ (C_R - E + C^w_{D,II})P_M \ . \tag{4.14}$$

Finally, the utility of the HQ can be written as a function of the cash flows from the two divisions as:

$$U^w_{HQ2} = \theta U^w_{D2} + U^w_{R2} \ . \tag{4.15}$$

Solving through backward induction as before, first, we obtain the optimal q^w_{II}, $q^w_{II}*$, that would maximize the utility of the manager of the donor division as:

$$q^{w*}_{II} = \frac{\theta P_{L2}\alpha_a \ - \ z}{\alpha_c K} \ . \tag{4.16}$$

This should also be the the optimum q^w_{II} for which the HQ is able to maximize its own utility. Substituting $q^w_{II}*$ from equation (4.16) in equation (4.15) and differentiating the resulting equation with respect to P_{L2} and solving the first order condition, we get the expression of the optimal P_{L2} (P^*_{L2}) as:

$$P^*_{L2} = \frac{(1-\theta)z + \alpha_a P_M}{(2-\theta)\alpha_a} \ . \tag{4.17}$$

Plugging this value of P^*_{L2} in equation (4.16) for P_{L2}, we can rewrite the expression for $q^w_{II}*$ as:

$$q^{w*}_{II} = \frac{\theta \frac{(1-\theta)z+\alpha_a P_M}{(2-\theta)\alpha_a}\alpha_a \ - \ z}{\alpha_c K} . \tag{4.18}$$

Comparing equation (4.18) and equation (4.6), we can observe that q^*_s will be greater than $q^w_{II}*$ if $P_M > \frac{(1-\theta)z+\alpha_a P_M}{(2-\theta)\alpha_a}$.

To demonstrate this, I prove it by contradiction. I assume that $P_M < \frac{(1-\theta)z+\alpha_a P_M}{(2-\theta)\alpha_a}$. Simplifying this inequality leads to the condition that $\theta > 1$ which is contradictory to the assumption of price distortion. Hence, it cannot be that $P_M < \frac{(1-\theta)z+\alpha_a P_M}{(2-\theta)\alpha_a}$.

Thus, for any $\theta \in (0,1)$, it must be that $q_s^* > q_{II}^w*$. This leads to the second hypothesis of the paper:

H2: *In the presence of price distortion in internal carbon markets, emissions internal carbon market firms will be higher than other firms operating in the EU ETS.*

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